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ACCOUNTABILITY AND ACADEMIC FREEDOM
The Battle Concerning Research on Cold Fusion at Texas A&M University

by J. O'M. Bockris
Department of Chemistry, Texas A&M University
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CONTRIBUTIONS TO THE FIELD OF LOW TEMPERATURE NUCLEAR REACTIONS FROM GROUPS AT TEXAS A&M

Directly after the March 1989 TV announcement by Fleischmann and Pons that they had achieved a nuclear reaction at electrodes in the cold, research began on the phenomenon at Texas A&M. The University was picked by EPRI as a recipient of funds to investigate the field. Thus, it possessed a Thermodynamic Research Center, a Cyclotron group, and three groups in Electrochemistry (Chemistry Department). In addition, there was the Center for Electrochemical Systems and Hydrogen Research in the Texas Engineering Experiment Center, housed in the university. All these groups received funds to explore "cold fusion."

A quick start was made by my own group (in the Chemistry Department) partly due to my personal knowledge of Martin Fleischmann,[1] who readily told me on the telephone some aspects of the technique he and his collaborators had used.

The intense period of work at Texas A&M lasted about one year. Work in my own group continued until 1994. The following results from it have been published in refereed journals.

1. Multiple observations of the formation of tritium from deuterium (1): The tritium production turns on for several hours, then ceases. It can be started up again by means of an increase of the cathodic electrode potential.

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[1]Fleischmann was a graduate student in the late 1940's in the Department of Chemistry at Imperial College, London University, in which I, as an Assistant Lecturer, had a 12 person research group. He joined us in some of our research discussions, and much of our social life.

K. Wolf (2) in the Cyclotron Institute also reported tritium in high concentrations in his own independent experiments. Later, he claimed that this must have been due to tritium present as an impurity in the palladium.[2]

2. We observed significant amounts of excess heat in a few runs (3).

The excess heat was observed in experiments of Appleby and Srinivasan (4) in the TEES laboratories at Texas A&M.

3. In one run we observed heat and tritium together (we had not sought this relation in our other runs). The amount of tritium produced was about 0.1% of that necessary to explain the heat (5).

4. We found He^4 in the Pd lattice after prolonged electrolysis, the difficult analysis being done at North American Aircraft. The helium was about 100 times above background (6). (Melvin Miles subsequently found He^4 in the gas phase equal to around 1/4 that necessary to explain the heat.)

5. We carried out about 20 experiments on the detonation of a mixture of solids. We found between 10 and 300 ppm of noble metals, in particular gold, in several experiments. However, the results were not reproducible (7)

6. In work on damage inside Pd electrodes we found that the impurities deposited on the electrode surface matched those in solution but that new nuclei (of species not present in the solution) developed inside the electrode after it had been saturated with deuterium or hydrogen (8). We also found Fe produced from spectroscopically pure carbon rods, arced under water of O_2 were present (9).

All these results have been subsequently verified in many independent labs (the detonation experiments by only two other labs).

The work has importance in the history of the field in several ways. The discovery of tritium production at or in an electrode kept interest in the field alive during the first wave of reports that the heat claimed by Fleischmann and Pons could not be reproduced. Eventually, it gave rise to positive results in a government investigation on the anomalous effect of heat on radioactive decay (10)

The transmutation in the cold discoveries began a new field and showed that the Pd-Li-D_2O system giving tritium and helium was simply an example of a wider field. The organization of two international meetings on transmutation, the first in 1995, at Texas A&M University, and the second in a hotel in College Station, Texas, in 1996, showed that metal to metal transmutational results were being found world wide.

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[2]This explanation was disproved by Will and Cedynska (11) who examined a large number of Pd samples from various sources, finding no tritium in samples unexposed to electrolysis. They identified an error in Wolf's method of analysis of tritium.

These novel results, despite their publication in refereed journals, met a negative reaction from one group in the Chemistry Department at Texas A&M. Thus, the first (1995) meeting on Transmutation at Texas A&M (13) (agreed to by the Department Head) was interrupted by a Professor of Inorganic Chemistry who loudly declared the participants (~ 75 people) to be "all gooks". Application to hold a second meeting at this university in 1996 (14), accompanied by the distribution to the Committee of a 1995 a review of the field by E. Storms (485 references) was turned down on the grounds that to claim that nuclear reactions occurred in the cold must be a hoax or a fraud. Those who unanimously voted so were observed to have the review literally in their hands in front of them at the time of the vote.

