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

WHEN TRITIUM FIRST APPEARED in John Bockris's "cold fusion" experiments in late April 1989, the effect was anything but subtle. Overnight the concentration of tritium in the Texas A&M chemist's electrochemical cells increased 10,000-fold. When the tritium appeared repeatedly, in six different cells in one week, it began to look like salvation for cold fusion.

After a year of ambiguous or simply negative experiments, Bockris's tritium data remain not only the single most extraordinary "cold fusion" effect, but also the only compelling evidence in support of the original cold fusion claims. Last June, for instance, it was Bockris's testimony before the Utah legislature, along with that of Robert Huggins of Stanford, that persuaded the state that cold fusion had been confirmed and deserved a $5-million investment. Nine months later, as *Chemical & Engineering News* wrote after the First Annual International Cold Fusion Conference, "[Proponents of cold fusion] point to the observed emissions of tritium as the unassailable signature of a nuclear reaction."

Yet almost from the beginning, researchers familiar with Bockris's experiment, and not enamored of cold fusion, have suggested that his data were perhaps too good and too easy. How was it that his group, within a month of the original cold fusion announcement, was able to produce tritium in quantities that no other U.S. researcher has come close to, even when following Bockris's recipe exactly? Was it truly a fusion reaction, which would require rewriting nuclear physics? Was it some inadvertent contamination? Or was it something more insidious?

Perhaps inevitably, suspicions were raised almost from the first that the tritium in the A&M cells was put there by human hands. As time went on, even members of Bockris's group would express their doubts about the "miracles" that seemingly favored the team. Other researchers, both at A&M and at outside institutions, warned that questions about possible fraud would have to be resolved before the results could be accepted.

But the response of the A&M researchers and administration to these concerns was limited at best. Instead of taking positive steps to guard their results against fraud, Bockris and his co-workers principally offered arguments as to why they thought fraud was unlikely, sometimes exaggerating their case in the process. And the Texas A&M administration, although it has been aware of some faculty members' suspicions and has kept an eye on the tritium work, has done nothing past some preliminary questioning.

Texas A&M University

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

The result is that after a year of experiments that most scientists view with a great deal of skepticism anyway, the A&M researchers are still haunted by this specter of possible fraud. Even Kevin Wolf, an A&M nuclear chemist who worked closely with Bockris on the tritium work, believes that fraud cannot be ruled out as an explanation for the tritium results, although he now believes that inadvertent contamination is to blame for his own results (see box, p. 1301).

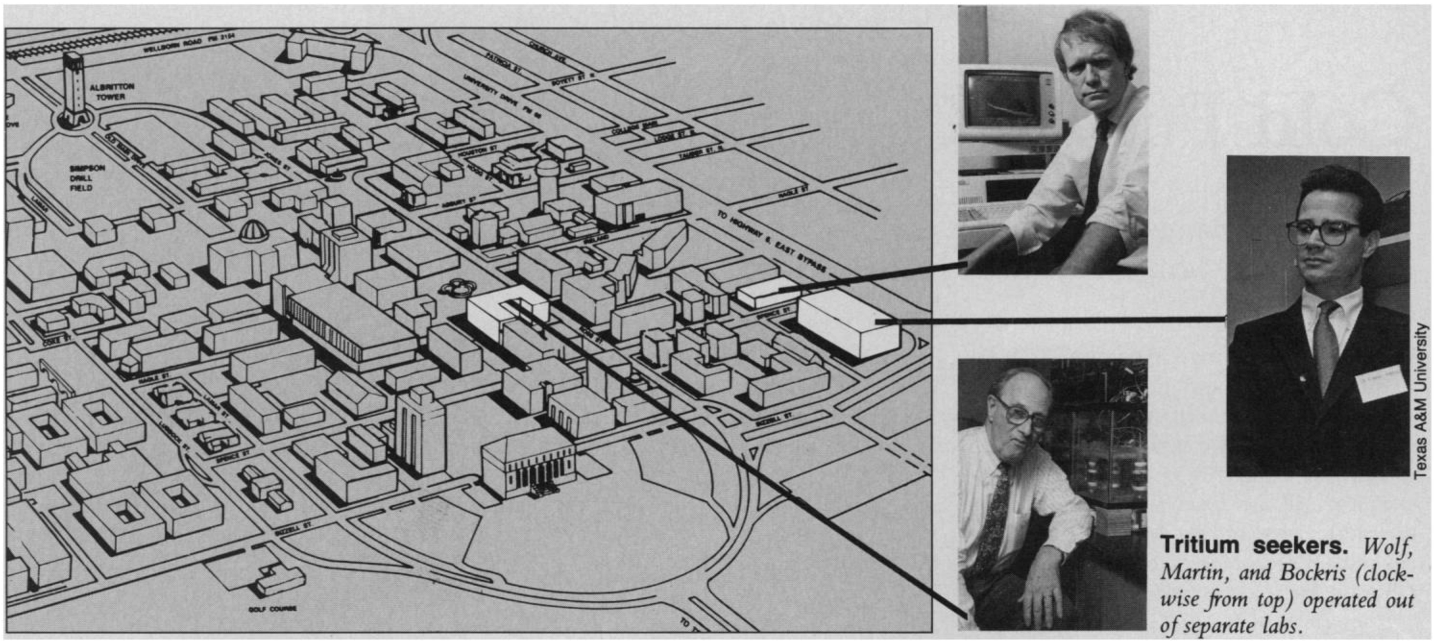
Although the origin of Bockris's tritium may not be resolved for years, the tritium episode has become a case study in the damage done when questions of fraud, legitimately raised, are not seriously addressed by either the lab chief or his institution. It raises crucial questions about how rumors and allegations of fraud should be investigated while ensuring academic freedom and protecting the reputations of scientists, whose careers may be at stake. In an atmosphere of increasing public scrutiny of the scientific process by legislators like Representative John Dingell (D–MI), the scientific community must have ready answers for such questions. And they take on added importance in this case, because of its high profile and the tens of millions of dollars and thousands of scientific man-hours spent chasing after the chimera of cheap, plentiful energy from "fusion in a jar."

