COLD FUSION AUTOBIOGRAPHY OF DR. EDMUND STORMS FROM 1989 TO 2015

A PROJECT OF THE LENR RESEARCH DOCUMENTATION INITIATIVE

DRAFT

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CONTENTS

INTRODUCTION

Cold fusion (CF) was announced on March 23, 1989, by Dr. Martin Fleischmann and Dr. Stanley Pons. The immense potential energy benefits of CF were immediately recognized at the time. Humankind's need for a source of cheap, clean, inexhaustible, and safe energy seemed to be realized. However, CF was rejected by mainstream science within a year or so, and it remains highly marginalized to this day. On the other hand, it has continued to be rigorously pursued by many capable investigators in several countries. It is now widely referred to as Low Energy Nuclear Reaction (LENR)

Many of the investigators who continued to pursue LENR in spite of the rejection began their work in the early years after the announcement. Now more than 30 years later many are leaving the field because of retirement or health issues. Because of the rejection and lack of funding, the researchers' records have not been systematically recorded and preserved. At the same time, these records may eventually help understand LENR and achieve its benefits. Their loss would be a tragedy not only for the field, but also for humankind.

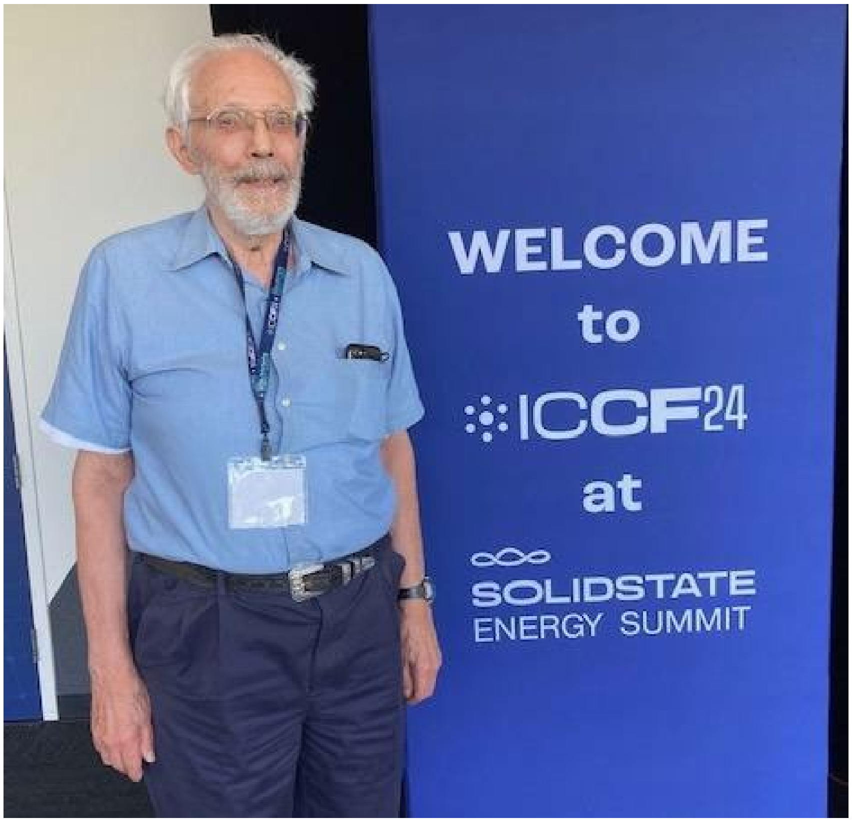
The LENR Research Documentation Initiative (LRDI) has been undertaken to help mitigate this loss. A project has been performed with Dr. Edmund Storms (Figure 1) to document his LENR research records. It was conducted under the umbrella of the LRDI and is referred to as the Storms LENR Research Documentation Project (SLRDP). Its objectives have been to collect, document, and archive his electronic and hardcopy files.

Dr. Storms was one of the first researchers to follow up on the cold fusion claims of Fleischmann and Pons. He has continued his LENR research in the 34 years since, first in his position at Los Alamos National Laboratory (LANL) and then in his home laboratory in Santa Fe, New Mexico.

When LENR was announced in 1989, Dr. Storms had already enjoyed a 35-year career at LANL, primarily in advanced materials research. His pre-LENR investigations were mostly in refractory materials, such as the carbides and nitrides, for high-temperature nuclear energy applications. He investigated the chemistry required initially to make nuclear rockets that could escape the gravity of Earth and subsequently to generate electricity for spaceships and satellites to operate long-term. His foundation in materials research enabled Dr. Storms to quickly become established as a premier investigator in the LENR field.

Figure 1

Dr. Storms at ICCF-24, Mountain View, California, July 2022



Dr. Storms has conducted many types of LENR experiments, utilizing most of the methods for achieving the effect, including the Fleishmann-Pons approach (electrolytic cells), the gas discharge, and gas loading methods. He has also designed and constructed many kinds of calorimeters for measuring excess heat. His work has included not only laboratory experiments, but also development of explanations of the LENR phenomenon. His LENR research career took place in several phases, which may be defined by the type of investigative approach and the entities he was working with or whom he was receiving financial support. These phases are as follows, with the primary collaborator or sponsor shown in parentheses: LANL (Carol Talcott), ENECO (Charles Becker), Lattice Energy (Lewis Larsen), and KivaLabs (Brian Scanlan). Between these phases Dr. Storms conducted independent research using his own resources.

During his 33+ years of investigations, Dr. Storms has developed one of the most extensive LENR research records in existence. Much of this work is available in the public realm through his publication of papers and presentations at conferences. There is in addition an extensive body of research results that are in his private files, which have been documented in the SLRDP.

A key component of the SLRDP is a set of interviews conducted by Dr. Grimshaw with Dr. Storms in April 2016, March 2017 and June 2017. All three interviews covered the full length of Dr. Storms' LENR career. Ruby Carat prepared this autobiography from transcriptions of the three interviews. It has been extensively reviewed by Dr. Storms and Dr. Grimshaw. A description of the SLRDP, including the three interviews, appears in the appendix to this report.

ROCKET SCIENTIST

For many years, people at Los Alamos National Lab LANL worked to design a nuclear rocket that could send people to the Moon and beyond. This successful effort was terminated without being used. Later, a nuclear reactor was designed that could provide the power to spy on Russia from space. I worked on both of these programs. But first, here is a little background.

After Sputnik in 1957, the Russians began to use nuclear powered spy satellites. Because their cameras weren't very good, the reactors were put in low-earth orbit, where the satellites would circle closer to the surface of the Earth. Because they were in low Earth orbit and catching more friction from the atmosphere, their orbits would decay and the satellites would come crashing down. These satellites were intended to come down in an uninhabited area such as the ocean because their reactors were powered with dangerous radioactive material. Unfortunately, they missed the ocean more than once. On one occasion, a satellite came down on land in Canada, contaminating the area where it hit rather severely.

Solar energy, at the time, was not an efficient source of power for a spy satellite. To generate the power needed would require a physically large system, which would be easily visible from Earth, creating a huge target to shoot out of the sky. A nuclear source of energy could provide a large amount of power that could power communication equipment as well as sensitive cameras.

The US government wanted the same ability as Russia but without the danger of the satellite crashing to earth. So, the Los Alamos National Lab (LANL) was given this task, which was called the SP100 program. The reactor was intended to be sent into high orbit where it would produce 50 kilowatts of electric power consistently for at least seven years. My job at LANL involved a study of the materials required to construct this reactor.

This was a challenging design. A reactor in space has to be very compact. Therefore, it has to operate at a very high temperature, even higher than boiling-water. Liquid lithium was used to extract the energy because it is a fluid capable of handling such temperatures. The best candidates were the carbides or nitrides of uranium as the nuclear fuel. Uranium carbide, in particular, has very nice properties for use in a high temperature reactor. Uranium oxide, which is presently used in terrestrial reactors, is not suitable.

These types of fission-based nuclear reactors produce a tremendous amount of radioactivity and radiation, which limits their use in space. Furthermore, they can no longer be tested. In the early days, a test could be done in Nevada. When radioactivity was spread all over the place, no one paid any attention. Now people have finally started paying attention, so the rules have changed.

When the Cold War ended, the need for the SP100 mission ended. Satellites could now be in low-Earth orbit and use solar, because the solar collectors and the electronics became more efficient. Nevertheless, this experience demonstrated just how difficult and unsuitable conventional nuclear power would be when used in space.

Meanwhile, another program, one that would change my life forever, was about to start. It was a program that promised to end our energy problems in space, and on Earth, forever.

MARCH 23, 1989[1]

For a short time, beginning March 23, 1989, the direction of research at LANL made an about-face. On that day, two of the world's top electrochemists, Drs. Martin Fleischmann and Stanley Pons, spoke at a press conference in Utah announcing their discovery of cold fusion. This discovery turned out to be a fusion-based source of heat-energy generated by a simple apparatus at room temperature without producing deadly radiation.

This was a shocking claim. Nevertheless, the discovery offered an ideal solution to creating clean abundant power for space and a solution to the climate change problem. However, the claim was in conflict with the 100-year-old theory about nuclear reactions that physicists relied on. The claim was impossible according to theory. The scientists at LANL were not stopped by this fact because we were frequently discovering new behavior that did not fit conventional theory.

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[1] Cold Fusion Press Conference at University of Utah, March 23, 1989.
https://www.youtube.com/watch?v=6CfHaeQo6oU

Besides, the need for a clean source of energy was too obvious to ignore even when the chance of success might be small.

When Fleischmann and Pons made their joint announcement at the press conference, everyone at Los Alamos was interested, but they needed some factual information before we could replicate the claim. Efforts were made to contact Pons and Fleischmann to get more details and, in particular, to get copies of their paper[2]. Their paper was not yet published, so people were confused about what they did.

The paper was eventually accessed through an organization in England. A few people who knew Fleischmann and Pons obtained a photocopy and faxed it all over the world. Somebody would fax it first, and then they'd send it to somebody else, and then *they* would fax it. By the time it got to us, it was almost unreadable. E-mail did not exist then.

That first paper of Fleischmann and Pons was an early version, and it didn't have the details needed to replicate the experiment exactly. Researchers were left speculating as to how Fleischmann and Pons went about doing the experiments. Oddly, most of the information was coming out of England through the *Financial Times*, a newspaper. Eventually, the claim was published all over the world by other newspapers and magazines.

The lawyers told Fleischmann and Pons to keep certain critical parts secret if they wanted to get a patent. Only after Pons visited LANL did we learn more. But even then, clearly *they* didn't know what was actually happening. They had been studying this phenomenon for five years, but still didn’t know when or where in the material the reaction would take place.

Soon, a readable version of their paper became available so that people could start to work. Everybody tried to obtain some palladium and heavy water (D_2O). At Los Alamos, we were already using palladium, so we had lots of it. Heavy water was easily available. Other people were not so fortunate. Many people at LANL started to focus their research efforts on cold fusion instead of what they had been doing. At one point during all of the commotion, I happened to meet the Lab Director (Sigfried Hecker) on the street. He commented, "This is just amazing, everybody is excited. Physicists are actually talking to chemists."

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[2] M. Fleischmann, S. Pons, and M. Hawkins, Electrochemically induced nuclear fusion of deuterium. *J. Electroanal. Chem.* **261** (1989) 301 and errata in Vol. 263. https://lenr-canr.org/acrobat/Fleischmanelectroche.pdf

WEAPONS INTO WATER

Within a few weeks of the announcement, I was asked to write a description of how I would propose to study cold fusion and how I thought the process worked. My explanation involved bubbles and how they interacted with the surface. The idea was incredibly naïve, but in the absence of a better explanation, it was sufficiently intriguing to cause some interest.

As a result, I gave a talk before our group, and then presented my description to the division leader. I requested to be allowed to work on what was then called the Fleischmann-Pons Effect (FPE). That presentation eventually became part of a request to the U.S. Department of Energy (DOE) for funding to support a lab-wide study of the anomalous heat effect.

The DOE granted LANL $250,000 to work on cold fusion. The money was divided between other people throughout the laboratory with a small amount being used to support a scientist (Carol Talcott), a technician (Maryanne Rodriguez), and myself.

FIRST, CALORIMETER. SECOND, TRITIUM.

So we set to work attempting to initiate nuclear reactions in small glass cells using electrolyzed palladium metal and heavy water.

Electrochemist Shimshon Gottesfeld, was first to get started. Gottesfeld set up a cell designed to be as close to a true replicate of what Fleischmann and Pons had used, as far as we understood at the time. He wanted to measure heat, the key signature of the FPE. He failed to detect any evidence to support the claim. Various other methods were designed to cause cold fusion using shock waves and different kinds of discharge techniques. People had all kinds of creative ways by which they thought they could initiate the nuclear reaction, while not actually replicating how Fleischmann and Pons did the job.

Carol Talcott was working in the same group as I was and asked to work with me. So, we started planning a replication project. Her background was in palladium-hydrogen chemistry, so she had an understanding of the material used in the electrolytic cell. However, as we started examining the problem, our approach to the design of the experiment evolved.

Initially we were thinking about measuring heat. That was what Fleischmann and Pons were focused on. This product also made the process useful. In late April of 1989, we started to design the calorimeter and quickly realized we didn't know anything about calorimeters. Besides, we had no idea how *they* were doing their calorimetry.

Fleischmann and Pons claimed they were causing fusion. Therefore, tritium should be made. Fortunately, many experts in tritium identification, measurement, and use worked at LANL. So, we set up electrolytic cells and looked for the production of tritium.

In order to eliminate any contamination, our cells were closed and totally sealed. This way, the gas generated by electrolytic action would be turned back into water by a chemical catalyst and this water would be retained. Nothing left the cell, and nothing entered the cell from the outside. All of the gases were captured into medical IV bags. For awhile, we were the largest user of IV bags in Los Alamos, which, of course, we got from the hospital. This allowed us to measure the tritium no matter where it appeared, either in the electrolyte or in the gas. This way, we could totally monitor exactly what was happening.

Another LANL scientist, Tom Claytor, also set up to measure tritium, but he used a gas discharge technique rather than the electrolytic method.

CLOSED CELLS CAPTURED TRITIUM

At least a couple dozen cells were set up on a table where they all were exposed to the same environment. If the environment contained tritium, all the cells would be expected to become contaminated. We used very pure palladium powder that we arc-melted into solid pieces. This way, any contained tritium would be removed. As a result, we had a good understanding of the purity and the history of the material.

To see how impurities might affect the results, Talcott and I added numerous materials to the electrolytes. We tried everything while having absolutely no idea of what we were doing or why we were doing it. But eventually, one of the cells in our lab started to make tritium. As a result, we got a lot of attention all the way up to the director's office.

