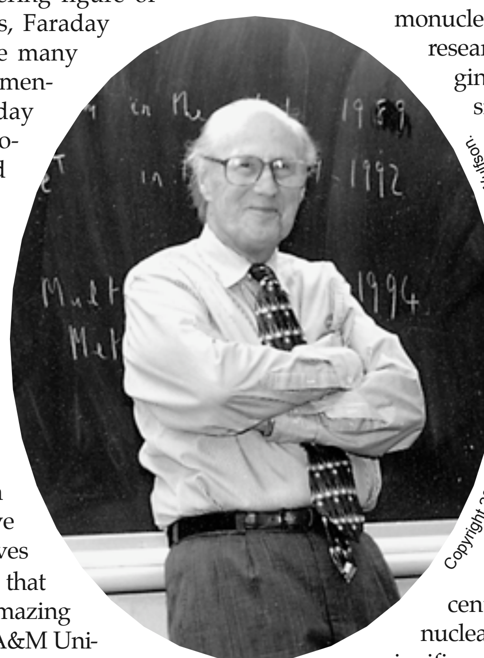
The Triumph of Alchemy: Professor John Bockris and the Transmutation Crisis at Texas A&M

by Eugene F. Mallove

Distinguished Professor of Chemistry at Texas A&M University, John O'M. Bockris, is one of the top two or three electrochemists of the twentieth century. He must be counted as a lineal intellectual descendant of one of the greatest scientists of all time, Michael Faraday, who was of humble birth but became a towering figure of nineteenth century science. Like Bockris, Faraday was raised in England and came to love many facets of science. In addition to his fundamental discoveries in electromagnetism, Faraday had much to do with the birth of electrochemistry. Among other things, he named the process called electrolysis, designated the anode and cathode, and coined the term electrolyte.



Copyright 2000, Texas A & M University. Photo by Jean Wulfson.

In a multifaceted scientific career from the 1940s through the present on three continents, Bockris and his student protégés pioneered many of the current directions in electrochemistry, and they confirmed several aspects of the Fleischmann-Pons cold fusion experiment.

Bockris was born January 5, 1923 in Johannesburg, South Africa—an extensive review of his lifetime of scientific work leaves no doubt about his prominence.[1] The story that follows is but a segment of that life, the amazing saga of John Bockris' experiences at Texas A&M University during a major paradigm shift in the history of science—one that may eventually match the impact of the Copernican revolution. Though that assertion may anger or amuse critics, history will be the final arbiter.

The Beginning: Cold Fusion

Although often overlooked in the glare and glitz of other branches of chemistry, such as biochemistry or fullerene (Buckyball) chemistry, electrochemistry is important to our industrial civilization. It turned out to be far more significant than anyone could have imagined prior to March 23, 1989. On that day, a scientific shock was felt around the world. It may lead to the end of civilization as we know it—the end of the age of fossil fuels and much else we could do without.

One of the students whom Bockris influenced at Imperial College in London when he was a Professor there (1945-1953) was Martin Fleischmann. Decades later, Fleischmann and Stanley Pons (who had been a student of Fleischmann's) announced at the University of Utah one of the most astonishing and bitterly contested discoveries in the history of science. Their finding, later verified by numerous laboratories around the world, became known as "cold fusion"—nuclear-scale excess energy in electrochemical cells that incorporated heavy water with palladium and platinum electrodes. Furthermore, because of the large energy release and absence of chemical "ash" to explain the reaction, the cold fusion discovery was immediately seen as a means to extract unlimited amounts of energy from abundant water. The scientific, technological, economic, geopolitical, and social implications were immense. It was as fundamental a discovery as the domestication of fire.

As a presumptive nuclear process, with originally poorly understood nuclear reaction pathways, cold fusion came into direct competition with the well-funded "hot fusion" establishment. Plasma physicists and engineers had been trying for decades to mimic the cores of stars, using controlled thermonuclear reactors at universities and government research establishments. They had had only marginal success. They had never achieved even a single watt of excess power out of their mammoth machines beyond the electrical power that was put in. In truth, they had little hope of achieving any practical working device before another half-century, if then, after billions of dollars more were spent. That they would not be the heroes who would rescue civilization from an energy crisis looming in the next century was too much for the hot fusioneers to accept. By contrast, though initially low power, some cold fusion experiments indicated high percentage excess power even in some very primitive initial forms, without the lethal neutron radiation of hot fusion.

Cold fusion was also an affront to almost a century of prevailing scientific wisdom that nuclear reactions could not possibly occur to any significant extent near ambient terrestrial temperatures. The highly positively charged nuclei of atoms were, it was said, unreachable by other positively charged nuclei, such as those of hydrogen and its isotopes. Above all, cold fusion was an assault on the current smug, self-assured high energy physicists—the veritable high priests of science who claimed to know almost everything about the fundamental laws of physics except a few remaining puzzles. They aimed for and wrote about a "Theory of Everything" that physics would finalize in their lifetimes. Just give them a few tens of billions of dollars more to build the giant Superconducting Supercollider (SSC)—ironically, under the plains of Texas, in the state where Bockris worked—and they would create this eternal edifice. That they could be utterly wrong about what some bench-top chemists, such as Bockris, Fleischmann, and Pons could have achieved, was unthinkable. The "Church of Science" had spoken. Cold fusion was branded "pathological science," "bad science," and "fraud."

In 1989, John Bockris and his students at Texas A&M University immediately fell into the whirlwind of activity surrounding the Fleischmann and Pons announcement. Hundreds, perhaps thousands, of scientists all over the globe struggled in the spring of 1989 to master the process—or prove it wrong. Bockris was to be rewarded with some of the earliest and most fundamental initial discoveries in cold fusion—in particular, that the radioactive form of hydrogen—tritium—could be produced in unexpected bursts within cold fusion cells. He and his colleagues later determined that helium-4 was produced in nuclear reactions in layers below the surface of palladium cathodes; others found helium-4 at the cathode surface. For his tritium discovery, in 1990 Bockris

Bockris was to be rewarded with some of the earliest and most fundamental initial discoveries in cold fusion—in particular, that the radioactive form of hydrogen—tritium—could be produced in unexpected bursts within cold fusion cells.

and his students were rewarded with a brutal press assault.

Science journalist Gary Taubes dragged Bockris through the mud, accusing him of being naive about fraud in the alleged spiking of cold fusion cells with tritium by one of his students. Taubes' attack was without foundation, but it was given wide currency by the main organs of the scientific media. It was repeated by other opponents of cold fusion, who were not bold enough to make the charge of fraud directly. Some of Bockris' colleagues at Texas A&M University participated in the witch hunt.

A few years later, the whole sordid affair against Bockris took a remarkable turn. The openness to new ideas about low-energy nuclear reactions (LENR) that the cold fusion discoveries had given to its investigators led Bockris onto even more heretical ground. Through a series of events with a cast of characters and circumstances that a Hollywood scriptwriter would be hard-pressed to conceive, Bockris began to test the claims of ancient alchemy—the transmutation of heavy metals, such as mercury, into gold and other elements.

Two amazing things happened, one predictable, one not. It was 100% predictable that when the larger world found out about the transmutation work at Texas A&M, along with the antics of the strange people who introduced Bockris to it, there would be an even more violent witch hunt against Bockris and his work. What had not been foreseen, however, was that some of those transmutation experiments apparently worked and became part of a growing body of experimental evidence that heavy-element transmutation at significant levels was possible in cold fusion and in cold fusion-related experiments. (For some time it was difficult to persuade even some "mainstream" cold fusion researchers that the evidence for heavy-element transmutation was in hand.) It soon became clear that radioactivity could be produced in what looked like mere chemical experiments. Radioactivity could be reduced or destroyed in similar experiments, and new stable elements and isotopes across a vast spectrum of atomic mass could be produced in heretofore exclusively chemical experiments. Had scientific alchemy risen from the grave—from its earlier death at the hands of twentieth century establishment science? Yes. John Bockris was one of the leading attendants at its rebirth.

What follows is the story of the scientific and personal courage of a great scientist under a most withering and unfair attack against the free experimental investigation of nature. The persecution of John Bockris at Texas A&M University hearkens back to the treatment of Galileo at the hands of the Catholic Church in the early seventeenth century. But this was near the end of the twentieth century, when scientists had supposedly assimilated those lessons of suppression and ridicule by inquisitors of old.

In some ways, the modern inquisitors were worse than their predecessors of the past: they had no excuse for their actions. They were not ignorant. They had seen and read about other historic paradigm shifts within science, but it seems they had forgotten the lessons, or they never really believed them. These modern-day anti-scientists with sheepskin certifications from academe were claiming that they were "objective." They said it was the purveyors of "pathological science," such as Bockris, Fleischmann, and Pons, who were polluting and betraying objectivity within science. By fiat of majority vote, they could say who was deemed a good scientist, who bad, what was possible to expect from nature's microcosm, and what was not. In short, this was tyranny within the house of science.

TAMU

Texas A&M (affectionately, "TAMU"), a large university of about 45,000 students, is located in the small town of College Station, which adjoins the old town of Bryan. It is well-known for its football team, "the Aggies," and its ROTC (Reserve Officer Training Corps) program.

The University has a great endowment, one of the largest in the country, from land donated to it in the nineteenth century, before it was known to contain oil. And, there are great plans for Texas A&M to become one of the ten leading universities in the country. Some departments indeed have world leaders in their fields. Though the official major emphasis is on agriculture and engineering, there is a Chemistry Department which ranks among the leading ten in the country, as measured by the activity of the graduate schools.

One of the features of Texas A&M, certainly exemplified by the Chemistry Department, is to appoint famous professors who are well on in their careers and who will lend instant distinction to their department. In 1997 there were seven Distinguished Professors in Chemistry out of a university-wide total of about twenty-eight active (*i.e.* non-retired) Distinguished Professors.

Bockris joined the Department of Chemistry at Texas A&M in 1978, a time when he had already authored 406 published technical papers and several noted books. He had begun his academic career at Imperial College of Science and Technology in London (1945-1953), then he was on the faculty of the University of Pennsylvania (1953-1972), and at Flinders University of South Australia (1972-1978). Between 1978 and 1992, when calls for his demotion or ouster began at Texas A&M, he had published some 250 papers at that university. For physical chemists, this is a very good record. Bockris had been very active in getting research grants. From 1979 until 1991, he ranked first or second in total research funds contributed each year to the Chemistry Department. Much of this money came from private sources, since the National Science Foundation (NSF) has no program in physical electrochemistry.

Tritium in the Cold

The Fleischmann and Pons announcement of cold fusion (actually the *re-discovery* of a primitive 1920s finding) happened on the afternoon of March 23, 1989. Bockris learned about it the next day, having missed the evening television reports. The young Martin Fleischmann had been a graduate student at the Imperial College of Science and Technology in London when Bockris started there as a lecturer in 1945. Fleischmann and his family, by the way, had escaped Czechoslovakia just before World War II. Since the field of top electrochemists in the world is fairly closely knit, it was easy for Bockris to call Fleischmann and ask him what was going on. Fleischmann told Bockris a few things about the way he and Pons prepared their electrolyte and the techniques they used to attain the unusual excess heat. Fleischmann considered the excess heat to be of nuclear origin, because of its high magnitude and lack of chemical explanation. But there were also associated—albeit initially weak—signs of nuclear activity, such as tritium and neutron production. This telephone input immediately triggered the cold fusion research in the Bockris group.

At the time Bockris was supported by a number of sources, especially the Electric Power Research Institute (EPRI). For a month or two, Bockris turned his whole group to trying to confirm or reject the Fleischmann and Pons claim. The temporary deviation from existing programs was encouraged by EPRI. The group sought to observe excess heat and tritium, and in these few weeks it manned a round-the-clock effort without formal contracts, which would have taken months to engage.

Because Texas A&M had a thermodynamics group, several electrochemical groups, and a strong nuclear science organization, it was an ideal university for EPRI's purpose. Bockris had encouraged EPRI to fund several groups at Texas A&M; indeed, EPRI funded three groups in Chemistry, one in Chemical Engineering, and one in the Center for Electrochemical Systems.

The first act of the unfolding drama was connected with a graduate student in Bockris' department, Nigel Packham. He and several others had been taking samples of the solutions of heavy water and lithium salts, which had been electrolyzed on palladium, to the Nuclear Engineering department. There tests were made on samples of electrolyte for the presence of tritium, the radioactive isotope of hydrogen, which has a decay half-life of 12.3 years. Bockris' group thought that it was important to look for this, because if the solution consisted of deuterium oxide (following the Fleischmann and Pons methodology) one of the most obvious pieces of evidence for nuclear activity would be tritium formation. Bockris realized that helium might also be produced, but its detection was beyond the capabilities of the group at the time.