FACTS RELEVANT TO EVALUATION OF THIS ARTICLE

1. I am a physical electrochemist and my intellectual background is similar to that of Martin Fleischmann. For example, I authored a 1993 textbook entitled *Surface Electrochemistry: A Molecular Level Approach*.

2. I knew Martin Fleischmann as a graduate student. Stan Pons I knew, too, from around the mid-80's. I evaluated him as a smart physical electrochemist, particularly able on the experimental side.

My opinion of Martin Fleischmann is the same as that of all physical chemists who know him: he is a brilliant contributor with an oft demonstrated flair for new ideas. He is a Fellow of the Royal Society.

3. I am skeptical of the permanence of theories in the Science. Some chemistry libraries go back to the 19th Century and it struck me early in my career that few theories hold for more than a generation. So, when it was said that the fusion of two D^+ ions was impossible except at 10^8°K because of the coulomb barrier, this theoretical deduction did not seem of much import to me. I immediately saw the difficulty solved by a hypothesis which would involve the formation of wandering neutrons. The only reliable thing are facts verified independently by others.

4. Historically, big discoveries have been made by following up experiments anomalous to the theory of the time. At a first class research university, the aim of the professors in the sciences, - as I see it, - should not be primarily to perfect the knowledge of the time by the publication of papers consistent with the known theory, but to carry out researches which have the aim of finding anomalies in the existing paradigm.

REACTIONS AT TEXAS A&M TO THE DISCOVERIES OF TRITIUM FORMATION

I had a smooth path for a few months after publication of the discovery of tritium formation in 1989 and was concerned with, e.g., the repeatability of the data and results in other laboratories, etc.

The first abnormal action was the visit to my lab of a journalist, Gary Taubes. He presented himself as an objective seeker after truth. He intended to describe the Texas A&M work in the magazine "Discovery". We seemed to have had a satisfactory interchange of questions and answers. I took Taubes at his word, was open and honest with him in respect to reproducibility, showed him graphs of unpublished lab results, etc.

Taubes returned again a few weeks later and this time he astounded us by suggesting that the anomalous tritium results were due not to error but to fraud! The graduate student who first observed them, - Nigel Packham, - had added tritiated water to his solutions to get the results. He had done this to hasten his Ph.D. My part was to either that of a fool, - not realizing, - or a knave, cooperating with the student. Taubes had flown to London to track the background of Packham and came back with the extraordinary statement that Packham had not been a graduate student there.[3]

I told the journalist he must talk to the student alone and ask to see his notebook.

Later, I discovered that in the one to one interview Taubes had threatened Packham with a publication in the next day's New York Times reporting that his discovery of tritium was a fraud. If Packham confessed the fraud at the interview, he could avoid the article and perhaps find a job in Albania before the book Taubes was writing about the work came to be published.

Nigel Packham told me that he had asked the journalist what else he could do but report the facts as he had found them. The experiment did not always work but <u>when</u> it worked the results were unmistakably strong.

Taubes then attempted to publish an article on Fraud in the Laboratory in *Nature*[4] but (after a full day's conference with the Editor) its lawyers ruled against the publication. About a year later, Taubes published a book called *Bad Science*. That part of it concerning the early tritium work was put over in a way which made one suspicious of its integrity, although in fact, there was no trouble about the facts, - except that what seemed to be the same electrode preparation produced bursts of tritium only sporadically.

One of the results of Taubes' visits to Texas A&M, however, involved the Dean of Science, a well known inorganic chemist. It was customary at that time for the various groups at Texas A&M working on cold nuclear reactions to have weekly meetings and at one of these, Kevin Wolf, a Professor of Nuclear Chemistry, who had been the recipient of the largest support from EPRI in Cold Fusion, suddenly announced that an article would appear in *Science* the following week which would be a detailed account of the alleged discovery of tritium, written by Taubes, stressing the irresponsibility of the A&M administration in allowing work on an impossible reaction to proceed, and hinting that fraud had been the basis of the apparent discovery of nuclear reactions in the cold.