Bockris's laboratory was one of several hundred worldwide, and three at Texas A&M alone, that began the chase to confirm cold fusion after the public announcement of Stanley Pons and Martin Fleischmann on 23 March 1989.

Pons and Fleischmann reported that they had initiated nuclear fusion in simple electrochemical cells that consisted of a palladium electrode and a platinum electrode submerged in a bath of heavy water. A current, passed through the cells, caused the deuterium ions of the heavy water to be absorbed into the palladium. At that point, Pons and Fleischmann claimed, the density of deuterium was such that two deuterium atoms would fuse together, producing heat and the requisite products of deuterium-deuterium fusion: neutrons, tritium, and helium.

Both their theory and their evidence, however, contradicted much of what was known about deuterium-deuterium fusion. Nonetheless, Pons and Fleischmann were well-respected scientists, and their claims suggested a cheap, virtually inexhaustible source of energy. The stakes were high.

At Texas A&M, Bockris, an old friend of Fleischmann's, began trying to replicate the claims in early April. Bockris's group constructed several dozen cells and began looking for evidence of the excess heat that Pons and Fleischmann claimed could only be explained by a nuclear process at work. But the calorimetric setup, as Nigel Packham of Bockris's group put it, was "primitive as hell." This was where Kevin Wolf entered the picture. A nuclear chemist who was not associated with Bockris, Wolf began checking Bockris's cells for neutron emission with his detectors at the A&M Cyclotron Insti-



Tritium seekers. *Wolf, Martin, and Bockris (clockwise from top) operated out of separate labs.*

tute. On 22 April, Packham started looking for tritium in the electrolyte—the solution in the cells that carries the current.

Packham carried samples of the electrolyte from three cells across campus to the Cyclotron Institute, where health physics personnel tested them for tritium. Two of the three were "hot," with huge doses of tritium—one trillion tritium atoms in each milliliter sample. "When I heard this number," says Packham, "my jaw dropped." The following Friday another cell turned up hot, and the Monday after that, three more.

On 8 May, Bockris and Wolf attended the special cold fusion session of the Electrochemical Society meeting in Los Angeles, and Bockris presented some preliminary tritium results. Two weeks later, Wolf would give the first full airing of the tritium data at the Department of Energy's cold fusion workshop in Santa Fe.

When he left for Santa Fe, Wolf was openly pessimistic about the relevance of the results. All six tritium-producing cells that he knew about so far had electrodes cut from the same strand of 1-millimeter palladium wire. To Wolf this implied that most likely the palladium wire had contained tritium initially, and that running current through the palladium electrodes had somehow released it into the cells. "I was on my contamination kick," he remembers. Once at Santa Fe, however, Wolf was contacted by the Bockris lab and told that a cell with a 3-millimeter electrode had come up positive. Wolf was temporarily appeased.

Wolf's presentation at Santa Fe sparked the first serious concerns about the validity of the tritium work. The data were simply that remarkable. John Appleby, for instance, an electrochemical engineer at A&M, recalls seeing the tritium results and then asking Bockris bluntly, "Look, concerning this tritium—are you sure that somebody hasn't been spiking your cells?" Appleby had also been running cold fusion experiments, and had reported excess heat but had not seen—and would never see—tritium.

