Peterson, C., *The Guardian Poplar, A Memoir of Deep Roots, Journey and Rediscovery*. 2012, Salt Lake City: The University of Utah Press.

Here are selected portions of chapter 12 from Peterson's book The Guardian Poplar.

"THEY WILL ONLY LAUGH AT YOU": Cold Fusion

"PRESIDENT PETERSON: REVIEWING THE UNIVERSITY'S DEPARTMENT OF Chemistry has been a pleasure. The department is strong and healthy with an able corps of young faculty, clearly poised to deliver national visibility."

Such is my memory of the words of Richard Bernstein, professor of chemistry and biochemistry at UCLA. Of all the stories I have shared, this encounter introduced events and issues that evolved into some of the most interesting and challenging I ever experienced.

Every five years, each department of the university conducts an external review. It is a thorough process, chaired by a distinguished peer from an outside university, assisted by two or three other experts. The preliminary report goes to the department, and a copy is sent to the dean of the graduate school. The dean evaluates the review and sends a summary to both the department chair and University president.

I had never received a face-to-face report, but in this instance, Professor Bernstein asked for an appointment. I agreed, and we met. "President Peterson, there is one additional matter you should know about that I am reluctant to put in the written report. You have what could be an extremely important project under way in the department that will be hard to keep confidential," Bernstein confided.

He went on to describe work done by Professor and Department Chair Stanley Pons, together with Research Professor Martin Fleischmann, focusing on "the generation of heat from an unknown process or processes that appears to be too great to be a result of a chemical reaction." Knowing a dusty bit of chemistry and physics, and impressed that someone of Professor Bernstein's stature would make a special effort to request a personal appointment, I took him seriously. I called in my vice president for research, Jim Brophy, a physicist, and asked him, "Jim, what can you tell me about some heat-producing process that two professors in the chemistry department are working on?"

That "process," as most remember, came to be called "cold fusion." Fusion because it appeared to produce more heat than a chemical reaction could generate and might be explained by a similar process to what occurred at high temperatures and pressures when atomic nuclei underwent fusion and released huge amounts of energy. It was described as cold because the process took place at room temperature. Within a week, Brophy told me what the world would hear some months later.

Martin Fleischmann was an internationally respected and honored electro-chemist—a field that I came to understand existed at the interface between traditional chemistry and physics. He was a fellow of the Royal Society, the British equivalent of our National Academy of Sciences, and had received the international Olin Palladium Medal in 1985, the top honor in his field of electrochemistry. Note the word “palladium”; it becomes increasingly significant. In 1988 he had received the Bruno Breyer Medal from the Royal Australian Chemical Institute. After a long and distinguished academic career, he had retired from the University of Southampton and accepted a part-time appointment at the University of Utah as a research professor to work with Professor Pons.

Like Fleischmann, Pons was also an electrochemist. He had studied at Southampton, where he received his PhD and had known Fleischmann. Thereafter he had been on the faculty at a number of Canadian universities before coming to the University of Utah. His fellow faculty members had recently nominated him to the important post of chair of the department, one that obviously carries with it the respect of his colleagues and the dean of science.

How I wish I could have had direct access to the minds and hearts of Pons and Fleischmann during this period and been privy to the pouring out of their passions as well as all the reasoning behind their decisions over the next months. But as well as being highly professional, both were very private people. Still, I will do my best to record this complex and still-relevant period in the history of the University of Utah and its impact on the faculty and president.

The work Professor Bernstein considered unusually important involved electrolysis of heavy water to promote the transport of a large number of deuterium ions from the molecule of heavy water into the submicroscopic lattice structure of the metal palladium. The investigators claimed that this process released more heat than could be explained by any known chemical reaction. . . .

