

DR. MEL MILES' AUTOBIOGRAPHY FOR HIS COLD FUSION RESEARCH

A PROJECT OF THE LENR RESEARCH DOCUMENTATION INITIATIVE

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1 Introduction

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

Dr. Mel Miles is one of the foremost researchers in the LENR field. He began his LENR research while at the Naval Air Weapons Center (NAWC) at China Lake. His goals were to determine if helium was produced in LENR along with excess heat and to show that the ratio of production of helium to energy release is the same as in plasma fusion reactions¹. Dr. Miles was successful in answering both of these questions affirmatively.

Dr. Miles (Figure 1-1) first heard of the LENR announcement on the radio. Because his NAWC research used methods similar to the electrolytic cell used by Fleischmann and Pons, he immediately recognized LENR's potential. He began experiments in his laboratory on his own time within two days of the March 23 announcement. He was able to do so because both palladium and heavy water were available at the NAWC lab. Subsequently, he conducted many LENR experiments at NAWC. As noted, he (with Ben Bush) was the first in the field to establish a correlation of helium generation with production of excess heat.

Dr. Miles then took a leave of absence from NAWC to conduct LENR experiments as a Guest Researcher at the New Hydrogen Energy (NHE) laboratory in Hokkaido. His research at NHE is described in a final report² and in an article in Infinite Energy magazine³.

¹ Rothwell, J., 1997. Introduction to the Cold Fusion Experiments of Dr. Melvin Miles. Infinite Energy, Vol. 3, Issue 15/16, p. 27. Revised and Updated 2004. <http://lenr-canr.org/acrobat/RothwellJintroduci.pdf>

² Miles, M., 1998. Electrochemical Calorimetric Studies of Palladium and Palladium Alloys in Heavy Water. NEDO Final Report. <https://lenr-canr.org/acrobat/MilesMnedofinalr.pdf>

³ Miles, M., 2000. Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan. Infinite Energy, Vol. 5, Issue 30, p. 22-25. <http://lenr-canr.org/acrobat/MilesMreportonca.pdf>



Figure 1-1.

*Mel Miles in the Back Yard of His Home in St. George, Utah.
Photo Taken May 2019*

After his work at NHE ended, Dr. Miles returned to NAWC, where he remained until he retired in January 2002. In the years since his retirement, Dr. Miles has continued to be a leader in the LENR field, attending the ICCF conferences and publishing papers in LENR journals – and in mainstream publications when the manuscripts were accepted. He operated a LENR laboratory at his home in Ridgecrest, California before he moved to his current home in St. George, Utah, the city where he was born and raised.

The purpose of this report is to present an autobiography of Dr. Miles' LENR research.



2 Development of Dr. Mel Miles' LENR Research Autobiography

The autobiography of Dr. Miles was prepared with the assistance of Ruby Carat (Figure 2-1). It was prepared from transcripts of three interviews of Dr. Miles that took place in an earlier project, which is described in Section 3 of this report. Each of the three interviews covered the full length of Dr. Miles' research career, with emphasis on his work in the LENR field. They were conducted on May 9, 2019⁴, July 22, 2019⁵ and August 10, 2021⁶.

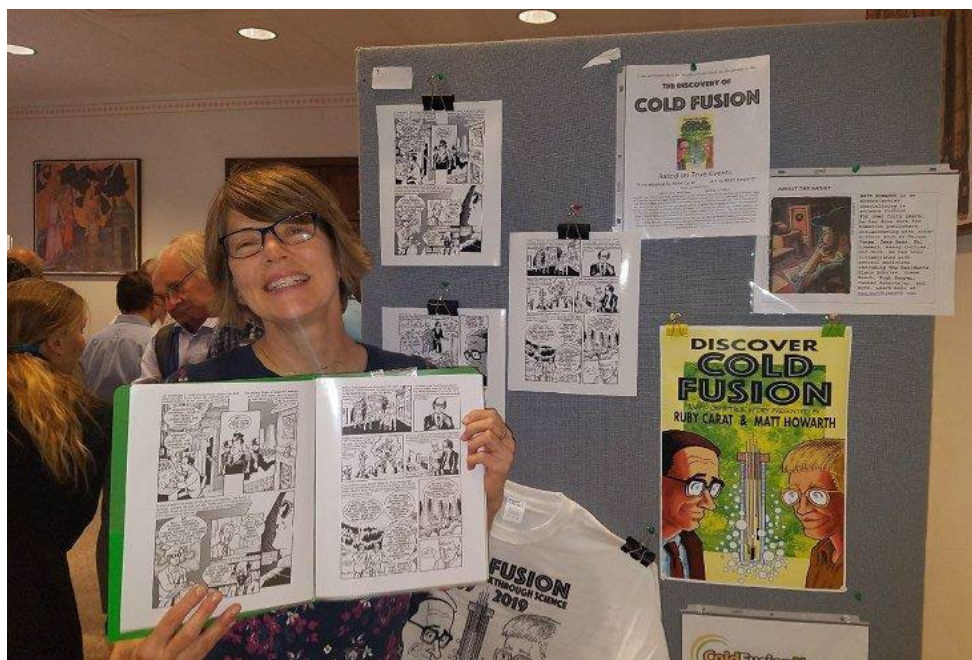


Figure 2-1.

*Ruby Carat at a Display Board for Her and Matt Howarth's Comic Book, "Discover Cold Fusion"*⁷

⁴ Grimshaw, T., Transcriptions of Interviews. Memo to Dr. Mel Miles from Dr. Tom Grimshaw, May 9, 2019.

⁵ Grimshaw, T., Transcriptions of Second Round of Interviews. Memo to Dr. Mel Miles from Dr. Tom Grimshaw, July 22, 2019.

⁶ Grimshaw, T., Transcriptions of Third Round of Interviews. Memo to Dr. Mel Miles from Dr. Tom Grimshaw, August 10, 2021.

⁷ Carat, R. and M. Howarth, 2020, DISCOVER COLD FUSION Comic Book. <https://coldfusionnow.org/author/rubycarat/>



Ruby began by closely reviewing the transcripts and then preparing a spreadsheet showing the major activities and events along with their dates and locations. It is shown in Attachment A. The activities were also arranged in date order. She then prepared the chapters of the narrative describing Dr. Miles' LENR research and other contributions.

The narrative is shown in Section 4. It consists of 79 chapters and includes a summary list of cold fusion dates and places prepared by Dr. Miles at its conclusion⁸.

The narrative has been extensively reviewed by Dr. Miles as well as by Dr. Grimshaw. The information on Dr. Miles' LENR work is supplemented by other major events and accomplishments of his life. The narrative prepared by Ruby seeks to "take the next step" in recording Dr. Miles' LENR contributions.

⁸ Most of the chapters include the interview (#10 for May 9, 2019; #20 for July 22, 2019; #30 for August 10, 2021) as well as the page number of the transcript where the information came from.



3 Background: The Miles LENR Research Documentation Project

As noted, the three interviews from which Dr. Miles' autobiography by Ruby Carat came is from a preceding project – conducted with Dr. Grimshaw – to document the records of Dr. Miles' LENR research career⁹. A photo of Dr. Miles with Dr. Grimshaw is shown in Figure 3-1. The contents of the Miles LENR Research Documentation Project (MLRDP) report are shown in Figure 3-2.



Figure 3-1.

Mel Miles and Tom Grimshaw. Taken in Dr. Miles Home, August 2021.

The MLRDP is being performed under the umbrella of the LENR Research Documentation Initiative (LRDI), which now has about 30 participants. The objective of the initiative is to mitigate the loss of LENR research records as the investigators are leaving the field now 30 years after LENR's announcement in 1989.

⁹ Miles, M., and T. Grimshaw, 2020. Documentation of LENR Investigations by Melvin Miles at the U.S. Naval Air Warfare Center, China Lake – and Beyond. LENRGY Unpublished Report, Vol. 1 and Vol 2 (Appendices), Third Draft, August 11.



1	Introduction
2	Publicly-Available Documents
2.1	U.S. Navy and Other LENR Reports
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3.1	Files on Laptop Computers
3.2	Files on CDs
4	Hardcopy Records: Notebooks
4.1	Lab Notebooks, Naval Air Weapons Center, China Lake
4.2	Lab Notebooks, New Hydrogen Energy, Japan
4.3	Activities and Events Notebooks
5	LENR Library
5.1	ICCF Conference Proceedings
5.2	LENR Books
5.3	Video Recordings (VHS Tapes)
5.4	Photographs
5.5	Conference Posters
6	Interviews
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8.4	Continued Professional Activities in LENR
8.5	LENR Research Summary
9	Future Opportunities
10	Project Methods
	Appendices (Volume 2)
	Appendix A. Cold Fusion Papers by Dr. Miles
	Appendix B. Identifications of Individuals, Etc. in Dr. Miles Photos
	Appendix C. Transcriptions of Dr. Miles Interviews
	Interview Set 1, May 2019
	Interview Set 2, July 2019

Figure 3-2.

Table of Contents of the Report for the Documentation of LENR Investigations by Melvin Miles at the U.S. Naval Air Warfare Center, China Lake – and Beyond.



The LRDI methods are shown diagrammatically in Figure 3-3. In most cases, a site visit is necessary to collect the records and conduct interviews. A report is prepared, and arrangements are made for preservation of the records. A number of visits were made to Dr. Miles’ home for the MLRDP as shown below.