As many as 13 samples made tritium out of about 230 samples that we studied. Two of them made a lot of tritium. The others were not as well studied and the magnitude was not as great. A couple of the cells made enough tritium for it to be completely unambiguous, even to a skeptic, so we published our work.

All of our work was described in a paper that was reviewed throughout the laboratory.[3] Of course, the skeptics complained that none of this was peer-reviewed, which was untrue. This paper was peer-reviewed by many people at LANL including by people in the director's office. A few changes were made to improve the presentation. Then it went to the journal where it was peer-reviewed again by other people. The paper was then published and ignored by the skeptics.

JOHN BOCKRIS WINS THE RACE

John Bockris, Professor of Electrochemistry at Texas A&M University, was a good friend of Martin Fleischmann. When Bockris heard the press conference announcement, he contacted Fleischmann and asked for the inside scoop. Fleischmann explained a few things to him and sent him more information. This allowed Bockris to very quickly replicate what Fleischmann was claiming to do. As a result, Bockris had a head start on everybody.

Because Bockris was also looking for tritium, we fell into a competition to be the first to find tritium because he was obsessed with finding tritium first. One day he called and asked how I was doing. I gave him a little background and eventually I told him, "it looks as though we were getting some tritium". He immediately went to the press and said, "Los Alamos has demonstrated production of tritium and has replicated our results." His action didn't go over well at Los Alamos and got me in trouble. LANL was not interested in revealing this work to the press. I wasn't happy with Bockris for making our personal call public, because I thought I was talking in confidence. Essentially, he had to brag about the fact that he had seen tritium first and that we at LANL had verified what he had seen.

I found out later *why* John Bockris found tritium in his cells first. We were keeping our heavy water totally pure by using a sealed system. This prevented contact with the atmosphere, which meant no light hydrogen was present in the heavy water. Bockris, on the other hand, had an open system that allowed entry of light hydrogen from the atmosphere. He even measured the amount of light hydrogen in his D_2O. Now we know that a mixture of light hydrogen and heavy hydrogen is required to make tritium. By using a sloppy design, he had created the most effective conditions for making tritium.

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[3] E. Storms and C. Talcott, Electrolytic tritium production, *Fusion Technol*. **17** (1990) 680. https://lenr-canr.org/acrobat/StormsEelectrolyt.pdf.

TRITIUM ORIGINATES IN PALLADIUM

The amount of evidence we experimentally observed was significant, but it still left many questions unanswered. The standard model of nuclear fusion shows that tritium production is linked to neutron production. For every tritium atom produced, a neutron should be emitted. People could not find the expected neutrons.

The “missing neutrons" allowed skeptics to reject the claimed heat generation as not being from a nuclear source. The extra energy must be the result of poor calorimetry, the re-combination of D_2 and O_2, bad temperature measurements, or because the cells that weren’t stirred. A parade of explanations of why the FPE was impossible was suggested.

However, the tritium we produced couldn’t be ignored. Nevertheless, the skeptics refused to believe it was produced inside the metal. Instead, the cell had to be contaminated. Professor John Bockris was even accused of adding tritium to his cells! This caused him a great deal of trouble at Texas A&M and the loss of many friends. His results were eventually verified after an argumentative defense and he was cleared of all charges. Such was the attitude toward new ideas at some universities.

I even tested the behavior of a cell to which I added tritium on purpose. I found that it did not act like the cell Bockris used. Even this fact was ignored by the skeptics. The skeptics were desperately reaching for even the most implausible explanations to explain the anomalous effects. This was insulting to the researchers who were finding unusual behavior. They were essentially saying to us, “We think you're totally incompetent and can not be trusted.” In response, we tested all of the claims made by the skeptics. We tested the laboratory air for tritium and found none. Even so, we could be sure that the cell was not being contaminated from any tritium that might have been in the room because the cell was sealed.

To be on the safe side, we placed a cell in a room known to be contaminated by tritium in the air. We discovered that the cell did pick up a small amount of tritium, but in an entirely different way compared how tritium appeared in a cell when it was in the tritium-free room. Then the skeptics claimed the tritium came from contaminated palladium. We eliminated this source by arc-melting the palladium, which would drive off any hydrogen in the material. We even electrolyzed a sample of palladium known to contain tritium. We found that the tritium appeared in the gas and not in the electrolyte where the unusual tritium was found. The skeptics then claimed the tritium contamination was in the heavy water even though the heavy water was tested before the study. To which we responded “Okay, let’s check.” We put tritium in the heavy water on purpose and electrolyzed it with palladium. We discovered that clean palladium removes the tritium from the water. In other words, the tritium in the electrolyte could not be the source of the abnormal tritium. Nevertheless, the skeptics ignored these results.

I was able to interact with a skeptic and future Energy Research Advisory Board co-chairman, John Huizenga, at the first Cold Fusion Conference in Salt Lake City Utah in 1990[4] . Clearly, he was unwilling to accept the validity of anything that was said there. “Well, what about the tritium? It’s real. We can measure it.” Huizenga would simply insist it was contamination caused by the amount of tritium in the air at LANL. He wasn't going to accept any evidence for the tritium being real.

The contrast between the objective scientists at LANL and the skeptics was overwhelming. This experience revealed which person followed the behavior normally required of competent scientists and who did not.

LOOKING FOR NEUTRONS

Malcolm Fowler led one of the teams investigating the emission of neutrons. Initially, people thought the FPE was a variation of hot fusion. Consequently, an equal number of neutrons should accompany the tritium. Talcott and I found tritium, but we weren't looking for neutrons. Malcom Fowler looked for neutrons but didn't look for tritium. Nevertheless, neutron emission alone would demonstrate that a nuclear reaction was actually happening in the material.

Italian scientists reported occasional neutron bursts first. They found that neutrons comparable to the amount of tritium detected were made when they put titanium in deuterium gas and then changed to the temperature, going from liquid-nitrogen temperatures up to room temperature. People thought, wow, this is cold fusion. This is the proof. Unfortunately, now we know that people were causing *fractofusion*, which is a version of hot fusion. This has no relation whatsoever to cold fusion. The LANL study was eventually published by Howard Menlove.

[4] E. Storms and C. Talcott, *A study of electrolytic tritium production*. The First Annual Conference on Cold Fusion 1990 https://www.lenr-canr.org/acrobat/StormsEastudyofel.pdf.

TOM CLAYTOR

Tom Claytor was successful in producing tritium occasionally when he used a discharge method. His success added further justification for believing that maybe Fleischmann and Pons were right. Shortly after Claytor began experimenting with his discharge technique, an expert on neutron detection, Howard Menlove, began working with Claytor. Their neutron detector was located deep underground, so the background was low. As a result, their detectors could see a small number of neutrons. They found that the neutrons had the energy expected to result from hot fusion. This created interest because this amount of energy was expected based on conventional theory. But we now know these neutrons did not result from cold fusion.

Claytor also worked with Steve Jones, who was detecting neutrons at the time. Jones worked at Brigham Young University where a tunnel located in the mountains was available. This was a perfect location for detecting neutrons because the background signal was very small. They also claimed to see a few neutrons. But many researchers did not find neutrons. They saw nothing because they never initiated a nuclear reaction.

Neutrons resulted only when a nuclear reaction occurred, which was rare. On the other hand, when the physicists saw nothing, they would conclude that cold fusion wasn't real. This is an example of seeing only what you expect to see.

Various ways of initiating the effect were attempted using equipment only available at LANL. For example, the material was exposed to high explosives and to very high forces. Scientists at the Lab did everything imaginable to the material. Only a few of the techniques worked. Only three of the LANL teams had success. Our group became one among the successful when we made tritium. Tom Claytor and his team also made tritium, but by a different technique.

Meanwhile, Malcolm Fowler was looking for and finding neutrons. However, they were not being generated by cold fusion, but by fractofusion. Nevertheless, this was unexpected, and gave evidence for the production of some kind of very odd nuclear reaction.

Now Claytor has a private laboratory in which he explores the cold fusion phenomena, just as I'm doing in my own laboratory in Santa Fe.

THE DOE WORKSHOP

In May 1989, the U.S. Department of Energy held a workshop in the old gym in Santa Fe. It was the first organized effort to bring people from around the country together in one place and present the information in a conference format. Chairs were laid out on the floor of the gym's basketball court and we listened to the presentations from the other national laboratories. Exhibits and posters were set up around the balcony. The supporting evidence for Fleischmann and Pons' claims displayed from other national laboratories was encouraging.

The Oak Ridge, Brookhaven, and Argonne National Laboratories had all seen positive results. Some people had the beginnings of success, while others failed completely. Although the positive results were somewhat ambiguous, this was a good start. We could never see the fire, but the smoke was obvious. People detected some tritium or some heat that they could not explain. After all, the Santa Fe meeting[5] was held barely a month and a half after the bombshell announcement in March. This was hardly enough time to do anything definitive. Maybe only 15% of the people who attempted to reproduce the FPE were able to get a reaction from this seemingly simple electrochemical experiment. Out of the total eight groups working at LANL to understand this claim, only three were successful at detecting a nuclear signature and our team was one of them.

However, the observations were not enough for some people. The skeptics came out in droves, especially the physics theoreticians, who believed cold fusion was impossible.

PONS, THEN FLEISCHMANN VISITS THE LAB

Stanley Pons was invited to LANL to tell everybody about cold fusion. The room was packed when he gave his talk. This was maybe a month or so after the announcement. At that time he was unwilling, and now we know unable, to answer most of the questions. His argument was that the lawyers advised him not to give information that could jeopardize their patent. (The patent was never granted.) This was frustrating because he did not describe how to make the reaction work. Now we understand that he did not know the answers himself.

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[5] U.S.Department of Energy, Cold Fusion Workshop, Santa Fe, NM May 25-28 (1989). https://digital.library.unt.edu/ark:/67531/metadc1093947/

Later, I arranged for Martin Fleischmann to visit the lab, but this resulted in a similar situation. He could not tell us how to make the effect occur. We later learned after they set up their laboratory in Nice, France that they paid no attention to the metallurgy of the material. They had no understanding of how to make the palladium nuclear reactive.

JOHNSON MATTHEY HAS THE RECIPE

Fleischmann and Pons had made a deal with Johnson Matthey, a global science and chemicals company; if you supply us with free palladium, we'll test it, do the studies, and then give you the palladium back. Johnson Matthey could then recycle the metal. But they could also examine the returned metal to determine the condition that produced cold fusion. According to Fleischmann, soon they were able to supply palladium that worked every time.

By analyzing the material, Johnson Matthey discovered the recipe of how to make active palladium, but they didn't tell anybody how they did this. Fleischmann and Pons not only didn't know, but they apparently didn't care.

Fleischmann and Pons supplied palladium to other researchers. Melvin Miles, a U.S. Navy electrochemist working at a laboratory in China Lake, said that "every piece of palladium he ever got from Martin worked." That was because Fleischmann got their metal from Johnson Matthey. On one occasion, Fleischmann described how they had a batch that did not work. They sent it back and informed Johnson Matthey that the batch didn't work. In response Johnson Matthey sent another batch and said it would work. And it did. Clearly, a treatment could be applied to palladium by Johnson Matthey to make it active. No one outside of Johnson Matthey knew the recipe. Fleischmann and Pons gradually understood that a special kind of palladium was required, but they did not focus on this need.

Vittorio Violante, a senior researcher and research program manager at the Universita degli Studi di Napoli in Italy, has worked to understand what makes palladium active. However, all of the information Violante gathered is not generally available. In any case, the characteristics of palladium are so complex that the limited investigation that Violante did was not adequate. He just saw the tip of the iceberg.

We at LANL wanted some of the same palladium that Johnson Matthey had given to Fleischmann, so we asked them for some, which they sent. I tested it and it didn't seem to work.

It turned out that Mr. Thompson, who had been in charge of the palladium program, had retired. I wrote to Thompson, “you're retiring and Fleischmann and Pons’ laboratory has shut down. Can you tell us how you made the palladium that worked?” He said no. He told me the method was proprietary information that Johnson Matthey owned. They were not interested in making it public. So as far as I know, that's still the case. I think they anticipate somebody figuring out how cold fusion works, at which time they would have a gold mine making active material. At one occasion, Johnson Matthey said that they would reproduce a batch of palladium in the same manner that they did for Pons and Fleischmann, but it would cost $50,000. Nobody could afford the cost.

SKEPTICS C’EST IMPOSSIBLE

Physicists believe that if they can't make cold fusion work, nobody can. Many physicists totally failed to get a reaction because they had no understanding of chemistry and the materials. They do not realize that this is a materials science problem. The nuclear reaction will occur only when the right material is used. Only then would the process involve physics.

Fusion Technology[6] was one of the few journals that would publish papers about cold fusion research. This was possible because the editor, George Miley, was a professor of physics at the University of Illinois Urbana-Champaign. Miley put his reputation on the line by accepting such papers, even though the papers were well-written and described legitimate scientific studies.

In contrast, Kirk Shanahan was a repeated skeptic who simply would not take no for an answer. He was a chemist at Oak Ridge National Lab and was asked by his supervisors to look into and evaluate the claims of cold fusion. He started coming up with explanations as to why we were all making mistakes. His explanations didn't hold water, but because he made a sincere effort to evaluate what was being done, he obtained a certain amount of legitimacy. People would say ‘Here is a scientist who is challenging your work, what do you have to say?’ Of course, we would respond.

Shanahan published a paper[7] in a top journal making the case that cold fusion was not real. He tried to explain why he thought he was right. He made the mistake of referencing my work so the

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[6] Currently known as *Fusion Science and Technology* https://www.ans.org/pubs/journals/fst/

[7] K. Shanahan, A systematic error in mass flow calorimetry demonstrated, *Thermochim. Acta* **382**(2) (2002) 95-101.

journal contacted me, asking if I'd like to respond. I told them, "You bet." My rebuttal was published along with his paper. Once again, his claims were completely wrong but a lot of time was wasted demonstrating this fact. Shanahan was absolutely convinced he was right. Fortunately, this was not typical of many open-minded people working at Oak Ridge National Lab, Brookhaven National Lab, and Argonne National Lab. Shanahan was not representative.

Then came an unpleasant surprise. Talcott and I had saved our collection of samples that contained tritium. At one point, the Lab decided to clean up and throw away all unused items. Unknown to us, all of the samples containing tritium were thrown out! This allowed the skeptics to say, "Well, now we don't have to listen to you. The proof is gone". The fact that we had measured the tritium and had the data didn't matter. We no longer had the samples, so in the skeptics' minds the tritium didn't exist.

ERAB PANEL SAYS NO TO COLD FUSION, 1989

In the late spring of 1989, the US Department of Energy appointed a panel of experts (Energy Research Advisory Board, ERAB) to investigate the claims of cold fusion. One panel member, Richard Garwin, talked to people at LANL. He came to my office. I told him what I was doing, he asked a few questions, thanked me, and never made contact again. When the report was made public, clearly he paid no attention to my work. He ignored what we had done. Once the report was available, all official work on cold fusion stopped at LANL.