One of the groups that was funded in parallel to Bockris' was led by Charles Martin, a professor in the electroanalytical chemistry division. His students were enthusiastic too and went to the same place to test their samples for tritium. Packham and others took numerous samples to Nuclear Engineering without any tritium being detected, but sometime in May 1989, Packham reported that the operating technician said, "What have you done with this one?" It contained a large concentration of tritium, in the 1,000 dps (disintegrations per second) range. The group had taken four samples from the solution at different times, and these results showed tritium climbing to an asymptote, *i.e.* the tritium production stopped after a few hours. The staff of Bockris' group agreed that someone had been present in the laboratory all the while that the tritium had been emerging. The research activity occurred during the day. However, it took about 400 hours of electrolysis for the electrode to begin to produce tritium. The Bockris team quickly put together a note for publication in the *Journal of Electroanalytical Chemistry*. It was returned twice for revisions but was finally accepted for publication. It was the first published account of tritium formation in a refereed journal and one of the first confirmations of the claims for a nuclear reaction "in the cold," made by Fleischmann and Pons.

Bockris' team announced that they had produced tritium at the meeting on cold fusion, held in Santa Fe, New Mexico under U.S. Department of Energy (DOE) auspices in May 1989, which encouraged everyone. The group continued to observe tritium sporadically for the next two years. The Bockris group devoted three reports to tritium formation. One of these contained a comparison of tritium and excess heat. It showed that the amount of tritium being produced was far too small to account for the heat by hypothesized nuclear reactions. In 1991, it was shown by Miles *et al.* of the U.S. Navy that helium was also being produced, but at a rate nearer to that needed to account for the production of the excess heat. This year, McKubre at SRI International and two groups in Japan *confirmed* helium production commensurate with excess heat production. (See Report on ICCF8, p. 25.)

The number of experiments at the Bockris lab devoted to investigating tritium production was 58 and the total number of times tritium was observed was 18, with 40 failures. In retrospect, Bockris wondered, had they left the other 40 cells to run more than 500 hours each, might they all have produced tritium eventually?

The Scandal Monger

Enter science journalist Gary Taubes, who had received a contract early on with Random House to write a book about cold fusion. He already privately believed cold fusion claims were "pathological science" and perhaps even fraud. Taubes visited the Bockris group where Nigel Packham was doing the research. Bockris initially thought that the tall, imposing Taubes was a genuine seeker of the truth, so he let him see everything the group had, including notebooks. Bockris discussed with Taubes the various pluses and minuses of the work in a spirit of openness. In the beginning, Taubes behaved normally, jotting notes and tape-recording the conversations with Bockris and others on his staff.

Bockris would later learn that Taubes had visited Texas A&M a second time without seeing him. Moreover, Taubes had also gone to London, England to investigate what Bockris had told him about the family background of Nigel Packham. He had interviewed Packham's parents and developed what turned out to be an utterly misguided theory of sinister motivations that might have impelled Packham to engage in scientific fraud—spiking the experimental cells with tritium.

A curious connection: Confirmation of this delusion of Taubes came to me in March 1990 at the First International Conference on Cold Fusion (ICCF1) in Salt Lake City. Taubes excitedly told me that he had developed an extensive psychological profile of Packham, which pointed to him being a fraud perpetrator within the Bockris lab. The alleged profile seemed to me then to have no bearing on the integrity of Packham, whom I would later meet and come to respect. Taubes' purpose was to get me to ask Bockris during a technical session at the meeting an embarrassing question about possible fraud in his lab.

Taubes thought that he could establish that Packham had never been a graduate student at Imperial College. When he learned about this preposterous notion, Bockris immediately obtained by fax from Imperial College Packham's registration papers for the graduate program in the School of Electrical Engineering.

Taubes made a third visit to Texas A&M in which he adopted a different public attitude. Now he was extremely aggressive, telling Bockris that the tritium results had been falsified by Packham. He said that other workers, particularly those in Charles Martin's group, had not been able to observe tritium. He suggested that Packham had falsified his results because he wanted to impress Bockris and get his Ph.D. more quickly.

Bockris remained calm under this attack. Taubes suggested

In some ways, the modern inquisitors were worse than their predecessors of the past: they had no excuse for their actions. They were not ignorant. They had seen and read about other historic paradigm shifts within science, but it seems they had forgotten the lessons, or they never really believed them. These modern-day anti-scientists with sheepskin certifications from academe were claiming that they were "objective."

to Bockris that he had played some part in the alleged fraud, because he had hoped for increased funding, which surely would be the result of the acceptance of such a remarkable claim. Bockris showed Taubes the record of all his research grants (eleven at the time), which proved that he had plenty of research funding. Bockris somewhat naively suggested to Taubes that he should talk directly with Nigel Packham, and he would doubtless be able to see the actual documented progress of the research in the lab notebooks. Taubes did just that, but afterwards, Nigel Packham strode into Bockris' office and exclaimed, "This man wants blood!"

Taubes had threatened Packham after he had talked to him for some time. He had told him that he should "confess" to having put the tritium in the solution from a supply of tritiated water that was in the lab. Packham told Bockris of the Taubes threat: If he (Packham) were to confess right then in a tape-recorded interview, Taubes would not publish anything about it until he wrote a book demolishing the "myth of cold fusion."[2] Packham would then have six to nine months to find a job. On the other hand, if Packham was *not* willing to "confess" right then and there, Taubes would quickly publish an article in *The New York Times*, where he had connections, that fraud was being committed in the Bockris laboratory by Nigel Packham. Packham's career would be ruined and with it some of the most important emerging scientific evidence for cold fusion.

The Taubes threat was extremely serious, especially since the issue of scientific misconduct was becoming a rather fashionable topic at the time in academia and in science journalism. It seemed that Taubes would stop at nothing to boost the prospects for his eventual book. Yet despite Taubes' attitude, Bockris invited him to lunch at his Club. There Taubes talked about his life, his exploits writing exposés for *Discover* magazine of less-than-angelic scientists, and his activity in writing scripts for Hollywood movies. Taubes had written a book, *Nobel Dreams*, in which he had attacked the reputation of famous Nobel laureate Carlo Rubia, a leader at CERN, the high-profile European center for nuclear research. (Rubia, it was rumored among science journalists, came close to suing Taubes.) Taubes swiftly departed the meeting with Bockris at TAMU, allegedly to get his article into *The New York Times* or elsewhere.

Despite the emerging threat of a scandal, Bockris recalls that his attitude at the time was rather casual. He knew what the group had done and there seemed to be little that Taubes could do to substantiate a non-existent fraud. But it was clear that Taubes was determined to prove his case in the press with circumstantial evidence; the false accusations would be damaging.

No article appeared the next day in the *Times*, but a short time later Bockris received a call from London from the powerful editor of *Nature* magazine, John Maddox. In his cultured English accent, he told Bockris quietly that a paper had been received by *Nature*, which claimed that fraud was being perpetrated in the Bockris lab. Maddox wanted a comment from Bockris himself. In the same reserved British tones, Bockris replied to Maddox that there was certainly no fraud. The work referred to was being carried out by an English graduate student from Imperial College, Nigel Packham, and other students and postdocs. Bockris told Maddox that although his group had been the first to publish a paper on tritium production in 1989, there were now several other independent groups that had found the same general result, notably Srinivasan's team at the Bhabha Atomic Research Centre in India (BARC). Bockris asked Maddox to forward the article for comment. Maddox agreed to fax it the next morning.

It was not pleasant that evening for Bockris. He went home to his charming wife Lilli (an Austrian-born Jewish woman who had escaped the Holocaust), knowing that he and his students had been accused of fraud at the most famous scientific publication in the world. It was not a restful night. When he returned to his office the next morning, he expected to find the article sprawling out of the fax machine, but nothing had come through. He waited until 4:00 p.m. London time and called Maddox to learn what was happening. Maddox's secretary said that he was in conference with lawyers and could not be disturbed.

Bockris called back an hour later and was told, "Dr. Maddox will call you soon." Finally, Bockris did get a call from Maddox, whose attitude had changed markedly. Bockris recalls the situation was like that of a "pricked balloon." Maddox spoke to Bockris in a tone of resignation: "We have put the article on the back burner." For what reason? Apparently *Nature's* lawyers had raised objections to its publication, as, indeed, they should have. (The accusations of fraud finally did come out in *Nature's* competitor magazine, the U.S.-based *Science*.) *Nature's* decision to reject the Taubes story had come as an immense relief to Bockris.

At the end of this second telephone discussion between Maddox and Bockris, Maddox asked hesitantly, "You say there are others who find tritium?" Bockris replied affirmatively, mentioned four groups that had detected tritium in cold fusion experiments by then, and sent Maddox the references and a report. Unfortunately, Maddox failed to investigate the matter of tritium production or any other discoveries within the emerging low-energy nuclear reactions field. *Nature* continued its attacks on cold fusion science and scientists, and to this day holds an anti-scientific position on cold fusion. Ironically, Maddox would later write a popular book titled, *What Remains to be Discovered* (1998). No mention of cold fusion in that book, of course!

Shock Treatment

Then came a great shock. At that time, the groups working on cold fusion at Texas A&M (one in Chemical Engineering, associated with the Thermodynamics Research Center; two groups in the Chemistry Department; a group in the Center for Electrochemical Studies in Hydrogen; and a group in the Nuclear Science Division) met once every two weeks to compare results. At one of these conferences, Professor Kevin Wolf made a startling announcement that would throw the whole matter of tritium detection at Texas A&M into disarray.

First, some essential background about Wolf and his association with the Bockris group. The late Kevin Wolf (who died unexpectedly in 1997, at age 55, see Obituary, *IE*, No. 18, p. 42) had been a well-known, well-respected nuclear chemist. Today, Wolf in many ways is still highly regarded within the cold fusion field, despite what many interpret as his unfortunate actions in 1990 and beyond. Wolf had received plenty of research support from the Department of Energy and other sources. He had been chosen by EPRI to be the recipient of the greatest amount of money that that prestigious organization was directing into Texas A&M for cold fusion research.

Kevin Wolf worked with Bockris' group, which had no nuclear chemist on staff. (Tom Schneider at EPRI had given the Bockris group $27,000 to buy a scintillation counter to measure tritium, in addition to the EPRI money already granted.) When the Bockris team began to observe tritium, the group needed someone who knew nuclear chemistry and tritium measurement. In the early days of cold fusion people suspected that the tritium was "coming from somewhere else." Tritium, created "in the cold" was regarded as impossible—yet it was being observed to form in cold fusion cells, without the high-energy (14 MeV) neutrons expected if it were being created even at low-level in an energetic plasma. The positive results for "cold tritium" were so unexpected that scientists wondered whether all sources of contamination by preexisting tritium had been checked. Tritium was being used in the Chemistry Department in other ways. It was not out of the question that there could be some tritium coming through in the ventilation system.

Kevin Wolf, knowledgeable in relevant tritium measurement techniques, worked with Nigel Packham and Jeff Wass, another graduate student who was associated with the very early work on cold fusion in the Bockris laboratory. One of Wolf's jobs was to thoroughly examine the lab in which the work was being done. For example, curtains and hangings, floor coverings, and many other items were tested. Wolf seemed to Bockris to be very helpful. Wolf would walk down the Chemistry Department corridor every day (to collect his mail, he said). He often talked with Nigel Packham and Jeff Wass and knew their work intimately.


News & Comment

Cold Fusion Conundrum at Texas A&M

The administration's laissez faire response to worries about possible fraud raises questions about the proper balance between academic freedom and the need to guarantee the integrity of research

Tritium producers. *Fusion cells in the Bockris lab.*

Science, **248**, June 15, 1990

All this is by way of background to the stunning announcement which Wolf made in one of the joint cold fusion group meetings. He blurted out that an article was to be published in *Science* in about two weeks (mid-June 1990), which would concern the Bockris group's work on tritium! He said it would be a lengthy article written by Gary Taubes, presenting the conclusions of what Taubes had learned. The main point of the article would be to castigate the administration of Texas A&M for allowing possibly fraudulent and unbelievable results of tritium production.

Wolf's announcement was a shock for two reasons. First, Wolf had to some extent collaborated with a muck-raking journalist, who would bring charges of fraud upon the colleagues whom he visited every day, without alerting them about Taubes' intentions. Perhaps this was the reason for the many unexplained visits by Wolf to the Bockris group

Second, Wolf *himself* had detected tritium in high concentrations in cold fusion experiments. Only months before (March 1990), he had presented a paper at the First International Conference on Cold Fusion (Salt Lake City) in which he claimed to have produced tritium.

At the very time that Wolf announced the forthcoming article in *Science* magazine by Taubes denouncing the Bockris group and the whole College of Science, Wolf sent Bockris copies of letters that he had apparently been secretly writing to Dr. David Worledge at EPRI, the program manager in charge of the Bockris work. He had alleged to Worledge that the Bockris group tritium work must be tainted by fraud. Wolf's tortured reasoning in these letters further revealed his apparent duplicity.

Being familiar with the Bockris laboratory on his daily visits, Wolf had no difficulty in surreptitiously removing a test tube of a solution in which the group had found tritium. Analyzing this in his own lab, Wolf had found some light water in the deuterium oxide (heavy water) solution. This seemed to him to support the idea that Nigel Packham might have put into the solution a significant amount of tritiated water. Wolf also revealed that he had been secretly writing to Dean John Fackler too (see Exhibit B p. 23 - excerpts from an interview with Prof. Fackler), telling him that the Bockris work might be fraudulent, because of the tritiated water which Packham allegedly had added to the solution.