THE ROAD TO NEW SCIENCE

Since World War II, the funding of science had shifted almost entirely from private laboratories to the federal government. Private labs at established companies like General Electric and Bell had largely given way to multiple federal agencies, such as the National Science Foundation, National Institutes of Health (NIH), the Departments of Energy and Defense, and the U.S. Naval Research Laboratory. This shift moved the decision making and funding of research from a mostly private corporate context to a quasi-public one. While the new process was designed for scientific peers to review proposals, these peers undoubtedly found themselves influenced by their colleagues, existing norms, and the power of institutional consensus. Congress specifically, as well as scientific associations within and outside the government, grew to have an interest and say in the awarding of research support. Still, peer review was probably the best possible process to assist in funding good science and weeding out academic cronyism.

Considering Professor Fleischmann's stellar international reputation in the field of electrochemistry, and imagining the potential importance of the discovery of even a low-grade source of energy generated by the supply of heavy water in the oceans, Jim Brophy and I took his findings very seriously.

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I have waited for the dust and mud to settle before writing this "progress report." Up to now the most thorough study is Charles Beaudette's book, *Excess Heat: Why Cold Fusion Research Prevailed*, published in 2000 by Oak Grove Press. As we say in medicine, "How is the patient doing?" My report, of course, will not be the last chapter in the story, for there is likely never a final chapter in science. But I invite you to join me in this journey that splashed angry mud on the early wet days and now forges ahead more easily in breaking sunshine.

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Fleischmann reportedly said (for reasons never clear) that the University of Utah had required the two investigators to go public when they did. When I subsequently asked for clarification from the relevant university office, people there clearly stated that their policy was to honor all faculty requests with respect to publication and announcement, not initiate them.

The announcement came on a spring day: March 23, 1989. Jim Brophy and I walked over to our campus's nearby Eyring Chemistry Building, named for the same Henry Eyring I'd known in my college days in Boston. At least fifty others—mostly chemists, but also physicists and community guests—were gathered in the auditorium.

Brophy opened the meeting with a few remarks and called on me for a brief introduction. In my prediction of a long period of debate about the material Fleischmann and Pons were presenting, I appear to have been more prescient than I imagined at the time. Here are the remarks I presented that day:

> We are here today to consider the implications of a scientific experiment…. First, what is an experiment? An experiment is an informed probing of the unknown under controlled circumstances. Does it always give clear and full answers? No. Science grows like rings on a tree, each larger, but shaped by the inner rings from which it grew. The full story of the research Professor Pons and Professor Fleischmann will announce today will not be known for months or years, as others confirm, challenge, and enlarge their ideas and their data. The breakthrough they will report today comes from the work of trained minds working at an old problem from new perspectives. This particular study examines a traditional problem in physical science from the chemist's point of view, specifically from that of electrochemists.
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> This university prides itself—whether in creative writing or dance or chemistry or genetics or artificial organs—on a long tradition of intellectual freedom and a willingness to try new ways to solve old problems. This announcement today is an expression of that

ancient and honorable process called "the university," where trained faculty and dedicated students work side by side in processes of teaching and research. Its products are educated minds and new knowledge. Those minds and that knowledge flow to the benefit of the people of the world generally, and to the cultural and economic well-being of the state of Utah specifically. It has been an honor to observe such processes.

Fleischmann and Pons then presented their findings, which—if confirmed by others—would significantly presage an interesting, but undefined, new area of science and a source of possibly nonpolluting energy. I remember the quiet, modest, and dignified tone of the press conference.

THE RESPONSE

Within days a great furor arose. Some said that the claim simply was impossible because it contradicted the laws of nuclear physics. Others attempted to replicate the experiment and could not. A few reported success.

Within a week, the question arose about what the state of Utah should do—if anything—to support such potentially important findings. Ian Cumming, a member of the state Board of Regents and a successful business developer, was the first to raise the matter of financial support for further research. The then-governor of Utah, Norman Bangerter, promptly asked the legislature to appropriate five million dollars to support the cold-fusion research. The legislature asked that I testify on the matter.

I testified that I was not in a position to "believe or not believe" in cold fusion. Nevertheless, I clearly favored the support of such highly regarded scientists in a matter of this potential importance. If the claim was confirmed, and it could advance to the level of commercial development, the world would change for the better. The university, the state of Utah, the nation, and the entire world stood to benefit. Yes, I said, I was emphatic that the work was worth five million dollars of preliminary support. Following my testimony and others, the legislature made the appropriation.