Wolf's report also prompted the Department of Energy's cold fusion review panel to schedule a visit to A&M. The DOE scientists arrived on 19 June. Among them was Jacob Bigeleisen, a chemist at the State University of New York at Stony Brook, whose expertise in tritium work dated back to the Manhattan Project. Bigeleisen was openly skeptical.

When Wolf presented his data, he included a cell in which the tritium appeared while the cell was in front of his neutron counters. No neutrons had been seen. If the tritium had been created in the cell by any known nuclear reaction, from a few hundred thousand to a few trillion neutrons per second should have accompanied its creation. As Bigeleisen told *Science*, the absence of neutrons suggested to him that the tritium had not been created in the cell but had entered through some type of contamination.

Packham presented the bulk of the tritium data to the panel. Packham was a fifth-year graduate student who had spent 3 years at Bockris's and Fleischmann's alma mater, Imperial College in London. With Jeff Wass, another graduate student, Packham had run the tritium studies.

Packham's key evidence was the appearance of tritium in a cell, known as A7, on 28 April. With A7, Bockris had wanted to catch a cell in the act of producing tritium. The current on this one cell was cranked up for 12 hours, and four samples were taken, each several hours apart. When the samples were counted, Bockris's group had hit the jackpot. Not only did tritium appear in the cell that day, but the multiple assays caught the tritium increasing with time: From background levels at noon to slightly above background at 2 p.m., to 5 trillion tritium atoms in the evening and 7.6 trillion near midnight.

To these four points, Packham had drawn a smooth S-shaped curve, indicative of the kind of gradual effect common in chemical reactions. Bigeleisen was unimpressed.

"He had four data points," says Bigeleisen, "to which they drew this hysteresis curve. I said, 'Well, your data do not uniquely define that curve. I could equally well draw the following kind of graph through your data—go flat across at zero, until a point around 6 hours, go straight up with a step function and go flat across again.' At that point Kevin Wolf said, 'Jake, are you implying that someone spiked that sample?' And I said, 'Kevin, you said that. I would never say such a thing.' "

Such spiking would be easy to do and difficult to detect. Anyone with access to a bottle of tritiated water—water with some of the hydrogen atoms replaced by tritium atoms—could remove a few drops of the radioactive water from the bottle with a syringe and inject it into the cells in seconds.

During a tour of the lab, Bigeleisen had asked Packham about possible sources of tritium in the lab, and Packham had replied that they did have a bottle of tritiated water, five millicuries worth. According to Packham, this was more than enough tritium to spike all the cells. Nevertheless, Packham still considered the possibility that anyone would have done so "ridiculous." Indeed, neither Bockris nor his researchers seemed ready to face the possibility of fraud. Several told *Science* that the accusations seemed unreal, that they simply could not take seriously the idea that one of their own colleagues would deliberately falsify data.

In early July, Charles Martin, another A&M electrochemist doing his own cold fusion experiments, tried to convince Bockris that if anyone thought the cells had been spiked, then Bockris's responsibility was to run cells in such a way that they would be beyond suspicion. At a July meeting of the various A&M researchers studying cold fusion, Martin offered to run Bockris's cells in his—Martin's—lab. "We will lock the lab," Martin said, "have very limited access, and see how it works."

Although Bockris would later say that he even had a suspect in the early days, he did little to ensure that the ongoing tritium experiments were not being tampered with. He never took Martin up on his offer to lock the cells in Martin's own lab, nor did he lock up the cells that were running in his—Bockris's—lab. And, says Ramesh Kainthla, an Indian postdoc who was the senior member of Bockris's team at the time, no one locked the tritiated water away or got rid of it entirely.

What Bockris *did* do was twofold: First, he removed Packham from his job of sampling the cells for tritium. But not, however, because he considered Packham a suspect. Packham, who was running the tritium experiments, had become the natural focus of attention. "I tried to get Packham off," Bockris says, "because by that time all these stories were floating around. Nigel spikes the tritium. Everyone thinks Nigel spikes the tritium." Bockris replaced Packham with Kainthla and Omo Velev, a Bulgarian physicist, both of whom had been working on the heat measurements. From then on, Packham says, he made a conscious effort to stay away from the tritium work.

Secondly, Bockris offered what he considered convincing arguments for why the cells that had already come up hot could not have been "sabotaged." In Bockris's first paper on the tritium work, written in mid-summer and published in the *Journal of Electroanalytical Chemistry*, he wrote off the allegations: "Interference with the experiments is considered improbable because of positive results from the Cyclotron Institute to which entrance is prohibited except by the usual personnel at the Institute."