5/5/19 to 5/6/2019
5/27/19 to 5/29/2019
7/18/19 to 7/19/2019
10/31/2019
6/25/2021
9/27/2021



Figure 3-3
LRDI Procedure

Thanks go to Dr. Miles’ spouse, Linda, for her essential role in supporting the MLRDP. Appreciation is also expressed to the Anthropocene Institute for financial assistance with project expenses.



4 Autobiography of Dr. Miles' LENR Research

1 MY GREAT-GRANDFATHER JOHN HENRY WILLIAM MILES 1861

On my father's side of the family, my great-grandmother Jane Mary Wyatt, was not married, but she was a mistress to a rich person in London with the last name Miles. John Henry William Miles owned the Red Lion Pub on Parliament Street in London from 1855-1875, just a block or so away from Parliament, and so my grandfather's biological father was actually quite a rich man in London. That's part of my background, that my great-grandfather was the owner of the Red Lion Pub in Parliament Street, in London.

My great-grandfather Miles was born in 1825, and when he died in 1878, one of his sons found out about this other, second family of Mary Wyatt and, I believe, it was four kids. This son had joined the Mormon church and planned to migrate to Utah. When he found out about the Wyatts, he paid their way to come to Utah, too. They took a ship across to New York, and then the railroad to Salt Lake City, eventually winding up in St. George, Utah.

John Henry William Miles' mistress Jane Mary Wyatt, and the four children, took the last name of Miles once in Utah. My grandfather George E. Miles and Josephine Jarvis met and had my father Maurice Jarvis Miles, M.J. who later met my mother Mary Lyon.

My Dad Maurice Jarvis Miles taught chemistry at Dixie College for many years and then went on to work at Titanium Metals in Henderson, Nevada. We didn't all move there because my father and mother basically split-up, and didn't get divorced. He lived down there in Henderson and he became a Chief Chemist of Titanium Metals later on in his career. This was after teaching at Dixie College, as well as running the St. George Airport, for many years. A few pilots that later flew bombing missions over Germany got their initial training from my father in St. George, Utah.

2 GRANDFATHER GEORGE E. MILES

My grandfather George E. Miles, the son of Jane Mary Wyatt, was born in London, England and he grew up rather poor. He would tell stories about how he'd have to go collect coal along the railroad track in order to keep warm. He came to St. George later on and married my grandmother Josephine Jarvis.

When my grandfather was a young man, the Silver Reef Mine was 20 miles up I-15 in a town called Leeds. They found silver there and a lot of money came out of that mine. The mining camp actually became a small town. Now my grandfather was not a miner, but he worked hauling supplies back and forth between St. George and the mining town. I guess he was frugal, and saved his money, so at some point he was able to buy a whole city block with a big pioneer home on it.

I recently read that my Grandfather Miles had saved up \$2,000 at the age of 29 when he married Josephine Jarvis, which he used to purchase the large Thomas Judd home and the entire city block



in St. George around 1899. I don't know what that would be equivalent to today, but it would be at least 40 times as much. I find that prices today are about 10-20 times what they were in the 1950's, and going up too fast.

It was kind of like the Southern style mansion, set quite a ways back from the street, maybe 200 yards in from the street. It had upstairs, and downstairs, and even a dirt floor basement. It was quite a nice home built by Thomas Judd.

I grew up with plenty of land. Grandfather Miles sold part of the block to my dad and my dad's brother. My Dad bought three acres of that block, and my uncle bought a couple acres on the south. We were on the north and they were on the south, and then sold off pieces to other people over the years, like real estate. Growing up, we had about two to three acres with grapevines, gardens, cows, and chickens. It was within a city, but it was kind of rural for us. As kids and cousins, we spent many hours playing near this home and in the shade of large Mulberry trees.

When that home was later sold, they were going to tear it down. However, some people came up with the idea to save the home and move it to Ancestor Square. They weren't able to move the whole entire home, but they moved at least half of it; the main entryway is still there in St. George in the main part of town, a place called Ancestor Square.

3 MY GREAT-GRANDFATHER GEORGE JARVIS 1861

On my mother's side of the family, great-grandfather George Jarvis had been all over the world with the British Navy, even in China, and then he lost an eye somehow, and he left the Navy. My great-grandfather and great-grandmother George and Ann Jarvis came from London, England as part of the original settlers of the first pioneer settlement of St. George, so my roots here trace back to 1861.

George and Ann joined the Mormon church and migrated to Boston, living there for a couple years. At some point, the church authorities told them to try to get to Salt Lake City, Utah. George and Ann Jarvis traveled from the Mississippi River across to Salt Lake City. My great-grandmother, always like to tell people that she'd walked every step; she never rode once the whole way.

My Dad's sister, Aunt Mary Miles Kleinmann wrote a book called "The Essence of Faith" about my Great-Grandparents from London, who traveled with a Handcart Company the 1030 miles from Florence, Nebraska to Utah with 6 kids. I have this book, and most of the Jarvis' have a copy of that book. It tells a lot about the Jarvis part of the family, how they joined the church, how they wound up in Boston and later moved on to Salt Lake City.

They left Florence, Nebraska on June 19, 1860 and arrived in Salt Lake City on August 30, 1860. This was an Independent Train led by Jesse Murday traveling after the 9th Handcart Company, but before the 10th and final Handcart Company. They did not even have a Handcart of their own but spread their belongings among others. Ann walked every step as did George except for a few hours when he became exhausted from carrying some of his seven small kids one day and rode several hours on a wagon.



It was the two Mormon handcart companies of 1856 who left too late in August (Willie, Martin) that got caught in winter storms crossing the Rocky Mountains and many did not survive. These two sad episodes can be found on the Internet.

George and Ann Prior Jarvis finally got somewhat settled in Salt Lake City when, in a meeting, Brigham Young, asked for volunteers to settle the St. George-area, and my grandfather stood up, and my grandmother was trying to pull him back down. He wanted to volunteer, but she didn't. However, since he did volunteer, she went along with it, and they eventually wound up in St. George when it was first settled late that year of 1861, along with some other settlements, like Washington, 10 miles to the east.

The St. George group was tasked to grow cotton and they were successful at it. There were a lot of Southern people in that company, because they already knew how to grow cotton. The Jarvis' themselves had never grown cotton before, but they wanted to help be part of the settlement. Of course, the cotton mission ended when the railroad came. It was less expensive to just bring in cotton grown in the Southern states. They were able to grow some cotton here, and did for several years, because they were isolated until the railroad came.

It was late in the year, and the story they tell is that there was a **period** of about 40-days where it rained every day. That's unusual here, but they were just living in wagons at that time, and they had to go through that winter with rain practically every day for about 40 days. And then when spring came, they were able to start growing food, and a lot of crops grew quite good here.

They found out there was irrigation water from the Virgin River, but it took a lot of years before they could really manage that river. They would build diversions for agriculture, but when you get rain up in Zion and up in the mountains, the river becomes a torrent, a flood really, and it would just wipe out everything they'd built. It was hard to control the Virgin River, and that was part of the story of the settlement here, the fight against the Virgin River to control it and to do agriculture with water from the Virgin River.

But my great-grandparents were part of the first encampment in St. George there. There's a plaque on the Dixie's State University Campus that lists the names of all the people in that first camp, and George and Ann Prior Jarvis are two of the names. They also had some kids with them, but they just list the parents' names.

One of the early leaders was Erastus Snow, and now, there's a lot of Snows in this area. When I grew up, Snow was a common name, and there's also the Snow Canyon State Park of Utah. I can't remember his last name, but St. George the town is named after a leader named George here that they thought was like a saint, but that wasn't my great-grandfather George Jarvis.

I sometimes subscribed to the Deseret News, and on Saturdays it has a section on church news. It said that from the years 1847 to about 1867, about 97% of the people migrating to the area were from Europe. The ancestry of most people here traces back to England, Germany, Denmark, Sweden and so on, with **75%** from England.

There's a small town west of St. George called Santa Clara that was settled by Swiss settlers from Switzerland. Common names are Frei, which is German for free, Hafen, which means harbor. And there's a lot of German-type names from the Santa Clara-area. It was a very, very clean farming



community, and well known for their produce. If you'd ever go there in the summers, they sold farming products from a Frei Market along the old highway.

My great-grandparents George and Ann Prior Jarvis then had my Grandmother Josephine Jarvis, who married George E. Miles.

4 MY DAD

My Dad, M.J. Miles taught chemistry at Dixie College for many years and then went on to work at Titanium Metals in Henderson, Nevada. We didn't all move there because my father and mother basically split-up, and didn't get divorced. He lived down there in Henderson and he became a Chief Chemist of Titanium Metals later on in his career. This was after teaching at Dixie College, as well as running the St. George Airport, for many years. Pilots that later flew bombing missions over Germany got their initial training from my father in St. George, Utah.

5 NOT A MISSIONARY 1957 - 1960

I attended Dixie High School and Dixie College here in Utah. I played both baseball and basketball on the high school and college teams.

I spent two and a half years in Germany on a LDS mission, which was common for most people here, and one thing I learned is, I'm not a missionary type. I found that the people that were assigned to us, they had trouble, too. I knew a mathematician who finally just wanted to be sent back home. He was out of his element and I was out of my element, too. I just tried to survive that time. But we had some success and some people joined the Church.

There was a girl, and I wrote to her when I came back to BYU for two years. If I had money I would have gone back and probably **tried** to marry her. She was one of the converts, and I know we liked each other. Her dad was a chemist and she was quite smart going to the university. I used to think maybe I can't find some way to do that, because my first marriage failed anyway. But she died of cancer in the 1980s.