On April 26, 1989, the U.S. Congressional Committee on Science, Space, and Technology convened hearings on the matter. In his opening remarks, the chairman, Robert A. Roe, averred that "this announcement [by Pons and Fleischmann] preceded the traditional submission to a scientific journal where the article would be reviewed by other researchers in the field."[1] In fact, the article had been subject to peer review.

This misstatement of professional sequence just days after the announcement took on a life of its own. The impression that a prepublication peer review had not taken place may have arisen from the article being published in a nonphysics journal. Still, the *Journal of Electroanalytical Chemistry* was widely acknowledged to be the leading publication in the field of electrochemistry. Therein, perhaps, began the controversy over "disciplinary ownership" of the findings, whether they belonged to chemistry or physics.

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The next weeks grew increasingly noisy and soon painful. The covers of many of the national weeklies showed the tabletop apparatus that seemed so simple but symbolized so much.

There was an early confirmation from the Los Alamos National Laboratory and Texas A&M. The Italian Atomic Energy Laboratory reported production of tritium, a signature of a nuclear process. Some other reports, though, were negative. Then there were one or two retractions of early confirmation reports. As it turned out, the preliminary report that Fleischmann and Pons had produced provided less information than what was required to replicate their work. This may have occurred because the Utah processes were in flux, or because Fleischmann, as he later said, was concerned about British atomic-energy laws, which he felt severely controlled public reporting on anything relating to atomic weaponry. Moreover, there was much still unexplored and undiscovered by Pons and Fleischmann about the process.

The heart of the criticism derived from certain canons of nuclear physics, positing that if the claimed excess heat came from a nuclear process, byproducts should have been readily measurable and even lethal to the investigators. The tritium reported from the Italian Atomic Energy Laboratory was the only confirmation initially of nuclear products. In any event, within a few weeks, there was growing condemnation of the Fleischmann/Pons claims because of the paucity of nuclear evidence and the failure of a majority of labs to produce excessive heat.

For some time, it was impossible for Fleischmann and Pons—and other labs that had recorded at least one episode of excess heat—to be certain of all the conditions required to produce a scientifically acceptable positive result. The major "event" had occurred over a weekend with no one in the lab. In the meantime, what had been a clamor grew to the level of disbelief and even charges of fraud. It was only months later that scientists at cooperating laboratories—particularly Michael McKubre at the Stanford Research Institute—found that heat production occurred only at a specific atomic ratio, or when 90 percent of the available sites in the palladium had been achieved or saturated, which required days and some-times weeks to reach.

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Unfortunately, the EPRI-NSF report was never published, and one can only speculate as to why it wasn't. However, as of June 2011, it is still available on the Internet at http://www.lenr-canr.org/acrobat/EPRInsfepriwor.pdf. I am not qualified scientifically to follow the more than one hundred pages of conference reports and discussion. But the tone of respect and desire to follow all data points and questions raised in the discussion are truly impressive.

At this point, the press—apparently unaware of the study made by the Washington, D.C., group—grew increasingly harsh. One can only conclude that if a wide disclosure of the findings of the Washington group had occurred—both to the scientific community and the public—the road to scientific, deliberate research undistorted by science/politics would have muted much of the criticism, which accelerated to the point of ridicule launched at Fleischmann and Pons. More importantly, the pace of research would have accelerated.

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CRITICISM, RIDICULE, AND INSTITUTIONAL REPUTATION

Intellectual and academic convictions provided only a partial antidote to the strident external criticism of the Fleischmann/Pons findings. Regrettably, this criticism strengthened vocal concerns on campus that the controversy and ridicule could do serious academic harm to the University of Utah generally. My concerns were as deep as anyone's, but I received some respite and a significant morale boost from Professor David Grant, a distinguished chemist on Utah's faculty. Grant visited his grant-liaison officer in Washington at the National Science Foundation a month after the Pons-Fleischmann press conference, when the criticism was growing hot.