The situation grew more serious by the day. Now the Bockris group was attacked publicly by the most influential magazine of science in the United States, *Science*.[3] It was strongly implied that the group's tritium results had been fabricated.This was being played out in front of the Dean of Science, and would go further up the University administration, because of the high-profile publication in *Science*.

Because Kevin Wolf announced the Taubes *Science* article just days before its publication, Bockris had no chance to do anything about it. There was barely time to write a letter to *Science*, with information that might have prevented the publication, or at least might have significantly modified the piece. Bockris went to see Dean Fackler about the matter. To his amazement, he learned that Fackler had known about the impending article for some weeks, and that Taubes had been talking to him by phone too! The Vice Dean, Abe Clearfield, also knew of the upcoming article. Although Clearfield was a colleague of Bockris in the Department of Chemistry, and Bockris knew John Fackler collegially, neither of them had informed Bockris. When the article finally appeared, it was a long, five-page feature. Length aside, it can be summarized easily: Absurd research was being carried out in the Chemistry Department, which should never have been continued in the first place after the "collapse" of cold fusion, and the work might even be tainted by fraud. The University administration was at fault in allowing this and should have imposed rigorous supervision, if it was going to allow the work to proceed at all.

The *Science* article was careful in that it did not actually say the Bockris group work *was* fraudulent, but much was said to hint in that direction. Prof. Charles Martin was quoted: "I warned Hall [the department head] that I thought there was a very good chance the experimental results were the result of fraud." There was a framed inset "box" in which Kevin Wolf opined that the Bockris work had been sloppy and poorly carried out. (Wolf suggested that his own tritium results might have been from precontaminated palladium, a suggestion that was later shown to be without foundation. In other published papers, researchers looked for tritium contamination in many different samples of palladium and found none. One of these papers was authored by Fritz Will, a former president of the Electrochemical Society.) The article was extremely damaging, and its negative implications for Bockris and everyone else working in cold fusion were all too clear. Though the U.S. DOE ERAB Cold Fusion Panel had rendered a rush to judgement against cold fusion in the fall of 1989, continued reports of tritium production, low-level neutron emission, and nuclear-scale excess heat continued to come from laboratories around the world. The Texas A&M work on tritium was one of the most important results, which helped sustain interest in the field during its formative stages. Taubes and his supporters in academe knew that very well, which is why they were anxious to cast doubt on the work.

were anxious to cast doubt on the work.

Bockris' first reaction was to consider legal action. He thought it might be possible to sue *Science* magazine for defamation. He listened to the advice of seven associates and friends. Only one advised that he sue. On the other hand, this dissenter was a law professor at Temple University in Philadelphia, who thought Bockris could easily win a libel suit or receive a settlement. Those who advised against legal action realized that while *Science et al.* could afford a $1 million or more to defend against a lawsuit, Bockris would be strained to pay even one-hundred thousand dollars—perhaps the minimum needed to challenge such powerful media forces.

Bockris chose instead to prepare a reply to the accusations in *Science*, with all the science he could bring to bear—picking apart the statements made by Taubes one-by-one. Bockris called the then editor of *Science* and told him that the article had been false in its implications. The editor was rather cold and said that he was "sorry." Bockris then wrote *Science* to ask if he could reply on so important a matter of science and ethics, with the same space that Taubes had been given. *Science* rejected the suggestion outright, claiming that a detailed reply would not be accepted for publication. Bockris recalls the surprising reason given, roughly this: "The public is interested in fraud, but they are less interested in normal science." Eventually, Bockris was allowed to publish a one-column letter in which he stated the plain facts of the discoveries and denied that there was anything experimentally wrong or unethical.

It was too early for Bockris to put up what would become a major defense, namely numerous replications by others. True, there were already a few confirming papers on tritium when the *Science* article came out, but Bockris had to wait until 1994 for some 147 papers to be published in support of tritium production in the cold. By then, Bockris had stopped counting the papers that claimed successful tritium production.

Several of the people from whom Bockris had sought advice said that his main concern should be his scientific reputation, which would depend on replication of tritium production, suit or no suit. But an accusation of fraud made at a high level does not readily disappear, scientific reports notwithstanding. By the time the huge number of independent confirmations of tritium came in, it was too late. Many had concluded that one or more people in Bockris' group had committed fraud. Cleaning off the mud proved very difficult.

Dr. Edmund Storms of Los Alamos National Laboratory, who could defend his own successful results in tritium production in cold fusion experiments there at the world's foremost tritium measuring laboratory, watched Bockris' ordeal from afar. Independent of Bockris, Storms devised a test which could determine whether or not the Bockris group results could have been due to spiking with tritiated water. If the tritium had been put in by Packham, it would be present in an ionic or molecular form and would remain there independent of time, emitting its tell-tale beta particle radiation. If the results were produced as gaseous DT at the electrode, the absorbed gas would be sparged out by the constantly bubbled D_2—the tritium concentration would decline with time.

Storms prepared a graph, based on an experiment, in which he showed the two different behaviors. Storms wrote a letter to *Science* in which he commented that time-history of the Bockris group results constituted clear proof that they were not due to tritium spiking. Tritium activity (radiation counts measured) did decrease with time, if D_2 continued to bubble through the solution during electrolysis. The tritium found in the solution had been formed on the electrode as a gas, partly dissolved in the solution and partly rising into the gas phase. *Science* magazine refused to publish Storms' brief article. Faced with proof that no fraud had been committed, *Science* preferred discrete silence and a cover-up rather than admitting that its story was flawed. Perhaps *Science* will someday atone for this ethical travesty of 1990.

Faced with proof that no fraud had been committed, *Science* preferred discrete silence and a cover-up rather than admitting that its story was flawed.

By 1992, Bockris' group had observed tritium many times. Bockris had been working with scientist C.C. Chien from Seoul, South Korea who had himself observed tritium, independently of Texas A&M work, *before* he came to work with Bockris. Working with Chien led on one occasion to a very remarkable electrode, which continued to emit tritium for several weeks. After it had emitted tritium for ten days, Bockris thought that it was reliable enough so that he could call neutral or skeptical colleagues to see the process for themselves. The rate of increase of tritium in the solution was such that one could make two measurements an hour apart and detect a significant increase in the tritium concentration. Bockris planned to tell colleagues in the nuclear science division, "Come and see for yourself. Do a test yourself!" The scintillation counter was in the adjacent room, so Bockris surmised that a colleague could stay with the apparatus for an hour, taking two samples. This would prove that no one was adding tritium artificially during the time of increase.

He phoned the Director of the Nuclear Science Division, who said that he was just about to go to Germany to carry out some research there. He could not come to see the tritium. Bockris phoned another person in the Chemistry Department, who was concerned with trace analysis and part of whose work was nuclear-related. This person said that it was his son's birthday and he could not come. Bockris tried two other colleagues, each of whom had an excuse not to come. Bockris realized that no one was interested in seeing the anomalous result.

Bockris recalled the eerie similarity between what was happening at Texas A&M on the matter of tritium—no one would come and look—and what is said to have happened in the early seventeenth century with Galileo and his telescope. The Church was then very much in control; its view was that the Moon was "queen of heaven and perfect," therefore, no need to look through a telescope to confirm what it already knew to be true. It did not want to be told that the lunar orb had imperfections. When informed by Galileo that much structure was evident on the Moon, the clerics turned away. They refused to look through the telescope. In four hundred years, the dynamics of intense paradigm shifts in science had not changed much.

Deeper Heresy

The work on tritium continued through that of Chien, Bockris, *et al.*, which was published in 1992.[4] In 1991 Bockris received a strange telephone call from a technician, who introduced himself as Joe Champion. This fateful call would lead to a new era of controversy, but also new discoveries at Texas A&M. Champion said that he had read about the Bockris work on tritium and wanted to demonstrate that he could initiate an excess heat reaction more quickly than the hundreds of hours which had previously been required. Champion claimed that he worked on the campus of a University in Tennessee, where he had a trailer containing his equipment. Champion promised that if Bockris could send someone to see his experiment, they would be convinced.

At that time Bockris had a very intelligent and able postdoc, Dr. Ramesh Kainthla. Also, Omo Velev, who had been working toward his Ph.D. in Sophia, Bulgaria, had come to work with Bockris. Bockris asked them to go together to see Champion's work in Tennessee. They returned to report that he had shown them the apparatus, given some instructions, and let them find out for themselves what it could do. They thought they had seen excess heat from the device within an hour of switching it on, so what he had promised appeared at first glance true, albeit some 30% less excess heat than he had promised. This seemed impressive, although the group was not told how he did it. Later, in March 1992, Champion called again to say that he had now found support money and would like to disclose more about his work.

When Champion first came to Texas A&M, he had an unusual appearance, Bockris recalls. He was tall and heavy-set, looking more like a football player than a scientist, yet he was very shy and diffident. He spoke with a slight stammer and told Bockris that he had been working for two or three years on the processes that he was to recount, but that he needed independent verification. He went on to describe the work, which was nothing less than heavy element transmutation at low energy—in effect, alchemy. Yet claims of alchemy, old or modern, bore some relation to the ongoing heresy of cold fusion, the production of helium from hydrogen under mild conditions. Bear in mind that by 1991, Dr. Melvin Miles of the U.S. Navy had confirmed that helium is created in excess heat-producing cold fusion experiments.[5]

The essence of Champion's approach was that one could calculate the frequencies of electromagnetic radiation that should be imposed on a material to make it undergo a nuclear transmutation to another element. He claimed that he worked only with materials, the nuclei of which had a quadrupole moment, for this brought them into the range of the types of frequencies which his device was said to produce. The nuclei would absorb energy provided by the magnetic and electric fields. If the amount of energy absorbed were great enough, transmutation would supposedly occur. He presented Bockris with a report, which included a number of calculations, none of which made any sense according to conventional understanding. Champion's claim was an even greater heresy than cold fusion, because it was much more difficult to understand how elements with nuclei of much higher positive charge (greater numbers of protons) than hydrogen could have their Coulomb barriers (electrical repulsion barriers) tunnelled through by charged particles.

Following the Champion visit, a Mr. William Telander arrived, who was apparently working in a financial capacity with Champion. Telander gave the impression of a genial, relaxed, wealthy Californian. His line was that he had inherited a restaurant chain from his mother. Telander said that he distrusted the United States as a safe place for his investments, because the government pried into everything. He said that he had taken to Europe the money he had gotten from the sale of the restaurant chain. He claimed various interests in Belgium, Germany, Russia, and China. He claimed he had an office in Switzerland. Bockris phoned that office to verify it and it did exist, but Bockris was told on two occasions, "Mr. Telander is traveling."

On Telander's first visit to Texas A&M, he offered $100,000 for the group to test Champion's unusual claims, but in the ensuing conversation Bockris got him to increase the offer to $200,000 to spend on whatever the group wanted within the general area of these "inorganic reactions." Telander said that he was intrigued by Champion's claims, which Champion said had been verified not only in Tennessee but in some work which he had done at the University of Guanajuato in Mexico.

Bockris phoned the scientist in Mexico with whom Champion said he had collaborated. A Professor Garcia gave a partial confirmation of what Champion had said. He had not really collaborated with Champion, but Champion had brought him samples which had been produced elsewhere. One set of samples was labeled "untreated" and the second "treated." The "treated" samples *did* contain some traces of gold and some other noble metals and were radioactive. However, he made the point that he had no idea where these samples had come from and whether the radioactivity was indeed due to some kind of process or had simply been put there. He seemed negative and hesitant about the whole thing; he made Bockris very suspicious.

When Telander finished his presentation, Bockris explained that he had become interested in this kind of work in the course of investigation of the Fleischmann-Pons work (*i.e.* tritium production) and would like to do it, even though it appeared a long-shot and extremely controversial. Bockris told him that the best way to fund the work was to approach the Development Foundation of Texas A&M and make a gift. The advantage of a gift was that the administrative overhead was then only 5%, a management fee, whereas if he went via the Research Foundation route, the university overhead would be 30-40%. There was a catch: by going to the Research Foundation, he could have a contract to carry out a definite program of research, whereas if he gave the University a gift, the University could determine what they wanted to do with the money.

It would be within the contract limits that the gift might not be used to fund his research at all. Bockris pointed out to him that in practice the gift path would be preferable. He could write an entirely legal letter to the University in which he donated the money, saying that it could be used by the University in whatever way it wanted. There would be a clause in which he could state that he would prefer the money to be used in the support of the work of Bockris. The University would not be likely to use the money except as desired by the donor, because it would want to encourage the donor to give more money in a second phase.

Bockris introduced Telander to the head of the Development Foundation on his second visit; Telander conferred with him. Bockris tactfully left them alone and was later told that the offer had been duly noted. Bockris would be told later whether it had been accepted by the University or not. He met with the Head of the Chemistry Department to tell him of the gift that would be coming and the fact that it was for strange, heretical work, which he outlined to the Head. Bockris thought that a general designation of "investigations into inorganic reactions" would be true, but discreet, yet would encompass everything.

The eventual authority who accepted the money was Dean Kemp. It took the University several weeks to consider Telander's offer. Although he was flying around in his private jet and wasn't often in College Station, Telander did visit the Bockris group on another occasion. He finally sent one of the lawyers, with whom he seemed to be in frequent contact, to ask officials whether they were going to accept the gift. Finally, official approval was given.