Taubes' article featured Professor Wolf in a sidebar declaring our work as "sloppy". During this period, Wolf had shown me and my collaborators a friendly face, helping us with our technique

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[3]It proved easy to obtain documents proving Packham had studied for the Ph.D. at London University.

[4]At the time concerned (1990), the Editor of *Nature* (John Maddox) had taken a very negative stance to chemically assisted nuclear reactions. He spoke to me on the telephone of giving the field a "good thumping."

in the nuclear measurements, although not asking for any help in the parallel electrochemical experiments aimed at producing tritium which he had in progress. Part of Wolf's "friendliness" consisted in a covert removal from my laboratory of a tube of D_2O-LiOD from an earlier experiment which had been stored there for about 6 months. He analyzed it for light water and, finding some, claimed this to be proof of Taubes' hint that the graduate student had added tritiated water to get the results which led to our claim to have discovered nuclear reactions in the cold.

During these months of "friendly visits" to my laboratories, Wolf had also been reporting his build up of evidence for fraud to the EPRI program manager, without telling me, the Principal Investigator of the work, of his suspicion (we met at least once per week). However, the EPRI manager pointed out that in 6 months in the laboratory air, moisture could have leaked into test tubes containing the solution in D_2O. Corresponding experiments which we subsequently did by taking pure deuterium oxide and leaving it exposed to the lab air showed that, indeed, water from the atmosphere did enter the D_2O. Over two to three weeks, the amount corresponded to that which was required to explain Wolf's finding.

The most disturbing thing about this unpleasant period came when I discovered that, - before the publication of the article in *Science,* - the Dean of Science had received phone calls from the journalist and knew that an article hinting at fraud, was to appear in *Science* magazine. Although I saw this man frequently (he also showed me a friendly face), he told me nothing of the pending article. The Vice-Dean at this time was one of my colleagues in the Department of Chemistry and agreed, later, that he, too, had known of the preparation of the Taubes (Wolf) article.

The article in *Science* did indeed appear. The question was whether to sue the magazine for defamation.[5] I took advice from seven different authorities on this issue, including the man at the National Science Foundation who dealt with fraud in science, - and everyone's opinion, except that of a professor of law at Temple University who advised me to sue, - was that a suit would be impossibly expensive for me but of trivial financial concern to the publisher of Taubes' book, and that what was really at stake was my scientific reputation. The only thing would be to wait and see. Would other people be able to replicate the results?[6] If so, all would be well. If not, no suit would help.[7]

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[5]I asked the Editor of Science for equal space to reply to Taubes article but this request was refused. Dr. E. Storms, of the Los Alamos National Lab, independently of me, devised a critical experiment to distinguish the behavior of solutions spiked with tritiated water (as implied in the Taubes article) and those in which the tritium-containing species had arisen as dissolved gaseous DT from an electrode reaction. The Note included a graph of the two behaviors expected in respect to decay of activity with time. It clearly refuted the tilt in Taubes article. Science refused to publish it (sic).

[6]By mid-1994, when I had stopped counting, I had received from Hal Fox, Editor of *New Energy*, references to 143 papers in which the observation of the formation of tritium from D_2O in electrolysis was reported.

[7]This seemed good advice at the time. In retrospect, the monetary factor prevents a scientist in my position suing the press. However, Taubes' article in Science spread throughout the world. The hundred independent replications of our original tritium work were known only to the few hundred researchers in the field.

Nigel Packham, the graduate student, was subject to an unreasonable degree of stress. He had to contend with writing his thesis during the furor set off by the article. At the same time, his wife was near to giving birth to their first child.

Packham's thesis consisted of two sections. In one he reported his work on the bacteriological decomposition of water to form hydrogen and in the second, work on the formation of tritium by means of an electrode process, and anomalous heat.

Because of the suggestion of fraud created by Taubes, I had asked that Packham's Ph.D. Committee should have added to it two eminent chemists from outside the Department. One of them was Norman Hackerman, former President of the National Science Board, the other was Ernest Yeager, former president of the Electrochemical Society and an electrochemist of international repute. Both of these experienced men had independently observed the formation of tritium during electrolysis in other laboratories. and had no problem in believing Packham's results.