The officer asked, "David, when are you guys at Utah going to stop that cockamamie cold-fusion stuff?"

A lesser person than David Grant might have been dismayed, wondering if his funding would suffer. Grant reported that he poked his finger into the chest of his liaison officer, replying, "Are you asking me to tell colleagues that they are not free to pursue a project of interest to them?"

"Oh no, Dave. I was only kidding."

Was he? Not everyone on the university faculty had the chutzpah of David Grant. A dozen or so of the nonchemistry science faculty asked to meet me a month or two after the announcement. It was a blunt, outspoken meeting in the same conference room adjacent to my office where Richard Bernstein had first told me what Fleischmann and Pons were doing. I am quite sure all the participants felt they were sincere in the concerns they voiced. We were all feeling the pain of public ridicule. There was standing room only where we met. Ultimately, one of the most outspoken of those attending blurted, "President Peterson, you must stop this work."

After a measured pause, my reaction was simple: "I never thought that on this campus I would hear one faculty person telling another that he or she must stop research that is breaking no laws." I suspect the gathered faculty might have agreed with me but felt themselves both victims of the national criticism and campus spokespersons for the need to wash the university clean of the presumed stain of the event. As I fell asleep that night, I recalled an ancient Harvard saw that suggested that faculty were "free to do whatever they wanted as long as they didn't get their names in the paper or scare the horses."

The University of Utah faculty member who spoke out in the meeting may have been honestly worried that his horses might flee, that current or future grants and reputations could be at risk. Though my conscience was clear, succeeding days brought considerable personal pain, pain shared by many others affected by the events and suffering from anguish that an intellectual or theoretical wand could not fully alleviate.

A university president's wife is not unaffected by institutional controversy. Imagine what must have spun through my wife's mind when she innocently unfolded a copy of the *New York Times* and encountered the editorial page. The words leapt at her with the declaration that her husband should resign his presidency for his role in the embarrassing "pseudoscientific circus" called cold fusion. It hurt a bit, but we could take it.

The same paper's editorial page had called Utah's unavoidable, full-and-open reporting of its artificial heart project six years earlier "grandstanding." In the science section of the same issue, the *New York Times* distinguished medical writer, Dr. Larry Altman, had credited the university with clarity and responsibility for the manner in which it reported that heart research. And, as is common knowledge, artificial heart pumps and heart-assist devices continue to enjoy growing development and modest use.

There is legitimacy in any university's desire and obligation to stop fraudulent research. Generally universities are sponsoring agents with the principal investigators in applying for and conducting funded research. On one occasion—a few years prior to the cold-fusion controversy—a minor paper published by one of the junior faculty members at Utah's medical school had been challenged. There is a standard protocol for investigation. Our university followed the rules carefully and found that the author had indeed falsified data. In response the university published a retraction of the paper, repaid the NIH for the investigator's research funds, and placed the author on probation. He left the university soon after.

"THEY WILL ONLY LAUGH AT YOU"

During the month before the public announcement by Fleischmann and Pons, I had called Professor Hans Bethe. He was a distinguished physicist at Cornell and a Nobel laureate for his work on the nuclear processes within the sun. He happened, parenthetically, to be the father-in-law of the sister of my son-in-law. Perhaps for this familial reason, he took my call.

I explained what Fleischmann and Pons planned to report. I asked if he, by any chance, had the time and energy to come to Utah and review their findings. He said he was too busy and perhaps too old to make the trip. Then he said in the clearest of terms, as a man whose career had focused on understanding nuclear processes at the temperature and pressure of the sun, "I advise you not to announce such a thing. They will only laugh at you." At that moment, I did not see what laughing had to do with such a matter-an announcement by two distinguished faculty chemists. Ultimately, Bethe's prediction was largely accurate.

I later discussed the research-related role of a university president with my friend David Gardner, a respected mentor and my predecessor as university president. He told me his choice would have been to play no public role in the matter, leaving it purely in the hands of the scientists. He might have been right. Doing that certainly would have shielded me from personal criticism. Would it have kept Utah's flagship university less connected to a story of "just two scientists"? I doubt it.