Champion Begins

The first reaction to Joseph Champion within the laboratory was that he was an oddball-type of inventor, not a person of any special scientific training. (Earlier, he had run a laboratory for calibrating test instruments, skills perhaps acquired during his military service.) Telander had sent a large quantity of specialized electronic equipment to accompany Champion, which was promptly moved into the laboratory. Discussions with Champion revealed that he needed an electrochemical cell to couple with his electronics. The lab had many such cells, so he was provided with one, plus ancillary equipment. Champion connected his device and proceeded to carry out experiments. The device produced pulses of a bandwidth and frequency which he could control, including a "beat frequency" mode. Champion had a list of quadrupole moments of certain elements and charts of other characteristics of nuclei, all in a computerized data base. For a given nucleus he could examine its properties to find the "appropriate frequencies"—those he thought would interact with the quadrupole moments of the targeted nuclei.

Champion set to work with a solution of ions, which he said he would transmute. Very quickly Bockris and his students got the impression that Champion was trying ideas that he had not examined before! It was unsettling, but who knew what a clever, intuitive tinkerer might come up with when so many other strange nuclear anomalies had already emerged in the "mainstream" cold fusion field itself.

This first phase of Champion's work at the Bockris lab lasted about six weeks. The Bockris team had become extremely skeptical that this was going to result in anything useful, so it left Champion entirely alone in the laboratory. In fact, they treated him as a postdoc—he was registered at the University as a "guest worker." Occasionally the group thought there were signs of success. Some solids did seem to be deposited and were subject to X-ray and other kinds of analysis. There was a hint of an anomalous production of gold, but the experiment wouldn't repeat, so the group gave it up. In view of what happened later, it is very important to note that in this period of unsuccessful work, *Champion had complete freedom to cheat if he had wanted to*. The group of academic scientists had little control over what Champion did at that time.

Telander was paying Champion's living expenses at a local hotel. Champion was risking his livelihood in admitting the failure of his work up to that point. He didn't know whether Telander would dismiss him on the spot and go off elsewhere. In fact, he retained Telander's interest by saying that he had used "another method" to carry out the work which had proved successful at the University of Guanajuato in Mexico. He called the new method "the explosion method." Bockris would later call it the "impact method," because a Russian group in 1998 had claimed to find nuclear changes occurring after it had subjected its samples to explosions.[6]

The group went ahead with Champion's impact method, because Telander had asked that this be independently verified. Postdoctoral students, Dr. Lin and Dr. Bhardwaj, were to work half-time on this. In practice, they would work for three to four weeks on the Champion work and then go back to their own research activities (on which they were getting one-half salary) for three to four weeks.

A rough outline of the impact method: There were initial starting mixtures designated by Champion, which typically contained inexpensive materials, such as lead chloride and mercurous chloride, together with carbon powder and potassium nitrate. Sometimes other chemicals were added, such as sulphur and silica, but the carbon powder, potassium nitrate, and the cheap metal chlorides were always present.

The mixture was put into a large coffee grounds can, and this was placed in a protective crucible, all within a fume hood, and ignited. This was done with a remote igniter, leading to a muffled explosion and dense fumes (sometimes from a sulphur constituent), which were removed by the ventilating hood. These were, in effect, low-level pyrotechnic gunpowder explosions. After a "burn," Bockris would approach the crucible and peer in just after the explosion. Much earlier in his career, he had had experience with high temperature optical pyrometry. It seemed to him that the color of the mixture in this pot just after explosion indicated a temperature that might approach 1,000°C, but would certainly be over 800°C. It is clear that this transmutation methodology has its origin in classical alchemy—from "recipes" that are hundreds of years old.

According to Champion's instructions, which allegedly came from his earlier work in Mexico, the post-burn crucible had to be left for two or three days before it should be analyzed. During this waiting period, the researchers applied a Geiger counter to the mixture and there seemed little doubt about it in their minds: radiation, apparently from radioactive materials in the residue, was present, which had not been there prior to the ignition. But these measurements were crude. They simply held a Geiger counter at a fixed distance from the crucible and took the count at intervals over twenty-four hours. It is important to define the set up. Telander had insisted that one of the offices in the corridor be occupied by Champion or by a secretary. There was also a lawyer, who was present sporadically, there presumably to initiate patent claims if a positive result were to be obtained.

The experiments were lengthy and tedious. The carbon had to be ground fine; other materials had to be obtained and ground up; and all had to be mixed for three days. The actual impact experiment itself, which occurred with a "woosh" sound, was over in a few minutes. The mixture would be cooled for three days, during which nothing could be done except measure the radioactivity to determine whether a characteristic time variation existed that might help identify the radioactive species. An exciting variation in the radiation counts was observed in the early days of the Bhardwaj-Lin experiments. The group plotted the declining Geiger counter readings (counts per second) as a function of time. The logarithm of the count was linearly proportional to the elapsed time after the ignition—the very behavior one would get in the decay of a radioactive isotope! The half life measured corresponded to that of platinum-197 (18.3 hours). This had been predicted by Champion earlier. He said that Pt-197 was an intermediate in going from mercury to gold. This seemed interesting, though it wasn't clear to Bockris why mercury, element 80, should become platinum first (element 78), and then onto gold, element 79. But the group was eager to see something measurable, so this ostensibly positive result heightened enthusiasm. There might be ways of explaining away enhanced radiation after a burn, such as a concentration effect of naturally occurring radioisotopes in the condensed ash. But a time-variation, indicative of a decay rate, was something else. The group later published the astonishing result.[7]

After the first runs had been carried out by Bhardwaj and Lin, the group had to analyze the material which Champion claimed would now contain noble metals. Bockris was extremely anxious to do this in such a way that it could not be faked. He didn't want Champion or anyone outside his research group to have any hand in it. Bockris therefore packaged some of the material himself and sent it to four analysts: to some friends in Australia, in the Government Research Organization there; one to a Canadian analytical organization; one to an organization the group had identified in Nevada, which specialized in analyzing mineral deposits; and one sample was kept at the University, to be examined by atomic absorption spectroscopy and an analysis offered by the local nuclear reactor staff.

The results of the first run were disappointing. One had to take into account that a considerable amount of material was expelled in the explosion, so the weight of the initial material in the crucible had to be measured and then the weight after the explosion. Finally, the concentration of any noble metals (analyzed in different ways by the various companies) had to be expressed as a fraction of the initial mass of material. The results of the first experiment showed a negligible change from start to end point, *i.e.* the experiment did *not* verify Champion's claims. Failure of this first experiment, using the method said to have been verified in Mexico, hurt Champion's credibility. The group tried again.

Champion's role in all this was that of an advisor. He talked to Bhardwaj and Lin freely and there were frequent conferences in Bockris' office during which detailed discussions of the experimental methods took place. The team carried out several experiments successively over the course of April, May, and June 1992. Remarkable results were observed, which all regarded as being very controversial. The group had found noble metals present, just as Champion had predicted! The general characteristics of these results, according to Bockris, were as follows:

1) The new metals found were gold, ruthenium, rhodium, and platinum. Gold was always dominant and its maximum concentration found was about 300 ppm. Other materials were in lower concentration, around 10 ppm and sometimes less than this, but above the error limits of the methods (about 1 to 2 ppm). The group counted these as significant. Each experiment took three to four weeks, including the time to send materials for analysis. The three successful runs occurred from April through June.

2) The analysis by the various analytical organizations were not always in good agreement; sometimes there were differences of as much as 50%. But qualitatively there was no doubt that in the three experiments using Champion's impact method, noble metals appeared to be produced. There always was a before and after concentration measured by the analytical people, so it seemed that the basic result, production of over 100 ppm of gold and lesser amounts of other noble metals, was secure.

3) The best analysis, in detail and thoroughness, was carried out by the National Institute of Metallurgy in South Africa. The organization might have been expected to obtain the most reliable result, because of the importance of noble metal deposits—particularly gold and platinum—in the South African economy. The National Institute of Metallurgy in Johannesburg was used to dealing with such analysis; they provided two methods of analysis, both of which worked out to give about the same result.

To Bockris' amazement, when Telander heard about this, he was *not* pleased! He was totally unaware of the anomalous nature of the results. Although he had come to Bockris with the attitude that he was a disinterested wealthy man who would like to find out if there was truth in an unlikely claim, he rapidly became a very interested businessman when the group reported that noble metals could be produced. He was dissatisfied: 100 ppm is about 0.01% of the mixture and it would only have satisfied Telander had they been able to produce actual *visible* pieces of metal. On some occasions the group could, in fact, see tiny specks of something gold in color, which did turn out upon analysis to be actual gold, but the amount of these yellowish specks must have been in the milligram range. This attitude of Telander was completely unscientific, focusing on the practicalities of future gold production from the method, while ignoring the astonishing evidence that gold might have been produced at all.

Photo courtesy Texas A&M.

The late Kevin Wolf
at the Cyclotron Facility.

By August 1992 Telander abruptly announced that he did not want to continue the work at Texas A&M, because of the ridiculously small amounts of noble metals the group was obtaining. He said that he would move to a commercial laboratory in Chicago and there the work would be done on a "proper scale." This made no difference to the $200,000 he had given to the University. Bockris was able to continue using it in other research projects. In September 1992, Champion left the Bockris laboratory with a positive feeling. He had come in April 1992 and left in September. Although there had been ups and downs, particularly the failure of the electromagnetic method, his claims appeared to have been verified, although the amounts of noble metals obtained were miniscule.

Despite the very dubious nature of Champion's testimony, Bockris' results seemed to be sound enough. There were Russian researchers who reported in Vancouver in 1998 at ICCF7 that it had used the "impact method" and found an altered ratio of the isotopes in cesium—if true, clearly a nuclear change. Then there is the extensive work by Dr. Tadahiko Mizuno at Hokkaido University, in which numerous transmuted heavy elements appear—including gold—in carefully measured electrochemical experiments.[8]

Kevin Wolf's Alchemy Nightmare

The transmutation results obtained by Bhardwaj and Lin, from the "recipe" given by Champion (however he had mysteriously obtained it from the "alchemy underground" of shady "adepts"), were obtained between June and August 1992. In October 1992, at the Third International Conference on Cold Fusion (ICCF3) in Nagoya, Japan, rumors circulated that Kevin Wolf had obtained remarkable heavy-element transmutation results in cold fusion experiments that had been conducted *covertly* using Fleischmann-Pons-type cells! The reported transmutation findings were completely serendipitous. If the rumors were true, a *second* front of modern-day alchemy had opened up at Texas A&M. Yet the Wolf transmutation results would not see the light of day until April 1995.

At ICCF5, which met in Monte Carlo, Monaco (April 1995), EPRI's Dr. Tom Passell revealed for the first time the results of the EPRI-funded work of Kevin Wolf, which had led to the transmutation findings. There it was: unambiguous evidence of the transmutation of heavy elements by some heretofore unknown nuclear process occurring in Pons-Fleischmann-type cells.

Why had Wolf wanted to cover up such a major discovery? (In fact, he met an untimely death in 1997 without ever having published a paper about them.) Recall that physicist Wolf was initially a pioneer in cold fusion, who had made announcements in the spring of 1989 about his detection of low level neutrons and tritium. It seems that Wolf had lost his nerve after the scurrilous attacks by Gary Taubes in 1990. Though Wolf had played an ambiguous role in the attacks by Taubes against Bockris, Wolf had concluded in the spring of 1990 that his own tritium results were flawed. They were most likely the result of pre-existing contamination of his palladium, he said. He "withdrew" these results in the general press, *e.g. The Wall Street Journal*, but never issued a formal retraction to any scientific journal, as far as is known—other than his negative remark to reporter Robert Pool in *Science*.[3] Thereafter, he became a quiet skeptic of the cold fusion field, even though he continued to be funded by one of Dr. Passell's colleagues at EPRI, skeptic Dr. Tom one of Dr. Passell's colleagues at EPRI, skeptic Dr. Tom Schneider, who was bent on tearing down cold fusion. Wolf continued his electrochemical cold fusion experiments, continuing to find low-level neutrons (and *not* publishing these either), but nothing else of interest.

In 1992, Dr. Wolf made what to him must have been a nightmarish discovery while chasing down low-level neutrons. Three of his palladium cathodes that had undergone Pons-Fleischmann electrolysis in heavy water were found in routine Geiger counter examination of the lab to be highly radioactive! Subsequently, the cathodes were examined in sophisticated gamma-ray spectrometers at various laboratories (including Los Alamos National Laboratory), which could observe the intensity and frequencies of the gamma ray emission lines. Experts, including Wolf, who saw the multiple spectral lines of gamma emission had no doubt that these were the signatures of radioactive isotopes with masses near that of palladium. The results were published by Dr. Passell in the ICCF5 Proceedings.[9]

It may be difficult to understand this, but Wolf could not imagine that these astonishing results—*in which he believed fully*, but could not thereafter reproduce—had anything to do with "cold fusion." He apparently died imagining that these radioactive isotopes might be due to the effects of cosmic rays penetrating the Earth's atmosphere—*which just happened to strike his "cold fusion" cathode*. He apparently believed that hypothetical Weakly Interacting Massive Particles (WIMPS)—the purported dark matter or "missing mass" of the universe—might have caused the transmutations. Wolf apparently wimped out, so to speak, on his own solid data. The world of science was prevented from hearing about this work at ICCF4 in December 1993. Wolf had been scheduled to talk at the ICCF4 conference in Maui, Hawaii. The program listed his talk as "To Be Announced," but Wolf was "encouraged not to attend" by cold fusion skeptic Tom Schneider, with the implied threat that his funding might be withdrawn if he went public.