As I mentioned before, John Huizenga was co-chairman of and had a major influence on the ERAB. As a professor of chemistry and physics, he was known to be knowledgeable about the techniques of electrochemistry. From a purely objective point of view, he was the logical choice to evaluate cold fusion. However, he was not, in any way, an objective evaluator, even from the very beginning. His biased attitude against cold fusion was truly extraordinary.

The ERAB panel was designed to get this cold fusion nonsense out of the way as quickly as possible. The draft report was ready in July and the final report was finished in November, less than eight months after the announcement from Fleischmann and Pons. The hammer was coming down even before the report was written. People who were respected by the mainstream science institutions were failing to initiate a reaction. They were not seeing heat, they were not seeing the tritium, or neutrons, and they were upset about their failure. Efforts were made at MIT and at various universities to look for the neutrons because they were absolutely certain the neutrons had to be emitted. After all, only one kind of fusion was known at the time. Cal Tech, MIT, and later Harwell, failed to detect even heat production. It didn't take the DOE long to make the case that cold fusion wasn't real and that the idea was a big mistake.

We saw that case was being made in the various reports[8] being distributed. They said, "We did many things to make it work and it didn't work." If people at an accepted level of skill and political connection can't make it work, then obviously it's not real. Never mind that other people did see evidence. Obviously, these people were seeing errors because they are incompetent.

Accompanying this colossal experimental failure was the sincere belief that such a process of cold fusion was impossible. A nuclear reaction could not occur in a chemical environment at room temperature. It just didn't make any sense. So, obviously Fleischmann and Pons made a mistake. And the few occasions when it did work, those were exceptions caused by some prosaic effect that we haven't identified yet. But we will. It never occurred to the scientific leaders that this was a different kind of fusion.

The DOE negative report nailed the coffin shut, which caused the support to disappear. However, some hope remained. The Cold Fusion Institute was formed in Utah with state money and headed by electrochemist, Fritz Will. Will was a careful worker. The team had the money to create a top-notch lab in a very modern setting with modern tools and modern data treatment. They bought high quality heavy water and used sealed electrochemical cells to make tritium. They were able to generate tritium and excess heat. In the process, they replicated the Fleischmann and Pons experiments. These really good studies were published. They had no effect on the skeptics. The myth was now in full bloom.

Even now, when the evidence for cold fusion is overwhelming, the facts are ignored. Even though the need for this energy is critical, the facts are ignored. Here we have an example of responsible politicians and scientists ignoring an energy source that has the potential to save humankind. I believe future generations will look back and think, "Oh my god, these people had the solution to global warming available and they ignored it. Why would they do something so stupid?"

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[8] D. Altman and J. Bland, Absence of evidence is not evidence of absence. *BMJ Clinical Research* **311** (1995) 7003. https://www.researchgate.net/publication/232268309_Absence_of_Evidence_Is_Not_Evidence_of_Absence

We can cite other examples of stupidity. The government spent several billion dollars on a successful nuclear propulsion reactor that was canceled after successful tests but before it could be used. As a result, all the work was wasted and the ability to send massive amounts of material into space was lost. A power reaction program was developed for use in space, the SP-100. It was also cancelled after spending many millions of dollars. Now that reactor could have powered the planned Moon base. Cold fusion could have solved the energy problems here on earth and could have made a manned trip to Mars possible. It was rejected. What do these rejections say about the ability of the US government to make rational decisions?

WORK CONTINUES, 1990

Although the studies were officially shut down, work at the group level continued. My group leader, Bruce Matthews, saw for himself that the tritium was real. So, I was allowed to keep working on cold fusion. At that time, the official SP-100 program was having problems that would cause it to be cancelled. Any further effort to understand how to design a nuclear reactor for use in space was obviously a waste of time. So, I spent my hours studying cold fusion even though it wasn't being funded.

In those days, a person could do important work even though it was not given official support. Later, the bureaucrats made such studies impossible. Only government-supported work would be allowed. Anyway, I worked in a no man's land after the official cold fusion funding had ended but before I actually retired and left the Laboratory in 1991.

The Lab had some discretionary money that was used to keep Claytor working on cold fusion. In this case, the decision as to how the money would be spent was not made by the DOE, but by the scientists themselves. The intention was to stimulate new ideas that the laboratory had no official reason to support. Professors and individuals from other national laboratories were assembled over a weeks' time to listen to the various proposals and to vote on which ones were to be funded and which ones were not.

Claytor presented his work. They concluded that his work deserved to be funded in spite of the DOE having rejected the claim and despite the fact that the laboratory administration was not interested. The scientific community, after listening to the evidence, concluded that cold fusion was real and it was worth funding. As a result, he had funding for several years. When the funding ended, he continued his work outside of LANL in his own laboratory at his own expense. Once again, we have a lesson in how official decisions conflict with how the system is supposed to operate.

DIAGNOSTIC DISCOVERY USING TAKAHASHI SAMPLES, 1990-1991

After writing the review of the experimental evidence that was published in *Fusion Technology*, I took the review to my division leader and said, “Cold fusion is real. I would like to use a calorimeter to look for heat.” He was very sympathetic, and found some money to build the calorimeter. He got in trouble later when the DOE discovered what he’d done, but thankfully, he had the wisdom, independence, trust, and most importantly, the data, to support the idea.

I built a calorimeter and started to look for the expected energy. I was lucky to get some samples from physicist Akito Takahashi at Osaka University. Takahashi had been looking for neutrons. In the process, he discovered that the batch of palladium metal was producing excess energy. He sent some of the palladium to various people, including myself. The first sample he sent produced excess energy. We were able to make as much as nine watts out of a couple grams of palladium. This was a significant amount of heat. The process took awhile to start, which was common in those days, but then all of a sudden the palladium started making energy. Takahashi sent samples of this active metal to other laboratories around the world and they all produced excess energy from that batch.

This behavior was even more definitive than the tritium production, because tritium was not the focal point of Fleischmann and Pons’ claims. The excess heat was. Fleischmann and Pons claimed they occasionally saw some tritium, but how tritium was related to this heat-producing reaction was still a huge mystery, especially in the absence of neutrons. The heat measurements made by the calorimeter at LANL using Takahashi’s samples were in direct support for Fleischmann and Pons’ claims.

At the same time, we explored the loading process. I measured what was called *excess volume*. Excess volume is produced after palladium takes up hydrogen, causing the lattice to expand. When the hydrogen is removed, the palladium contracts in a very strange way. The palladium sample does not return to its original shape or size. Instead, it retains extra volume while the thickness increases and the longer dimensions get smaller. In other words, it tries to turn into a cube. This excess volume measurement was very important, as we discovered.

When my sample stopped working for some unknown reason, we asked Takahashi for another sample. He sent another sample but it was from a different batch. That batch did not produce excess energy. But then we discovered that it made a tremendous amount of excess volume, up to 10%. We contacted Takahashi again and said, “This metal isn't working”. We discovered he was having the same problem. He went back to Tanaka Metals, who made the samples for him, and complained that this palladium was not like the first batch. Tanaka Metals admitted they had not made it the same way as before but thought this difference did not matter. Takahashi made clear that the next batch needed to be just like the first batch.

Takahashi then got a third batch and he sent us a sample. This sample made excess energy. In addition, we found that it made excess volume but more than the first sample but less than the second. Now we had three points showing the role of excess volume. We also had a diagnostic tool we could use to evaluate the palladium to decide whether or not it had any chance of producing excess energy. If the excess volume were too high, no excess energy would be likely. Samples showing smaller excess volume would more likely produce excess heat. We published this information, but nobody paid any attention. I never discovered from Takahashi or Tanaka Metals how the active palladium was made.

Electrochemist Michael McKubre at SRI International was having the same experience. He was seeing heat that was related to the nature of the material. He found that if a part of a batch were active, then the whole batch would be active. Something was being done to the material to create a universal characteristic that was able to support a nuclear reaction. This was important, but again largely ignored.

I continued to explore the materials issue when the New Hydrogen Energy project in Japan provided many palladium samples to test. The Japanese were interested in the excess volume measurements as a way of understanding what's going on, but also as a criterion for judging whether the palladium was going to be active. Those tests showed that, yes, the excess volume was an important criterion, but it wasn't perfect. However, it did eliminate palladium that would have been a waste of time to study.

TIGER TEAM ROUND-UP

About this time, LANL became very hostile towards cold fusion. Informal work continued at the group level, in my case looking for heat, and in Claytor's case, looking for tritium by the gas discharge technique, until the Tiger Teams came to the lab. The bureaucracy decided that Los Alamos was not using the money to do what was authorized, which was developing nuclear weapons. Instead, all kinds of other good ideas were being explored, like the study of cold fusion. So, my studies were shut down. Eventually Claytor was shut down also, although he continued a little longer than I did because he could hide it better than I could.

About that time I considered retiring because life was being made miserable by all the inane new rules. On top of that, the Lab had developed financial problems. People who were close to retirement were given a bonus if they retired early. A large fraction of the laboratory retired, including myself. This created a problem because now the lab could not function because most of the senior staff had gone. So, many of us were hired back as consultants. This allowed me to continue a study of cold fusion for another year.

Meanwhile, Carol Talcott and I fell in love and married. Carol continued to work while I started building a new home in Santa Fe on a lot I had bought years before. We designed the house, cleared the land of trees, staked out the foundation, and hired workers to help. We first built a space for Carol's art studio. This building would also become a woodworking shop and cold fusion laboratory. We lived there for a year while we built the main house nearby. Carol designed and created the original stained glass art we put in the windows and laid all of the tile. I built much of the furniture and did the plumbing and electrical work. The house became a very personal expression with a fantastic view of Santa Fe.

After moving into the main house, I set up a small laboratory. We didn't have much money, so I did only what we could afford. I started seeing excess energy from samples of palladium, which allowed me to explore the effect of different variables like temperature, current, and various treatments using the electrolytic method. I also started constructing various calorimeters in an effort to find the perfect design. I was doing in a home laboratory what the DOE would not support and most scientists believed was impossible.

COLD FUSION BEFORE CONGRESS, 1993

In 1993, a congressman from New Hampshire arranged for me to visit Washington D.C. and testify about cold fusion before a Congressional committee. Congressman Swett was a friend of Eugene Mallove, who had convinced him that cold fusion was absolutely essential for saving the world. Swett was an architect, so he had some technical training. He was also on the committee that was planning to evaluate the hot fusion program once again. Every time the hot fusion program managers came for more money, they would say they only needed a few more years. They would get the money and then they'd come back again next year and say the same thing. This time the Committee wanted to hold their feet to the fire more than usual.

Congressman Swett thought he could kill two birds with one stone. He wanted to give the hot fusion program a hard time. He also wanted to educate Congress about the possibilities of other kinds of energy that would perhaps eliminate the expensive hot fusion program. So, Swett told the LANL administrator, “I would like Storms to brief Congress on cold fusion.” At that time, I was officially retired from the laboratory, but still working as a consultant. That request didn’t go over well. “Cold Fusion? You must be kidding? We have official people who interact with Congress and Storms is not one of them." LANL didn’t want me to talk about cold fusion, especially before Congress. Swett was insistent. “I'm a Congressman on the committee that funds your work, so this is exactly what's going to happen.” So, the lab had to agree and proceeded to coach me so that I would not embarrass them completely.

It was fun going to DC. We found Swett's office early in the day; the hearing was scheduled for later that afternoon. During the morning he showed us around the Capitol building. Being an architect, he knew exactly how the dome of the capital was made. So, I joined one of his parties of constituents and got to see how the dome was built. It was constructed around the Civil War and only recently did they reinforce and redo it because it was starting to fall apart. People were afraid it was going to fall down to the floor. That was also one of his concerns. After that, Carol and I had lunch in the cafeteria where all the various people who work there ate. One of the lobbyists sat down at our table and wanted to know what I was doing. I told him about the testimony.

The hearing was in the afternoon. Researcher Randy Mills was invited to talk about his work. Dr. Maglich described a different type of design for a hot fusion reactor. The three of us were asked to explain before the Committee on Science, Space, and Technology, of which Swett was a member, how our different approaches for making energy might be better than the present hot fusion program. The committee members listened to all the talks and asked questions. Although the lab did not tell me what to say, they insisted I make clear that I was only speaking for myself and not for LANL. Nevertheless, I was able to put a lot of information about cold fusion in the official Congressional Record[9]. I was later sent a transcript of the entire hearing. Although the experience was fun, nothing changed. The hot fusion program got their money and cold fusion remained an ignored idea. I never heard from Congressman Swett again.

PRIVATE LAB

I knew that cold fusion was a real phenomenon and had overwhelming importance, not only scientifically, but also as a commercial source of energy. Cold fusion proved to be the ideal hobby for me during my retirement because I could do the study in my own laboratory. I also found it to be an intellectual challenge. When I wasn't working in the lab, I was reading the literature and putting the papers on the computer so that they could be easily found and read. When I started work in my own laboratory, I already had a large library about cold fusion, probably the most complete. In addition to providing valuable insights, the collection also made writing papers much easier.

Very few people were studying cold fusion. The national labs had stopped their programs. Most people did not have the tools, skill, and the interest to perform experimental studies as a hobby. I was one of the few. I probably had better equipment than most because of the skill I had acquired by working at Los Alamos. I could do all my own glass blowing, machining, and computer programming. I also did not have to earn a living, which gave me time to study the subject.

My approach has been focused on looking for heat energy using a couple of calorimeters. I used either an electrolytic cell or the direct reaction with deuterium gas. The apparatus is fairly small, self-contained, and designed to do one particular thing at a time. After that study, I moved on and used the same space for something else. I don't have many things going on at the same time.

Some of the first work I did in the new private lab used samples that people would send me. Dr. James Patterson, a chemist, was interested in having his samples tested, so he sent many. Dennis Cravens sent me some samples. Dennis Letts sent some samples. I also got some from Scott Little of EarthTech. I also received 90 samples of palladium from IMRA in Japan. I had earlier explored a study of excess volume in palladium and its relationship to LENR. That work got the

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[9] *Statement of Dr. Edmund Storms*, in Hearing before the Subcommittee on Energy of the Committee on Science, Space, and Technology, U.S. House of Representatives, One Hundred Third Congress, First Session US Government Printing Office, Washington, DC (1993) 114. .https://books.google.com/books?id=JApkwQEACAAJ&pg=PA114#v=onepage&q&f=false

attention of scientists in Japan. They sent me a number of samples that I tested. The palladium was later used to explore some of my ideas.