At the time, however, I did not think I had the option of simply being an onlooker. Professor Bernstein had handed the matter to me and strongly advised me to scrutinize it. Fleischmann and Pons had asked me to join and help arrange a meeting with Professor Jones, the BYU president, and the provost. After the announcement, the Utah governor and legislature had asked for my testimony about state funding for further research. It is doubtful if funding would have been provided had I declined to testify or if the university was unwilling to assume responsibility. Finally, Congress had asked me to testify at a hearing in Washington to consider the topic and exceptional funding approved earlier for superconductivity, the Genome project, hot fusion, and much of NASA's work. An impressive number of scientists and even some university presidents had supported such projects, not as representatives of their institutions, but rather as advocates for the importance of the project. Finally, who more than a university's president is in a position to support the importance of academic freedom when it is aggressively challenged?

Yes, I was a university president. But, before and perhaps beyond that, I was a physician-bound to ensure no harm. And before and beyond that, I was the son of a university president, perhaps thus carrying an inbred sense of campus responsibility. A wise psychiatrist once either admired or accused me of being the sort of person who would feel obliged to paddle a stream to be certain it successfully made its way to the ocean. Mea culpa!

Examining the mind-set of medical practitioners may also help explain my choice in the role that I played. A doctor is committed to participating in the equation of diagnosis, healing, and care. This is quite different from the detached stance afforded most professions.

THE FLEISCHMANN/PONS EFFECT, TWENTY YEARS LATER

The current status of the Fleischmann/Pons effect, or the FPE as it has come to be known, can be summarized as follows: As of 2009, more than a hundred respected scientists from dozens of distinguished domestic and foreign labs-such as the U.S. Naval Research Laboratory, the Stanford Research Institute, the Italian Atomic Energy Laboratory, the Bhabba Atomic Research Centre (India's first and primary nuclear-research facility), even Russian, Japanese, and, most recently, privately funded Israeli labs-now regularly report experiments producing excess heat. The ability to duplicate their experiments has improved as metallurgy has advanced.

Before he died—in a lecture given on December 7, 1990, in Japan—Julian Schwinger, who shared the 1965 Nobel Prize in physics with Feynman and Tomonaga, urged "open-minded research—not suppression:' And in view of the physicochemical nature of the subject, he added that he “would have thrown all the resources of the Institute of Physical and Chemical Research into the study and development of cold fusion?[2]

In addition to the original electrolytic process that Fleischmann and Pons used to load the palladium, there are now reports of success with gas loading. A few other labs have reported transmutation—the emergence of elements not present before the palladium loading—which indicates a nuclear process. Others have reported the specific production of tritium, a product of nuclear fusion.

Thus, in each year of the last twenty, objective data from a growing number of respected laboratories worldwide have seemed to confirm the FPE. My guess is that many critics will never acknowledge the corpus of supporting data until a fresh generation steps forward to extend the science or it becomes possible to ramp up the generation of energy from the FPE to heat houses or power vehicles.

In 1963 J. B. S. Haldane wryly suggested that new theories often undergo four stages in the process of acceptance:

1. This is worthless nonsense.
2. This is an interesting, but perverse, point of view.
3. This is true, but quite unimportant.
4. I always said so.[3]

In 2009—during the very week of the twentieth anniversary of the Fleischmann/Pons announcement—Robert L. Park updated his earlier strident criticism of cold fusion with a backhanded acknowledgement (stage 3 on Haldane’s list), saying,

> Incredibly, the American Chemical Society was meeting in Salt Lake City this week and there were many papers on cold fusion, or as their authors prefer LENR (low-energy nuclear reactions). These people, at least some of them, look in ever greater detail where others have not bothered to look. They say they find great mysteries, and perhaps they do. Is it important? I doubt it. But I think it’s science.[4]

In the same month as the American Chemical Society meeting, an unrelated, but thorough, report by the CBS program 60 Minutes reexamined cold fusion. Sensing the polar controversy, the reporters found a respected physicist who had no published opinion on the matter, Robert Duncan, vice chancellor for research at the University of Missouri. They asked his assessment. Duncan had earned his undergraduate degree at MIT, and during a rich and varied career, had been a visiting member of the faculty at Cal Tech and a supervisor at Los Alamos National Laboratories before coming to the University of Missouri.