To those who knew the Cyclotron Institute, however, Bockris's defense was unconvincing. "Any graduate student could have gotten into that lab," says John Huizenga, cochairman of the DOE cold fusion review panel on which Bigeleisen served. "It's not a bank vault."

Moreover, when Bockris wrote his paper, only two of the positive cells had come from Wolf's lab at the Cyclotron Institute. The Institute had no guards on nights or weekends. Anyone with the necessary keys could get in unquestioned and those keys had readily been given to Bockris's researchers so they could tend to the cells when necessary.

Bockris's group also took to presenting the protocol for cell A7—the cell that produced tritium while being monitored over a 12-hour period—as proof against the spiking accusations. But they would exaggerate the details to do so.

In October, for instance, at a workshop co-sponsored by the National Science Foundation and the Electric Power Research Institute (EPRI), Wolf would report that this cell "was done at the Bockris laboratory by dedicated graduate students, four of them, standing over the cells."

In November, when Packham was asked about the spiking accusations, he would explain that the cells "were under guard for that time, 24 hours a day, 7 days a week. There was one cell [A7] . . . that shows the

Wolf: My Tritium Was an Impurity

Many scientists have considered Kevin Wolf's reports of tritium the hardest to dismiss among all the claims of cold fusion. Wolf, a Texas A&M nuclear chemist, is widely regarded as a careful and skeptical researcher, so when he said last fall that he was having difficulty explaining how tritium was appearing in electrolytic cells run in his lab and that of A&M chemist John Bockris, people listened. Now Wolf appears to have knocked perhaps the last prop out from under the shaky claims of cold fusion.

"It's pretty clear that our low-level tritium was [due to] contamination," Wolf says of the cells that turned hot in his lab. And since several other labs that saw tritium in fusion cells got their palladium from the same supplier as Wolf—Hoover & Strong Inc. from Richmond, Virginia—the contamination that Wolf found throws added doubt on much of the tritium data reported in this country.

To look for tritium contamination Wolf completely dissolved a number of palladium samples, including electrodes from cold fusion cells, electrodes run in light water blank cells, and virgin palladium. He found low levels of tritium contamination in both the virgin palladium and the blank cells. The latter result was particularly surprising since the palladium electrode from the light water blank had been vacuum annealed before use, a process believed to drive all the tritium out of the metal.

His contamination findings do not necessarily apply to the tritium results in the Bockris's lab, Wolf says, even though both labs bought palladium from the same company. Bockris's cells showed tritium levels much higher than are consistent with the amount of tritium Wolf discovered hiding in the palladium. In trying to understand where the tritium came from, however, Wolf made a second finding that raises other questions about the Bockris data.

Wolf also tested some of the electrolyte from a fusion cell run in the Bockris lab that had shown a very high level of tritium. He found that the electrolyte, which had been stored in a sealed container since last year, contained a large amount of light water. Though there are other possible explanations, this result is consistent with the hypothesis that the cell had been spiked with tritium. Tritiated water contains a large amount of normal light water, and if someone had spiked the cell with tritiated water, he would have left the telltale light water behind as well.

After hearing of Wolf's discovery of light water in one of their cells, the members of Bockris's group examined eight more of their cells, says team member Nigel Packham. All eight cells, including two that had been sealed, had large amounts of light water, between 30% and 90% of the total water content of the cells, he reports. The light water could have gotten there if the electrolyte were exposed to the open air for some time, he notes. Over time, water molecules from the atmosphere will trade places with the heavy water in the solution.

"It's just incredible, I don't understand it," Wolf said. He has checked 50 cells in his own lab and found no more than 1% light water—usually much less—in 48 of them. "The proper conclusion," Wolf said, "is that things [in the Bockris lab] were so uncontrolled and so sloppy [that] those studies don't mean anything."

Packham, although concerned with the light water contamination and Wolf's discovery of tritium in the palladium samples, says he and Bockris are not ready to abandon their tritium results. "Our feeling here is that it is unlikely that we would have produced the tritium levels that we have from latent low-level contamination."

■ ROBERT POOL

buildup of tritium as a function of time, where four people were standing there the whole 12 hours in front of the cell when the samples were taken."

The reality was much less iron-clad. Kainthla had taken the third and fourth assays on A7, the only two that showed high tritium concentrations. These two were taken after hours, when the lab was empty. "If you think people were watching the cells all the time," Kainthla said, "that's not true. Watching the cell meant a person is in the lab, and once in a while [that person] came in and checked that the current was passing through the cell and nothing unusual was happening." And he added, "If you want to do some mischief, you don't need a couple of hours. You can do it in a very, very short period of time."