The work then was mostly trial-and-error. I made the studies in various ways just to see what would happen. Gradually, this provided insights about what worked and what did not.

TESTING THE DOE

Around 1994, a request was made through Steve Jones to submit a program to the DOE that they might fund. After all, the Department of Energy had basically said (in the ERAB report), "Cold fusion should be funded, but through normal channels." This was really a bureaucratic way of saying, "We don't want anything more to do with it, but we don't want to tell you."

Jones and various other people took that opportunity to submit a proposal through normal channels in order to test whether the DOE was serious. A group of us, including Jones and myself, wrote up a research proposal and submitted it through normal channels. It was a sincere effort to take the DOE at its word with a proposal submitted by legitimate people. We weren't a bunch of crazies; we were from a national laboratory, a university, and other institutions. We were submitting a proposal based upon the success that we've had in the past. Of course, it was totally ignored, as everybody thought it would be.

ON THE COVER OF COLD FUSION MAGAZINE

In May 1994, the first Cold Fusion Magazine was published by Wayne Green who had great enthusiasm for crazy ideas. He decided to create a magazine and then went around looking for money. Charles Becker offered to fund the magazine with Eugene Mallove as the editor and many of the major researchers on the advisory board. These were the days when we thought the idea would be quickly accepted and made useful. This optimism did not last.

Charles wanted a picture of me on the cover. So, the cover shows me holding a sample that had made tritium. I think there were two other issues of *Cold Fusion Magazine* and then it died. Eventually, it morphed into *Infinite Energy Magazine* with financial support from Charles Entenmann. Becker went back to his company and proceeded to try to recoup his money in a more legitimate way. He was very sympathetic to the possible applications and the money that could be made with cold fusion, once it was patented. Everything depended on getting a patent.

CHARLES ENTENMANN

Charles Entenmann provided the support for *Infinite Energy Magazine*, and was the first to start providing financial support towards my private lab. He would attend ICCF meetings and talk to people. He gave me some money because he liked what I had to say. I had a few long conversations with him and his son-in-law about the field. Every now and then, an envelope would show up with a check and a note saying, "Use this to take a vacation or do anything you want to do." I used his financial support to buy equipment for my laboratory.

He supported a number of people. Entenmann had made money unexpectedly through the baking company that his father had established. When the business was sold after his father died, he inherited more money than he could possibly dream of spending. He became aware of cold fusion through his son-in-law. The subject gave him an opportunity to talk to interesting, creative people while spending his money in a way that was socially beneficial. Charles Entenmenn thought cold fusion would do something good for humanity. Too bad the DOE did not have this belief.

SCHOOLING UP ON CALORIMETRY

In 1995, I set to work studying and designing calorimeters, while trying to understand exactly how they worked. I used isoperibolic calorimetry initially. Later I use the flow-type calorimeter or a mixture of the two kinds. Finally, I chose the Seebeck technique as being the best method.

Calorimetry is commonly used in chemistry, but it is complex and requires uncommon skill. On-the-job training was the only available teacher. The process begins with trying to find a sample that will actually make excess energy. I started by using palladium and heavy water to learn how to use a calorimeter and how the electrolytic process worked. Excess energy came later as a pleasant surprise.

I wanted to understand how temperature gradients within the material and within the electrolyte would affect the measurement. So I actively stirred the solution and determined the effect on the heat measurements. This approach revealed a lot of information about the basic behavior of these gradients. Even when I stir actively, I could not fully get rid of the gradients. As the stirring rate increased, the gradient would be reduced but a small error would remain. This was one of the big issues skeptics harped on.

When the thermal power is measured using the temperature change, the gradients would affect the measurement. However, Fleischmann and Pons used a thin, long cell in which bubbles caused a convection current. They argued that the stirring action of the bubbles tends to offset that gradient, which reduced the effect of the temperature gradients within the material.

In contrast, the studies I did showed there was a limit to this convection effect that could not be overcome, which limited the accuracy of the method. However, this effect did not nullify the various reports of excess energy when the energy was well in excess of that limit. But, when rather small amounts of excess power were measured, the result could be wrong.

Melvin Miles, a Navy electrochemist, initially used the F-P design in the lab at China Lake. Eventually, he turned to the double wall, isoperibolic-style cell, which eliminates the gradient problem. In this type of cell, the temperature is measured outside the cell, which eliminates the temperature gradient concerns.

I set up a fairly elaborate flow calorimeter that made the measurement two different ways, both as a flow calorimeter and as an isoperibolic calorimeter. I found out that those two styles of calorimetry were not compatible. When the flow is optimized, the isoperibolic method gives poor results, and vice versa. However, I did study a number of samples that showed interesting behavior, which led to a much better understanding of flow calorimetry[10].

Flow calorimetry is complicated. While being an upgrade from the isoperibolic method, it clearly has a lot of problems. The entire cell has to be isolated from the environment in a box and the box has to be kept at a constant temperature. Use of this method was a challenge. Eventually, I used only the Seebeck calorimetry, which is the ideal method.

NEW HYDROGEN ENERGY IN JAPAN, 1997

The New Hydrogen Energy (NHE) project invited me to Japan for a week. NHE was a government-sponsored consortium of various universities and companies that worked together at the same laboratory. This allowed experts to study cold fusion, learn how it works, and then take that information back to their organizations. They had a very fancy laboratory and were doing first-class work.

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[10] E. Storms, Description of a dual calorimeter, *Infinite Energy* **6**(34) (2000) 22. https://www.lenr-canr.org/acrobat/StormsEdescriptio.pdf

But they had trouble making sense of what Martin Fleischmann was saying when he visited. When Fleischmann tried to explain something, he was frequently incomprehensible to ordinary people, but the Japanese found him particularly difficult to understand. They wanted somebody to look over their data with whom they could communicate. After a week in Japan, they invited me to stay for several more months. I didn't want to stay that long, so I returned home. Melvin Miles was then invited to visit and stayed for several months.

After I got back from the NHE lab, IMRA, one of the companies involved in the NHE project, asked if I might test some of their materials. They would provide 90 samples. I would then put them in an electrolytic cell, load them, and measure any excess energy, the D/Pd ratio, and the excess volume. Four of the samples produced excess energy, while one of the samples stood out for making a more noticeable amount of excess energy[11]. This low success rate demonstrated just how rare heat production could be. I got to keep the samples and used the palladium for years. palladium is very convenient to study because it can be melted with a torch and recycled.

JAMES PATTERSON

In the mid-90s, James Patterson claimed that beads of plastic covered with palladium would make excess energy. He sent some to me and to Tom Claytor for testing. Claytor set up a device similar to what Patterson was using and found out that excess energy was measured depending on where the thermocouples were placed. Not good. Unfortunately, no real evidence showed that the Patterson method was successful.

Dennis Cravens had a private lab at the time. He was asked by Patterson to replicate his work and demonstrate it at one of the conferences. Jed Rothwell got interested, so he brought a temperature measuring device to the conference. Rothwell tried to get Patterson to let him make some measurements but Patterson refused. Of course, so did Cravens, because he was working for Patterson. That soured Rothwell on the claim for excess energy. He said, “it looked like” they were making excess energy, but unless he was given a chance to make sure, he wasn't going to believe it.

Patterson gained fame because he was the first one to get a patent through the Patent Office. He did this in a very underhanded way that embarrassed the Patent Office. Afterwards, getting a

[11] E. Storms, 1999, My life with cold fusion as a reluctant mistress, *Infinite Energy* **24**(24) 42.

patent was even more difficult. The patent had no value whatsoever because he did not describe anything that was reproducible.

General Atomic tried to replicate what the patent described. The method Patterson used to make beads did not work. Instead, they made the beads using the known method. After applying a coating of palladium, according to the patent, they got no excess heat at all. When General Atomic couldn't make the beads active, Patterson said, “Well those weren't the same beads, that's why it didn't work.” Apparently, Patterson purposefully left out details. Why did he do that? Was he trying to hide the secret or was he totally incompetent? Whatever the case, the people at General Atomic determined the patent could not be replicated, which makes the patent useless.

Even now, no one is using the bead technique to initiate LENR because the coating kept coming off the beads. The method wasn't sustainable even if a little excess energy could be made, which was doubtful. One of the many ironies of the cold fusion field that is full of such instances.

ENECO AND FLEISCHMANN AND PONS’ PATENT, 1994

Fleischmann and Pons had sent a patent application to the U.S. Patent and Trademark Office (USPTO) for their heat-producing cell. Assuming they received their patent, they would need to find a company that would be willing to lease the patent technology and implement a plan to make the phenomenon useful. ENECO was that company. Its primary mission was to get the Fleischmann and Pons patent through the USPTO and then to lease it to companies that would develop the technology. Money would be made from the lease fees.

Charles Becker set up the company with investors to fund the patent process. Half a dozen different investors created ENECO. Its basic goal was to get the Fleischmann and Pons patent granted. Unfortunately, the USPTO refused to accept the patent. I was part of an effort to convince the patent office that cold fusion was real and deserving of a patent. I was asked to write a review of what had been done by other people. Later, Becker asked me to join the corporation and be on the board of directors.

At this point, I had already written a review of the field that had been published while I worked at Los Alamos. ENECO asked me to update the review. Sometime in 1994, I assembled a description of what was known about the science at that time. The lawyer at ENECO put it into a legal framework and sent it off to the USPTO. It was totally ignored. Later, I provided them with another review, which was an update of the first one. Eventually, this effort became a book called *The Science of Low Energy Nuclear Reaction*[12].

We had hopes, idealistic beliefs, that the USPTO would play by the normal rules and would grant a patent. But the USPTO raised all kinds of objections with imagined explanations as to what kind of possible source this energy might be. Our team at ENECO would test their ideas only to discover they didn't apply at all. We'd go back to the USPTO to report this. They would then reject our argument by using a New York Times article saying that cold fusion was not real. In their eyes, cold fusion violated the theories of conventional science and therefore was not patentable.

To the USPTO, cold fusion was in the category of perpetual motion. The evidence did not matter. The legal arguments did not matter. They just simply rejected everything based upon their beliefs that this was not a real technology; that Fleischmann and Pons had made a mistake. Apparently, the USPTO was under political pressure to see that the patent was never granted. After the USPTO rejected the patent, the patent rights reverted back to the University of Utah.

ENECO FUNDS MATERIAL GAINS

ENECO had spent an enormous amount of money trying to get the patent accepted. To justify this expense, they set up a laboratory and did some experimental work of their own. While they were putting a private lab together in Salt Lake City, ENECO hired me to do some of the experimental studies in my own laboratory. They wanted me to replicate what Fleischmann and Pons had done as further confirmation that they were right. ENECO provided some palladium as well as some of the equipment.

In the process of replicating what Fleischmann and Pons had done using an electrolytic cell, I had to create a calorimeter that could be trusted. In the process, I discovered a lot of the pitfalls and frustrations by not being able to replicate what they had done. The key is in the nature of the palladium cathode material. That fact was not fully appreciated at the time. People thought that just by electrolyzing some palladium, excess energy would result. If excess energy was not produced, it was because the work was not done properly. Now we understand that even if

[12] E. Storms, *The Science of Low Energy Nuclear Reaction, (*World Scientific Press, 2007). https://www.worldscientific.com/worldscibooks/10.1142/6425#t=aboutBook

everything is done exactly right, excess power will not happen unless the right palladium is used. Success depends on the material.

I spent a year and a half with ENECO before I started studying their samples in the lab. ENECO would give me some money and send a sample to study. I would write a report and send them the result. I never found a sample that made excess energy.

ENECO'S LAB RESEARCH

ENECO created their own laboratory. Yan Kucherov was hired as their laboratory director and he brought with him a number of experienced people from Russia. At that time, Russia was going into economic free fall and the scientists in Russia were available for hire. ENECO hired a group of Russians who had been doing work in this field or related fields. The program designed by the newly hired crew was very clever and they proved an asset in the field.

They proceeded to try to replicate the Fleischmann and Pons claim. In those days, no one understood cold fusion. Everybody had their own theory and Kucherov was no exception. I vigorously disagreed with the idea he proposed. My opinion carried little weight because I couldn't prove he was wrong. Because nobody had any better idea, they accepted his explanation. Unfortunately, this theory failed to show how LENR could be caused.

I got into a conflict with the CEO (Fred Jaeger) over the interpretation of the data and what needed to be done to move forward. He wasn't paying any attention to my suggestions so I resigned from the Board of Directors.

As became increasingly clear, the patent would never be granted. ENECO had to turn their attention elsewhere to justify the investment. Kucherov continued the work until ENECO folded. He returned to Russia to continue to work mostly on theory. His health was not good and he eventually passed away from pancreatic cancer.

JAMES PATTERSON ENDS ENECO EFFORTS

After Fleischmann and Pons failed to get a patent accepted by the U.S. Patent Office, they submitted one to the European Union. If they could get a patent in Europe, that might set a trend. It would have shown the USPTO that somebody with authority believed LENR is real. ENECO could lease the technology to the Europeans for further development. This wasn't ideal, but it could be a path forward.

The rules are different in Europe. The European Union was much more accepting. However, after a patent is published, the EU waits to see if anyone objects before granting the patent. James Patterson objected to the patent. This prevented the patent from being granted. Patterson didn't want Fleischmann and Pons competing with him; so, he prevented Fleischmann and Pons from getting a patent in the European Union.

Everybody in the cold fusion field was disgusted with Patterson. He ultimately contributed nothing to the field. He will be remembered only because he kept Fleischmann and Pons from getting the support they needed. The Pons and Fleischmann patent was eventually granted in Brazil and a few other countries.

ENECO essentially ran out of options short of suing the USPTO in the District Court in Washington. They were advised that they might win, but the USPTO would drag out the process until ENECO went bankrupt. ENECO couldn't possibly win, so they gave up. As history will demonstrate, the US government actively denied mankind the energy source that would have slowed global warming.

THERMOELECTRIC ENECO

ENECO continued for a couple more years independent of me. They shifted from trying to replicate LENR to trying to create a very efficient thermo electric converter. They hired Peter Hagelstein, a theorist and professor at Massachusetts Institute of Technology, as their scientific advisor and theoretician to direct that effort. Since they couldn't easily solve the LENR problem, they changed direction in an effort to make an efficient thermo electric converter. They could then apply this to LENR to make electric energy from the heat, should they ever solve the LENR problem.

ENECO signed a contract with the Navy, which needed that kind of conversion of heat-to-electricity on ships. The Navy had an ideal application because the ocean was available as the constant temperature reference that such a device needed. Besides, the thermoelectric converter might be a very compact method for energy conversion. However, the efficiency was only about 10% . If this could be doubled, the device might be practical.