Duncan initially reported that he tended to be skeptical of the Fleischmann and Pons claims. CBS then requested that he take the time to study the more-recent published reports and visit laboratories reporting regular excess-heat production-including a trip to a privately funded facility in Israel that was working on commercialization of the process. Duncan did so over a number of months.

In his summary, Duncan concluded that good science done by skilled scientists has resulted in a high, but not 100 percent, record of replication and that the scientific basis for the phenomenon of cold fusion is yet to be understood. This report paralleled presentations at the iath International Conference on Cold Fusion that had convened a few months earlier. With the Naval Research Laboratory, the Stanford Research Institute, dozens of university and national laboratories here and abroad, and a handful of commercial enterprises continuing to conduct active research, it is unlikely that the genie can be put back into the bottle.

A PERFECT STORM

Since discovery announcements are an ancient and accepted part of science, one naturally wonders how Fleischmann and Pons could have made their announcement without prompting the violent reaction they received. It had all the characteristics of an unavoidable perfect storm.

In an interview Fleischmann gave in 1996 to Christopher P. Tinsley, a contributing editor of Infinite Energy magazine, the scientist came to the same conclusion, saying, “I think the press conference was a mistake. But it was inevitable:’

Tinsley asked, “Can you, looking back, see any alternative to what happened?”

“No.” Fleischmann responded. “... I think it was inevitable—and it would happen again, and in other fields it will certainly happen again.”[5]

My question: What, in the events of cold fusion, was too hot to handle from the beginning? Certainly Fleischmann and Pons had the right to claim primacy for their potentially important findings. The history of science is replete with controversy over early claims of primacy. The discoveries of insulin by Frederick Banting and Charles Best in Canada and the double helix of DNA by James Watson and Francis Crick are two well-known examples. One hopes that, in the case of cold fusion, history will untangle who did what and when and, in the process, clarify the role of Iones.

While the initial *Journal of Electroanalytical Chemistry* paper took the form of a preliminary note, the potential importance of the findings was too enormous to be accorded a wait-and-see reception by critics. What was at stake if Fleischmann and Pons were right?

By comparison the suggestion of movement by tectonic plates to explain the contours of South America and Africa and species isolation contradicted existing science but posed no threat to related lines of research and development. The geologic-science establishment was therefore content to scoff at the validity of the observation, but there was no rush to censure it for much of a century until irrefutable evidence slowly emerged and confirmed the theory.

A case more comparable to Fleishmann and Pons's is a study on the African American family written by my personal friend, Professor, and later Senator, Daniel Patrick Moynihan. The study prompted strong criticism for even considering negative aspects of the sociology of the postslavery black family. Moynihan dryly commented after weeks of public outcry that the subject perhaps deserved a period of "benign neglect." The implications of the Fleischmann/Pons experiment were probably simply too large and challenging to receive such a refreshing, calm break.

Fleischmann and Pons often said that they wished they had had six to eighteen months or more to shore up their claims and uncover the reason(s) the experiment produced inconsistent results. No one initially knew all the elements the process required to produce excess heat. The first full paper on the experiment was published in 1990. Thus, the instructions for replication in the initial 1989 article likely gave a misleading impression of simplicity, igniting disbelief and suspicion. Effort in time and money that many laboratories spent to replicate the phenomenon without success in the first week or two after the announcement must have added more skeptical wood to the fire. In that light, it was probably easy for frustrated investigators to cry "sloppy science" or "fraud" given the huge implications of the claim.

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MISTAKES WERE MADE

There were at least three unfortunate misjudgments or mistakes relating to the cold-fusion drama that added to the turmoil. The first was our naively being unprepared for the magnitude and degree of public and professional criticism. Utah might, I believe, have better anticipated and prepared for the public interrogation and criticism. Fleischmann and Pons were gentlemen throughout. Still, Pons particularly could have been tougher and more prepared for the onslaught.