I had a dream, and this maybe goes back to religion. This girl appeared to me in a dream and wanted me to go help her husband. I didn't know that she'd died and I found he'd left the church. He'd been a leader in the church and because she died, he turned against the church. I didn't know what I could do, but I did go to visit him twice during the time we went to Italy, once with my daughter when we traveled, and once with my wife Linda. He's remarried. I like him. He has another family, so life turned out pretty good for him. But it turned him against the church that his wife somehow **died**. Religion doesn't guarantee you against things like that. Sometimes it can be misconstrued. I am still in contact with this family. I write to them at Christmas.

After I got back from the mission in Germany, I went to BYU for two years and got a bachelor's degree in Chemistry and a minor in Mathematics. Then it was on to the University of Utah, where I got a PhD in Physical Chemistry and a minor in Physics.



6 UNIVERSITY OF UTAH 1962 - 1965

I worked for two people at the University of Utah. One is very famous, and a lot of people thought he should have received a Nobel prize for his Reaction Rate Theory. That person was Henry Eyring. The other was his son Edward M. Eyring, who was hired just a few years before I came to the University. He'd done a postdoc in Germany with Dr. Eigen on studying fast reactions in solution.

Some reactions are very fast in solution, like the reaction of hydrogen ions with hydroxide ions. But if you could measure the rate of those types, then you could study other acids. I studied organic acids and the rate of the reaction of their hydrogen ion with hydroxide ion. We even did some studies in D₂O. Kinetics are different in D₂O, especially if the reaction involves the hydrogen species, because of the much heavier deuterium than hydrogen atom. I got into details on that a little bit.

I also worked with the temperature jump method of studying faster reactions, which was invented by Dr. Eigen in Germany. He later won the Nobel prize in chemistry for that work. My PhD dissertation Temperature Jump Kinetic Studies of Substituted Malonic Acids.

7 IN GERMANY WITH HEINZ GERISCHER 1965 - 1967

Henry Eyring was well known and had been invited to places all over the world. He advised me on where to go for research, and what group to go to. He had already given a talk at the Technical University in Munich, where he met Dr. Gerischer, who was the head of that group. He thought I would like working for that group, and so, at Dr. Henry Eyring's recommendation, I applied for the NATO postdoctoral fellowship program at Dr. Gerischer's institution. I spent a year in Munich, Germany, learning electrochemistry with one of the great electrochemists of the time, Dr. Heinz Gerischer, and that's where I got started on electrochemistry.

I also knew German quite well, and that was another reason I wanted to go to Germany. In fact, when I first met Dr. Gerischer, he was struggling to speak English with me. I just spoke with him in German saying "Sie können ruhig Deutsch sprechen". Ich verstehe Deutsch", meaning, "You can freely speak German. I understand German." Well his jaw dropped open and he never spoke another word of English to me the rest of the time.

Even all the coworkers and I communicated only in German and very seldom in English, unless they wanted to learn a little English. I remember one person I worked with, he always liked to go home for lunch, and he used to say, "Es schmeckt besser zu Hause", "it tastes better at home". While I was there, I wound up helping to translate Dr. Gerischer's papers written in German into English for submission to the English journals.

The electrochemistry project he gave me was to study the electrochemical reaction kinetics in mixtures of water and methanol, and how the methanol affects the rate of the reaction. My earliest studies in electrochemistry began with a reduction of zinc ion to zinc metal. Apparently, the reaction goes through an intermediate from a zinc plus two ion to zinc plus one ion, and then to zinc metal, and we were measuring what they called the exchange current density, which is the rate of the reaction at equilibrium of going back and forth. In electrochemistry, the exchange current density is



designated as a small 'i' with the subzero on it. We were studying how that variable changes with the mole fraction of alcohol in the solution.

This was later published in the Journal of the Electrochemical Society, I think. With a little bit of alcohol, the rate goes down sharply, then it kind of levels out, and rises gradually, and then when you get into near pure alcohol, then it goes up again. This has to do with the fact that ions have to exchange back-and-forth, and normally the zinc-ion is solvated with water molecules in order to react, and the methanol solution has to make exchanges with water-solvent molecules instead of with methanol-water molecules, and that tends to hinder the rate, and slow it, as you add alcohol to the solution. This was a kinetic study of electrochemical exchange current density.

I had some electrochemistry previously, theoretically at least, when I took Henry Eyring's class on absolute reaction rates. Some of that was related to electrochemistry through how the over-potential affects the reaction rate. And so at least I'd been exposed to it by, through his theory at least, but this was my first experimental electrochemistry.

After my post-doc ended November 1967 in Germany, I was hired by a battery group in Corona, California.

8 THE ORDNANCE LAB JANUARY 1968 - 1969

There was a professor who visited our group in Germany, Professor Tobias, and he told me about this group in Corona, California, and said I ought to apply for a position and go there. He said he would talk to people there and advise that they hire me. Corona, California is in southern California, about 20 miles from Riverside, and maybe 30, 40 miles to LA proper. I was hired by the Naval Ordnance Laboratory directly while I was still in Germany. I didn't even have to interview. I started work at the Ordnance Lab in January, 1968.

I was there at the Corona laboratory almost two-years and our work there was on battery systems involving liquid ammonia. You can do electrochemistry on liquid ammonia and that's where I got started with that.

Also, lithium forms a very interesting solution. Lithium metal dissolved in liquid ammonia will form a blue solution first, and then, as it becomes more and more concentrated, it becomes coppery. It becomes metal-conducting actually, because the lithium ion, and what it forms, is a very conducting metal-like solution in liquid ammonia. Our focus was on a lithium anode in a liquid ammonia battery and then some cathode. I did publish some of this later after I went to Middle Tennessee State University in the Journal Electrochemical Society.

Within a year after I started, the Ordnance Lab at Corona became part of China Lake. They combined the two labs and were going to close the Corona laboratory. Some people were invited to China Lake, but they weren't going to move everybody, just ones they wanted. Other people were not invited to go to China Lake. They did not invite the research people there.

I got a job in Middle Tennessee State University teaching and spent the next nine years teaching there. But the Head of Chemistry at China Lake, he'd come down, and meet me and he told me he would've hired me, but he didn't want to hire the whole group, but he hired me to come work every



summer. If I wanted to, I would go and work at China Lake in the summer and then go back to Tennessee for the school year, which I did, except for two summers when I went to Brookhaven National Laboratory on Long Island in New York.

9 MIDDLE TENNESSEE STATE 1969 - 1978

When I was offered the job at Middle Tennessee State University, they had hired three new faculty members in chemistry that year. We were all hired because they wanted to get more research going, and I was hired because of my research experience. Still, I had a heavy teaching load, so even though it was hectic with teaching, I bought equipment in electrochemistry, including liquid ammonia, and then I continued some of the liquid ammonia studies, electrochemistry and some of that got published. I taught physical chemistry, so calorimetry became somewhat familiar, besides electrochemistry.

I went to work at Brookhaven National Laboratory during the summers of 1973 and 1974 to do water electrolysis to produce hydrogen, then store the hydrogen in a metal-hydride, and then run the hydrogen when you needed it back into a fuel-cell and get the energy back out.

They started inviting me back for the summers. I would go work there in the summer. And, it added to my income. The only 2 summers I didn't go to China Lake were the two summers I got invited to work at Brookhaven National Laboratory, and, not on cold fusion, but on water electrolysis which is somewhat related. They wanted to, this is back in the 1970's, they wanted to do for energy sources, they want to get hydrogen from water electrolysis, and store the hydrogen in a metal compound like titanium, iron-titanium. And, then release it out when they need energy into a fuel cell. They were kind of ahead of their times. But they did have a lot of that worked out. I worked on that at Brookhaven for two summers. That was before cold fusion existed.

I had two summers at Brookhaven National Laboratory, which was a nice experience. I thought it would be like New York City but it's out on Long Island, rather rural at that time. Maybe not today but, in the 70's you could pick strawberries and places like that on land.

And, you drive all the way to the end and that's Montauk Point in Long Island. If you wanted to go to New York, you could. And, we did that a few times to see a baseball game or something or just walk around and the Statue of Liberty and did all that stuff.

They were thinking of the hydrogen energy economy back in 1973, before most other people got interested in it, and that was the work I did at Brookhaven. When I went back to MTSU, I started to have a project in water electrolysis, and how the electrochemical kinetics were on different electrode materials. We investigated almost every metal in the transition metal area of the periodic chart. And we would report on a periodic fashion how the overvoltage would vary with the metal, and the best catalysts are always in the family: nickel, palladium, and platinum. Those are the best catalysts for water electrolysis, and of course with oxygen evolution, a lot of metals didn't hold up, they would corrode under anodic conditions. Only a few metals are stable enough, those being metals such as nickel, palladium, and platinum. That's where I got started in palladium metal, was in that study.

The catalyst they used to store hydrogen was iron-titanium alloy. It's actually an intermetallic compound, one iron per one titanium. And it would absorb a lot of hydrogen at room temperature,



and it would release it again back out, near room temperature. I never got around to it, but that would have been another metal to study for cold fusion, iron-titanium. Those were the summers 1973 and again in 1974.

I had been working summers on a new thermal battery program and the lab got enough money for it. Eventually, they wanted to hire me to work full-time on developing a new electrolyte for a thermal battery. A thermal battery is a key component in practically every missile and China Lake was a Naval laboratory for air-to-air missiles, and they had a lot of interesting ones. The Sidewinder missile was developed there. They can sit for 10 - 20 years, and then suddenly be activated because it's a frozen solid that's suddenly melted.