While Bockris continued running the same cells through the summer and fall, Martin and Wolf separately set about testing the "inadvertent" or "spot" contamination theory, which both considered the most probable explanation for the results. "The sudden appearance of tritium activity in the cells," as Wolf said later, "requires the tritium to be loaded in a component prior to the beginning of cell operation."

In late July, the university relocated Bockris's laboratory to a new wing in the chemistry building. Wolf had health physics personnel search the old lab for possible sources of tritium contamination. They gave it, Wolf said, "a clean bill of health." Wolf also recruited Packham and his colleague Jeff Wass to take shavings off the lab equipment. They tested them for tritium contamination and found none.

In August, Wolf and Martin both began running a series of new cells along with light water control cells to do a proper statistical analysis of the results. If spot contamination was producing the tritium, they expected to see it in light water cells as well. Wolf had Del Lawson, a graduate student who works in Martin's lab, construct a dozen cells for him—six light water controls and six heavy water cells—in a basement laboratory at the Cyclotron Institute.

Lawson also made duplicates of the Bockris cells for Martin to run in Martin's lab, with an equal number of light water blanks. Martin wanted Bockris's protocol duplicated exactly. He had Lawson go so far as to use the same heavy water that Bockris did, and even the same color of rubber stoppers.

"We weren't getting results," explained Lawson. "People would say, 'Well, you're not doing it right.' The indications were that it was some kind of black magic. You had to do it exactly this way, for this long, to get the results. So we wanted an exact duplication. And if we did get something we would need a lot of blanks, light water cells, to answer the critics."

This time, two of Martin's new cells were made from palladium donated by Bockris. With these, Martin wasn't satisfied with locking them away in his own lab. He took them home and ran them in his second bedroom to ensure that they couldn't possibly be sabotaged. Martin never saw tritium in any of his cells.

In the last week of September, two more cells came up positive—the first in nearly 3 months—and worries about spiking immediately resurfaced. One of the two cells was among Wolf's dozen, cell D6. It came up positive in front of Wolf's gamma ray detectors, but no gamma rays were seen, which indicated that no nuclear process had taken place. The other was one of Bockris's cells. Labeled cell 4, this one had been running since May and had just begun producing excess heat, as measured by Bockris's calorimetry. (This was the only cell in which the team saw both heat and tritium.)

Mike Hall. *"There's a very large burden on the accuser" to prompt an investigation.*

Omo Velev, the Bulgarian physicist who had been doing the tritium assays with Kainthla, found the timing of the positive results suspicious. He had been away on vacation when the two cells came up hot. When he returned and heard about the newest findings, he went to Bockris with his suspicions. In particular, he was uneasy about the correlation between the dates the cells came up hot and visits of funding agents from EPRI.

EPRI had sponsored fuel cell work at A&M for years. In 1989, for example, EPRI split $150,000 between the research of Bockris, Martin, and John Appleby. When cold fusion came along, EPRI quickly doubled this amount. In July, EPRI spent $25,000 on Bockris's new tritium counter. In the autumn, A&M submitted a proposal to EPRI, requesting $1.4 million for the cold fusion research of Bockris, Wolf, and Appleby.

Between June and December, EPRI funding agents made two trips to A&M to discuss additional cold fusion funding. After tritium appeared in a cell the last week of May, during the Santa Fe meeting, no cells turned up hot until 3 July. On 5 July, Rocky Goldstein, the EPRI project manager for A&M, visited Bockris's lab, the first of the two visits. Then no tritium appeared for 3 months, nor did any EPRI officials.

On 27 September, Dave Worledge and Bindy Chexal of EPRI visited A&M for 3 days to discuss the A&M cold fusion proposal. Between 24 and 28 September, cell D6 came up hot in Wolf's lab; and between 21 and 25 September, cell 4 came up hot in Bockris's lab. (Three cells were discovered with tritium in the last week of July and the first of August, but no one could say exactly when the tritium appeared in the cells. One was a cell that had been abandoned in the old laboratory, and the other two were found by Kainthla and Velev when they took over the tritium detail. There was no way to tell if the tritium appeared in the cells the day before they were assayed or 6 weeks before. All three had low levels of tritium.)

Velev recalls that when he went to Bockris with his suspicions, Bockris told him not to worry about the coincidences. So many other laboratories had seen tritium, Bockris said, that to suspect theirs to be illegitimate was foolish. This would be Bockris's recurring position.