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PERSONAL REPUTATION

On a personal note, in 1990-the year after the announcement on cold fusion-I was named chair of the congressional science-advisory committee called the Office of Technology Assessment (OTA). In that position, I followed William Perry, former secretary of defense, and was succeeded by Joshua Lederberg, Nobel laureate and president of Rockefeller University.

In the same period, the National Association of State Universities and Land-Grant Colleges, or NASULGC, elected me as its chair. I am the only Utahan I know of to have occupied that post. That is the same organization that took my father to Washington yearly as an executive-committee member and me with him once when I was twelve.

Those appointments suggest that there was no embarrassment or censure felt by my national peers, who were not naive about the rituals and images of education, research, and academic politics.

As I said at the time of the 1989 announcement and continue to believe, “What we now term the Fleischmann/Pons effect, or low-energy nuclear reaction, deserves serious study. There is not yet a full understanding ofits theoretical basis. The university, its faculty, and I believe in the importance of diligent research and study on a matter of such potential importance to science and possibly humanity.”

Cold-fusion’s basic scientific claims have now been widely confirmed as new and interesting science. Within this new scientific niche may lie merely a curiosity or a significant contribution to one of the greatest problems the Earth faces. Any challenge to a paradigm is painful in its evolution.

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THE TIMELESS CONFLICT INHERENT IN THE CHALLENGE OF NEW SCIENCE

A few months into the controversy, I learned of a book by Michael Polanyi that details his own experience of the tension between orthodoxy in science and challenges to it. In a chapter entitled “The Potential Theory of Adsorption,” the respected scientist describes being caught in the crossfire between scientific order and the claims of a new idea. Polanyi was widely ridiculed for more than twenty years for having made an observation in 1914 that violated the then-current science theories. Had he not been able to present subsequent research sustaining his reputation, he believes his professional career could not have survived. By the late 1930S, finally, he received acceptance of what hed proposed. His conclusions are worth noting:

> I am making, therefore, no complaint about the suppression of my theory for reasons which must have seemed well founded at the time, though they have now been proved false. It is perhaps more difficult to understand why more than fifteen years passed after the presentation of my paper in 1932, in which the original objections had been proved unfounded, before the rediscovery and gradual rehabilitation of the theory set in. I suppose so much confusion was left over from the previous period that it took some time for scientists to take cognizance of the new situation, and that meanwhile my own work, which had been so long discredited, remained suspect. If the problem had been more important, this period of latency would no doubt have been shorter.
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> The dangers of suppressing or disregarding evidence that runs counter to orthodox views about the nature of things are, of course, notorious, and they have often proved disastrous. Science guards against these dangers, up to a point, by allowing some measure of dissent from its orthodoxy. But scientific opinion has to consider and decide, at its own ultimate risk, how far it can allow such tolerance to go, if it is not to admit for publication so much nonsense that scientific journals are rendered worthless thereby.
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> Discipline *must* remain severe and is in fact severe .... Even so, the opposition to my theory would have cut off any hope I had of a scientific career ... had I not done other

> scientific work that brought me recognition which outweighed the discredit brought upon me by my theory of adsorption [italics in original]."[6]

Let me be clear: I am not asking that the tension between established science and new theories be otherwise. I'm merely attempting both to frame and spotlight the grave risks that characterize dissent. This centuries-old tension often promotes a productive debate. Obscuring this tension—as happens too often in our times—dilutes the intellectual honesty of science.

How should one judge the words of Bertrand Russell?

> The triumphs of science are due to the substitution of observation and inference for authority, Every attempt to revive authority in intellectual matters is a retrograde step. And it is part of the scientific attitude that the pronouncements of science do not claim to be certain, but only the most probable on the basis of present evidence. One of the great benefits that science confers upon those who understand its spirit is that it enables them to live without the delusive support of subjective authority."[7]

Such statements, while emphasizing inference, obscure the fact that the authority of current scientific opinion is indispensable to the discipline of scientific institutions; that authority's support is invaluable, even though, ironically, its dangers are an unceasing menace to scientific progress. I have seen no evidence that this authority is exercised without claims of certainty for its own teachings.