When it came to question time in the thesis defense, I gave Kevin Wolf, the most informed critic, eight minutes to question Packham publicly and then I suggested that that would be enough as there were many other hands up. Other people got their questions and answers given and then after discussion had continued for about 30 minutes, the youngest member of Packham's Ph.D. committee rose and presented Packham with a document of several pages which contained a large number of questions. He said: "answer those."

Expecting "trouble", the Graduate School had sent a representative to be present at the Oral. I (as Chairman) speedily went over to this man (who was sitting in the front row) and asked him what he thought we should do because it was not practical at this stage for Packham to answer the many questions which had now been posed. The Graduate School Representative ruled that Packham give his answers to the list in writing, later. I therefore told the audience of the decision and the normal questions and answers continued. Eventually, the questioning died down and the audience was dismissed.

The eminent gentlemen from outside and the two A&M professors on the Graduate Committee who were to examine the biological work, immediately voted to pass Packham, but the youngest committee member announced that he would not sign off on the thesis.

After about one hour's discussion it was agreed that he would sign if Packham would put answers to his questions at the back of the thesis in an appendix.

Some of the long discussion about the youngest members refused had involved an emotional tone, but at this point, all seemed resolved. The usual handshakes to the waiting successful student were given outside the examination hall. I invited the Dean, the Department Head of Chemistry, and Prof. Yeager to have dinner at my club. (Dr. Hackerman was to return to Houston in a waiting limousine.) It seemed to be a pleasant occasion redolent with success: the academic process had been tried and found true in very difficult circumstances.

Next day, the situation had undergone a radical change. The Head of the Chemistry Department now announced that after all, he would refuse to sign off on the thesis, i.e., would not allow the Ph.D. to be awarded! There then followed 2-3 days of turmoil and confusion, and it was finally worked out that he would only sign <u>if Packham would eliminate the cold fusion work</u> from his thesis. The thesis would consist only of the work on the bacterial decomposition of water to hydrogen (the two professor who represented the biological side agreed that this would be "just enough"), and the only mention of cold fusion would be an appendix containing reprints of the four publications in refereed journals about tritium and anomalous excess heat of which Packham was a co-author.

I am glad to say that ex-graduate student Packham was able to confound Taubes'+ prediction that he would have to move to Albania by getting an excellent position as Staff Scientist in the Lockheed Corporation with much work connected with life support for NASA. As of this writing, he has been thus employed for eight years.

TRANSMUTATION

Since 1989, I had had phone calls from a technician, Joseph Champion. He told me that he could switch on production "of a radioactive gas" in the palladium-D_2O system in minutes instead of the hundreds of hours which it took in our own experiments. Two of my post docs visited Champion and gave me a positive report, - he had encouraged them to operate his apparatus themselves alone and they had seen a fast switch on of anomalous heat effects. Some months later, Champion said that he had recruited research funding and he wanted to extend the deuterium to tritium work (and nuclear reaction in the cold) to nuclei of higher atomic weight. A gift of $200,000 was given by a William Telander to the Development Foundation at Texas A&M with the request that it be devoted to investigations directed by me.

Champion's idea (accompanied by much calculation) recorded in reports was that, if the nucleus had a quadrupole moment, it would be possible by the application of electric and magnetic fields of practical magnitude, to have the nucleus receive energy, and build it up to a value which would cause fission.[8]

Champion tried this idea at Texas A&M without success. However, he said he had done other experiments involving a detonation method and would like to illustrate that he could indeed thereby bring about transmutation of cheap heavy elements to noble metals.

The fund giver was eager that Champion's detonation method should be tried out by independent experiments under my direction. I asked two of my postdoctorals to work on this project at 50% time. A graduate student was occasionally involved.

Four experiments in succession gave productions of gold (up to 300 ppm) and smaller (1 to 10 ppm) productions of other noble metals from a mixture of cheap chemicals involving lead and mercury. After a pause of several months (during which time we were interested in β and (alleged) γ emission from the experiment), we tried to repeat the experiments, though without success.