For instance, in an 18 December memo to John Fackler, dean of the College of Science, he wrote, "The best evidence that the tritium we see is real, not subject to these extraordinary explanations, is the stream of people who now have verified our work. We were simply the first." The labs, as Bockris listed them, were:

"Packham *et al.*, TAMU [Texas A&M]
Wolf *et al.*, TAMU
Iyengar, Bhabha Atomic Research
Storms and Talcott, Los Alamos
Menlove *et al.*, Los Alamos
Yeager and Adzic, Case Western
Ramirez, Institute of Petroleum, Mexico
Scott (C.D.), Oak Ridge
Schoessow and Wethington, Gainesville
Guruswamy, Utah."

Of these, however, only Iyengar had claimed to see tritium at the levels 100 to 10,000 times background that had appeared in the Bockris lab. The others either reported much smaller levels of tritium—levels that could easily be explained by small amounts of contamination—or hadn't formally reported anything at all.

Throughout October and most of November, no tritium appeared at A&M. During this period, Kainthla left the lab to work in industry. Velev was left in charge of the tritium assays.

Then came 27 November. Although Bockris had taken Packham off the tritium detail in August, Packham nevertheless decided on the night of the 27th to assay a handful of cells in the Bockris lab. He had a flight to Utah early the next morning for a job interview at the National Cold Fusion Institute, he said, and he wanted to check for any new results before he left. Among these were two cells with titanium electrodes that had been running for months.

"Having gone through what Bigeleisen was saying," Packham explained in March, "what a lot of other people were inferring, I had decided to stay away from that environment, if I could. And yet, these two cells [were] sitting there for 3 months, without having been sampled. I kept saying, 'Why doesn't somebody sample those, why doesn't somebody sample those?' So I did."

He found that both the cells with titanium electrodes were hot. Packham called Velev and told him about them, and said he had a witness, Del Lawson, who had been in the lab checking assays from his own cells.

The next morning Velev said to Lawson, "Too many goddamn miracles in this laboratory." Lawson agreed.

Once again, Velev took his worries to Bockris, in particular this latest coincidence: Packham sampling cells for the first time in months, and hitting two right off the bat. As Velev remembers it, Bockris again dismissed his fears as irrational.

"I was getting depressed," said Velev. "I told him, 'Listen, I'm very suspicious about the results. I'm not convinced they're true. The timing is very suspicious.' He said, 'Well, okay, thank you for your consideration. I'll keep this in mind. . . . ' " A week later, Velev told Bockris he was leaving the group to work for Appleby, because Appleby would not make him work on cold fusion.

While Bockris's responses to concerns about spiking were weak, the response of the Texas A&M administration was even weaker. Several administrators had been keeping an eye on the cold fusion research on campus and had been aware that spiking was a possible explanation for the tritium results. But they had decided there had been nothing that warranted official action, says Dean Fackler. After 27 November, however, Martin started to push the issue.

"I resisted for a long time the possibility that there was fraud going on at Texas A&M," Martin says. He had discounted the spiking theory because he believed Bockris and Wolf when they insisted that security at the Cyclotron was too tight for the three results there to have been due to spiking. But when he looked into that security, he found it considerably less than advertised. It would have been "easy" for someone to spike the cells there, he concluded.

Martin went to Mike Hall, head of the chemistry department, and voiced his suspicions. "I warned Hall that I thought there was a very good chance the experimental results were the result of fraud," Martin recalls. Hall then checked with Fackler about A&M's policy toward fraud.

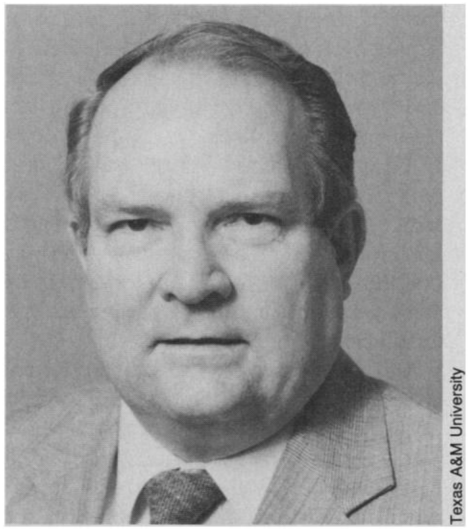
At the time, the A&M administration was revising its fraud policy. The current version seemed to have no provision for an investigation without a faculty member willing to press the case. "I had to publicly act as an accuser," Martin says. Although Martin was seriously concerned about possibile fraud, he says, he felt that all the evidence was circumstantial. "I can't go before a committee and accuse anyone of scientific fraud when all I have is circumstantial evidence."