The unreasonable unwillingness to reconsider a rejected proposal is revealed by a recent response by an otherwise-respected nuclear physicist from Texas. When asked his current view of cold fusion, he crisply responded that the issue had been closed years before when the theory was proved to be error. The questioner then asked, "But have you read any of the more than one hundred articles that have confirmed the phenomenon?" The Texan's response was simple: "Of course not; I do not have time to waste on matters that have been closed."

PERSONAL CONCLUSIONS

Intellectual and academic freedom are essential elements of experimentation, observation, data collection, and, ultimately, human welfare and progress. Observation and data collection are essential in the generation and confirmation of theories. The usefulness of data rests on their ultimate replication and confirmation by other skilled investigators. While immediate verification is ideal, more often additional experimentation and time are necessary to understand the conditions required for reliable replication. Hence, one novel observation is rarely the end in probing a mystery of nature. The period of uncertainty surrounding a new observation calls for modesty and restraint from both the proponent and the skeptic.

The tenets of established science are a means of avoiding chaos, promoting order, and acting as a road map for new discovery. In the process, these scientific tenets risk engendering intellectual myopia unless they simultaneously encourage a healthy culture of skepticism for the known as well as the unknown.

Intellectual property deserves no less attention and stewardship. This stewardship serves the interest of the individual scientist and institution while promoting the advancement of humanity at large. This aim is exactly the intent of the Bayh-Dole Act in the 198os, which was designed to promote timely transfer of bench science to industrial development.

The Fleischmann/Pons announcement—coupled, as it was, with the uncertain research of BYU's Stephen Jones—created suspicion that resulted in confusion. The passage of twenty years appears to have answered many of the questions about the primacy of the discovery of cold-fusion's production of excessive heat. With respect to any parallel work Jones did on the issue of nuclear measurements, it is best to reserve judgment. . . . Science and wisdom await all questions.

WHAT WILL THE FUTURE BE?

What will be the significance of the excess heat generated by the Fleischmann/Pons effect—LENR? Will it be a wedge in a new chapter of science, or merely an oddity? Will it produce energy in a manner and magnitude, ideally as decentralized energy, that is useful to civilization? As I write, many lines of development are being pursued. But we do not know the answer ... yet. There's that wonderful, honest, and optimistic word: yet.

Will the lesson of cold fusion have taught us to live more wisely between the useful, comfortable, but uncertain clarity of established truth and the disorder posed by the challenge of as-yet-unverified new truth? The jury, as they say, is still out.

Finally, the process we at Utah experienced is a direct expression of a special, sometimes fragile freedom—academic freedom. And though this is a freedom that has never sailed on smooth water, I cannot imagine not paddling for it or helping to steer it to a safe harbor.

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[1] House Committee on Space, Science, and Technology, *Recent Developments in Fusion Energy Research*, 101st Congress first session, April 26, 1989

[2] Julian Schwinger, "*Cold Fusion-Does It Have a Future?*" (Lecture presented at the Yoshiro Nishida Centennial symposium, Tokyo, Japan, December 5-seven, 1990). http://www.lenr-canr.org/acrobat/SchwingerJcoldfusiona.pdf

[3] J. B. S. Haldane. Quoted in Wikipedia.

[4] Robert L Park, "Cold Fusion: 20 Years Later, It's Still Cold," *What's New*, March 27, 2009.

[5] Tinsley, C., *An Interview with Professor Martin Fleischmann*. Infinite Energy, 1996(11). http://www.infinite-energy.com/iemagazine/issue11/fleishmann2.html

[6] Michael Polanyi, "The Potential Theory of Adsorption," in *Knowing and Being: Essays by Michael Polanyi* (Chicago: University of Chicago press, 1996), 93-94.

[7] Bertrand Russell, *The Impact of Science and Society*, 1953, quoted by Polanyi, *Knowing and Being*, 94.