Several weeks after a Pons-Fleischmann-type cold fusion experiment had ended in Wolf's Texas A&M lab, at least one cathode was found to be inexplicably radioactive. Gamma rays from at least seven radionuclides were observed. The number of counts observed per peak was on the order of 10^4 to 10^6 counts, with a high signal-to-noise ratio of about ten. The statistical significance of the data was high. Radionuclides of silver, rhodium, ruthenium, and palladium were detected. (See *Infinite Energy*, No. 2, p. 30, and Reference 9 for a more complete discussion.) Since there is no conventional explanation for how palladium can be made radioactive with this pattern of isotopes (not even in a nuclear reactor), it can be assumed that a "cold fusion" reaction was involved in some way. To this day, no one knows what could cause the bizarre Wolf data presented by Passell at ICCF5. In any event, it has been clear at least since 1992 that deuterium-fusion reactions are not the only participants in cold fusion phenomena.

The Wolf results had been obtained in September 1992, *after* the first impact method experiments in May in the Bockris group had shown new elements apparently forming under mild conditions from lead and mercury compounds. On returning from the 1992 Nagoya, Japan ICCF3 meeting, at which the Wolf results were only *rumored*, Bockris urged his co-workers to try the impact method again. He wanted them to redouble their efforts, because of Kevin Wolf's results.

New impact experiments resumed in December 1992. Much to the group's chagrin, the amounts of gold found were within the limits of error—a null result. During the Christmas vacation of 1992, about eight runs were carried out by Dr. Bhardwaj to try to recover the results that had been obtained in the summer, but no anomalous noble metals emerged. (See Exhibit A. Dr. Monti suggests that a "seasonal effect" described by ancient alchemists—results are only good from about March 25 to June 15—might have been responsible.) By February 1993, Bockris became convinced that the group had to withdraw the support it had given during the summer to the results of the impact method—there were too many doubts about it. Bockris wrote a letter to the lawyer with

Exhibit A: The Role of Dr. Roberto A. Monti

Dr. Roberto Monti, an astrophysicist based in Italy, had been developing a revisionist model of atomic and nuclear structure for years, based on his study of the historical development of chemistry and physics from the eighteenth century through the early twentieth century. When cold fusion was announced in Utah in 1989, Monti attended some of the earliest scientific meetings in Italy on the topic. Some of his initial reactions to the Utah claims are recorded in Italian newspapers of the time; these, in turn, were quoted by U.S. papers. So John Bockris had been in occasional scientific communication with Monti, prior to the arrival of Joe Champion and Telander. When the "Philadelphia Project" began, Bockris thought it would be useful to have Dr. Monti lend his expertise in monitoring the heretical alchemical studies at Texas A&M. Monti was a silent observer of the happenings among Bockris, his students, Champion, and Telander. Monti noted both the apparent successes and failures of the experiments and learned from them. As I have discovered from extensive discussions with Dr. Monti, the history of atomic theory, classical alchemy, and the happenings at Texas A&M are very rich with interconnections, strange conflicts, and colorful happenings.

Since 1993, Monti has continued to conduct transmutation experiments of his own based on his "alpha extended model" of nuclear structure, which implicitly permits the kinds of low-energy ("cold fission/cold fusion") nuclear reactions that have been claimed by some scientists and "real" alchemists for centuries. He is now supported as the Director of Research of Monti America Corporation, a Vancouver, Canada-based company involved with low-energy transmutation. (Monti resides mainly in Italy.) He published extensive descriptions of his thermal-alchemy transmutation experiments in *The Journal of New Energy*, Vol.1, No. 1, 1996, p.119 and in the same journal, Vol. 1, No. 3, 1996, p. 131.) At ICCF8, his poster abstract claimed continuing progress in experiments that show the feasibility remediating nuclear waste:

Nuclear Transmutation Process of Uranium

The possibility to cause nuclear transmutation of stable isotopes by means of ordinary chemical reactions suggested the possibility to cause nuclear transmutation of unstable isotopes. A first series of experimental tests was made from 1993 to 1995 with positive results. A new series of independent tests has been performed at ENEA laboratories, starting October 1997 up to April 1, 1998. The results of the first and second series of independent test were reported in ICCF7 (Vancouver 1998). A third series of independent tests was made in the same laboratory (ENEA, Saluggia) on May 21, 1998 and May 25, 1998, using uranium nitrate, again with positive results. A new series of independent tests will be performed in an independent laboratory in the USA in April 2000. The results of the experiments made in May 1998 and in the new ones of April 2000 will be reported in ICCF8 (Lerici, May 2000).

Experimental reports provided by Monti describe tests in which gram-level destruction of radioactive material occurs. We plan to follow up on the Monti story in future issues of *Infinite Energy*.

whom he had been most associated in dealings with donor Telander. He noted that the group could not repeat the results. Shortly after this, the work supported by Telander had to stop, allegedly because of difficulties Telander said he was having.

The Bockris group continued its transmutation research with other funds. It now worked on the electric arc, carbon-to-iron reaction with the help of Dr. Sundaresan of the Bhabha Atomic Research Center (BARC), in Bombay, India. A small amount of iron was apparently produced in the experiments. It was well above the level which corresponded to the tiny amounts of impurity iron remaining in the spectroscopically pure carbon electrodes. A dependence on oxygen (O_2) emerged; no iron was produced without oxygen. There was an attempt to hypothesize a nuclear transmutation that would be consistent with the excess heat evolved in the process.[10]

For another seven months, the group continued its low-energy nuclear reactions work. Then unexpectedly, an inflammatory letter appeared in the local newspaper, the *Bryan-College Station Eagle*. It was written by Dawn Wakefield, a former student of Bockris. Although the group had not done any transmutation work involving the impact method for six months (the approach that resembled classical alchemy), Wakefield accused it of the "heinous crime" of performing medieval alchemy at a state university. The letter stirred a hornet's nest of other troubles.

A call came from Joseph Weiss, a reporter with the *Dallas Morning News*. Weiss acknowledged that Dawn Wakefield's letter had led to his inquiry. He knew of Joe Champion and the gift of $200,000, the final disbursements of which had been interrupted by actions of the California Securities and Exchange Commission. Weiss wanted an interview. Bockris could see trouble coming, so he consulted the Chemistry Department Head, who obtained a recommendation at the TAMU Vice Presidential level that Bockris should grant the interview.

The interview was held on a Saturday morning. To Bockris' surprise, shortly after the meeting began, Dean Kemp, whom he had never met before, entered his office and said that he wanted to be present. Kemp had apparently heard of the interview from the Office of University Relations, whose representative was also present. Dean Kemp, indeed, had a personal interest in the *Dallas Morning News* story, for it was he who had approved the grant from Telander. Telander had told everyone that the donation had come from his personal funds, which originated with the sale of a restaurant chain inherited from his mother. Telander consistently claimed this, and it may be true, despite other disturbing news that emerged. California securities authorities had formally accused Telander of misappropriating $11 million from investors. Telander had apparently accepted funds from investors for investments in Switzerland, where arbitrage schemes on currency fluctuations allow risky but high rates of return. When questioned, Telander argued that he had merely extended their investment gamble in backing a development, which, if successful, would bring even higher returns. But Telander had not gotten his investors' approval for this, and there was also the discrepancy between the amount given to Texas A&M and the amount authorities deemed misappropriated. Telander claimed he had spent millions of dollars at other labs to follow up the results obtained in the summer of 1992 at Texas A&M. Bockris had no evidence that this was true.

Bockris was very frank with journalist Weiss. The discussion was recorded on four audio tape machines: one owned by Bockris, one by Weiss, one by a representative of University Relations, and one belonging to Dean Kemp. The interview lasted several hours, two hours before lunch and at least one hour after lunch. Mr. Weiss had a lot to write about, for Bockris had no reservations in telling him everything he knew about the entire business of the funding, the scientific work that the group had done, the results it had obtained, etc. He gave it to Weiss "straight," pointing out that no one understood the mechanism of the impact method, which had seemed to produce tiny amounts of noble metals. Bockris emphasized that the work sprang out of his verified and published work on the tritium-producing cold fusion reaction. He stated that he had wanted to see if a similar kind of nuclear reaction "in the cold," obtained with hydrogen isotopes, might also be found with elements of higher atomic number. Bockris told Weiss that the results had been disappointing, because after the promising experiments, the group found that it could not repeat the results, although some new anomalous radioactivity had again been observed.

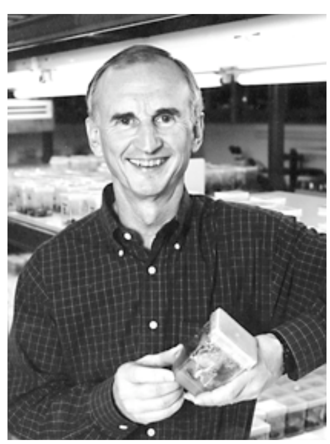
Exhibit B

The Failed Petition at Texas A&M to Demote Bockris (This was signed in 1994 by 23 out of 32 Distinguished Professors at Texas A&M.)

A Request

Professor John O'M. Bockris' activities since 1989 (the inception of the "cold fusion" embroglio), and particularly recent allegations that he lent his name and that of our university to the fraudulent scheme to promote a bogus engineering enterprise, has brought this university into disrepute. Note that on page 6 of the "Policies and Procedures Regarding Distinguished Professor Appointments" (September, 1993) it is stated that "The Distinguished Professors. . .bring honor and recognition to the University. . ." Instead, we that believe that Bockris' recent activities has made the terms "Texas A&M" and "Aggie" objects of derisive laughter throughout the world among scientists and engineers, not to mention a large segment of the lay public. The "Alchemy" caper is, everywhere, a sure trigger for sniggering at our university. And so it should be. For a trained scientist to claim, or support anyone's claim, to have transmuted elements is difficult for us to believe and is no more acceptable than to claim to have invented a gravity shield, revived the dead or be mining green cheese on the moon. We believe it is sheer nonsense, and, in our opinion, could not have been done innocently by one with a lifetime of experience in one of the physical sciences.

In view of the above consideration, we the undersigned Distinguished Professors of Texas A&M University hereby request the Provost to take steps to revoke the title of Distinguished Professor now carried by John O'M. Bockris. We do this because of our belief that Dr. Bockris' alleged disregard of the accepted standards of scholarly and professional behavior has brought great embarrassment upon this university and his colleagues. In our opinion he no longer merits the title of Distinguished Professor.



Dr. Robert Kennedy
Photo courtesy Texas A&M University. Photo by Jean Wulfson.

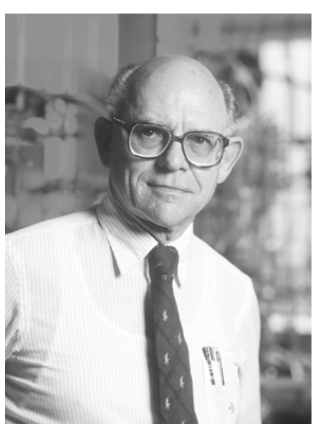
Shortly after the interview, Bockris was astounded to get a letter from Dr. Robert Kennedy, Vice President in charge of research at Texas A&M. Kennedy said that Dean Michael Kemp had accused him of "misconduct of research." It seemed that Kemp read into the interview things which Bockris had never meant. During the heady times after the group had gotten good results, but before it tried to replicate them, Lin and Bockris had been invited by Telander to go to Mexico City to make a presentation about the work to a group of science journalists. Bockris was pleased to do this and both Lin and he spoke for perhaps five minutes each about the research. Bockris pointed out something which has now been widely verified: that if transmutation in the cold were indeed true, there would have to be a major revision in the theory of nuclear chemistry.

Dean Kemp read this statement quite differently. He thought that Bockris had gone to Mexico as an advocate of sponsor Telander. Kemp believed that Telander wanted to commercialize the products of the group's findings. Since transmutation in the cold was impossible, thought Kemp, any supportive statement that it could occur, or might have occurred must also be fraudulent, hence constituting misconduct of research. In his view, Telander wished to deceive the Mexicans and sell a process which was nonsensical. Kemp thought that it was a travesty that a Distinguished Professor at Texas A&M University would support a man like Telander.

The disturbing accusation was backed up shortly afterwards by a remarkable document (see Exhibit B), which came from the group of other "Distinguished Professors"— twenty-three of them. "Distinguished Professor" is the highest title for a professor at Texas A&M University—all are world-famous in their respective fields. The purpose of the accompanying document was to suggest that anyone who was crazy enough to believe that tritium could come from deuterium reactions in the cold, and then go on to say that metals transmute to other metals, including gold, must be certifiably scientifically idiotic. Worse, it would be *prima facie* evidence of fraud—fraud for the sake of money.