Fackler now took a closer look at the tritium results. A week earlier, he had received a memo from Bockris in which Bockris suggested that Fackler may wish to set up a committee to monitor the work. He did not do that, but he did query Dave Youngblood, director of the Cyclotron Institute, about security there. Youngblood agreed with Martin: Security on nights and weekends was nonexistent, and many people on campus had keys to the building.

Wolf insists that his lab in the basement, at least, where D6 had been running, was locked at all times and needed a different key. Lawson, however, who had been tending those cells, says the door to Wolf's lab had only been locked after tritium had appeared in D6. Either way, it may not have mattered: Youngblood told Fackler that the key to Wolf's lab was not unique. At least 35 faculty and lab personnel had keys that would open that door.

In response to Fackler's concerns about the security at the Cyclotron Institute, Bockris offered a second argument against spiking. In his 18 December memo, he wrote: "This possibility [that the tritium was put there by someone] has been taken seriously by us from the beginning. . . . We have monitored a certain flask containing tritiated water purposely left in its original position. Not only did we note the level of the water in the flask but we also measured its tritium content. It has remained unchanged. . . ."

Bockris's confidence, however, seemed to be unjustified, judging by yet another memo posted at the time in Bockris's laboratory.



John Fackler. *"We were concerned right from the beginning."*

Dated 4 September 1989, it was from Packham to "the electrochemistry lab." Packham wrote that he had just completed the inventory of their supply of radioactive substances. What he found seemed to imply that it would be difficult to know how much tritiated water was in the lab, let alone monitor its level and concentration.

"There are many radioactively labeled bottles," Packham wrote, "which have no description of radionuclide, total and specific activity, the person using the material, the date on which it arrived in the lab, etc. There are also some bottles or flasks which are broken in the freezer, and which are standing in beakers, again unlabeled . . . ."

On 26 January, Martin sent Fackler the final results of his experiments at A&M: "*None* of the 83 cells which were run by my students in my laboratory have produced tritium levels above those predicted by the (known) separation factor."

Once again, Fackler took up the issue with Bockris. Why couldn't Martin replicate his results? A memo Bockris wrote on 2 February in reply showed he was unfazed by this argument.

"My tentative judgment as of today," he wrote Fackler, "is that a new field of chemistry has been formed." As for "[w]hy cannot Dr. Martin succeed? . . . *We* cannot succeed either for long periods of time (e.g., 6–8 weeks). The important thing is when we do succeed which may be 10 weeks after we switched on the electrolysis."

Through the end of May, A&M still had mounted no formal inquiry into the Bockris tritium data, although interviews with members of the administration showed that they had their own concerns. Hall reflected on the need to have a policy that allows for a confidential inquiry into possible fraud—

without demanding a Grand Inquisitor. "That's been part of the problem—there's a very large burden on the accuser. We have to find someone who is willing to put his own reputation on the line."

Although Hall and Martin saw the draft policy as implying that an accuser was necessary, Duwayne Anderson, associate provost for research and graduate studies, says that is not the case. With enough evidence, the administration can set up a committee to investigate, he says.

Anderson added that he and Fackler and Hall have been following the tritium results closely to see if an inquiry was warranted. Last week, they met after hearing that Wolf had found light water—a possible sign of spiking—in an electrolyte sample left over from one of the Bockris cells that had produced a lot of tritium (see box). The result of the meeting: "I haven't quite passed the threshhold of being sure that we have enough evidence to go forward with an inquiry," Anderson said.

The A&M experience illustrates how tricky it can be dealing with possible fraud. A university must find a balance between making it too easy to start an investigation and making it too hard. One lesson here may be that demanding someone act as a formal accuser or whistle-blower is too restrictive. An even clearer lesson seems to be that a university should have a well-defined fraud policy in place before problems arise. Martin says, "Part of the problem of why the university didn't do more is that it is just now coming up with a policy. There was a lot of confusion on what the policy is."

But there is an even deeper issue that is not so clear-cut: the question of at what point scientists should stop being scientists and start being fraud investigators. Fackler and Anderson say they believe this point has not yet been reached in the case of Bockris's tritium results. "Our people say this is an area of dispute arising from conflicting data," Anderson says, and the proper way to deal with it is to continue to do experiments to determine what has been going on.