10 BACK WEST 1978

That summer I had three choices of where to go. They wanted to hire me at China Lake for the thermal battery program, Brookhaven National Laboratory wanted to hire me permanently, and also, the Naval Research Laboratory was interested. I picked China Lake because I wanted to live out west again. Most of my family lived in the West, in Utah, and other places, and I liked the western country and scenery.

I was hired by China Lake in 1978 and began work as a Research Electrochemist on their thermal battery program. I continued with thermal battery work for about four or five years, and then later worked on other programs as well, like electrochromic materials that changed the color of glass, on the surface of glass, they'll change color based on electrochemistry.

11 BATTERY RESEARCH 1979

The Navy changes organization names quite often. When I was first hired [here](#), in China Lake, it was called the Naval Weapons Center, NWC, pretty simple. However, a lot of Navy labs and facilities started combining, like China Lake joined with the Corona Laboratory, and then Corona Laboratory got sort of closed down, though it never really, completely, closed down. Then, the name changed to Naval Air Warfare Center, with Weapons Division the group I was working with, so it was NAWCWD. But starting off it was simpler, NWC.

My research at China Lake really started with thermal batteries. Every missile has a thermal battery, and because it's a solid electrolyte, it can have a shelf-life of about 20 years. You can suddenly heat it up and melt the electrolyte, like perhaps lithium chloride and potassium chloride, and it'll start operating as soon as it becomes ionic conducting. We wanted an electrolyte that would melt at a lower temperature.

Now the lithium chloride + potassium chloride eutectic melts at 352 degrees Celsius, and molten nitrate mixtures melt at 124 degrees Celsius. Something like the nitrate mixtures would have a lot of advantages in that it would be lower temperature, and still melt at about 124 degrees Celsius, which is above the boiling point of water.

We had a battery program on lithium anode and a bromine trifluoride that serves as both the electrolyte and the cathode material. It gives you about five or six volts per cell, which is better than what anything else can get. It was very dangerous material to work with.



We did show that we could get that cell voltage level. The only thing is that you could not get a high current density with it. You could get lower current density, but somehow you probably form a lithium-fluoride coating that doesn't pass current very well and that would tend to limit the current.

This even went on to another program at another laboratory back in Massachusetts. They looked at it, and they got similar results that we did. It actually became another program funded by the Navy, because it does actually work. It gives you high voltage, although it doesn't give you enough current.

This was all part of President Ronald Reagan's Star Wars program, to fund new ideas. Bob Nowak was in charge of that program, along with some other people at ONR. That program continued mainly with the help of some Post Docs.

12 ELECTROCHROMICS AND CO₂

When I had Navy funding for several years, I had funding for multiple research projects and I passed a lot of that off to the two Postdocs working for me, Kwang Park from Korea, who had a PhD from UCLA, and also, David Stilwell from the University of New Mexico.

After about five or six years of battery research, I went on to other things, like electrochromics, things that change color by electrochemistry.

It could be used in darkening or lightening windows for example. By electrochemistry, you could darken the light in glass like windows or car windows, say. I think it's now available on some cars, especially like your rear-view mirror. You can press a button and it'll darken the mirror, and so you don't have the reflection of headlights. I don't think it's common, but it's used in some cars and there was interest in electrochromic materials in the 1980s there at China Lake.

Another project I worked on was the electrochemical carbon dioxide reduction, which is still of great interest today. That's where I got into using palladium as a reference electrode, because you load it with hydrogen, and it would remain at the same potential for a day or so. You can do your studies with it over a couple days. Palladium loaded with hydrogen was a good reference electrode for that kind of work. Working in things other than water, you needed some other type of reference electrode.

I also had funding for a project on the electrochemical reduction of carbon dioxide. You could theoretically reduce carbon dioxide to methanol, or maybe even methane, though it's hard to get past formic acid. To do that, I did studies in non-aqueous solvents, because I'd worked on those at Middle Tennessee State University. I had to devise a reference electrode for those non-aqueous solvents, and that was a palladium reference electrode loaded with hydrogen. It would last for about a day or two before the hydrogen had all escaped, but, for one day, it set up a pretty stable potential.

13 HEARD IT ON THE RADIO MARCH 1989

On the day of the announcement by Fleischmann and Pons, I had missed the news on TV. I first heard about cold fusion listening to the news on the radio driving to work on Friday, the day following the press conference. An announcement came on that there'd been a huge discovery. That really attracted my attention.



My first thought was that it **was** a really good idea. Why didn't I think of it? I was already working with palladium and hydrogen, and I knew palladium would absorb a lot of hydrogen. I had to find out more about it.

When I got to work, I saw that my supervisor **Geoff** Lindsey had left me a voicemail. He was on travel, but he heard about the news before I did and thought it was something that we should take a look at, and maybe write a proposal. It's always about getting money in the Navy, at least for supervisors.

I had most of the things that I needed there in the lab at China Lake; the palladium, the electrochemical instrumentation, and they had bottles of heavy water just across the hallway in the NMR lab for their nuclear magnetic resonance studies. We had lithium metal, you just add that to the heavy water, and it would react and give off oxygen and form LiOD. It was not hard to get the things I needed to start this study.

I had already used palladium as a reference electrode because palladium absorbs hydrogen, and if you're working on a non-aqueous solvent, like I was doing at that time, you cannot use the commercial reference electrodes as they usually contain water, so you had to make your own. Palladium wire loaded with hydrogen is a good reference electrode. As long as palladium has hydrogen in it, it stays at the palladium hydrogen electrical potential.

14 FIRST TRY

I had everything I needed, so why not go on and try it? That Saturday was my free time; I could do whatever I wanted. I went into the lab and set-up a cell that afternoon, but it was very hard to know what you're doing with such limited information. Since I already knew all about hydrogen and palladium when I first started, I didn't really know much about what I was doing with my first experiments on calorimetry. I just used the heavy water and the palladium that I had around the lab, which I found later doesn't absorb deuterium very well, and hard to get very high loading, so I probably never would have found excess heat with it anyway.

Preliminary research was just trying to measure the energy going in and the energy coming out. I think it was okay with the measure of the heat going in but measuring the heat coming out was difficult, and that's where the problem was. I found ways to measure the heat in and the heat out, but they didn't match up. I wasn't very accurate. The heat out should equal the heat in. There was less energy coming out, than was going in.

For the heat going in, I only measured about 80% of the heat that was coming out. I knew then that the real problem was being able to do calorimetry accurately. Heat in was easier to calculate. It was more difficult to get an accurate measurement of heat coming out. I knew we had a lot of work to do on calorimetry, but at least I knew basically what to try.

My first experiments did not show I had the right calorimetry yet, and that took the next couple of months probably, to really come up with good calorimetry. Doing calorimetry, finding a good way of doing it, and an accurate way of doing it, was a big challenge. It **was** a learning experience. Fortunately, at the time, I had my two postdocs, who then worked with me mainly on cold fusion.



You'll see their names on some of the early papers, David Stilwell and Kwang Park in the 1989 – 1990 timeframe. They were both interested in cold fusion and wanted to get involved.

Kwang Park had a PhD from UCLA. He did some of the early work but not as much, and he moved on to another position and finally, I think he went into dentistry eventually.

David Stilwell received a PhD at the University of New Mexico in Albuquerque in Electrochemistry, and he'd done previous work trying to develop electrochromic materials for Navy applications on one of my funded programs.

But he was mostly free to do what he wanted. David Stilwell did a lot of the early work and he was on most of my early papers. The first few months into his two years, he could do pretty well what he wanted, because he'd already mostly finished the electrochromic work and so he could focus full-time on cold fusion while I had to do the work on other projects. But that was up to him.

Soon after that, he got very interested in the excess heat experiment. During the week, he wanted to switch over on cold fusion as soon as I told him about it. He wanted to switch what he was doing and focus on developing calorimetry with me. That's what he wanted to do, so he worked on getting the first calorimetry worked out.

We had a lot of discussion about how to do calorimetry and we tried a lot of different things, and we finally got to where we could break even, where the heat coming out would equal the heat going in. He started doing different calorimeters, different studies, like using a plastic thermos bottle.

I knew exactly what he was doing and contributed ideas that he would try. The best idea was this secondary compartment filled with water that would minimize that drastic effect of electrolyte level changing the cell constant.

He was nearing the end of his time actually. He was there two years I believe, but this was near the end of that two-year period and he'd have to find another job or else get hired on, and he apparently wasn't going to get hired on at China Lake so he was looking for a job for that summer.

15 A SMALL CELL

One problem we had was the cell we were trying to use was kind of small in size, this only contains about 20 milliliters. This caused the electrolyte levels to fall rapidly, and that changed the cell constant, as we found out right away. If the cell constant changes with electrolyte level, you have to compensate for that in some way.

What we finally did was set the cell in a second cell, a second, bigger test tube, surrounded by water. It would minimize that electrolyte level effect because the surrounding water would stay at the same level, therefore, we got away from such a drastic change in the cell constant. Then, we started to get more accurate calorimetry.

We wanted a larger piece of palladium, even though I had a small palladium that was used as the reference electrode. We found that the other palladium in the stock room, a little bit larger in diameter as I remember, is from a company not very well known, but maybe an older company called Westco.



We started running experiments using that palladium. We later found out that that palladium was very poor loading, so it is not any surprise now that we never found excess heat using that.