Hagelstein provided ideas as to how to create the right conditions to make this work and the team at ENECO was able to improve the efficiency. Unfortunately, this newly improved design failed at even modest temperatures. The device could not be used under practical conditions. When the design did not meet the qualifications that were needed, ENECO lost the Navy contract.

Cold fusion wasn't of interest and now the thermoelectric idea wasn't working. They ended up getting involved with some shyster who promised to buy out the company and use it for other purposes. Instead, he proceeded to run the company into bankruptcy and everything went downhill from there. Around 1998, a lawsuit finalized the end. As a result of his efforts to help mankind, Charles Becker lost about six million dollars thanks to the USPTO. Becker was one of the great, courageous investors early in this adventure.

The USPTO hurt the field tremendously because of their incompetence. The USPTO action was clearly illegal based upon the statutes. They simply were not going to allow a patent to be granted no matter what ENECO had to say.

WIRED MAGAZINE AWARD, 1998

Michael McKubre and I were featured in *Wired Magazine* in 1998. We went to Los Angeles to accept the awards and had the chance to talk to other people who also received awards.

Wired Magazine published a list of people who were doing things that they thought would have a profound impact on the future. Somehow, they became aware of cold fusion and realized that it would, in fact, have a significant impact. They contacted McKubre to join this group, acknowledging his contribution to cold fusion. McKubre told them to contact Storms as well. So, they asked me to join McKubre as a representative of the cold fusion effort, to which I gratefully accepted. *Wired Magazine* paid for our trip to Los Angeles and the hotel stay. They had a big ceremony with many people from the press and from the surrounding Los Angeles area. All of the 25 awardees were introduced. Mike and I were honored. Everybody had wonderfully supportive things to say. They gave us prizes. In my case it was a pair of shoes that didn't fit!

Then we went home and waited for the phone to ring and for someone to offer us a million dollars to support more studies. But nothing changed. The event and the award did, however, reinforce the idea that maybe some educated people did care about this science and the future of mankind.

ELECTROLYTIC NICKEL CATHODE, JANUARY 1999

In January of 1999, Italian physicist Francesco Piantelli claimed he could activate nickel using gas loading at a high temperature. Presumably nickel could also be actuated using electrolysis. I began to look at nickel as a cathode material in the electrolytic cell to see whether or not it could be activated that way. I tried various combinations of materials; nickel, silver, palladium, plating, not plating. Nothing worked.

I also explored different kinds of palladium. One question the theory asked, "Was an active material a superconductor". I attempted to measure the superconductivity of the material as it loaded, using magnetic susceptibility. I never found anything. If superconductivity happens, it's so minor that it's not readily detectable. What's worse, I had the same problem as everybody, getting LENR to occur at all. If the reaction does not happen, the results have no meaning.

A VISIT FROM EUGENE MALLOVE, APRIL 2000

Eugene Mallove took an interest in information he could give to the press and to various people. In April of 2000, he came to visit me. I don't think the house was fully finished at that point. He documented the entire trip. He made a video showing the visit from his arrival to the end. As far as I know, this video has not been made public and I do not have a copy.

KEN SHOULDERS' EVOs, AUGUST 2000

Perhaps the Exotic Vacuum Objects (EVOs) might have a role in cold fusion because they have a very, very high negative charge. This charge would be available to offset the Coulomb barrier. Ken Shoulders formed the EVOs by gas discharge. I wasn't set up or interested in studying gas discharge at that time, but from a purely theoretical point of view, I felt that if they could be made their presence could initiate a nuclear reaction. Shoulders showed evidence of their causing nuclear reactions when they moved through a material.

Shoulders would keep me apprised of what he was doing because I was one of the few people who thought his work was worthwhile. He even visited my home in Santa Fe using his own helicopter. When he needed gas, he would just drop down to a gas station and fill it up. When he wanted something to eat, he would land in the parking lot and go to the supermarket. Of course, that attracted attention. He would give rides because people were interested. This was a fascinating way to travel.

He worked at SRI and had developed a lot of instruments for them. He was independently wealthy as a result of many patents. His devices were really well designed because he had an advanced high-tech scientific background. Shoulders would periodically send a CD-disc describing his work. That's how he made his information known. Unfortunately, few people were interested in what he was discovering. He submitted a patent and wrote a book, but did not make the details of his discovery available to the general public.

He discovered that electrons could form an unusual type of assembly around clusters of ions. He found these clusters to be very common in every gas discharge, by which he could make the EVOs very easily. When they passed through matter, they created a very precise hole. He was able to identify nuclear reactions on the sides of the holes. Once again, the phenomenon was rejected by conventional science because it conflicted with conventional understanding, just like LENR.

GEORGE MILEY AND LEW LARSEN, 1999-2000

At one time, I believed the nuclear active environment occurred in the surface region of the cathode. Therefore, the rest of the sample didn't really matter. If so, perhaps a thin layer of material (e.g., palladium) on platinum, which is inert, would work. This might allow the location of the nuclear active environment to be identified. I studied platinum under different conditions including applying deposits in the cell using co-deposition and out of the cell by depositing different materials on platinum before putting it in the cell. I got involved with George Miley and Lou Larsen[13] at this time.

George Miley is a Professor of Physics at University of Illinois Urbana-Champaign. He had courageously published cold fusion papers as the editor of *Fusion Technology*. Miley analyzed the coating on the beads supplied by Patterson to determine the transmutation products. Obviously, LENR is occurring because transmutation products are being produced. Therefore, this can't be that big a deal. It can't be that difficult.

Miley was claiming to be able to make excess energy on a regular basis. That claim attracted Lou Larsen, an entrepreneur in the cold fusion technology field. Larsen thought, “why not modify the conditions and file another patent from which we can make money.”

[13] The efforts made in 2000 began receiving support in 2003. That work with Lew Larsen continued until 2006.

Larsen then made a deal with Miley to replicate the process in a different way. They created a company together called Lattice Energy. Each had 50/50 ownership of the company. The whole idea was that Larsen would get money to fund the work that Miley was doing at the University of Illinois. Then, Larsen and Miley had a falling out because Miley could not keep the work secret. As is characteristic of all universities, it's difficult to keep what is happening in the lab secret. Larsen was paranoid about the information being known by other people. He was a strongly suspicious person. He suspected that everybody, including the Chinese, was trying to steal the science that he wanted to develop and profit from. Larsen was afraid that the students at the university were blabbing to competitors and giving away the secrets.

In addition, Miley believed he owned the information so he patented the work that Larsen had paid for. Larsen felt this was a betrayal of their agreement, so they parted ways.

LATTICE ENERGY

Lew Larsen now needed a scientist in Lattice Energy because he was not a scientist, though he had some background in biochemistry. He was essentially a person who was trying to raise money, but he needed a scientist who would do work in the laboratory and give some scientific legitimacy to the company. So, Larsen called me. He proposed I work for him. He would supply the money. In exchange, he would give me 7% of the corporation that he had created. I was a little insulted, but clearly, after his break-up with Miley, he wasn't taking any chances.

That offer didn't make me enthusiastic about working for him. On the other hand, the chance of him making any money wasn't very high. However, if I *did* discover how to cause LENR, the company might be worth a lot. So, we started working together.

He wanted everything encrypted with passwords because he was very afraid the Chinese were hacking into his computer. He claimed he saw this happening on a regular basis. He didn't even trust me to keep a secret. Ironically, no one was interested in what was known about LENR at that time. In addition, the explanations popular at the time were wrong and useless.

MILEY THIN-FILM TESTS

Miley had claimed he could make excess energy using thin deposits of material on microscope slides. He said the technique was essentially a replication of Patterson's work. Instead of using the beads, he was applying the coating to flat glass. Miley claimed he was successful in making excess energy when the layer was reacted with deuterium in an electrolytic cell. He made this claim at the ICCF-8 conference. He claimed the calculated energy from transmutations matched the energy he was measuring experimentally. But, Larsen wanted an independent test.

So, I was given the task of testing what Miley had done. This involved building a very specialized calorimeter. He sent samples from which he had claimed to have produced excess energy. I tested these in my calorimeter.

The cell was rather elaborate with a recombiner. I could measure temperature, open circuit voltage, and excess energy very accurately. In October of 2000, I started testing the samples that he supplied. In the process, I discovered he was using an incorrect value for the neutral potential. Because he was using open cells, he had to make a correction for the amount of gas that was leaving the cell, which uses the neutral potential. When the correct neutral potential is used, his measurements showed no excess energy. In other words, his claimed excess energy was not valid.

The *enthalpy* of the $D_2O = D_2 + \frac{1}{2}O_2$ reaction is used to calculate the correct nuclear potential. Instead, he was using the *Gibbs energy* to make the calculation, which gives a large number. I told Miley about the error. Miley didn't really understand the issue, so he said I should talk to Lipson. Physicist Andre Lipson had been working with Miley at the time, and he had made those calculations. I went to Lipson and said that's the wrong number. The value you were using is based on the Gibbs energy, but that's the wrong number. He said to me, “No, you don't know what you're talking about. That's the right number. Go away.” The error was left uncorrected. However, in my book (*The Science of Low Energy Nuclear Reaction*), I point out the error, why it's wrong, and how the correct number is calculated.

When a recombiner is used, as I did, this correction is not required. Consequently, I got zero excess power when I studied his samples. But if I removed the recombiner, and made the correction he made, the calculation gave the same amount of power he claimed to make. So, the data in the literature claiming he made excess power is wrong. This meant the method he used to make active samples had no value.

PALLADIUM AND DEUTERIUM

Lew Larson provided some palladium. We explored each piece of metal, treating it in different ways. Larsen also provided an SEM for my use. This allowed the samples to be examined at high magnification. We needed to find a way of treating the palladium to produce an active sample. That is, of course, everybody's goal. An active sample would occasionally be produced, just enough to keep Larson and me interested. But every time we tried to replicate it, we failed. Of course, that was the basic complaint of the skeptics.

They were absolutely right, replication was difficult. But skeptics would go one step further, and say, "Therefore, your success wasn’t real." That's where they made their mistake. The success was real, we just didn't know how to make it happen repeatedly. The critical conditions needed to initiate a reaction were special. The condition could be created on purpose, but with difficulty. The process was influenced by many variables over which we had absolutely no control. At that point, the creation of the nuclear active environment was accidental but real when it happened.

Carol and I went to Italy for a conference where I presented a paper describing the platinum thin-film study. I was able to make excess power several ways. I would deposit palladium onto platinum, which would produce excess energy[14]. I was making samples fairly regularly, trying to find the magic combination. And the magic combination would periodically show up. Each time excess heat was made, a little more information about the process could be obtained.

LES CASE STUDY, AUGUST-OCTOBER 2001

In parallel with the Lattice Energy work, I continued to explore how to make the cold fusion reaction 100% reproducible. One of those side projects was the study of Les Case’s claim. Les Case was a chemical catalyst expert, who had developed a relationship with the United Catalyst company. He asked his friends there to make a special catalyst for him. This was not a commercial product that was made in large amounts. It was made solely for him to his specifications. The deal was, if it made excess power, Case would pay for it. If it didn't work, he'd send it back.

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[14] E. Storms, *The nature of the energy-active state Pd-D,* in II Workshop on the Loading of Hydrogen/Deuterium in Metals, Characterization of Materials and Related Phenomena, Asti, Italy (1995). https://www.lenr-canr.org/acrobat/StormsEthenatureo.pdf

The people at United Catalyst needed charcoal on which palladium was deposited. They chose a charcoal made from coconut shell. This charcoal had been used in the past when they made other catalysts. The process involves first wetting the charcoal with a solution of palladium chloride and HCl. The excess water is removed and the palladium chloride is reduced to palladium using hydrogen or deuterium. This material was so successful in generating excess power, Case decided to scale up. He went from a small calorimeter to a huge calorimeter that was too big to even get into his laboratory!

Les Case was very creative, but very non-communicative. His descriptions, both written and oral, were totally incomprehensible. But people took notice of his claim to be able to make excess energy using this more or less conventional catalyst of palladium on charcoal.

SRI REPLICATES CASE

Michael McKubre, the Chief Electrochemist at SRI International, and the team at SRI attempted a replication. At first, they failed. Then Case was invited to visit the lab, and demonstrate personally what he had done. When Case supervised the activation of the material, material did indeed make excess energy. McKubre was able to measure energy production simultaneously with production of helium. The lab at SRI was able to obtain many measurements over a period of time as energy was produced along with accompanying helium. They made very accurate simultaneous measurements of the ratio of energy production and the helium production. The helium production and the excess heat were found correlated. This was a spectacular result.

I wanted to try a replication as well. I asked McKubre for some of the Case material and he sent some.

MY LES CASE STUDY

At that time, I had a scanning electron microscope with EDX. It allowed me to see what the material looked like as well as to determine the chemical composition, which had not been measured or reported before. This was very complex material. The charcoal contained many elements that would have been part of the original coconut. It also contained a small amount of palladium as advertised.

At the end of 2001, I was using flow calorimetry. The Case study required a different style, so I used the occasion to build a couple of Seebeck calorimeters, starting from scratch. These used a polyethylene tube about six inches in diameter and about two feet long. I drilled holes in the wall, and put in a thousand thermocouples that I spot welded so that they were in series. The outside was then cooled by flowing water. The calorimeter actually did a fairly good job of measuring heat produced on the inside. The cells I used for the Case study were made of Pyrex to which I added deuterium gas. But I wasn't having any success. I called McKubre to find out why. He revealed the secret that Case was keeping as to how the charcoal had to be activated.

This material had not been chemically reduced. That is, it still contained the palladium chloride that was distributed throughout the charcoal. This had to be first converted to palladium metal in a finely-divided form. To do this, the material had to be heated to allow the $PdCl_2$ to redissolve and then have the water evaporate rapidly as steam. This caused the $PdCl_2$ to be deposited as very small crystals. The small particles of palladium chloride were then reduced to palladium metal by heating the sample in deuterium. Without this treatment, the material would not produce LENR.

I tried to replicate this procedure as best I could, but there was nothing in writing, and it did not come from Case directly. Furthermore, Case wasn't here to say, "Oh, no, no, you did it wrong. I didn't tell you about this other little detail, but that's important." I'm sure I did not replicate precisely what Case required because the material did not produce LENR. That didn't mean that it couldn't work. It simply meant that I did not have the skill to make it work.

Apparently, the type of charcoal was absolutely critical. It only worked with that one kind of charcoal. Nobody knew this until Case ordered more catalyst. He had been so successful, he decided to scale up, and needed a rather large amount. This batch did not work. He asked the supplier, "What's going on here?" He learned they had a big cleanup campaign at the end of the year. The drum of charcoal they were using was tossed. So they used another supply of charcoal. This charcoal did not work. Now Case could not make excess energy, but had a huge calorimeter that couldn't fit into his house. Eugene Mallove volunteered to store the thing at his place. Dennis Cravens acquired much of the equipment after Case died.