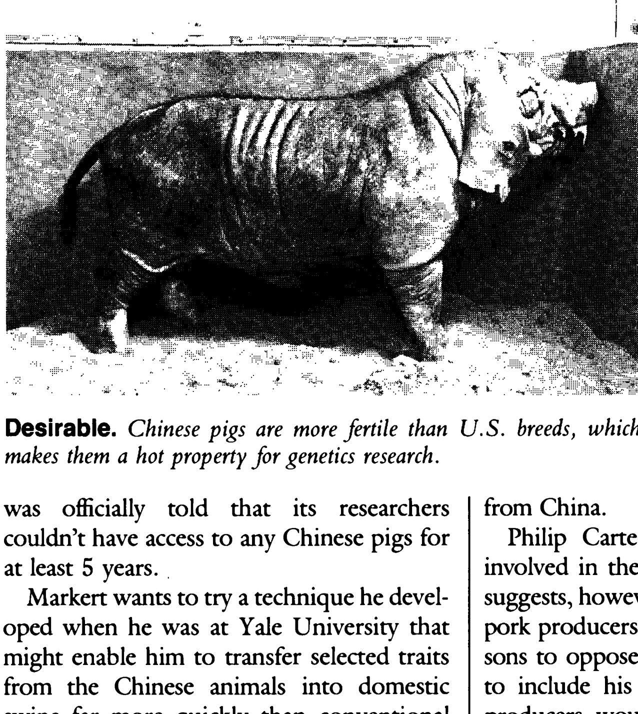
Both Fackler and Anderson pointed to the case of "polywater" 20 years ago, where dozens of researchers chased a chimera created by minute contaminants in measuring instruments. That experience showed that bad science comes out in the wash and, barring further evidence of possible fraud, that is what the A&M administration will let happen. It is important to keep in mind, Anderson pointed out, that "honest error and misinterpretation" are excluded from the definition of "fraud." ■ GARY TAUBES

Gary Taubes, a science writer, is working on a book on cold fusion.

North Carolina Protests Chinese Pig Cartel

NC State researchers want access to Chinese pigs but all the animals are owned by other institutions who won't give any up

CLEMENT MARKERT, a biologist at North Carolina State University, would love to get his hands on a Chinese pig. But he has a problem: Every Chinese pig in the United States is owned by the Agricultural Research Service (ARS), Iowa State University, and the University of Illinois, and they have a pact not to let any of the animals out of their facilities. Last month, North Carolina State was officially told that its researchers couldn't have access to any Chinese pigs for at least 5 years.



Desirable. *Chinese pigs are more fertile than U.S. breeds, which makes them a hot property for genetics research.*

Markert wants to try a technique he developed when he was at Yale University that might enable him to transfer selected traits from the Chinese animals into domestic swine far more quickly than conventional breeding techniques would allow (*Science*, 6 October 1978, p. 56). The attraction of Chinese pigs is that they have much larger litters than U.S. breeds—an average of 15 offspring per litter, compared with 10 for domestic breeds—and they reach sexual maturity much earlier. Markert says he is "outraged" that the three institutions are "monopolizing a scientific resource paid for in part by public funds." Says Markert: "It's clear they don't want anybody to compete with them."

Reg Gomes, dean of the College of Agriculture at Illinois, says the three institutions paid to bring the pigs into the United States last year under a carefully worked out contract and they decided it would be unwise to alter the arrangement now. (The ARS's share of the costs was, of course, federal money.) NC State had an opportunity to join the Chinese pig consortium when it was formed 2 years ago, Gomes says, but the university turned the offer down. "To reopen this now would not be productive," Gomes says.

Part of the reason for the tight controls over the animals, Gomes says, was to allay fears of pork producers that undesirable traits of the Chinese pigs—they are fat and grow relatively slowly—might be transferred into domestic breeding stock along with their high fertility. There was also a desire on the part of the pork producers to keep germ plasm out of the hands of private companies, but this was rendered moot last year when Dekalb Genetics of Sycamore, Illinois, imported its own batch of semen directly from China.

Philip Carter, an NC State researcher involved in the negotiations over the pigs, suggests, however, that the Illinois and Iowa pork producers have direct commercial reasons to oppose broadening the consortium to include his university: North Carolina producers would then have access to the research. Gomes acknowledges that the advice of the Illinois and Iowa pork producers' associations and the National Pork Producers' Council must be sought before any changes are made to the agreement. In Illinois, at least, the pork producers helped support the university's request for state funds to bring the pigs over, but "there was no promise, real or implied, that they own the germplasm or control the germplasm," says Gomes. "We have no intention of locking this research up and making sure it doesn't get out of Illinois."

But NC State officials are not taking "no" for an answer. They have enlisted the help of their senator, Republican Jesse Helms, who has called senior Agriculture Department officials. Helms is particularly miffed be-