A group of four of Bockris' peers was assembled, all Distinguished Professors. Bockris met them in the building of Texas A&M housing the office of the General Counsel. In the inquiry, Bockris gathered six of his collaborators, each of whom had had experience in either cold fusion or transmutation work. He wanted them to be on hand if questions of experimental design or handling were raised. Bockris had paid for the services of a lawyer after the accusation. Though the University hesitated at first, attorney Gaines West was allowed to accompany Bockris into the conference room in which the "trial" took place. Present also was the Assistant General Counsel Genevieve Stubbs, a vigorous and capable attorney who a few years earlier had assisted Bockris when the Taubes allegations appeared in *Science*.

Bockris first asked permission to make a ten minute presentation, in which he pointed to questions of legality in what the University had done. For one, the rules of the University Policies and Procedures Manual state that no one in the Administration may speak to reporters or give interviews regarding a Professor's work without his or her permission. A *Newsweek* magazine article had, in fact, quoted a spokesperson of the TAMU administration, ". . . the work on transmutation was embarrassing the University."[11]


Distinguished Prof. F.A. Cotton, A signer and major promoter of the Petition.
Photo courtesy Texas A&M University.

Then Bockris summarized the work of his research group. He pointed out that the team had repeatedly generated tritium in electrolysis of heavy water, and that this was an indisputable "nuclear change in the cold," which had been published in refereed journals. Thus, he told the panel, it had not been unreasonable to examine similar behavior with heavier elements. Bockris related that against all odds the work seemed to succeed, but then after a pause of some months, the results could not be reproduced. Regarding questions about Joe Champion (who later turned out to have had an imbroglio with the law at an earlier stage on an unrelated matter) and William Telander, who was then under investigation for whether he had permission from his clients to invest 1% of their money in speculative research at Texas A&M, Bockris could only say that he knew nothing of any improprieties by Champion or Telander while he collaborated with them. In any event, whatever the ethics of Champion or Telander, this didn't seem to affect the work that had been carried out by Bhardwaj and Lin. [In a Los Angeles court in 1994, William L. Telander plead guilty to four counts of securities fraud and two counts of tax evasion. He had stolen $11 million from 380 investors, for which he served time in prison. In 1993, Joe Champion went to prison in Arizona on charges not directly related to the fraud charges that put Telander in jail. Later in the 1990s Champion returned to prison in Arizona (reportedly for a parole violation) and may still be there in 2000. See "Cold Fusion and Modern Alchemy" in *IE,* No. 15/16, p. 95, on the further travels of Joe Champion and hot fusion physicist Dr. Barry Merriman, who spent a significant effort in an attempt to verify Champion's later claims.]

The four distinguished professors who were "trying" Bockris were pleasant, which encouraged Bockris and his attorney. There was no need to call in any of the six postdocs who had carried out the research. After only a week or so, the best possible outcome happened: Bockris was given a "complete exoneration" from the charges in a letter dated January 31, 1994.

The Distinguished Professors who had tried Bockris gave an account of their investigation: They had examined more than 1,000 pages of documents. They had obtained evidence from four or five people. (Dr. Wakefield, the initiator, had been asked to provide evidence, but had refused.) One of the exculpatory pieces of evidence cited was a note, hand-written by Bockris from a hotel in New York City. It was a draft of what had presumably been made into a typed letter later. It contained a specific warning from Bockris to Telander that he must not in any way use the successful results obtained in the summer of 1992 to imply that there might be some commercial value in it. This was a key point in the defense. After all, the accusation had been that Bockris had encouraged Telander in fraudulent gold-making activity. But how had the investigators even found the note? Bockris had suspected that his office at Texas A&M had been under surveillance for a long time and that various documents had been stolen, presumably by unauthorized entry at night. Apparent thefts of certain documents also occurred, apparently via unauthorized entry at his rustic home office. Who was paying the possible "private investigators" carrying out these intrusions? He never found out.

After the trial, Bockris continued with his research and teaching and had another four or five months of peace and quiet, just as he experienced after the end of the work supported by the dubious Mr. Telander. Unfortunately, news of a "new inquiry" erupted around June 1994. An article in the *Eagle* implied that the "new inquiry" had been set up to see if any "personnel changes" were needed as a result of the "Philadelphia Project"—the informal code name the group had given to the Champion-Telander work. How could this be, after the letter of complete exoneration? Bockris understood that a big initiative of some kind was underway and that decisions in secret "political trials" are not necessarily made according to the truth, rather according to the power exerted. Bockris had enemies at Texas A&M, and perhaps beyond, who were not satisfied by his exoneration.

The new committee came to be known as the "Ad Hoc Committee." When Bockris' lawyer inquired of the Assistant General

Counsel what was the objective of the inquiry, he was told only that the University could investigate whomever and whatever it liked. The invisible "inquiry" went on and on. After some months, Bockris wrote to the Committee, pointing out that it was he who knew more about the "Philadelphia Project" (the informal name for the work that was used within the Bockris group) than anyone. He suggested that they could shorten their investigative work by inviting him to one of their meetings to ply him with questions, the answers to which could later be checked.

Bockris would later learn from a member of the Ad Hoc Committee that the primary mover against him was a professor in the Inorganic Division of the Department of Chemistry—Distinguished Professor F.A. Cotton. At a meeting with the Dean of Science, this ambitious professor had pointed out that he had published more than 1,000 papers, whereas Bockris had published only 700—a rather tenuous measure of relative rectitude.

Bockris received no reply to his letter asking to be questioned. Christmas 1994 was approaching. Bockris thought it would be a good idea to speak to the Chairman of the Committee, Dr. Robert Kennedy, with whom he had a good relationship in the past. Over the phone Kennedy was reluctant to speak, but he finally agreed to meet. Yet when they met he couldn't tell Bockris anything! He said that the Committee was doing its work. When Bockris asked him what the result would be, he said he didn't know, but that personally he was fed up with it all. He said cryptically: "There is a message from the Provost. He has asked me to tell you: 'Bockris will not be the only one.'"

The chilling message meant that they were thinking of firing Bockris, on what grounds he did not know. Any new charges had been kept secret from him— not exactly a model of ethical legal procedure. Bockris would soon learn from the leaky Committee that a principal reason for renewed investigation was that he had obtained results that were simply "impossible" and a cause for the University to be the object of ridicule. Bockris decided to spend a few thousand dollars more on the attorney who had helped bring the first inquiry to "total exoneration." The two agreed that the best approach might be to take the whole thing to the American Association of University Professors (AAUP).

An eleven page document was prepared,[12] which described what Texas A&M had been doing to Bockris since 1993. There had now been two years of continued persecution, a "trial," the exoneration, then the "new committee," the eleven months of investigation, the refusal to tell Bockris what any charges were, etc. The AAUP is regarded as a powerful body within the university community of the United States. It investigates what it considers to be unjust treatment of professors and it can punish a university if it finds cause. If a university is blackballed by the AAUP, new faculty of high quality will be less easy to hire. Texas A&M had good reason to be concerned about this: in the 1980s it had been under a cloud from the AAUP. Perhaps the University would not want to again risk censure. The Texas representative of the AAUP said that the Association might well send a team of investigators to Texas A&M to find out just what the university was trying to do to Bockris.

Though there is no proof that what happened next had anything to do with Bockris' letter to the AAUP, in the light of the chilling message given him a few months earlier, Bockris was both surprised and relieved that on May 5, 1995 he received a welcome letter from Acting Provost Charles Lee. It said that the eleven month investigation had shown that in no case had Bockris done *anything* that contravened the Rules and Regulations Manual of the University. It was tantamount to another complete exoneration, although the letter was not as warm as that from the first group of professors. It was written with a regretful tone, implying that there were no technical or legal grounds on which to convict him, "but . . ."

The Shunning

One of the most difficult aspects of the treatment to which Bockris was subjected was social ostracism, starting with Dean Kemp's accusation and not even ending with the second exoneration. There were about sixty-five professors in the large Chemistry Department at Texas A&M. Most ignored Bockris for much of the two-year period in which the University, egged-on by ring-leaders in the Department, acted against him. After the first complete exoneration, two professors did congratulate him, but he was isolated. Bockris' wife Lilli felt it perhaps more than he, because she had a number of faculty wives whom she had known as friends. When she met them now in the supermarket, instead of having the usual kindly chat, they turned their backs on her. Lilli recalls that the year she spent in Vienna after the Nazis took over seemed to her less unpleasant and threatening than the isolation and nastiness which she felt in College Station, Texas from 1993 through 1995.

One would have thought that after all that had been done, everything would be settled now. This was not the attitude of many of Bockris' colleagues. The motivating force for the antipathy may be the subconscious fear that the discoveries of the Bockris group might eventually be proved and recognized. Then his original contributions would be rated as discoveries of great magnitude. There were at least two professors in the Chemistry Department who had made it known that that they expected to receive the Nobel Prize in Chemistry some day. The possibility that it might go instead to a colleague whose work they so much denigrated must have been an unwelcome thought. (They did not have the attitude of physicist Richard Feynman, who was displeased by the artificial focus on one person's accomplishment that the Nobel Prize system encouraged.)

Having failed in the three official investigations that had been carried out against Bockris, they decided that all they could do would be to persuade the head of the department to have Bockris shunned—as in an excommunication for religious heresy. No one was supposed to speak with the errant Bockris. For a long time, absorbed in his work as ever, he didn't understand that shunning was underway. Most of the colleagues had been ignoring him anyway since the inquiries had begun in 1993. He did notice, however, that whenever he wanted to talk to the Head of the Department, perhaps once every few months, he came to his office and did not invite Bockris to come to his. Of course, he was more than twenty years younger than Bockris, but later Bockris realized that this was an example of the shunning. The Head did not want anyone to see that he was talking collegially with Bockris!

Bockris' colleagues in the physical chemistry division took no notice of the shunning order, which might have gone around unofficially. In practice, the shunning made no effective difference to how Bockris carried out his work, though it was a very considerable act of spite. It proved once again that at least in the Chemistry Department at Texas A&M University, research results which do not agree with existing theory are not tolerated.

More Intolerance

In 1995, an energetic young student in Engineering, Todd Hathaway, wanted to have a symposium on new energy sources. He was headed for service with the U.S. Navy, interested in serving in nuclear submarines, perhaps eventually to become a Captain. Hathaway had the characteristics of a leader, Bockris thought. He was polite, but also quite domineering and a bit arrogant. He persuaded the Head of his Department, via a committee of students who recommended seminars, to agree to a symposium on new energy sources. They invited four people to speak, of whom Bockris was the one who would talk about cold fusion.

The posters for the New Energy Sources Symposium caused a sharp reaction from certain professors, who apparently wanted to show that it would be impossible for Bockris to speak, but the line was that the entire symposium should be cancelled. Bockris learned that the movement to ban the New Energy Sources Symposium came from Chemistry. The quasi-official line was that "the speakers were not of good quality." The Head of Hathaway's Engineering department told him that there would be no lecture theaters available in Engineering for a symposium of this kind. Furthermore, if he tried to hold it elsewhere in the University, he would be held responsible for the consequences. He was later accused of having improperly used state property, just for having communicated with the speakers using an office typewriter! The *Eagle* newspaper treated the matter as though it cast doubt on the student and the proposed symposium.

Fortunately, Hathaway was not so easily put off. He went off campus to a local Catholic Church, to Father Sis, with whom Bockris had had occasional theological discussions. The church immediately gave him a room for the symposium, which was held with the original speakers.

The *Eagle*, which had so denigrated Bockris's experimental work on transmutation, sent a photographer to the symposium. This was presumably to ridicule it and perhaps show, if possible, that few students would turn up. The photographer went away empty-handed, however. The symposium took place perfectly normally with about thirty-five people present, including several members of the Chemistry Department. All went well with the four lectures and a discussion period. The *Eagle* fell silent. University censorship of new ideas may have been thwarted, but it was yet another example of how Texas A&M didn't really know how to deal with groundbreaking research that threatens the paradigm, contrary to the official line about "openness" in many Texas A&M publications.

International Meetings on Transmutation

After the Bockris group obtained the results in the work of Sundaresan (carbon-to-iron) and the results of Minevski (protons plus palladium to numerous new metals within palladium electrodes), they wondered whether others were claiming to have results parallel to theirs that nuclear reactions did occur "in the cold" within solids. It was Dr. Lin who suggested to Bockris that they should hold an international symposium on these matters. So Bockris went to Dr. Emile Schweikert, Head of the Department of Chemistry, to asked him for permission to have a one-day symposium held at Texas A&M. He replied, "Of course."

After much organizational activity, mostly by Dr. Lin, the symposium was held on June 19, 1995. (See *IE*, No. 3, p. 8.) and attracted about eighty-five people, including one from Russia and several from other countries. A student interpreter assisted the Russian scientist. The symposium went very well, beginning with Dr. Tom Passell of EPRI speaking about the hidden, controversial transmutation results of the late Kevin Wolf. It was the high point of the symposium, and certainly set the theme, for by itself this work proved that transmutation in the cold did occur in some metal systems.