The work was defunded in 1993 because the California SEC claimed that there were irregularities in the original funding (the Broker had promised to use funds given to him of around $10M in arbitrage investments in Swiss banks) but we were able to continue with the support of companies and found iron transmuted from carbon by an arc in water; and, later, new nuclei in Pd after lengthy H evolution thereon. The latter work was prior to the publication of similar findings by Miley (University of Illinois) and Mizuno (University of Hokkaido) although these later works (by

[8]Ideas published by Yan Kucherov in 1996, are qualitatively similar in principle to Champion's thought. Kucherov relies upon the phonon frequency of the hydride to overlap with the frequency of the quadrupole oscillations of certain nuclei (found in impurities in the Pd).

nuclear scientists) were carried out in a more thorough way, particularly in respect to isotopic abundance measurements.

Trouble surfaced towards November 1993 in a letter which an ex-employee of the Texas A&M Development Foundation published in a local newspaper. It opined that it was disgraceful that at a State University medieval alchemy was being practiced! This attack was launched six months after the detonation experiments had been finished.[9]

It was followed by an angry outburst in the local press, which (cf. The treatment of the tritium work) presented the work on Cold Transmutation as though it were a fraud. In December 1993, a Professor of Inorganic Chemistry organized the writing of a petition to have the Distinguished Professors ask that my Distinguished Professorship be removed from me "for this Cold Fusion caper." Most signed the petition, none asking if there were refereed publications (there were six at the time).

A rather intelligent reporter of the Dallas Morning News, Joseph Weiss, asked me for an interview to discuss the allegations made by the ex-employee. It was decided by a Texas A&M Vice President that I should indeed give the interview and I asked him to come see me on a Saturday morning so that we would have plenty of time. He stayed for six hours. To my surprise a Dean (it turned out he was the one who had made the decision to accept the Broker's gift) came and recorded the interview.

I gave an account of my tritium work and how I wanted to see if a nuclear reaction, - proved for deuterium, - could be brought about in the cold for other atoms.

The journalist published a two full pages spread article in the Dallas Morning News.[10] It implied that I had been seduced by the magnitude of the gift to carry out work which had, at the best, produced only small quantities of new material.[11] I then received a letter from the Vice President in Charge of Research which informed me that the Dean who had accepted the gift had now accused me of "misconduct in research." There would be an "investigation." Four Distinguished Professors, i.e., my peers, were named as those making up the relevant committee.

I hired a lawyer to advise me on what was virtually a trial. Someone in the university had come across the suspicion that, in fact, I was conspiring with the fund giver to <u>pretend</u> transmutation so that he could obtain money!

The result of this first investigation by Texas A&M was stark and unambiguous. The Committee voted for my "complete exoneration" on the charge. They seem to have done a remarkably thorough job, which included the use of voice enhancement technology to listen to a tape of the interview with the journalist. To my astonishment they had discovered a hand written draft of a letter which I had written in a hotel in New York warning the sponsor that he must in no way suggest that the work had a commercial significance. How the Committee got hold of my forgotten letter I do not know. I have evidence, however, that someone was copying correspondence in my office unbeknown to me: the letter had evidently been found there by the snooper, a case indeed of kicking the ball through the goal one is defending.

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[9]In experiments by Filimonov and Korbets, published in ICCF 7, the detonation method has been shown to change the isotopic abundance ratio of C^{137}.

[10]This is a newspaper, said by some at Texas A&M, sometimes to show a negative tilt to that university.

[11]Mr. Weiss clearly did not realize the importance of finding even small quantities of new nuclei if actually produced by detonation of atoms nearby in the Periodic Table.

Of course, the letter contributed to the decision of the Committee to vote for complete exoneration.

The result led to congratulations from members of the chemistry faculty, though it made other members unhappy and one of these, the Dean who had kept back from me the news of the oncoming attack in *Science*, has admitted in a letter to me that it was he who then gave rise to the much more prolonged examination of my actions which was carried out by a further (11 months long) investigation by an ad hoc committee of senior administrators which was convened in June, 1994 and did not complete its deliberations until May 1995.

A local newspaper came out immediately with the statement that the Committee had been convened to see (in spite of the exoneration) if it would yet be possible to remove me from the university because of my publications against known science were bringing odium and ridicule upon the university.