Within, I think, our first set of experiments, we didn't show any excess heat, but we got the ratio of heat-out to heat-in down to 1.00 plus or minus 0.04. Looking back, that palladium would never have given excess heat because it did not load very well, we found out later. But for our first experiments, we just used what we had in the laboratory at the time. It was from a somewhat unknown company called Westco in California. We didn't know until later that you only see this excess heat effect with certain types of palladium made in special ways.

David Stilwell diverted from what he was doing and started working on it full time, and he came up to where he needed to get a thermistor system, so we ordered that. The thermistor could measure temperature to one-hundredth of a degree. It was a lot more accurate than the thermometer.

We tried various calorimeters and we had problems because of the small test tubes, the electrolyte level changes too rapidly, and so your cell constant is changing rather rapidly. It made it hard to get accurate measurements. I remember we tried a thermos that you buy for drinks, we tried a thermos and different ways of doing it. I think I eventually came up with the idea to use a plastic container and then an outer test tube filled with water that wouldn't change levels, and then put a smaller test tube with the D₂O in the center that would change levels. And it kind of helped wipe out the effect of the electrolyte. It wasn't nearly as great. And we could calibrate it quite accurately and that was the calorimetry we finally wound up with. It was this, the China Lake calorimetry we called it. And it was accurate to maybe 20, plus or minus 20 milliwatts, I would say.

At that time, I think we'd heard that Fleischmann and Pons were using lithium dissolved in heavy water as an electrolyte, which gives you lithium deuterioxide, LiOD, similar to lithium hydroxide, except it has deuterium in place of hydrogen. The D₂O that we were using came from the company Cambridge Isotopes. We later ordered our own from that same company, and that was our electrolyte, D₂O plus the LiOD with a concentration of about 0.1 molar or 0.2 molar LiOD. It was always isoperibolic calorimetry because that's what Fleischmann and Pons had utilized, and we thought we'd just follow what he used.

In those first experiments, we just gained information on what you need to measure. For each cell, you had to measure the cell temperature, the bath temperature, the cell voltage, and, to be sure, we used two thermistors in each cell so we measured temperature at two different points.

We relied on the gassing coming off for stirring, which is quite adequate as long as you use higher current densities. We never used less than 50 milliamp current in our experiments, but we would often dial-up around 200, 300, maybe 500 milliamp current. Those higher currents will give you bigger stirring.

The test tube itself was quite small. We filled it with just 18 milliliters of electrolyte. However, the smaller the cell, the more sensitive it is to heating the entire content of the cell, so this small size made the cell quite sensitive to any excess heat. Now you gain somewhat by doing that, but you lose, too, because the electrolyte level changes more rapidly, and that changes the cell constant. That was a serious problem, and in order to get around that, we put that cell inside of another, bigger, cell



filled with water. Then, the thermistors were against the cell wall on the outside of our active cell wall.

So, unlike Fleischmann and Pons, we did not measure temperature directly inside the cell. We measured the temperature on the outside of the cell wall. The extra water in the outer tube helped to minimize electrolyte level effect, and allowed the electrolyte level effect to stay at about the same constant level. It still evaporated a bit, but you could do studies pretty well for a day or so before you had to refill the electrolyte.

We weren't sure of anything. Eventually we got more than 50 milliwatts out of it, though we never saw that starting off because we were still using the old palladium. We initially came up with the value of heat out to heat in at 1.00 ± 0.04 , which is pretty accurate. I'm sure we did not have any excess power. Still, I thought at that time, to get 1.00 was pretty good.

16 SANTA FE MEETING MAY 1989

There was the so-called Santa Fe Meeting put on by Los Alamos National Laboratory, but I didn't go to that meeting. David Stilwell wanted to go, so I let him attend. We worked together making the view-graphs and presentation. I don't know whether we used viewgraphs back then or slides, but we prepared the presentation together, and David Stilwell went to the meeting and gave the presentation.

We probably had the calorimetry perfected and just a few weeks before the Santa Fe meeting that May 1989. We had done an experiment where we had zero excess heat, with an error of ± 0.04 . Otherwise heat out, equaled heat in. It was presented at the meeting that this ratio was 1.00, plus or minus 0.04 or, plus or minus 4%. We just were not seeing any excess heat at that time. It was going to be a couple of months to develop our calorimetry.

I have the publication from that meeting.¹⁰ It was published in a journal with a fusion title. I don't know exactly the name, but these conference papers are all published in one volume of that journal, which I have. Both post-docs, David Stilwell and Kwang Park, were on the paper with me, I think.

They were on several initial publications that we had. Even with later positive publications, I put all their names on it because they contributed a lot in getting the calorimetry working and so on. So we usually had three names, at least on the publications we did. Even the first report through XSE had all three names.

17 NEW PALLADIUM SUCCESS SEP 1989

The first six months, we did all sorts of studies using the same palladium in every study, and we always got heat-out equals heat-in. It wasn't until that fall, in September, 1989 we placed the order to Johnson Matthey for a new palladium rod. I might have ordered it from the Alpha Chemical catalog, but it was the Johnson Matthey name on the package. It was a six-millimeter diameter palladium rod, and a lot thicker than what we were using.

¹⁰ LANL. *Workshop on Cold Fusion Phenomena*. 1989. Santa Fe, NM: Los Alamos National Laboratory. <https://lenr-canr.org/acrobat/LANLworkshopen.pdf>



By the time we got that new palladium rod in, we had developed the calorimetry pretty well, and we set up the experiment. Well, we didn't see anything at first. We saw heat-out equals heat-in. But, after a week or two, we started seeing more heat-out than we were putting heat-in.

I didn't do that right at first, but the second time I ran it, I set up two cells. We had two calorimeters, so two studies ran simultaneously. Two pieces of the 6-mm palladium rod were cut off, somewhere between one and two centimeters long. So, we had two different samples from the same palladium rod, and we ran those over and over again in the three or four different experiments.

They both always showed excess heat. Then I turned it off and let it sit, and then I would turn it back on again, and every time, the excess heat would come up. It would deviate, sometimes it'd be larger, sometimes smaller, but in every experiment, we measured excess heat. I repeated that excess heat study for that same palladium, I think three or four different times, and every time, I got excess heat, and it seemed to be quite reproducible.

We started seeing excess powers of maybe 10, 20, 30%, where the ratio of heat-out to heat-in would be 1.2 or 1.3. I didn't believe it was correct right off. I knew we had a lot of questions, and we had to try it again. We had to test a lot of different variables.

Fleischmann told me that Johnson Matthey did not release any of this Type A palladium that gave excess heat nearly every time for Fleischmann-Pons until about September of that year. Most people did not have this palladium. Caltech and MIT had already run experiments, and came out with negative papers. I had a negative result for all those months too, but I was maybe one of the first ones that got this Johnson Matthey good palladium type, apparently.

We had been using about a one-millimeter palladium wire and this new cathode material was a thick palladium rod, about six millimeters in diameter, the thickness of a pencil. Suddenly, we had a lot bigger volume.

I didn't know that at the time, but Fleischmann reported excess heat of about 1 watt per cubic-centimeter at the normal current density of 100 milliamps per square-centimeter. Most people like to say the cold fusion reaction is a surface effect, but the excess heat seems to be roughly approximate to the volume of the palladium. For bigger volume you get larger excess heat. And I found that to be approximately true in all my studies I've done.

That was our first sign of any excess heat effect, about six months after first starting. We had a lot of the rod left and I used it over and over again.

You know Fleischmann once stated that people should report on whether, once the rod gives excess heat, does it give excess heat in the next experiment, or another experiment. My experience is that if you run a successful experiment, and then repeat an experiment with the same rod, it often gets better. I had a streak of success, and didn't realize how unusual it was that every experiment would give the excess heat effect, as long as I used those same new palladium rod samples.



These experiments were all written up. The first time reporting excess heat was in a refereed journal, the same journal that Fleischmann and Pons used, called the great common journal for electrochemistry, a Journal of Electroanalytical Chemistry.¹¹

Before that, we'd written up a paper reporting no excess heat, and that was in the Journal of Fusion Energy, which was published later in the proceedings of the Santa Fe meeting on cold fusion held in May 1989.

18 ERAB REPORT NOV 1989

I had two initial conversations with my Department of Energy contact where I told him we hadn't seen any excess heat. After getting the new palladium rod, I reported that excess heat was a possibility; I didn't say it was, or wasn't, happening for sure, but I thought we could possibly be measuring excess heat. It was still as yet an open question. I only gradually became convinced.

But that never made it into the DOE Energy Research Advisory Board (ERAB) Report. Caltech had already reported that there was nothing to the excess heat claims. MIT and Harwell, all the public scientific opinion, was negative on cold fusion. ERAB said it was probably not real, and I was still left on the side with CalTech and MIT, in finding no excess heat.

I can't think of his name right now, but my contact worked at a government laboratory in Colorado. I have his name and the conversation I had with him in September written up in a notebook.

I never ever even talked to any members of the ERAB panel directly. They hired somebody who worked at the Colorado Energy Research Institute and he called me at the end of July wanting to know if I had seen any excess heat effect. At that point, my answer was no, and I told him and then that's why I ended up on their list as being on the negative side. But in September, after getting this Johnson Matthey palladium, I called him back and reported I think I see an effect, but I don't know for certain yet. I thought we might be seeing excess. I guess he didn't think that was a real thing, yet, and I didn't either.