The rest of the symposium went well. Tom Ward, an independent spirit from the DOE, was there. He spoke at the end of the symposium, praising it and saying that DOE money might well be available for such efforts "very soon." However, the funding never came.

A most unfortunate incident confirmed the worst fears about Texas A&M University and those who opposed the publication of new scientific findings. Doherty-Welch Distinguished Professor of Chemistry F. A. Cotton approached the meeting room with two colleagues in the early afternoon. Dr. Ward and another speaker were outside the lecture hall. When Cotton realized what was being discussed in the lecture theater, in a loud voice he shouted that these people were "all gooks." Ward took great exception to this behavior. He wrote a letter of protest to the Texas A&M President, pointing out that he had come to hear science, not to be insulted by a man who apparently could not bear to hear about this developing field.

This symposium was immediately discussed by the *Eagle*, which talked about ruffled feathers at Texas A&M. The article implied that there was something nefarious about the meeting. Bockris was just about to leave for Australia for a three-month period, but managed to write a letter to the *Eagle* about what Prof. Cotton was reported to have said. Bockris' letter was not published. Its main point was that the results of a large number of scientists from various countries were being reported at the symposium; that the process of science was to listen and to accept or reject these experimental results, to see where they might lead in possible revisions to theory.

In 1996 it was time to consider whether a second symposium should be held. Requests were sent out for speakers, with an encouraging response—about one hundred people registered for the symposium. Bockris approached the Head of the Department again and asked him whether they could hold the symposium at Texas A&M University as in 1995. This time, however, the Head had been told that he must submit any such request to a Committee which had been formed. The Committee, consisting of about a dozen members of the Department, listened to Bockris' five-minute presentation about the proposed symposium. Bockris circulated a review that had just been published by Ed Storms, which contained 468 references on research in cold fusion, a substantial number of them in refereed journals. Each member of the Committee had the review in hand the day before they were to be asked to agree to the symposium. Bockris received a memorandum the next day from the Head telling him that the votes had been unanimous to reject the symposium. It could not be held in the Chemistry Department. This was the standard of academic freedom at Texas A&M.

Bockris spoke over the telephone with one member of the Nuclear Chemistry group and asked him the reason for the unanimous vote against. The man replied, "They think it's a fraud or a joke." Ironically, the Cyclotron Institute at Texas A&M was devoted to transmutations, but at high-energy conditions in which particles had been accelerated. The equipment was valued in the millions, while what Bockris *et al.* had been using cost $10,000. Might this have something to do with the problems?

Another venue was found, the local Holiday Inn. Professor George Miley of the Nuclear Engineering Department at University of Illinois, a well-respected member of the nuclear community and editor of *Fusion Technology*, co-chaired the meeting with Bockris. Professor Joseph Natowitz attended, as he had the first symposium. He is the leading nuclear chemist at Texas A&M and Head of the Cyclotron Institute. There was plenty of time on the second day of the symposium for free-ranging discussion. Bockris asked Prof. Natowitz publicly: "Were you Kevin Wolf's boss?" He replied affirmatively. "Why then have you allowed Wolf's transmutation results of 1992 to remain unpublished by the Cyclotron Institute for four years?" He explained that the results "were not reproducible."

The two symposia on transmutation at Texas A&M University might have been a turning point in the attitude of many toward such reactions, although they are certainly not accepted by the majority of chemists even in the year 2000. But at the same time the American Nuclear Society has for three successive years held Low-Energy Nuclear Reactions (LENR) sessions at their national meetings. So the meetings which took place at Texas A&M would no longer be required; they were meant simply to start the introduction of the subject into the mainstream and were successful in doing that.

Packham's Oral Examination: Aftermath of the Tritium-in-the-Cold Discovery

It came to pass in 1992 that Nigel Packham, who had been the main worker in the initial research on electrochemical production of tritium, wrote his Ph.D. thesis. Packham had been working with Bockris for about two years on and off on cold fusion, but the subject in which he had begun with Bockris had been entirely different. It was aimed at examining the production of hydrogen from water using bacteria.

Packham's earlier background in England was partly in biochemistry, so the topic was ideal for him. His thesis consisted of two very different parts, one on hydrogen production from bacteria and the other on cold fusion.

Bockris suspected that bad problems might arise during the oral doctoral examination of Nigel Packham, because of the long-past article in *Science* by Taubes. So he arranged with the graduate school representative (who is present at all orals for the Ph.D. to assure fair play) to be prepared to remove the oral exam from the big lecture theater to Bockris' office. Bockris feared that the potential shouting and general disruption might adversely affect the academic process. Before the oral exam, Bockris held a meeting in his office to discuss procedures, although one member of Packham's committee, Dr. M. Soriaga, did not attend. Bockris also asked that two people knowledgeable about tritium production in the cold be members of Packham's oral exam committee. These were famous electrochemists at the time in their own right. One was Dr. Norman Hackerman, President Emeritus of Rice University. Hackerman is a well-known electrochemist, who had also been to South Korea where he had seen the work being carried out there on tritium production in the cold. He had informed Bockris after that trip that he had seen tritium produced under completely different circumstances from those that Packham had employed.

The late Prof. Ernie Yeager of Case Western, the second participant requested by Bockris, was perhaps the most well-known physical electrochemist in the United States. He had been President of the Electrochemical Society, and had received many other honors. Bockris knew that he had obtained tritium in research with Robert Adzic, but that he had chosen not to publish the work, possibly because of the general atmosphere of ridicule of the topic. When they arrived at the lecture theater for the oral, it was full of people. Usually, these orals take place in small rooms, and the persons who attend are just the members of the committee concerned, about four people, and the candidate. Legally there can be other members of the university present, and it was already known that that this would likely occur.

The examiners, Hackerman, Yeager, and two professors in Biochemistry, together with Bockris, the Chairman of the Committee, sat in the front row. A large number of graduate students were there. Dean of Science John Fackler was there, as was Prof. Michael Hall, Head of the Chemistry Department at the time.

Packham began by summarizing his whole thesis. He talked first about his bacteriological work on the decomposition of water to yield hydrogen. He seemed to Bockris to be spending too much time on this—everyone present had come mainly to hear about tritium and not about bacteria decomposing water. Bockris asked him to get on with the tritium story.

Bockris as chairman of his doctoral committee had the task of choosing among the many hands held up at the time of questioning. Kevin Wolf was present, and Bockris favored him because he thought it would be most fair. Wolf had greatly opposed the work, and now was his time to say why. Wolf was allowed eight minutes to question Packham. After Wolf's questioning, Bockris exposed Packham to many other questions. After half an hour, Bockris was just about to close the discussion when Dr. Soriaga rose to his feet and walked down the aisle with a bunch of papers in his hand. He handed them to Packham and said: "Answer these!" Packham stared at the sheaf of papers, each of which contained a question. It was obviously impossible for him to deal with this publicly—it might have taken a couple of hours. One of the time constraints was that Bockris had to get Hackerman back to Houston in the ground transportation that had been arranged and was waiting.

Bockris queried the graduate school representative who was sitting in the front row, "What now?" He recommended that Packham should be asked to respond to these questions in writing, which could be included at the back of the thesis. Bockris announced the decision and the oral exam ended. The audience left and the committee remained, including Dr. Hall, who asked if he might be present at the subsequent deliberations. Bockris agreed.

All the members of the committee were quick to assent that they were satisfied with Packham's performance. They thought that his work certainly came up to the standard that deserved a Ph.D. degree. Only one person, Dr. Soriaga, held out against the work. He said that he could not sign the thesis because the formation of tritium in the cold was "impossible." Subsequently, Soriaga became rather heated. Dr. Michael Hall then made a suggestion: acceptance of the thesis should be conditional on replies to Soriaga's many questions being printed out in the thesis as an Appendix. Would Dr. Soriaga then sign? Yes, he agreed to that. It was the end of the oral examination and all the people present, except Dr. Hall who was not part of the Committee, signed the official forms which are generally regarded as giving the graduate student his Ph.D. degree.

They went up the stairs to the lecture theater and at the top of the stairs Dr. Hall shook Packham's hand, and said "Congratulations on your Ph.D!" Packham had, of course, been outside the lecture theater while the deliberations were conducted. He had become rather anxious, because usually these deliberations last ten minutes and this had lasted more than a half-hour. Hackerman left for his trip home and Bockris invited Fackler, Hall, and Yeager to come with him to the Plaza Club in Bryan for what turned out to be a pleasant dinner. The academic process had worked satisfactorily—an oral exam had been completed in a very controversial area.

The next day, however, everything had changed. One of the conditions, which is usually assumed to be a formality, still had to be fulfilled in the awarding of the Ph.D.: the department head's signature. Usually after the graduate school committee completes its recommendation, the papers are sent to the head of the department and he routinely signs off the thesis. Dr. Hall refused to sign off. His handshake with Packham and congratulations had evidently not been meant seriously! He now said that he couldn't accept the thesis either, because it was well-known that tritium could not be formed in the cold.

The next few days became a furor of negotiation and discussion. Finally an arrangement was worked out, largely on the suggestion of Kevin Wolf. Packham would rewrite his thesis, cutting out all reference to tritium production, and the thesis had to stand or fall on the basis of the biological work alone. Packham would be allowed to have an "appendix," which would consist not of the answers to Soriaga's questions, but the very papers he had already published in refereed journals on the formation of tritium. This arrangement was agreed to by the biochemistry professors, who said that the biochemical work that Packham had done was "just enough" for a Ph.D. degree. Now Hall signed the thesis and Packham had his degree. Today, Dr. Packham works for Lockheed on NASA's closed-cycle life-support systems for long-duration spaceflight, such as manned missions to Mars. He occasionally is seen on CNN or other television coverage of these exciting projects.

The matter was not quite over. A journalist for the *Dallas Morning News* had been in the audience for the oral exam. Her two-page article appeared in the paper's Sunday edition, describing the oral exam in detail. Bockris was pictured suppressing the discussion, while not allowing the junior members to speak properly or to ask questions. Nothing was said about the tension—the torture—applied to a student who had worked for six years on his Ph.D., had been congratulated on having done it by the Department Head, being certified as having it by the graduate committee, and then having it torn from him at the last moment by the Department Head's overnight change of mind.

The Department Head sent around a memorandum to the faculty the next day after Packham's oral. He promised that no other orals of this type would ever occur. He apologized to the junior professor, Dr. Soriaga, whose feelings he claimed had been hurt. He wrote, "You have witnessed the chairmanship of a committee by an autocratic professor. . ." Bockris sent this note to the Smithsonian Institution in Washington, DC—a quaint addition to its collection of memorabilia about the discovery of nuclear reactions in the cold and the transmutation crisis at Texas A&M during the 1990s.

The process of academic freedom at Texas A&M University had been strained, some would say broken, by the suppression of Packham's research. Since the work had been published before the Ph.D. degree was resolved, it was in the public domain. Its suppression as a part of Packham's thesis reflects badly on Texas A&M, as do all the other outrages that we have reviewed. Perhaps in the fullness of time, Texas A&M will realize what happened on its campus and take steps to honor John Bockris and his students, rather than to revile them and devalue their historic work.

Excusing Texas A&M?

Since Bockris retired from Texas A&M in 1997, he has thought about the treatment meted out to him. He prepared this assessment for this article:

"The fact that the so-called cold fusion phenomena has been so much confirmed in various parts of the world (2,000 publications!) and that the American Nuclear Society has agreed for the last three years to host sessions on low-energy nuclear reactions, all shows that we were right in 1989 with the first scientific measurements of tritium, and again in 1992 with the first published measurements of transmutation among metals. I stopped counting at 174 papers with the tritium confirmation, because there seemed no point in obtaining further confirmation of our pioneering work. Tritium had its day when its finding was primary evidence for nuclear reactions in the cold, but now the barriers to analyzing helium have been overcome and Melvin Miles and Michael McKubre have shown that helium production is one of the main products, and accounts for most of the excess heat. The production of tritium is no longer of primary importance.

"How is it that a University can react so strongly against a Distinguished Professor who obtains new and unprecedented scientific research results? Is not a university the place for this kind of thing? Such fundamental new and disturbing results might never be tolerated in industrial labs. Further, heads of groups in government agencies are not pleased when something unexpected and fundamentally new is discovered, because it upsets their plans. Thus, where in the United States, is New Science to be created? Is it not in the universities? Do not the words 'academic freedom' mean quintessentially that a man or woman can research what he or she likes and publish results according to what is found? Isn't the fact that they are published in refereed journals sufficient for their intended integrity to be confirmed? All these questions are apparently answered in the negative for Texas A&M University, and this is a tragedy. However, I have tried to look at it from the point of view of the President's office.

"First of all, Texas A&M is without doubt, a football school. I mean nothing pejorative in this, but the fact is that when one speaks in Pittsburgh or Boston or Los Angeles about 'the Aggies,' they are not talking about the Distinguished Professors of the Physics or Chemistry Departments (nor even those in Agriculture); they are talking about the football team.

"One of the higher administration officials at Texas A&M has described to me just how strong the influence of the success of the football team is and what influence it has on the Board of Reagents. First of all, as in other universities, the coach of the teams is reported to receive an income larger than that of the President. The Board of Reagents is the controlling body of the University, and its degree of satisfaction is strongly influenced by the football team! When the Aggies win a game, the donations from rich people to the University increase. But when the Aggies lose, it declines!