I was told the Committee met 11 times between March '94 and May '95. During this time, of course, my wife and I were under considerable strain. A letter written by me, and a more formal one written by my lawyer, both asking after the nature of the renewed inquiry, led to a refusal by the University Assistant General Counsel to define any. My offer to be subject to questioning by the Committee on all matters of which I had knowledge was ignored. The existence of the new Committee led once more to a feeling of isolation and unpopularity at the University and many people were not recognizing us anymore.

After more than 9 months of meetings, the ad hoc committee to reinvestigate me had me sent a chilling message. It was delivered verbally by the Vice President in Charge of Research, and said: "Tell Bockris he will not be the only one."

In exasperation, I returned to the lawyer who had helped me in the first investigation and we composed a detailed letter to the American Association of University Professors (AAUP), the essence of which was to present the evidence that the University was treating me in a capricious and unfair way. I asked for the University's treatment of me to be the subject of an AAUP investigation.

Universities fear the AAUP which can blackball them. Texas A&M had been the subject of such a blackball for its ill-treatment of a Professor at an earlier time. I do not know if this factor influenced the decisions of the ad hoc committee. At any rate two months later on May 25, 1995, I received a letter signed by the Provost informing me that the eleven month re-investigation had concluded that no action of mine had been contrary to the rules of the Policy and Procedures Manual of the University.

The statement given here of what was done by a University to a faculty member who published research results inconsistent with the existing paradigm of the time does not tell of the anguish involved; of the rejection by one's peers; of the effect upon family life; of the isolation and rejection.

My wife was a victim of the Nazi occupation of Austria and a refugee who reached America in a British liner convoyed by warships. She has told me that during the years she lived in Vienna under the Nazis, she never felt so rejected and threatened as in College Station, Texas, 1992-95.

A PARTIAL DEFENSE OF THE ACTIONS OF TEXAS A&M

Readers should know that this University is one with a history of training military officers. Six percent of the student body is still Officers in training. <u>Order</u> is the watch word. Crazy scientists who come up with results utterly in contradiction to well established Science are not wanted. It is what is in the book which counts.

It would distort the picture to imply that all my colleagues in the Chemistry Department behaved, as those who, e.g., secretly collaborated with Journalist Taubes, interrupted scientific conferences or attempted to secure demotion. The Head of the Cyclotron Institute, Joseph Natowitz, behaved consistently as scientists are pictured to do - with cool and balanced discussion. My department head, Dr. Michael Hall, treated me fairly, although evidently under pressure to do otherwise, as exemplified by his volte face in respect to Packham's Ph.D. degree.

Further, I must accept some blame for not taking time and trouble to face down the hostility of the group of colleagues in Inorganic Chemistry and present them with my results. This I did, in respect to tritium, for the physical chemistry and nuclear science group (I made them a two hour presentation, occasioning rancorous but fair discussion). I should have tried harder with the others, although I did send some of them relevant publications and essential reviews.[12]

ACADEMIC FREEDOM

At Texas A&M, I was made to feel that I could only work on "approved fields". (A list of approved fields for research in the Department was, indeed, later circulated to the faculty!). The results of publishing work which gave evidence that the present theory of nuclear change needs was two trials over three years; rejection and isolation. Towards the end of the period of investigation, I actually received a relatively friendly letter from a faculty member, imploring me, in future in my research, to "just go by the book."

PEER REVIEW

In my experience, peer review works fairly if the author is young and unknown. A well known author must belong to a clique of like-minded scientists who are friendly to him; and be in tune with his program managers, for then the latter will send his Proposals to his allies and they will give him evaluations which will lead to funding and coworkers, i.e., publications. Of course, he will reciprocate by good evaluation of his allies' proposals. An independent scientist, one who thinks new, stands no chance of support from peer reviewed grants. The reason for this is that scientists within their time have always thought that what they know is "the final truth". They do not understand the temporary nature of the theoretical construct. Hence, a man whose proposal does not simply add and support the theoretical constructs of the time will not be approved but, in fact, ridiculed and rejected. His funding will rapidly sink.

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[12]The difficulty of getting a change of paradigm is well illustrated by the occasion on which I had a long lasting (1 month!) Tritium production experiment. I invited four professors individually to come and witness the increasing tritium concentration. They all refused, thus, providing in 1993, a repetition of the type of occasion on which the Cardinals refused to look at the moon through Galileo's telescope.