I wasn't positive it was excess heat. "Are you a hundred percent convinced?" No. I didn't call the ERAB and tell them, I did not go to any press conferences. I just wanted to take my time trying to figure it out." I was a little bit unsure and felt that it was not either their fault I didn't get changed over [to be in the positive results list because I was not convinced myself yet. I was always very cautious as a scientist. I don't want to go out on a limb and say something that's not true.

I never actually talked to anybody on the ERAB Board. None of them ever called. Martin Fleischmann told me, nobody from the Advisory Board ever contacted him. Nobody came to his laboratory at University of Utah to even look at their system. I don't know if they even visited Mike McKubre for example. The DoE didn't really do much work in actually investigating what people did. They just had a person call people, and they reported groups that reported excess heat on one side, and then a group reporting no excess heat on the other; they were about even in numbers, as I

¹¹ Miles, M., K.H. Park, and D.E. Stilwell, *Electrochemical calorimetric evidence for cold fusion in the palladium-deuterium system*. J. Electroanal. Chem., 1990. **296**: p. 241.



recall. I don't think they ever did much actual work except have somebody call laboratories for them. They never made trips as far as I know to look at any laboratory.

Of course, that's just about the time we started seeing something, which was just a possibility then. So that's how I found out it never got changed. I reported all those written things, I telephoned him and reported my first excess heat, but they just ignored it.

I just mentioned it, and didn't emphasize that I knew it was right or anything. I didn't know for sure. "This is the first time I've seen excess heat. I'll have to check it out more." I tried to look at a lot of different things and gradually I became convinced it was really excess heat, but that's how I got on the ERAB report with laboratories at Caltech and MIT and Harwell, and other labs, claiming a negative effect.

Norman Ramsey had them revise the introduction of that report to say that even one experiment showing excess heat would be significant. He's since passed away. But after I reported the heat-helium correlation a year or two later, somebody from that report called me and said he agreed with me, and he liked the fact that I did report this. It could have been after I published my calorimetry paper in the Journal of Physical Chemistry, reporting on errors made by Caltech and MIT. That might have been when he called me.

And he more or less thought it really was related somehow to cold fusion. I can't remember his name. Maybe Landis, if that's the name.

19 ICCF1 The 1st International Conference on Cold Fusion Mar 1990

Our first Navy experiments showing excess heat were reported at ICCF1 in Salt Lake City. It wasn't named ICCF at the time, but it was the first international cold fusion conference and was sponsored by the National Cold Fusion Institute. That was the first time I was convinced that we had real excess heat.¹²

That was in March 1990, the strangest scientific conference I ever went to. Critics were trying to influence the press that was there, by using terms you'd never use in a scientific conference, things that related to ridicule after a speaker spoke and poking fun at it. It was not being scientific in any criticism. I thought that was very strange.

We had about a 50/50 mixture of people who had positive results and critics that came to ridicule. We had hot fusion people from MIT that wanted to shoot down the cold fusion idea, and people from different parts of the world like Dr. Srinivasan from India that reported positive results. And anytime anybody had a positive talk, one of these negative people would jump up and they would just make statements of ridicule.

They made an attack of ridicule in a way that I've never seen ever happen in a scientific meeting. A lot of press was there, and they were just playing to the press. I've never seen a scientific meeting where a person gives a positive talk, and then somebody jumps up and tries to use ridicule, and

¹² NCFI. The First Annual Conference on Cold Fusion. 1990. University of Utah Research Park, Salt Lake City, Utah: National Cold Fusion Institute. <https://lenr-canr.org/acrobat/NCFIthefirstan.pdf>



ridicule would be the right word, like one I remember exactly, was this guy got up and said “this is something like Alice in Wonderland”, you know. Things like that.

I began to realize that it was hot fusion people trying to protect their interests. MIT was well-represented. I don't remember the names, but well-known critics from MIT were there at the meeting. Richard D. Petrasso was very vocal on practically every talk. MIT had hot fusion programs going on and they didn't want to get defunded because of the cold fusion. Their whole careers are based on getting government money year after year on this never-fulfilling promise of useful energy from hot fusion. Government funding sustained their careers, so they have to continue receiving it. Of course, they would fight against cold fusion getting accepted. Maybe I'd do the same, but I don't think so. An honest person would not do the same thing. But if you were dishonest and just interested in your career and not actual correct science, then you would tend to be that way. I don't blame them, but that's not scientific. That was March 1990.

The thing I reported in Salt Lake was the use of a new Johnson Matthey palladium rod generating excess heat. I even showed where we had a power failure during the experiment and when the electrical power was out, it deloaded. When I started it back up, it was back to zero. That's a good test really, if you do that, by the way. It was accidental, but if you turn it off and deload it and restart up, you should be back at zero excess power and that's exactly what happened. Then this excess power would kick in again a little bit later. Then that was another good sign it was a real effect, it went to zero and then recovered after the power failure. I had to just let the cell sit and deload, then turn it back on and it was back to zero excess heat. Then, slowly, as it loaded up, the excess heat came back up again.

I had heard a lot about Fleischmann and Pons as electrochemists, because they were top in their field. A couple of years previous, I think it was 1987, I was at an Electrochemical Society meeting where Martin Fleischmann received the Palladium Medal Award, the top reward of that Society. He gave a talk to the entire audience of everybody attending the meeting, and I remember his talk very well. I said, "It would be great to know a great electrochemist like that," but I didn't know him at that time.

I'd never talked to either Martin Fleischmann or Stanley Pons, and nobody knew anything much about me or my work at that time. At the ICCF1 conference, my presentation was on the last day of the meeting, and I remember after I finished my talk and sat down, here comes Stan Pons from across the room and congratulates me. He said what I reported agreed well with what he and Fleischmann were doing, and I had shown many things that they had found as well. I think that one thing that Pons liked about my results was that it showed that when the power went off, and it deloaded back to zero excess power, it would come back up to produce excess power again. I realized he realized that it was really definite proof that what they had done was correct because somebody else had done practically the same thing.

That's the first time I met Stanley Pons, following my talk there in Salt Lake City.

I didn't meet Martin Fleischmann at that first ICCCF meeting. It wasn't until the next conference in Como Italy that I got to know Martin Fleischmann and talk to him. I remember before the meeting



started, they had name badges out there and I'm standing next to Martin and he pointed out to the girl here, he said, "My name, it has two ns at the end." They had it with just one "n".

He was a stickler for details. Of course, it's important that you spelled his name properly.

But Martin Fleischmann was well known in the field. He was a member of the Royal Society, which is a high honor in Great Britain. Stanley Pons was very active in a lot of different publications. He was head of the chemistry department at the University of Utah, as well. I don't know if that's an honor, posts like that generally rotate through faculty, but they said, "Nevertheless, he was the Chairman of Chemistry at that time."

Martin Fleischmann said that they had been working on cold fusion for about six years at the time of the announcement. So that would even place it at about 1983 when they started their research.

Pons is the quieter of the two and less comfortable giving talks. But he did give the talk at the meetings that they had there in Salt Lake City. Of course, Fleischmann came on and answered a lot of questions afterwards. I remember their talk because it was the first time I saw the detailed equations for calorimetry and I realized right away that they were certainly way ahead of me and they had a lot of things figured out that I had not quite figured out. Like the fact that there's some excess power due to the gasses coming out of the cell. They had that modeled. One of the most complicated equations in their whole modeling was accounting for the heat of the gas escaping from the cell. It comes out, of course, being hotter than the environment, and takes heat away from the cell, and that has to be accounted for.

That's always a term I don't like to calculate because it's complicated and you have to know what the atmospheric pressure is of the room at the time, and you have to know what the partial pressure of D₂O is at the temperature of the cell at that moment in order to do that calculation. I've never known anybody else to even be concerned about the pressure in the laboratory, but if you're going to use the detailed equation of Fleischmann and Pons, the gas term, the heat carried away by the gas, you have to know the pressure in the laboratory at that time.

After that, every time I took measurements, we had a big round meter that gave the atmospheric pressure of China Lake. You just go look at that and write it down. At an elevation of about 2,500 feet, the typical pressure of China Lake was around 690 millimeters instead of the standard 760 millimeters due to the elevation of China Lake. Interestingly enough, that's about the same elevation as here in St. George.

20 BACK BURNER RESEARCH CONTINUES 1989-1991

I was hoping for funding for cold fusion after that meeting, but I did not get it at that time.

In 1990, I was still working on the new lithium-type battery program, with the lithium anode and a bromine trifluoride electrolyte, BrF₃, for the Office of Naval Research (ONR). Bob Nowak was the head of that program and he knew about Fleischmann and Pons' work. He had even funded some of their early work. Before him, it was another person (Jerry Smith) at the ONR Laboratory.

It was around July 1989 when I got the official word from Bob Nowak that he could not fund cold fusion research. He told me to go ahead and work on it, but kind of on the back burner. He was



actually told by the Office of Naval Research not to fund anything in cold fusion. He said there were a lot of enemies of cold fusion at ONR, and they made it difficult for him to fund it officially.

He did say I could work on it to the degree that I could have the postdoc work on it, and that's how I first got started in cold fusion research. It was not part of any program. It was something we did on the back burner, in addition to what we were supposed to be working on officially, with a postdoc that did the research. He gave me permission, in his words, "to do it on the sly, do it on the back burner," but he did not tell me I had to stop. It's just that he couldn't fund the work.