"The second aspect of Texas A&M, which has affected what happened to me, I think, is the militaristic background. By now, only about six-percent of the student body are in the officers training corps, but it seems that the idea of 'command from the top' pervades the atmosphere at Texas A&M. Indeed this has come to the fore much more in recent years with a new president who seems to want to have a hand in 'controlling' everything and who has caused a decrease in the atmosphere of relaxation on the campus, which is so necessary to the prosecution of disinterested inquiry. A case in point: The recent persecution of a man in the computer sciences department, based upon the fact that he had used grant monies to support a course he was teaching extra-murally.

"Briefly, the kind of publications which a University of this kind likes are those which confirm the paradigm. Of course, the papers have to be original and have to constitute an advance, for example, as refereed papers published in the *Journal of the American Chemical Society*. These papers should be a little better than the papers which have been published there before. This will disturb nobody and also not make much difference, but it will not scare people and that leaves everyone smiling and happy.

"Texas A&M University, military history, concentration on football, should not, however, be criticized too severely for giving in to the requests of the professors who tried to harm me. In spite of all, the final results were favorable to me, the due academic process held, although I undoubtedly underwent two to three years of totally unjustified persecution. It is worth quoting the situation at Harvard when John Mack published a deep study of what a number of his patients related during hypnosis. The essence of Mack's book, *Abduction*, is to say that the persons concerned passed every test for sanity, but claimed they had been abducted and operated on in space vehicles to provide genetic material. Mack was duly investigated, as was I, but his trials were much shorter (three to four months) and the result more friendly and encouraging to the goal of basic scientific research, the establishment of the new."

References

1. Conway, B.E. 1993. "John O'M. Bockris: A tribute and survey of his research over 47 years on the occasion of his 70th anniversary," *J. Electroanalytical Chemistry*, **357**, pp. 1-46.

2. The scurrilous Gary Taubes book, *Bad Science: The Short Life and Weird Times of Cold Fusion* (Random House) came out in 1993.

3. Taubes, G. 1990. "Cold Fusion Conundrum at Texas A&M," *Science*, **248,** June 15, pp. 1299-1304, with a side-bar by Robert Pool.

4. Chien, C.C., Hodko, D., Minevski, Z. and Bockris, J. 1992. "On an electrode producing massive quantities of tritium and helium," *J. Electroanalytical Chemistry*, **338**, p. 189.

5. Miles, M., Bush, B.F., Ostrom, G.S. and Lagowski, J.J. 1991. "Heat and Helium Production in Cold Fusion Experiments," in *Proceedings of the Second International Conference on Cold Fusion*, Como, Italy, *The Science of Cold Fusion*, Eds. T. Bressani, E. Del Giudice, and G. Preparata, Italian Physical Society, Bologna, Italy, pp. 363-372.

6. Chernov, I.P., Nikitenkov, N.N., Puchkareva, L.N. and Kolobov, Yu. R. 1998. "Change of Isotopic Composition of Metals at Deuterium Charge," *Proceeding of the Seventh International Conference on Cold Fusion*, Vancouver, BC, April 19-24, pp. 441-450. ICCF7 work by Russians, transmutation via impact method.

7. Lin, G.H., Bhardwaj, R., and Bockris, J. "Response to Noninski *et al.*: Observation of β Radiation Decay in Low Energy Nuclear Reaction," *Journal of Scientific Exploration*, **9**, 2, pp. 207-208.

8. Mizuno, T. 1998. *Nuclear Transmutation: The Reality of Cold Fusion.* Infinite Energy Press.

9. Passell, T. 1995. "Charting the Way Forward in the EPRI Research Program on Deuterated Metals," *Proceedings of the Fifth International Conference on Cold Fusion*, April 9-13, Monte Carlo, Monaco, pp. 603-618.

10. Sundaresan, R. and Bockris, J. 1994. "Anomalous Reactions During Arcing Between Carbon Rods in Water, *Fusion Technology*, **26**, 3, November, pp. 261-265.

Exhibit C

"Excuse Me, I've Got a Meeting at 3:00"

Brief Excerpts from an interview with Professor John Fackler at Texas A&M, November 25, 1996, conducted by Eugene Mallove

Eugene Mallove: For what period of time in 1989 would you say you still. . .had an open mind they might find something important, very significant? At what time did you perhaps begin to turn away from that view?

John Fackler: I suppose the time that I began to be less positive about the information fed to us with regard to whether it meant cold fusion. . .The time that I started getting concerned was the time when people started to fail to reproduce results.

EM: For example?

JF: Mark Wrighton [of MIT]. Mark worked very, very hard. I talked with Mark at length when he was doing it. I think Mark was probably more than anyone else quietly went about trying to reproduce the Pons-Fleischmann results. Went out to Utah himself, talked to Pons directly, attempted to set up the lab in a way that he could reproduce the results... It impressed me that Mark was probably the brightest of the people who were really after an effort to try to reproduce those results.

EM: And what else besides MIT, what other results do you remember. . .one that influenced you negatively?

JF: What influenced me negatively was John's results himself, that he had himself here. . .

EM: Which result was that?

JF: The tritium results. The initial tritium results. The work of Nigel Packham, the actual observation of the evidence for formation of tritium as observed by John and as published by John had a flaw in the publication process and I. . .

EM: What was that?

JF: The flaw of the publication process was that they had not calibrated their photolysis system for the formation of tritium. They were using. . .They were in a hurry. . .

EM: What I'm trying to establish, Dr. Fackler, is that at the time of the tritium first being seen, long before Taubes first came into view, was the spring of 1990. So we're back to the spring and summer of 1989. You began to have doubts at that point?

JF: I saw the results. I saw the spectral traces. I asked Nigel Packham and I asked John, "Have you gone back, not only to reproduce it. . ." I knew they couldn't reproduce it. That was the number one problem always, the lack of reproducibility. As a scientist, I don't believe anything until it's reproducible. Okay? That's how I've been conditioned to perform as a scientist. It's an interesting idea, an interesting concept; I don't believe it as factual until there is a defined way of reproducing the result. . .

EM: Taubes originally wanted to go to *Nature* with it, but *Nature* refused after their attorneys looked at it. So then he shopped it around and got it into *Science*. It was five pages and you saw it in advance. What was your reaction, do you remember, when you saw it in advance? I'm curious.

JF: If Gary had interviewed with me, I would have told him he has forgotten about the fact that inorganic chemistry has some very interesting aspects to this question of photoluminescence that were not taken into account in his article, not taken into account by Nigel Packham himself, and so Gary Taubes focused on the fact that John Bockris had tritium in the laboratory in his office, which he did. I saw it; I know he had that. So, he took the easy road out. Now, I would never accuse John of spiking it with tritium. . .

EM: . . .Were you one of those signatories [of the petition against Bockris]?

JF: I'm sure I was one of the group that, in fact, asked that the administration revoke his Distinguished Professorship. Simply because he did, in my opinion, not follow proper scientific procedures and procedures particularly with regards to this press conference. He had been told administratively that he was not to be involved with any press conference activity with regards to transmutation. Prior to Labor Day [1992], actually.

EM: . . .Was that your main reason or was it because you felt not only he was doing something that he was told not to do, but you didn't believe the result at all?

JF: Well, the results have never been produced in a way that are reproducible. Even to this day, and so I have never believed them. Right, that's correct.

EM: Let me ask you something about reproducibility because this is a very interesting point. There are certain phenomena, as you know, that are reproducible in the sense that they eventually get to be highly reproducible. But even then not totally, so they are in some sense sporadic, certainly when they're first discovered, such as in the case of a transistor. Okay, the effect was seen and when the people were trying to . . .when they regarded it as confirmed and it was announced and so forth, but subsequently in trying to make batches of this stuff...that would work...obviously they don't all come out well, you get defects, and some of them don't work. That's an instance. . .

JF: Why don't you use polywater as another example, which is the opposite result?. . .

EM: What is your feeling about now there are two reports that came out from the university. . .

JF: What reports are you talking about?

EM: About the Bockris situation. One report of the first investigation exonerated him fully, it said. Then there was another report that Dean Curry gave me the other day and it also exonerates him with respect to the Philadelphia Project. It just says he's exonerated, okay. There is no punishment for him. There is no demotion of any sort, but there is basically a clean bill of health. What is your reaction to that?

JF: My reaction was very clear and I've stated that very strongly. That the committee that did the first report was given a very narrow charge and they—John Calhoun and John McDermott—are two people I talked to who were on that committee and they're hands were tied in terms of anything more than what they did. They clearly, from the evidence they were given and the charge that they were given, there was nothing further that should be done or could be done with regard to John. It wasn't an exoneration of the question of whether John had overstated the case publicly in Mexico City; that wasn't the issue. It wasn't an exoneration of John with regard to trying to convince his colleagues, like myself, that he had actually produced and reproduced, from the Champion results, gold—without testing whether or not the gold came from that source. That was not the questions that the Calhoun and McDermott committee were addressing. And so, consequently, the exoneration was obvious given those conditions, but my concern was John's failure first of all to deal with the issue properly of presenting scientific information to the public without convincing proof that that result was really correct and getting in bed, as a result of that, with people who were obviously making money on the process.

EM: So, people shouldn't make money over, say, data that isn't. . .

JF: I feel very strongly that if science is to be done that way, then we're in for trouble. I mean, it's easy to get money if you present to the public some ideas and creative concepts that are potentially lucrative, whether or not they're right or not.

EM: What about the opposite situation, where somebody has already a very lucrative program and they conduct research to defend that program...? [*Editor's Note: Mallove was considering the flawed work at the MIT Plasma Fusion Center against Fleischmann and Pons.*] Suppose that research, that money that's already been there, is based on data that isn't quite kosher. What then? I mean, if it's already an established program, then this program says, "Look, we're getting all this money" and then this other idea comes along and they shoot it down in defense of their own effort— partially in defense of their own effort.

JF: I don't know what you're driving at in this particular case in terms of. . .I mean, you do research to test ideas and you do research with a point of view to never until that idea is tested properly to be convinced that that idea is correct. You start from a non-convincing point of view and attempt to disprove what you're observing.

EM: Sure, but you certainly don't want to throw the baby out with the bath water.

JF: No, but it. . .No, that's true you don't. You want to try to deal with science in the world you're dealing with in an open fashion, but you have to be willing to accept every result that is pointing against your result and try to understand it and deal with it. I mean, there are a lot of cases in my own particular field where people have published garbage and it in some cases has cost the world a lot of money to demonstrate that it was garbage. Because the material looked alright. Everything was fine. In X-ray crystallography you can make mistakes. I mean, there was a time when people didn't believe you could make a crystallographic mistake, but that's been shown to be wrong.

EM: What do you think of Dr. Cotton, who apparently doesn't want to meet with me?

JF: You can meet with him in Paris.

EM: What?

JF: You can meet with him in Paris. He's there now.

EM: He has refused to. . .

JF: Well, sure, he read your side of the [cold fusion] story.

EM: And what does he think of it?

JF: He thinks it's not been established.

EM: He thinks it's not established? Let me ask you this... This was a letter that he wrote to *The Eagle* and I'm kind of curious what do you make of that? [**Editor's Note:** Texas A&M Prof. Cotton, in a letter published November 24, 1993 in *The Eagle*, among other negative remarks, states: "Bockris has repeatedly violated every known cannon of responsible scientific research. . .has perpetrated three of the most egregious examples of sick science ever seen. . .made claims that are completely irrational...capped a disgraceful career. . ."]

JF: I would say that Cotton has—and I've read this before—has a gift with words.

EM: Do you think he went too far in that letter to *The Eagle*?

JF: As far as I'm concerned, I couldn't write a letter like this because I don't have that talent for writing.

EM: Do you agree with the sentiments and the choice of language? I mean, let's face it, he's making various accusations against the colleague, Bockris. Do you feel that even though you couldn't write it because you don't have that talent with words, that this is an accurate and. . .?

JF: I can't say that because I can't reproduce those adjectives.

EM: Here it says...

JF: I've read it; I know what it says. But the point is that—trash, I've said trash.

EM: You've said trash?

JF: I've said that publicly.

EM: I'm curious, do you think cold fusion is trash today?

JF: I still have not seen anything that convinces me.

EM: Would you go over and talk to Dr. Wolf and look at his data, his transmutation data, and see that?

JF: At the moment...

EM: He did it in a Pons-Fleischmann cell.

JF: At the moment I am not particularly interested in that, but the point is that there are phenomena that do take place and you've talked about a couple that we don't understand and there may be phenomena here that we haven't completely understood.

EM: Absolutely. Here it is...[*Mallove shows him the published Wolf evidence.*]

JF: Whether or not it's a development of electrochemical energy in such a way that causes deuterium to fuse is a process that, in my opinion, I don't have any authority to talk about.

EM: Well, it turns out that the man over there has these gamma spectra—you can either accept them or not, but he's got them. He was one of those that rejected tritium, which was rejecting. . .

JF: Excuse me, I've got a meeting at 3:00.

EM: Sure. Well, basically, thank you for your time and I'm glad to get your perspective even though it's not mine, obviously.

❑❑❑