He will not get funds to support coworkers to test his new ideas. If he writes up his ideas without the experimental base which the refusal to fund precludes, the papers will be rejected with contemptuous comments.

It is simple to think of ways to help this parlous situation. One could introduce procedures accompanied by a potential federal law which would make it illegal for the program manager and/or reviewers to accept or evaluate (respectively) a research proposal requiring government funds if they know the identity of the Principal Investigator making the Proposal.

DECISIONS ON RESEARCH FUNDING

Apart from Anonymity as a New Principle in the decision on funding, there should be a percent of all research funds which are reserved for out of the paradigm (i.e., really original) research proposals. Let the appropriate fraction not be debated here, - but it <u>is</u> vital to have something of this kind to preserve the liveliness of the system. Fundamental research can only be done if government funds are given. At present there is no mechanism[13] by which ideas which are regarded as impossible on the (always temporary) theories of the day can be funded.

HOW TO DECIDE THE VALIDITY OF "ANOMALOUS FINDINGS?"

At present the attitude towards paradigm-inconsistent findings is automatically to reject them, with anger, insisting that they are due to sloppy experiments or fraud. That is dangerous, for it may keep alive a horse which should be led out to pasture. Science is a changing, developing body. The key to progress is to find experimental anomalies to the present view and investigate them (but, evidently, not at Texas A&M University).

How to prove that anomalous findings are not indeed experimental trash? One has immediately to fund <u>two</u> independent investigations to find out! To obtain absolute independence one should keep the identity of the two groups hidden from each other and perhaps one group should be in another country (research costs in Russia (bloated with an excess of scientists) are a small fraction of those here).

HONESTY CONCERNING THE OBJECTIVITY OF SCIENTISTS

A comfortable illusion of the 20th Century, - held not by scientists themselves, but by the tax payers, - is that scientists are, somehow, above the fray and highly honest. What a lot of nonsense this is! Certainly, the penalties of outright falsification are <u>so</u> great that, in my 50 years experience of

[13]One can always go to the local billionaire. However, he is used to being petitioned and will send your request for evaluation to his lawyers. They will, in turn, submit it for comment to a Scientist in the field concerned. He will, of course, reject it ("Silly Nonsense") because it goes against the reigning paradigm.

university research, I never became conscious of a case. However, it must be understood that scientists in universities at any rate, are like business men. Both depend on income from clients and getting that is a complex business in which every legal device can be and is used. Scientists are as subjective as those in other professions. Such a realization really destroys peer review (often a process for downsizing of rival colleagues, made under the guise of feint praise). It demands a new protocol for decisions of monetary disbursement which is based upon the fact that emotion (largely negative) will influence the reviewer who sees a rival (anyone else in his field).

SUMMARY

(1) The author pioneered (or was co-author to publications of) a number of new results in nuclear chemistry, all of which have subsequently been replicated by others. He organized two pioneering international meetings on transmutation in the cold.

(2) The reaction of the Administration at Texas A&M was to subject him to an investigation for misconduct in research; and then (upon his complete exoneration by the first committee) to a second year-long series of meetings of an ad hoc committee set to investigate him further.

(3) The Press (local and national) brought odium and ridicule on his work. When it was finally officially recognized by the holding of sessions in Low Energy Nuclear Reactions in the National Conferences of the American Nuclear Society, no mention was made of the momentous event although the local press was made aware of the change.

(4) Reflections on these matters stress the subjectivity of decisions made in funding research. A tough Principle of Anonymity backed by the law, should be used to guard decisions on funding from subjective judgment. It is the new and different anomalous result which should be cosseted and encouraged to grow.

APPENDIX: COLD FUSION IN 1999

The field has continued to be studied world wide. About two hundred papers per year are published. Two books describe it. In the USA, one major nuclear authority, Prof. George Miley at the University of Illinois, has taken up and confirmed the work on transmutation in the cold, pioneered at Texas A&M. Thus, scientific progress is being made. Although no consensus in the theory has evolved, there seems much in favor of the creation of wandering neutrons from the discharge of protons onto electron rich portions of the surface. The use of thin films increases reproducibility.

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