At the ICCF1 conference, I was greatly impressed by Julian Schwinger, a mathematician and Nobel prize winner.¹³ In his talk, he said that what we should be looking for is helium-3, because the reaction of a Deuteron plus a hydrogen nuclei produces helium-3. With that reaction, there's no neutrons and there's no gamma rays given off, and nobody was seeing neutrons or gamma rays in the lab. I thought, "Well, that is likely what's happening." Of course, you'd need some trace of light-hydrogen, or water in your cell, but there always was some trace of water in the D₂O.

The only thing I'd wondered about, was there really enough hydrogen in D₂O? In our experiments, we tried to keep the H out, but it could be that there's an impurity, and it might be enough.

I thought a good way to test for helium-3 would be to collect a sample of gas during an excess heat episode, and then do a mass spec analysis of it. I'd already asked people at China Lake and they said they could not measure mass that low for helium-3. Their mass spec was more for organic chemists who had molecules with rather large masses. That instrument could not measure helium-4 accurately. What I wanted could not be done at China Lake.

Soon after, I had a phone call passed onto me by my branch head from Ben Bush who was inquiring about a postdoc position at China Lake. I was the main one hiring postdocs at that time. Ben Bush grew up around Sacramento and wound up at the University of Texas running laboratories for Professor Lagowski, who was in the chemistry department.

I don't know whether he knew I was already doing it or I guess maybe found out or else I told him. I don't remember the entire conversation, but Ben Bush, once you talk to him, he can go on for a long time. And so, we covered a lot of topics.

We had several phone calls before we got into discussing measuring helium-3, and he said he could measure helium-3 at the University of Texas with their mass spec. I said, "We should probably look for both helium-3 and helium-4." They show up separately so that'd be no problem.

This is what led to my consulting with Ben Bush. I was pretty sure I could get excess heat, and then we would measure any helium-3 and helium-4 that would come off.

¹³ Schwinger, J. *Nuclear Energy in an Atomic Lattice*. in *The First Annual Conference on Cold Fusion*. 1990. University of Utah Research Park, Salt Lake City, Utah: National Cold Fusion Institute. <https://lenr-canr.org/acrobat/SchwingerJnuclearene.pdf>



21 HELIUM-3 or HELIUM-4 OCT 1990 – DEC 1990

I set up a new experiment for the helium study around October 1990 and it correlated with some other things I wanted to measure, and all the details and parts of that experiment were completed by the end of 1990, following the ICCF1 meeting and before the ICCF2 meeting.

Ben Bush had a lot of good ideas. He set up a Schlenk line that you could do chemistry in without air getting into the system. He was already an expert on keeping air out of things, and so that's where he really contributed, by knowing how to prepare the flasks and how to protect them from breaking. You put film or tape around them, so on shipping, it wouldn't break. Plus, he knew how to flush the system out.

My contribution is knowing how to do the electrochemistry and how to get the excess heat effects going and so on. With Ben Bush's help, I followed his instruction on how to collect the gasses and I sent them down to the University of Texas for analysis.

We were mostly interested in detecting helium-3. But we figured, as long as we were looking for helium-3, we might as well measure helium-4, too. If you're going to measure for helium, it's pretty easy to measure both. One is mass three, the other is mass four. They just come off a little bit separately on the mass range of the spectrometer. Of course, you have to separate out the D₂. The bottle that you collect the gasses in is mostly D₂ and oxygen that comes off during electrolysis, so it's mostly D₂ in the sample when you collect it. The main problem is helium-4 shows up at the same place that D₂ gas shows up, and you have to separate the helium-4 from the deuterium molecules in order to do mass spec correctly.

Activated charcoal is the way they did it at the University of Texas. You put activated charcoal at liquid nitrogen temperature and make the gas flow through it. There's a lot of surface area in the charcoal. In the low temperature, the D₂ gets adsorbed in the charcoal and gets slowed down, at least temporarily, and the helium passes right on through and comes out first. He used an activated charcoal filter at liquid nitrogen temperature, and the D₂ and helium-4 would get separated and not appear in the mass spec at the same time. Helium-4 will show as a separate peak from the D₂ molecules.

Helium-3 would have been very easy to detect, because there's not much that interferes with it. The only thing that would interfere with helium-3 would be tritium, since tritium has atomic mass 3. Tritium would not be exactly the same as helium-3, but close. Let's see tritium has two neutrons and one proton, that'd be about roughly mass three. Deuterium, a DH molecule would also be mass three, but that would get adsorbed by the activated charcoal and get separated out anyway. The only thing that would really interfere, if tritium were really a product, it would get adsorbed by the activated charcoal. Nothing would get through except for helium-3 and helium-4, and they'd both be separated by their masses. You're not too worried about oxygen really, because it's much higher mass.

Ben Bush didn't actually do the mass spec measurements, somebody else at the University of Texas did the analysis, but that person didn't want their name on any paper. He would do the work, but didn't want any mention of it. I guess cold fusion had a bad name by then.



22 NO CONTAMINANTS

We thought we'd have to first find out what gasses didn't have any helium in them that we could use to purge our system clear of contaminants. At China Lake, we had a nitrogen line from a huge tank outside next to the lab that actually came right into my laboratory. The boil-off nitrogen would tend to be pretty pure, because anything like helium-4 would boil off much earlier and be gone, but there was also a small argon tank inside the lab, too.

We did the first test on the argon and boil-off nitrogen systems. Nitrogen was already being piped into my lab in a hood, so I could turn on nitrogen anytime I wanted. We had a huge tank about several stories high outside the lab, right outside the window filled with liquid nitrogen.

Any other gas impurities would be removed, and there would just be pure nitrogen into the system. I filled some flasks with boil-off nitrogen and sent them to the University of Texas to run tests on them, and there was no helium-4 detected. Nothing was detected basically. We had found a way to avoid helium-4. Helium-4 was not getting into our system from the atmosphere.

I sent him samples from our argon tank, and also, from the boil-off nitrogen tank and the test showed that if it did contain any helium-4 that could be detected. We found that the way to do the experiment was to use boil-off nitrogen because it did not contain any helium. This was all done before we sent out any samples relating to cold fusion.

The way we were doing things, there was no helium-4 even getting in our system, even though it was at five parts-per-million in the atmosphere. The atmospheric helium-4 was not interfering with anything that we were doing, as our critics would later claim. That was the main purpose of those initial tests.

We had to gradually get to our actual experiments, but we used the same system set-up in our first excess heat experiments. We found a water bath and made the cell fit in it quite well. The calorimetry was a plastic bottle filled with insulating material with the cell in the center. Inside, it was a little bit larger cell that contained the water on the outside.

And I used the same palladium I had used before, because it had been given excess heat in every experiment. And, in this experiment, it actually turned out to be the largest excess heat I had ever measured with any palladium. And that was quite fortunate because the large excess heat allowed us to see the helium-4 more readily, because we did have rather large excess power effects.

23 HOW TO MEASURE HELIUM

Ben Bush started by making these special flasks there at University of Texas. They were basically a modified, 500 milliliter volume, round-bottom flask. He modified the flasks so you'd just have a single inlet for gas, and a single outlet and he would tape them over so they wouldn't break on shipment. He used filament tape so they wouldn't break or chip, which could be easy to do. It turned out that it probably also helped prevent helium-4 from getting into the flask, too, because it had this tape on the outside. It turned out the tape would also impede the helium more than just the glass alone would. Then he shipped them to me, where I filled them with sample gas from my active cells.



After everything was connected, before we would collect a sample we would flush the flask for maybe 30 minutes with this boil-off nitrogen running it through the whole system. And then we would turn off the nitrogen. The gas always flowed out the exit and through an oil bubbler, so air could never come back in. The pressure would build up, and then a bubble would form and it would come up through the oil. So that way air could never flow back into the system. The only way helium could get into the system could have to diffuse through some part of the system, not by any backflow.

24 GAS SAMPLES DURING EXCESS HEAT

Then, we would run the experiment for maybe 30, 40 minutes and collect samples when we had excess heat.

This is part of the work where I found the Johnson Matthey palladium in the six-millimeter diameter rod would work every time, and so I thought reproducing was easy and it was as long as we had that rod. I ran two different cells in the same water bath. I ran the cells in series, that is the same current went through both, and Ben Bush was an expert in keeping air out of systems.

This happened to be one of the best excess heat effects I've ever seen. The largest excess power was in this experiment, so we were getting a large effect, which was good because the helium could be measured well above any contamination by any atmospheric helium diffusion through glass.

I didn't realize at the time but that turned out to be the biggest effect I would ever measure was in that experiment. It got up to about almost 50% excess heat, the ratio of heat out to heat in was about 1.5. Now, that doesn't sound like a lot but that's more than I've ever seen since.

We would insert the glass flask into the line of the gas that's coming out the cell, and then would run through this flask. And then, any other extra gas that gets through continues on through an oil bubbler, which prevents any atmospheric gas flowing backwards into the cell.

But what we did, we ran and I got excess heat and I sent him the flask where the gas is coming off and I measured the excess heat.

After sampling some gasses, we closed-off the flask by stopcocks in and out, and disconnected it from the line. I sent our samples down to Ben Bush at the University of Texas for analysis.

25 BLIND TEST

Now I'd never met Ben Bush. We just arranged everything by telephone. I don't think we even had any internet back then. There was no email, it was mostly just by telephone conversations.

I thought, "I don't want to have him be influenced by any of my excess heat results."

I sent Ben Bush maybe like 8 or 10 flasks, somewhere in that area, probably about three different times, and I didn't tell Ben Bush which flasks showed large excess heat and which showed the low excess heat, and which showed the medium or none. He didn't know anything beforehand.