I believe the palladium metal is not nuclear active. This material only allows the deuterium to be converted to single atoms. The charcoal itself is the location of the nuclear activity. The hydrogen atoms are able to travel over the surface of the charcoal and interact with the various morphologies that existed in the charcoal.

THE SEEBECK CALORIMETER, 2002

I used Seebeck calorimetry while I was working with Lew Larsen. I made two calorimeters, both of them alike. They had an error of about 15 milliwatts. I used them for years, but gradually they deteriorated. Recently, I replaced those with a better design, which is the one I'm using now. This design is based on using commercial thermoelectric converters. I made a water-cooled box out of aluminum and covered the inside with 54 converters connected in series. The size allows the study of electrolysis up to 90° C or gas loading at temperatures up to 500° C. The random error is ±0.005 W over the entire temperature range. The design has been copied by Google and by NASA.

JOHN DASH TITANIUM

John Dash claimed to make large amounts of energy using titanium as a cathode and sulfuric acid as the electrolyte. He told me what he did and I decided to try it. I had visited his lab at one point, so I had seen his setup. Later we corresponded about the details. I was able to make some excess energy using his technique. However, the problem was more complicated than he was aware of.

As I remember, he did give me some samples and I had some titanium sheet as well. Titanium doesn't hydride readily because it's covered by an oxide that is not reducible by hydrogen. It’s difficult to get current to flow. When current flows, it starts at the corners where the voltage is particularly high. This high voltage can decompose the titanium oxide, which allows the hydride to form at those locations. The hydride forms as a black powder at these locations, which then falls to the bottom of the cell. While that's happening, some excess energy will be produced. Presumably, between the time the hydride forms and the time it falls off, some excess energy can be made. The time during which energy is made is so short, the process is not very effective, so I abandoned this method.

TOM VALONE AND THE INTEGRITY INSTITUTE, AUGUST 2002

Tom Valone prepared a compendium called *The Collected Works of Edmund Storms About the Science of Chemically Assisted Nuclear Reactions*. He didn't check with me first. He just took papers I had previously published and compiled them into a collection. He put this on his website and started selling it. I needed to find out what he was publishing, so I bought a copy. I realized that this is what I've already made available.” I figured, "Well, okay, that's fine. He is not going to make any money, but the effort will make this information more available, so it’s all good."

DENNIS LETTS, 2002

Near the end of 2002 and into 2003, I started studying the Letts Effect. Dennis Letts found that when a cathode is radiated with a laser, excess energy would result. But, the sample had to be coated with gold. This claim was new and really rather amazing. He didn't know whether he could turn it back on again after the cell was turned off. So, he put an active cell and a battery in the trunk of his car and drove from Austin to California to show it to Michael McKubre at SRI. McKubre became convinced that he was making excess energy.

I set about exploring the process in more detail. What exactly did the gold do to the surface to make this happen? Could it be used *without* the laser to initiate a reaction? He lent me the same laser he was using and showed me exactly what he was doing. He was using isoperibolic calorimetry, which created some doubt about the validity of the values. He didn't have a good absolute calibration. He only knew that the laser caused the calorimeter to drift away from zero.

I used a Seebeck calorimeter that had been calibrated to duplicate using his protocol. He had an elaborate 12-step process for sample preparation. I prepared my samples in a much less complex way. His protocol came about simply because he was able to make excess energy. Then he would try to duplicate the treatment as close as possible, without paying any attention to the *part* of the treatment that had an impact. Instead, I only used the important part of the treatment. I got positive results. However, the nuclear reaction had to occur before the laser was applied. The laser alone did not turn the reaction on. Some excess heat had to be generated first. Then the laser would amplify the amount of power. The laser did not *cause* the excess energy. It only *amplified* the effect.

In my case, I could see the excess energy, whereas Letts could only see a change. He didn't know whether excess energy was being made or not. His calorimeter wasn't well enough calibrated to be able to tell where the zero was, but the Seebeck calorimeter was[15]. Plus, I also used an

[15] Storms, E., *Use of a very sensitive Seebeck calorimeter to study the Pons-Fleischmann and Letts effects,* in P. Hagelstein and S. Chubb, Tenth International Conference on Cold Fusion, (World Scientific Publishing Co., Cambridge, MA, 2003) p. 183. https://lenr-canr.org/acrobat/StormsEuseofavery.pdf

isoperibolic calorimeter, which confirmed the heat. When I made no excess heat and put the laser on the sample, the laser had no effect at all.

I also found that the gold layer seemed to have some beneficial effect to getting excess heat, but it was not necessary. Later when I had the SEM available, I looked at some of those gold layered samples. The surface of his samples was a mess. It had gold, iron, and copper on it. The surface was very uneven and filled with little chunks of stuff. Where the nuclear active environment was, or what part of that surface played a role, was hard to know. Knowing the role of the gold was impossible.

When the laser is applied to the metal cathode, maybe a square millimeter or two, the surface is illuminated. The rest of the surface doesn't know the laser exists. That means the laser has to be applied to a part of the cathode that is already nuclear active. This event would happen only by accident. This adds further complexity to the interpretation.

The effect might be only related to the temperature because the laser heated the sample. It applied 35 milliwatts to a region of about a millimeter in diameter. The 35 milliwatts could be measured by the calorimeter, so I knew the absolute power that was being applied by the laser. But this energy was applied to a very small area that would get hot.

This study really needs to be done again, with really good control of where the laser is applied so that the region could be examined using the SEM to determine what the active site looks like.

WASHINGTON DC MEETING ORGANIZED BY LARSEN, 2004

In 2004, a meeting in Washington, DC was arranged by Lew Larsen involving people from the Department of Energy. It wasn't a formal meeting, simply a half-dozen individuals. I gave an update on what I was doing in the lab and Larsen described his ideas. This was before the Widom-Larsen theory was published. He was trying to educate people in the government about the nature of this research. The meeting started with an overview by Lew on cold fusion and the effects found in the laboratory. Then he talked about his business. Two other people gave talks about cold fusion. Then the people from the DOE educated us about their attitudes.

The attitude in the DOE was conflicted. The official attitude was expressed in the 1989 ERAB report in which the government had rejected the whole idea. But many individuals in the Department were aware of the reality and were interested. But they couldn't support any study either with a policy change or money, despite knowing cold fusion was a real and useful phenomenon. It was fascinating to realize the contrast between the official, public view and the attitude of individuals within the organization. A summary document was given to the people present, but nothing changed.

SECOND CHANCE FOR DOE, 2004

The DOE's rejection of cold fusion is apparently part of official government policy. No change is possible. We were told by the ERAB report that we could submit proposals through normal government channels. People submitted proposals that were summarily rejected. Although the DOE promised the submissions would be treated in an objective way, in fact they were not.

Peter Hagelstein, a researcher at Massachusetts Institute of Technology, had a friend who made contact with the head of the DOE at that time. Peter suggested that they hold another review, which was implemented. This particular review was, from my point of view, organized solely to discredit the field without having to take blame for rejecting the claim. This was a typical bureaucratic way of dealing with a situation they don't want to address in a direct way.

They assembled a group of 22 scientists, a good fraction of whom had absolutely no relationship to or knowledge about cold fusion. Only a couple of these people showed up at the actual meeting held in Washington, DC. I was invited at the last minute by Mike McKubre a week before the meeting was to be held. He sent a summary of what they planned to present and asked if I would attend. I read the summary and told him, "This approach is not going to have any effect. Furthermore, this is obviously a bureaucratic fig leaf and you need to treat it that way. You need to know what you're getting yourself into. I could rewrite what you're submitting if you want and give a talk." The response was, "No, it's already set in stone. Peter Hagelstein is going to describe the subject. A few other people are already scheduled to talk. If you want to show up, fine, but otherwise that's it." I said, "Well, this would be a waste of my time so no, I'm not going to bother to show up."

The meeting was a total disaster. As described to me, most of the time was spent by somebody from the DOE describing boilerplate, which is their method of using up time. Then a few people gave their talk. When Peter started to talk, everybody's eyes rolled and they went to sleep. Then the meeting got totally out of control. The skeptics and the people who really wanted to get some useful information started quarreling among themselves.

When the report was written, the conflict between the official summary and the various scientific authors was remarkable. Some of the scientists actually gave a response, most didn't. The basic attitude was, “This is really interesting. We suggest more emphasis should be placed on a study of cold fusion.” In contrast, the executive summary gave the impression that nobody thought this subject was useful at all and that the original rejection by the DOE was totally justified. I was particularly annoyed because the summary was a gross distortion of what was actually in the document itself. It was clear that the politicians had one attitude and the scientific establishment had a different attitude. I wrote a paper, along with Jed Rothwell, describing how the DOE lied about the facts. As a result, a mixed feeling was created in the cold fusion community. Some people believed, “Be careful, don't piss them off too much.” My feeling was, “It's way beyond that. Pissing them off is going to have absolutely no effect whatsoever. We need to acknowledge what is actually happening so that historians in the future can understand what was going on and our reaction to it.”

RANDOM SAMPLING WITH LARSEN, 2003

In April of 2003, Larsen and I actually made our relationship official. I joined Lattice Energy as Senior Scientist. I would write the Progress Reports summarizing the successful results. If the result was not successful, I didn't bother. The information was thrown into a file and never seen again. But periodically, nature would give me a slight gift that I would describe in a report. They are a rich source of what was going on at that time.

Larsen hoped I could discover how cold fusion worked so that Lattice Energy could start making money. So, I was given a lot of freedom as to what I could study. He provided the money to buy palladium. I would treat palladium in various ways to see if I could encourage the process to occur. I studied many samples, most of which produced nothing. The process would occasionally work at a low level. But figuring out how to increase success was the real problem. And that's still a real problem.

In 2004 I started using silver as a substrate on which palladium was plated. I would add various chemicals to the electrolyte just to see what would happen. People believed the active material was on the surface. Getting the deposit just right was the challenge. Unfortunately, that approach was not successful because the surface is complicated and sensitive to many uncontrolled variables. Getting the same surface to form was difficult. I had a scanning electron microscope available so I could see what was happening. I became very aware just how complex a surface actually is. Many theoreticians make up theories that have absolutely no relationship to what is actually present on the surface. The real surface of active samples has no relationship to the ideal surface that theory likes to model. The active surface is incredibly complex, and very difficult to reproduce. The surface is like a view of Canyonlands National Park from 10,000 feet.

Various people used what is called *fibrous nickel*. It's essentially a cloth, consisting of a compressed structure made of nickel wires. It's useful as a filter because the gaps between the wires are small. Sometimes it's also used as a catalyst. People thought high surface area might enhance cold fusion.

Following this suggestion, I set about using this material as the cathode. It did not activate. It would not have been a very useful form anyway. The interior had such poor physical contact with the surface that the fusion of hydrogen would've involved only a very small part of the material. The surface was so irregular that only certain regions carried all the electrolytic current. In other words, people were not considering what is actually happening in the suggested material.

Every once in awhile I would get an inspiration. I would choose one of the samples and treat it in a novel way that it had not been tried before. I had a small collection of samples that had turned on in the past. Most of the time, they would not make energy again. According to my present theory, in order to keep a sample alive, it must not lose all of its deuterium. So now I make sure the samples are stored so as not to lose deuterium.

THEORY ENDS EXPERIMENTS

We did see evidence for excess energy on a few occasions. But we could never reproduce the success. It happened just often enough to keep us sucked in but not enough to go to the next step. After awhile, Larsen concluded he wasn’t going to make any progress using this incremental experimental path, so he turned to theory. That is when he asked Widom to find an explanation. He thought, “Perhaps, a theory could attract some money”. On one occasion, Larsen told me he had a theory. I said, "Well, let me look at it."

He said, "No, only when it's published."

"Why not?"

"I don't want anybody else to know about it. I'm sorry, I don't trust anybody”

That was his nature. I had the first chance to read the theory after it was published. I was floored by the amateurish way in which he tried to explain cold fusion. I told him, "You have a problem." He replied, "No, no, you don't know what you're talking about." So I said, "Okay, let me talk to Widom."

So, he got Widom on the phone in a three-way conversation. Widom was an arrogant theoretical physicist who considers other people to be ignorant. I told him, "You're using a Green function to explain the energy.”

Widom said, "Yeah, that's right. You understand what we're doing."

At that point, Larsen terminated the conversation because he did not want Widom to know about my objections. That ended our relationship. Many people in print criticized the theory, which was embarrassing because this made the leadership of the field look incompetent.

LARSEN’S EDX

When Larsen and I stopped our relationship, I was allowed to keep the SEM. He figured it's going to cost more to take it and store it. So he told me, “It's yours to use,” but he insisted I sign a statement saying I would not allow anybody else to use it and that I would not use it for any other purpose. Of course, the agreement had no legal binding.

The EDX part of the SEM was fairly primitive because he bought the cheapest version available. It went bad when I was working with Brian Scanlan. Brian bought the upgraded version, which is on the SEM now. So Brian owns half the machine, and Larsen owns the other half. Larsen died and the SEM was given to Claytor because we had to make changes in the room where it was being used.

STEVEN KRIVIT

Krivit started out with good intentions. He and his wife Nadine Winocour, a clinical psychologist, wrote a book called *The Rebirth of Cold Fusion*[16] together. He contacted me initially for an interview, and I encouraged him and sent him some information.

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[16] S. B. Krivit and N. Winocur, *The rebirth of cold fusion* (Pacific Oaks Press, Los Angeles, CA, 2004).

But they split up. He needed to make a living, so he set up a website New Energy Times and tried to get money to run a website keeping track of cold fusion. Being a good writer, he wanted to be the objective judge and explain it to the common folk. He tried this approach for awhile but then became more antagonistic and cynical. He became more willing to create controversy when none was justified because that's what sells information.

Krivit had received some funding from Charles Entenmenn. I suspect his major funder has been Larsen because Krivit went out of his way to support the Widom–Larsen theory without showing any signs of being objective or open to discussing the real issues.

For awhile, I considered him my friend. He'd call and want information. Normally, a journalist will check before publishing information obtained from a personal conversation. He did not. Instead, he quoted me in ways that distorted what I was actually telling him and without my permission. I complained. "If you want honest information, then you have to be willing to give me an opportunity to review it before you make it public." His philosophy was, "No, anything you tell me is public knowledge. I'm a journalist and I do not make judgments." He did not believe in confidentiality. I've never been treated that way by anybody in the press. Almost always they will allow you to read what they wrote to make sure it's accurate and truly represents what you meant to say. Krivit did not do this. So, I told him that I would not interact with him at all and we went our separate ways. Other scientists in this field have experienced a similar situation. Krivit misquoted Mike McKubre on numerous occasions. So, McKubre stopped talking to Krivit as well.

Nevertheless, he was good at making contact with people and creating interest. He encouraged the American Chemical Society to publish a volume of various descriptions of cold fusion. He asked me to contribute, which I did, along with a number of other people.

BIGGEST LENR LIBRARY IN THE WORLD

I started a collection of LENR papers when I worked at LANL. Then ENECO supported a search of the literature about cold fusion. Since then, the collection has continued to grow with over 5600 citations in 2022, all indexed with personally chosen keywords. Most papers are physical or electronic copies. This collection was used to create www.LENR.org that is now administered by Jed Rothwell.

JED ROTHWELL LENR LIBRARY

When everybody else went to the ICCF-9 conference in Beijing, I was sitting at home when a guy called and said he would like to create a website for the LENR subject. He said, "I'm interested in cold fusion and I'm a computer programmer. I know how to generate websites and I'd like to work with you and generate a website for the cold fusion subject." I was hoping somebody would do that because we really needed a website. I didn't know how to do this myself and I couldn't get anybody else interested.

He started to construct a website. I looked at it and said, "This is useless. This is not the way to go." He was very insistent that this was exactly the way to go. I said, "Look, I'm the expert in this field and you're trying to describe what I know something about. You're not doing it in a way that's going to be useful." He replied, "No, I’m not going to do it your way. I'm the one writing your website and this is the way we're going to do it. Period." Of course, he wanted financial support for himself as well. When Jed Rothwell got back from the conference in China, I asked Jed, “Would you be interested in taking over and doing the website that this guy's proposing?"

Jed said, "Yeah, that sounds like a good idea," and he started interacting with this guy. He called the guy and said, "Okay, I'm going to take over." The guy said, "No, you're not". Our problem was that he owned the domain LENR.org and he would not give it up unless we paid him. I didn't have the money and Jed didn't have the money. Talbot Chubb donated the money so that the domain could be transferred to Jed. In case we could not get the preferred name, we called the website www.LENR-CANR.org. The CANR part was the abbreviation of *chemically assisted nuclear reaction*, which was my early name for the process. Unfortunately, this description never caught on.

Jed and I discussed the design that would be most useful. We decided that having access to the papers about LENR would be the first goal[17]. I had the EndNote collection, so I transferred this to Jed who put it on the website. He has kept the collection organized and up to date since. Jed did a very fine job and he continues to do a very fine job[18]. Initially, we would try to reconcile between what he had and what I had so that we would have the same thing on our machines.

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[17] J. Rothwell and E. Storms, *LENR-CANR website, its past and future*, in P. Hagelstein and S. Chubb, Tenth International Conference on Cold Fusion, (World Scientific Publishing Co., Cambridge, MA, 2003). https://lenr-canr.org/acrobat/RothwellJthelenrcan.pdf

[18] J. Rothwell, Librarian, *LENR-CANR.org – A collection of papers about cold fusion.* https://lenr-canr.org/

Eventually, this became too much of a nuisance so we went our separate ways. I have everything that's on his website but he doesn't have everything on mine. My collection is more complete because Jed needs copyright permission to post on a public site. In my case, my collection is not public, so I don't have to worry about this restriction.

COLD FUSION FOR THE PUBLIC

People who are not scientists, and even some of the scientists in the field, had a hard time understanding what the papers really meant. Jed and I realized an overview in layman's language was needed. Jed Rothwell wrote *Cold Fusion and the Future* as a book available for free to anybody. This describes the nature of cold fusion and what he thinks will happen in the future.

My contributions were more focused on the science. Thus was born *A Student Guide to Cold Fusion*[19]. I wrote the Student Guide to make cold fusion more accessible to a scientifically educated person, but one who was not knowledgeable about some of the technology.

I also wrote *Cold Fusion for Dummies*[20] as another descriptive tour. Several reviews of the field that I wrote, designed for various levels of scientific expertise, were also published. Other overviews were drilling down on a particular aspect of the science, like *Calorimetry 101*[21], which was focused purely on calorimetry. These were all efforts to try and provide a little more background for students and scientists. People around the world have downloaded those general overviews and have appreciated them more than the scientific reports.

DIETER BRITZ COLLECTION

Dieter Britz created a library of cold fusion papers. He started in parallel with what I was doing. His collection was limited because he was mostly focused on papers written in Europe and papers that were published in conventional journals. He didn't have any of the conference papers. His library was very convenient because he had copies of the very early papers in digital form. He made his library available to the world by putting the collection onto CD disks and

[19] E. Storms, *A student's guide to cold fusion*, *updated 2012,* Unpublished Report (2012). https://lenr-canr.org/acrobat/StormsEastudentsg.pdf

[20] Storms, E., *Cold fusion for dummies*, Unpublished Report (2006). https://www.lenr-canr.org/acrobat/StormsEcoldfusione.pdf

[21] Storms, E., *Calorimetry 101 for cold fusion methods problems and errors,* Unpublished Report (Undated). https://lenr-canr.org/acrobat/StormsEcalorimetr.pdf

distributing them to anyone who might want them. I got a copy of this collection and incorporated it into my library. He continued his collection for some years but has essentially stopped adding to the library now. To some extent, it's become irrelevant because Jed Rothwell is already updating the LENR collection regularly in a much more accessible and more complete form.

I don't think the whole Britz Collection is incorporated into lenr.org because Jed has to worry about copyright. Many of these papers appeared in journals that do not allow a copy of the paper to be made public. The copies that Jed has on the public website are the copies obtained from the author based on the Fair Use Law.

THE PREPARATA AWARD

The International Society of Condensed Matter Nuclear Society (ISCMNS) chose to give an award as an acknowledgement of a distinguished contribution to the field. The first award was a truffle. This proved problematic because they were very expensive and couldn't be taken out of Italy. Bill Collis, the CEO of ISCMNS, suggested having an actual medal commemorate the scientific achievements. But whose face should go on it? He wanted to give the award to Martin Fleischmann, so he didn’t want to use his face. He needed somebody else.

Giuliano Preparata was an Italian theoretician who made an effort to create a theory. He had significant influence over Fleischmann's thinking about how cold fusion could operate. He wrote some really good papers that describe not only his work but also other people's work. He thought the cold fusion heat-producing reaction was happening within the bulk of the metal, between the atoms. He had a significant influence over the suggested theoretical models in the early years of the field. This made Preparata a logical choice to have his face on the medal. Also, he was no longer alive, which was a requirement.

Preparata was a very argumentative person. From his point of view, his theory was correct and anyone who thought otherwise was an idiot. A few memorable occasions occurred at meetings where Preparata and theoretician Scott Chubb would argue. Prepatara and Chubb were very much alike. They would have shouting matches while trying to explain to each other what their theories meant! Of course, everybody just stood back, smiled, and tried to avoid getting involved. Both theories were later shown to be wrong.

Collis had a large number of medals made with Preparata's likeness on them. They were made from an inexpensive metal in the form of a large coin. ISCMNS first started to award the Preparata Medals at an ICCF conference. They were given as a group, maybe four or five at a time. I was among this group. I raised an objection. I said, “An important contribution is not rewarded as a group. This award should be focused on a single individual. The person getting the award should give a talk describing the reason for the award. The nominating person should describe why that particular individual was selected. The choice should be based on the decision of the entire organization, not just by an individual.” My ideas were implemented. The award became an important part of the ICCF banquets. The nominations would be submitted to the ISCMNS organization. One of those individuals would be chosen based on a voting process.

On one occasion, I was given the job of shepherding the award process through the organization. This was not an easy job. Finally, the choice was made. In this case, the nominee was John Dash. John was introduced by the person who had nominated him. He then described his work. The event became a true award with meaning.

THE TOYODA AWARD

The Minoru Toyoda award is a gold coin on which is the likeness of Minoru Toyoda, of the car corporation fame. He was a big supporter of Fleischmann and Pons when they had a laboratory in France. The plan was to give this award very rarely for contributions truly above and beyond. The first one was given to Martin Fleischmann at the ICCF-15 conference in Italy. They proposed to give one to Stanley Pons as well, but he said he had no interest in attending the conference or being part of such an event. So, he did not receive the award.

Fleischmann attended the conference in a wheelchair. He had some worsening health problems by then. It was really nice that the group had one last opportunity to acknowledge his contribution. I was awarded the medal in 2022 at the ICCF-24 conference in Mountain View, California. I was grateful for being acknowledged. But as far as I could tell, only the people who actually attended the ceremony knew about the award. My relationship to people in the field and my financial support did not change.

CELL DESIGN, SEPTEMBER 2005-2006

After Larsen terminated our relationship, my experimental work was reduced while I wrote the book *The Science of LENR*[22]. Nevertheless, some experimental work continued because the calorimeter was run by a computer and did not require my attention. I studied several different kinds of materials. These efforts were designed to make various materials active by depositing palladium on them.

I used a recombiner catalyst inside the cell. When the recombiner stopped working, the stoichiometric mixture of D_2 and O_2 would build up and cause the cell to explode. When this happened, the top would pop off with no other damage being done. The consequence was trivial, except for the nuisance. Because the cells were made from Pyrex glass tubing and the tops were Teflon with an O-ring seal to the glass, the cells were cheap, simple, and easy to replace without being dangerous.

BRIAN SCANLAN AND KIVA LABS, 2007

I met Brian Scanlan at an ICCF conference. Later, he contacted me to discuss how we might collaborate. He invited me to his home that he had designed and built. He had a laboratory he was using to study LENR. We agreed to study LENR in our respective laboratories and share the success we had. Brian, like me, hates paperwork. He hates the legal details. So we had a handshake deal. Later when some success was in sight, we created a company. His brother-in-law came up with the name, Kiva Labs. I could not have reached my present understanding without his support.

GAS DISCHARGE TECHNIQUE, 2007

Brian was using gas discharge in his own laboratory, so we decided to use gas discharge in my laboratory. Other people were reporting success using the gas discharge method, particularly the Russians. They were producing energy *and* transmutation products. So, we felt that we might take advantage of that success. Gas discharge operates by applying a voltage to the gas at low pressure to form plasma.

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[22] E. Storms, *The Science of Low Energy Nuclear Reaction, (*World Scientific Press, 2007). https://www.worldscientific.com/worldscibooks/10.1142/6425#t=aboutBook

I constructed a very sophisticated apparatus that allowed the ions emitted from the cathode to be detected and the energy measured using a silicon barrier detector. I observed a very complex series of peaks the height of which decreased as the energy increased. We used various techniques to identify the element being emitted. The signal appeared to be generated by energetic deuterons. Now I believe the ions were H^4, which would act like D.

We used different kinds of materials for the cathode. The discharge modified the surface in very strange ways. An insulated shroud insulated the vacuum system from the high voltage on the cathode. The shroud material would deposit on the cathode. Consequently, the ions were not being emitted only from the metal cathode. In addition to the ions, other radiation was emitted. The group Coolescence[23] had been using gas discharge as well. They claimed we were actually seeing photon emission, not electrons. We set up to find out which idea was correct by using a magnet. As it turns out, they were right, we were detecting photons.

We used a silicon barrier detector (SBD) to which various absorbers could be inserted to measure the energy of the emission. A little magnet on the end of a swinging device on the inside allowed different absorbers to be placed in front of the silicon barrier detector. This enabled us to explore the characteristics of the radiation. The silicon barrier detector measures the energy of the radiation, but not the kind. By putting absorbers between the source and the detector, we could determine the kind of ions being emitted.

We were seeing ions with more that 0.5 MeV of energy. Ions having this amount of energy can only result from a nuclear process. In addition, they had a complex energy spectrum. Until recently, I had no idea what the energy spectrum meant. Unfortunately, we couldn't measure heat production.

GAS LOADING TECHNIQUE, 2008

Scanlan and I started using gas loading around mid-2008 with the ability to measure energy production. I built a calorimeter that would measure heat production under pressurized gas over a range of temperatures. We were able to expose materials to deuterium gas, heat them in the gas,

[23] Coolescence was located in Boulder, Colorado. It ceased operations in 2019.

and measure the amount of energy. It was a really neat little calorimeter for that purpose, and we used it to explore many different materials.

Brian bought a sputterer so that we could apply coatings of various kinds to palladium. I also applied different kinds of coatings to CaO and to zeolite. Later, we worked with nickel in light hydrogen. We reduced palladium nitrate and palladium chloride in hydrogen to try to create finely-divided particles on silicon dioxide or on calcium oxide. This would have been very similar to samples created by Les Case. Nothing worked.

I have a friend who worked at Grace Catalysts company. He claimed that a zeolite that they made might be a useful substrate. He sent me a sample, which I looked at with no luck. In the fall, we looked at palladium on graphite. We treated platinum plated on palladium and palladium black mixed with boric acid. The boric acid was an attempt to incorporate boron into the mix because boron was found to have some benefit in the solid palladium. I used sterling silver, which is an alloy of silver and copper, as the anode. We tried sodium hydroxide with palladium black on copper. This palladium black was made by decomposing palladium chloride with sodium borohydride, which makes very finely-divided palladium. We wanted to see if that could be activated, but we had no luck with that.

We tried a palladium-boron sample, and then, with a higher concentration of boron. I still have those samples. Throughout 2009, Brian Scanlan and I continued to try many different combinations of materials. In February, we tried copper plated with palladium and gold. In March, we tried sterling silver and palladium plated with gold. Around April in 2009. we tried a palladium and gold alloy, palladium heated in vacuum with lithium.

We tried the dual laser method proposed by Letts. We applied magnetic fields, changed the frequency, and the polarization. No excess power was produced.

We also used palladium nanoparticles. In September, we studied a number of samples using gas loading of platinum, copper plated on platinum, and palladium-nickel alloy. In November, more plated material and more gas loading experiments were performed. In December we continued the gas loading project, using some fairly exotic material, like neodymium carbonate and lithium aluminum oxide.

We used all kinds of strange substrates on which we attempted to produce active palladium. At that time, I wasn't aware of the possibility that the palladium was not the site of the nuclear activity. Instead, the substrate on which the palladium was deposited was active.

At the time, people thought that the smaller the particle of palladium, the more active the palladium would become. An extreme example is produced when palladium is reacted with zeolite. The atoms of palladium assemble in a very small atomic cage in the structure. This creates a very small particle that was expected to be nuclear active. NRL sent a variety of samples they have made and explored. They found no excess energy and I found none.

Later in 2010, we looked at silicon dioxide plus palladium using Omega-Bond. Omega-Bond is a compound that is used to bond at very high temperature. It's a powder that when mixed with water hardens into a ceramic. It did not produce excess energy. We tested carbon aerogel treated with palladium nitrate. Nothing worked.

LETTS SAMPLE 672, 675, AND 676, OCTOBER 2008

In October of 2008, Dennis Letts had sent me his sample number 672 to examine using the scanning electron microscope. I found the surface was covered with impurities of all kinds, including copper, iron and nickel. The surface was very complex. Why it made excess energy would be really hard to understand and almost impossible to duplicate.

Later, in 2009, I got some more samples from Letts, his numbers 675 and 676, and I looked at those as well. Same result. He *had* to plate gold in order to get his laser effect to work. I didn't need to do that when I studied the laser effect. In his case, he felt that the layer of gold was beneficial and necessary. The gold was in little spots. It wasn't uniform; it wasn't like a shiny gold electroplated coating where the gold is uniform. These surfaces were very dark, essentially black, and the gold was in little balls, or similar shaped regions, stuck to the surface in various places. In other places the palladium was visible. He had made an incredibly complex surface.

LETTS-HAGELSTEIN DUAL LASER EFFECT, APRIL 2009

During testing all types of treatments to palladium in 2009, Dennis Letts made a second claim involving lasers. He said that he could enhance the anomalous heat effect of certain frequencies when two lasers were used to create a beat frequency. Scanlan and I attempted to replicate this method.

I made a small, flow type calorimeter devoted solely to this experiment. We used two lasers of slightly different frequency to shine on the same spot. We did not produce any excess energy. However, the experiment was so crudely done, I don't think this failure means anything. Nevertheless, I learned a good deal about how lasers behaved.

This was a very complicated measurement because according to him the process is sensitive to the magnetic field strength, to the polarization, and also relative polarization. Both lasers have to be polarized and superimposed in the same manner. We weren't able to successfully replicate the claim, and I don't think anybody else has either. As a matter of fact, I’m amazed that Dennis made the behavior work as claimed.

Letts pursued this dual laser effect for a long time and logged a lot of data. The behavior looks real. The big question is what causes it? Hagelstein thinks the behavior supports his theory. Everybody used this as evidence saying, "Oh, here's a theory that has led to a successful prediction so therefore the theory must be right." I argue, yes, they saw something but the explanation is wrong. In addition, previously Letts could produce extra heat using a single laser. This time a single laser would not work. Only the use of two lasers worked. So, the effect was not consistent with a previous behavior. Why not?

ARATA FINELY-DIVIDED PALLADIUM, 2010

In February of 2010, we looked at various other exotic mixtures while trying to cause activation. People believed magnetic materials were important. We added magnetic iron oxide to see if it would do any good. It did not. In April, we continued the effort to activate various substrates by applying palladium, all without success.

Arata sent samples to McKubre. He found that they made not only heat, but tritium as well. Arata’s material was obviously nuclear active. Arata showed his samples were making helium. Although he didn't make quantitative measurements, he did show a correlation between heat production, tritium production, and helium production. McKubre then detected heat and tritium. But the conditions that allowed these reactions to occur were still a mystery. The active material was never identified.

Arata oxidized an alloy of zirconium and palladium. He expected the zirconium would be converted to zirconium oxide and the palladium would precipitate as finely-divided palladium. He believed the finely-divided palladium would be active.

Ordinary finely-divided palladium will sinter and eliminate the finely-divided palladium. The oxidation process would leave finely-divided palladium dispersed within the oxidized structure so that it could not sinter. The Japanese have been using this material to make excess energy for the last couple of years.

We finally realized that the fine palladium was not the active form. Instead, the substrate on which the palladium was deposited was the important material – or the gaps that formed when the fine powder sintered was active.

ANDREA ROSSI, 2011

Scanlan and I had heard about Andrea Rossi's claim before he demonstrated it publicly. Rossi had been studying the water-gas reaction in order to make a fuel. Presumably by accident, he hit upon a condition that caused excess heat to be produced in his cell. He already knew about cold fusion and recognized the importance of that extra energy. Matts Lewan described this discovery in detail in his book *An Impossible Invention*, which focuses on Rossi's work.

In Italy, Rossi had heated trash to make a liquid fuel. This effort was successful until he got crosswise with the Mafia because the Mafia has a monopoly on trash in Italy. He was turning trash into liquid fuel but this trash began piling up after the Mafia shut him down. Then he was accused of environmental pollution because of too much trash, which resulted in a prison sentence. While he was in prison, he read about cold fusion. After getting out of prison, he came to the United States, set up a company and got some money from the DOE to turn coal gas into a liquid fuel using a catalyst. He was in the process of doing this when one of his reactors unexpectedly got hot. He realized he might have caused cold fusion.

He spent two years working with Sergio Focardi, a physics professor at the University of Bologna, before he announced his discovery. Focardi, along with physicist Francesco Piantelli at University of Siena, had claimed to generate heat using solid nickel and light hydrogen. Rossi was using powdered nickel in the form of a catalyst. Rossi was getting a lot more power than they did. Apparently, he learned how to better treat the nickel to produce an efficient production of energy.

When Rossi started working out of Florida, Brian Scanlan went to Miami to see him and offered to support him. He told Rossi, "I can provide you with large amounts of money." “But,” Brian said, "I need to know for sure that this is real. I have to see your apparatus, how you make the material, and I want to replicate it myself." That’s pretty much what Industrial Heat asked. Of course, Rossi said no. Brian came away with the impression that Rossi was not able to interact in a rational way. So he walked away. Industrial Heat did not walk away so they got taken to the cleaners.

NICKEL STUDIES WITH BRIAN SCANLAN, 2012

Scanlan and I tried to replicate the Rossi claim. We set up a small reactor with both calorimetry and radiation measurement in which we exposed nickel to carbon monoxide, hydrogen, and water in various combinations. We also started with different kinds of nickel, since initially we didn't have the nickel that Rossi claimed to use.

We speculated that the oxidation and reduction of nickel would have produced strange morphologies on the surface of the material. We proceeded to make nickel powder in many different ways. We hoped that by sheer luck we might achieve the condition that Rossi got by accident. This was a nice education about the chemistry of nickel, but at no time did we see any evidence for nuclear reaction.

Two kinds of nickel are commercially available. One kind looks like little balls about ten microns in size. The other kind is made by decomposing nickel carbonyl. The material has a cauliflower-like structure with a size somewhere in the neighborhood of ten microns. Rossi claimed to use the latter kind. Nickel can be made in other forms by decomposing nickel formate, nickel nitrate, or nickel chloride. We learned a good deal about the chemistry of nickel. We tried many ways to make nickel powder and to react it with hydrogen.

We eventually got some of the nickel he allegedly used. However, I think this was a false lead because this nickel was not the catalyst he claimed was active. This nickel I'm sure had no relationship to anything Rossi had done.

UNCLEAR RESULTS

That work continued throughout 2012. Scanlan and I both frequently recorded behaviors that didn't make sense. We didn't know whether a weird reaction was happening or if the apparatus had failed. The results were always on the ragged edge. In contrast, if the heat occurred at the level Rossi claimed, the result would be unambiguous. Instead, if we were seeing a heat signature, it was at too low a level to be certain.

Scanlan used a different approach. He was set up to do quick and dirty experiments while exploring a wide variety of conditions. I was set up to explore at depth with more precision. If he found something useful, we could look at it using my calorimeter in more detail. I was also exploring different kinds of conditions simultaneously with his. We were seeing strange behaviors periodically, but never anything that made sense.

We saw no hydrogen reaction with nickel in my apparatus. In his apparatus, occasionally he'd see examples of very high loading. Sometimes the nickel would become so highly loaded, it would be a good storage medium for hydrogen. We never figured out why or how this was possible.

When the EDX part of the SEM stopped working, Brian had a state-of-the-art EDX installed. We discovered we could not make the same morphology each time. Even within the same batch, a variety of morphologies would be present. We could only hope that one of these morphologies would be active.

Given the complexity, how did Rossi figure out how to reproduce the active material? If it's so difficult to reproduce, or make in the first place, it's also going to be very difficult to reproduce. How did he go about doing that? My suspicion is that he really didn't. I suspect he used an entirely different material from the one he claimed to use. He would have used a commercial catalyst that would have been manufactured in a very reproducible way. He only needed to treat the material to cause it to become active, as it did initially by accident.

ROSSI CATALYST

I do not believe the samples of nickel Rossi gave to people is the catalyst that makes excess heat in his reactor, the E-Cat. He spent years figuring out how to treat the catalyst before he actually made the announcement to the world. But his announcement is inconsistent with what he was actually doing.

Alfa Aesar is a supplier of catalysts for the world. The Alfa Aesar catalogue lists a nickel-based catalyst that is designed for this purpose. It is not available. It is sold out.

Brian suggested that Rossi never revealed the real material so that people would not compete with his discovery. We were so frustrated in trying to make nickel work, Brian said, "Maybe we're going down the wrong path. Maybe it wasn't nickel. Instead, perhaps he was using a particular catalyst". What other elements would be combined with the nickel in this catalyst? The substrate is very important because it determines the nature of the molecule that can gain access to the hydrogen. These catalysts are proprietary because the morphology is very important. Various elements can be used to split the hydrogen, but they have to be compatible with the substrate. He obviously found a catalyst that worked. I believe this catalyst is no longer available because he bought the entire supply.

WHAT HYDRIDE?

Rossi initially reacted his catalyst with hydrogen gas. But this was the nuisance. Being clever, he might have said, "Why not mix a hydride with the catalyst? What hydride should we use?" He started with magnesium hydride, which is MgH_2. This provided a supply of hydrogen internally so he didn't need the tank. Then he tried a hydride containing *more* hydrogen, which was lithium aluminum hydride ($LiAlH_4$). Then he discovered that the use of a higher temperature produced even more energy. Unfortunately, this catalyst melts and decomposes near 200°, which causes many changes.

The catalyst and the lithium aluminum hydride have to be kept separated because after the hydride decomposes, the liquid wets everything and runs all over the place. He probably had this separation designed into the system without telling anybody. Other people mixed the catalyst with the nickel, which caused a mess in the cell.

We recognized that the nickel has to be activated. Initially, the nickel is dead. Most people do not understand this fact. They believe the nickel can simply be heated with lithium aluminum hydride to make it active. That's not what Rossi did and that doesn't make any sense in terms of what we know about cold fusion. The nickel has to be activated first.

Rossi essentially created a distraction by writing a patent describing his way of making excess energy, leaving out the fact that the material had to be first activated. He got a patent without revealing this important detail. Industrial Heat tried to implement the patent and failed. He left out this important detail because this is the secret of his technology. He was not going to tell anybody his secret.

INDUSTRIAL HEAT

The president of IH visited Kiva Labs as part of his effort to provide money to study the subject. I think he viewed cold fusion as just another piece of science from which the company might make money. They didn't believe the phenomenon was different from normal discoveries and technologies. They don't realize just how different and unique cold fusion really is.

We were trying to sell them our idea of how a laboratory should be designed. It was clear they weren't interested. They were setting up their own laboratory with their own ideas. They didn't need another one, but they would support someone who had a laboratory from which they could gain benefit. They gave Cravens and Letts money. Just about anybody who had a laboratory or had claims of success could get some money. I didn't have any claims of success at that time. I had success in the past, but not then. They went elsewhere. From my point of view now, that was a gift. I consider being independent of IH is like missing the sailing of the Titanic.

NASA CONTRACT

In October 2012, NASA hired me to do some work. The first effort was to design a calorimeter. It was sort of a back-of-the-envelope design, which they then implemented. I didn't hear anything from them for a long time. Then they sent word, "It's all done. Come and take a look." I went to NASA in Cleveland. They made a few mistakes, but it was thoroughly and completely engineered, including having its own room. Although it was over-designed, it worked. However, it was sensitive to room temperature. The room was not temperature controlled. It was designed to prevent the buildup of hydrogen. They had focused on stopping a hydrogen explosion rather than on having a workable calorimeter.

APPENDIX. DEVELOPMENT OF THE STORMS AUTOBIOGRAPHY FROM THE STORMS LENR RESEARCH DOCUMENTATION PROJECT

The Storms LENR Research Documentation Project (SLRDP) was performed under the umbrella of the LENR Research Documentation Initiative (LRDI). The objective of the initiative is to mitigate the loss of LENR research records as the investigators are leaving the field now 34 years after LENR's announcement in March 1989.

The LRDI methods are shown diagrammatically in Figure A1. One or more site visits are usually made to collect the records and conduct the interviews. A report is prepared, and arrangements are made for preservation of the records.

Figure A1

LRDI Procedure

The principal objectives of the SLRDP have been to secure and archive the public and private collection of hardcopy and electronic LENR files and to make the materials more accessible for Dr. Storms and others who are interested in the LENR field to conduct more enhanced review for additional insights in the future. The project was developed by Dr. Thomas Grimshaw with Dr. Storms (Figure A2). The scope is from March 1989 through December 2015, the date selected for cutoff. It began in August 2015, when Dr. Grimshaw made his first site visit. A number of site visits were subsequently made to Dr. Storms home for the SLRDP.

Figure A2

Ed Storms and Tom Grimshaw. Taken at Dr. Storms' Home in about 2015

As a consequence of his many years of LENR research, Dr. Storms developed a large body of experimental data along with many publications and unpublished reports. The records collected for the SLRDP have been organized into the Components based primarily on the source of information: publications, unpublished progress reports, work history (lab notebook entries), electronic files, hardcopy materials, LENR library holdings, and interviews of Dr. Storms.

An incremental approach was used to collect information because the full scope of the research materials was not known in advance. The first steps were to prepare memos describing each element as it was found. The Project was conducted in three stages – information collection, organization, and documentation. Reports were prepared for each stage based on the memos. The Stage 1 report documented the information obtained. The Stage 2 objective was to organize the Stage 1 information. The organization was accomplished by developing timelines for each Component. The Stage 3 (Final) report includes appendices with timelines for each Component as well as copies of the publications and progress reports (as separate report annexes). Annexes with Dr. Storms' interviews and copies of the memos prepared for the SLRDP have also been developed.

A summary report has also been developed based on the Stage 1, 2 and 3 reports. The table of contents (Figure A3) shows how the SLRDP is organized first by the phases of research, the project development in stages, and the components of the records.

Figure A3

Table of Contents of the Summary Report for the Storms LENR Research Documentation Project

A key component of the SLRDP is a set of interviews conducted with Dr. Storms in April 2016, March 2017 and June 2017 by Dr. Grimshaw. All three interviews covered the full length of Dr. Storms' LENR career. Ruby Carat (Figure A4) prepared the autobiography for Dr. Storms from transcriptions of the interviews.

Ruby began by closely reviewing the transcripts and then preparing a spreadsheet showing the major activities and events along with their dates and locations. The activities were also arranged in date order. She then prepared the chapters of the narrative describing Dr. Storms LENR research and other contributions. It has been extensively reviewed by Dr. Storms and Dr. Grimshaw.

Figure A4

Ruby Carat and Tom Grimshaw at ICCF-24, July 2022