I labeled the flasks with the birth dates of some of my kids and family members and so they were just, they had actual 1/18/37 that would be my birthday and then one of my kids was 5/5/75 and things like that. So, I just labeled the flasks that I sent him with birthdays; my kids birthday, my



birthday, my wife's birthday, and other numbers I recognize as important things. These were on the flasks. Ben Bush had no idea which flask had what sample.

Now I did not tell Ben Bush which samples had which amount of excess heat, or whether they had any excess heat. So, he did not know if there was any excess heat or not, or how much there was. All he measured was if there was helium in the sample and how much.

And the reason I'm kind of glad I did that is I found Ben Bush was really so anxious to prove cold fusion that he might have changed the analysis, and I'm glad he didn't have that chance, because he just thought cold fusion was absolutely true, and didn't seem to second guess the work enough, I thought.

I later found that he was so gung ho on cold fusion. That's the word I use in describing it. I think he could maybe go a little bit overboard trying to promote what he found. So I'm glad I did that.

Other people noticed that in him also, that he had no thoughts of cold fusion being wrong. I always had thoughts of being wrong and making errors and making sure we didn't make errors. I think that the helium-4 we did with the boil-off nitrogen were good controls, as were later experiments in H₂O, that gave no excess heat.

Now we did not know how much helium-4 we were measuring, but you can calculate how much helium-4 should have been seen based on the excess power measured divided by the current. That would determine how much helium-4 you would expect. And that's an equation I just worked out recently. And it pretty well fits with what Ben Bush reported.

And I didn't really expect a correlation, I was hoping for one but I didn't really know. I'm always critical of my own work and I try to figure out why and I never could come up with a good reason other than that it was being produced in the experiment by the excess heat. And I was kind of surprised when it started to really correlate with everything quite well.

Some of the samples earlier on, I would collect at different times and I think we had the dates listed when the experiments were actually done, but the way I reported it to Ben Bush was using my family members' birthdays. Later, when we published it, I think by then we did include the date when the sample was collected.

We had the graphs of the excess heat by date and we, on the excess heat chart we would show the collection points, when it was published. At least the later publication but perhaps not on the initial preliminary paper.

But on the final paper, it was about a year or two later before we completed everything. We had to study the diffusion of heat through glass and things like that.

We did the control actually for the first paper as well. But it took longer to write the full paper – it was a much more thorough paper and was probably published in '92 or '93 I think. But the first helium study and all the details on heavy water studies was completed at the end of 1990. It Was published early in 1991 and reported at ICCF2 in Como, Italy in 1991.



26 CONTROLS JAN 1991

All of that first helium study was done with Ben Bush still in Texas and me at China Lake, and it was all completed in about the last three months of 1990. The announcement was in 1989, so that was pretty quick. Nobody else had come up with those types of results yet.

We knew we had to have a control set so people knew that we didn't have helium-4 contamination from the air. I turned the experiments off over Christmas and when I came back in January of 1991, we did the control study. We ran the regular water H₂O control studies, and collected the gas samples the same way as before. There was no excess heat and we did not find helium-4 in any of those controls.

That was good proof that we knew how to keep helium from the air out of the system, but I guess a lot of the critics ignored the controls.

It was a little confusing though. At one point, I thought there might be a little bit of excess heat. The regular water is perhaps not the best control because H₂O does contain small amounts of deuterium as well. I don't know if there was a calorimetric error, or whether you can get a little bit of false excess heat using H₂O.

What Martin Fleischmann and Stanley Pons used for controls, which I think is the best control, is D₂O electrolyte and a platinum cathode. Then you don't get excess heat.

Have everything else the same, but for the control, use platinum instead of palladium for the cathode.

Also, the palladium we were using, we'd used in a previous experiment. It had been loaded previously with deuterium, so it might still have had traces of deuterium. So early on, I think I saw maybe a little excess heat, but that could be explained also by a little bit of deuterium still in the palladium I was using.

It was such a small amount anyway, at the limit of the calorimetry, maybe 10 or 15 milliwatts, and my calorimetry error was +/- 20. It was in the error range anyway, probably, and I never mentioned it. I remember the later experiments did not show the excess heat.

The earlier experiments showed maybe excess heat. The later ones did not.

27 HELIUM RESULTS

I thought that if we found helium-3, that would be definite proof of a nuclear reaction, because there is not very much helium-3 in the air around. We did look mainly for helium-3, but we never saw enough helium-3 that we could measure in any sample, and as it turned out, helium-4 showed up in samples. I would have actually preferred finding helium-3, and then it wouldn't have been so controversial. But nothing showed up with mass around three.

A good instrument can separate helium-4 and D₂ and the instrument at the University of Texas, Austin could do that as well. You see separate peaks, and I've got some of Ben Bush's actual photographs of the screen that show the separation. Also, the way that Ben Bush did it, and the way that Rockwell International's Brian Oliver did it, is that you pass this gas sample through filters that will absorb hydrogen, but they don't absorb helium. Like activated charcoal at liquid nitrogen



temperature, helium will pass through that and keep going, but D2 will get absorbed on it, and get delayed. This separates the helium-4 which arrives first to the mass spec, while the D2 arrives later. In order to do accurate work, you have to do that. You have to separate them physically in a filter system where you cause the D2 to be absorbed and the helium goes on through.

What we reported as the large peaks of helium turned out to correspond with the larger excess heat measurements. The medium peaks of helium were usually the medium excess heat I measured. And the low peaks, the small helium peaks were the lower excess heat values. There were one or two samples I sent him where I had not measured any excess heat, and he also did not see any helium-4 at all in those.

I worked up some calculations for a chapter in a book and almost everything I calculated theoretically fits with his peaks that he reported, except for two samples. If they were interchanged somehow, that would be exactly right, also.

Also, if anybody's going to measure gas samples in this study, they need to be able to measure one part per billion. There's a paper I'm reviewing, and I don't know if this is anything, but I know Tom Claytor's name was on it, though I don't know who else is on it without going back and looking, but I think it should be published. They're claiming they can measure one part per million. That's a thousand times different. That is not accurate enough to measure gas coming off a cold fusion electrolysis cell.

In fact, I've done a calculation to get 1,000 parts per billion, you'd need about 10 watts of excess power. 10 watts of excess power would cause any electrolytic cell to boil away. You'd see boiling before you would be able to measure the helium. So, one part per million will not work. In fact, in Nate Lewis' first paper against cold fusion, he has a section on helium-4 and his accuracy is one part per million, and he doesn't realize that you'd have 10 watts of excess power before he'd even reach the detection of that. I pointed it out to some people, and also in some of my papers. But one part per million won't do it.

28 MULTIPLE MEASURES

I was already planning on running an experiment and I wanted to do several other things, which I did.

I wanted to test neutron activation of gold and another metal. My brother was a nuclear engineer and I could send a sample to him, and he could test for nuclear activation by neutrons of the samples. So that was the second part of the experiment.

The sample I sent off for the neutron activation test was mailed during the Christmas season and it got lost, delayed in the mail, and so it didn't get there right away. I should have sent it by FedEx. But my brother did give me an answer that the neutron production would have to be less than 10⁴ neutrons per second, which is certainly true. Actually, I found out later that neutrons are generated experimentally more like a few neutrons per second, at most.

The third part was to use film I got from the dentist and put those in and see if they got exposed.



We had three ways, three things we were testing for. Film exposure, metal activation, especially gold foil, and helium-4. And those were the three things we measured in that experiment, besides that we found helium-4, but no helium-3.

29 PUBLISHED ON THE DOWNLOW

I completed most of the work in late 1990 and the control was done in January, 1991. The preliminary note came out in March, 1991.

I normally would have written that up at China Lake myself, but at that time, there was so much negative opinion about cold fusion, I was concerned about having a paper published on cold fusion as a US Navy researcher. I knew there were a lot of people that did not believe that cold fusion was real. I would have to go through a lot of different layers of bureaucracy for permission, and anyone could have prevented me from publishing that paper. There were so many negative things written about cold fusion in early 1991, that any paper I wrote would have to go through my branch head, my department head, the head of research, and then people above them, and it had to be approved to get publication, and I wanted to bypass that. It could have been blocked, because of the negative atmosphere at that time.

If I published through China Lake, it had to go through review and be approved by several layers, starting with my division head. Then head of research and then people higher up that I didn't even know. And I thought cold fusion by then was not a very popular topic. A lot of people, a lot of enemies. And I thought it could easily not get through review at China Lake.

I was concerned because there were so many anti-cold fusion people everywhere, including the Navy. I think some people in China Lake, I've heard this from several sources, thought I was making a mistake, it wasn't true no matter what I reported. And these were people in the Navy and in fact probably should have gone through different chains, and if I ran into one of those types of people and they might not want to allow the Navy to publish it, so that's what my concern was. So, to get published quickly, we had it submitted through the University of Texas.

I arranged for Ben Bush and Professor Lagowski to publish the paper there through the University of Texas. That's how it wound up with Ben Bush and Lagowski being the lead authors, despite the fact that it was our China Lake lab that did the research, and they just analyzed for helium-4. But anyway, that was the preliminary note.

I think Ben Bush did the final writing of it. I did a lot of the writing, but, and he put his name and Lagowski's name as first and second, which I thought was not right because everything was done at China Lake except for the helium-4 measurement. That still bothers me today.¹⁴

I had Ben Bush write it, at the University of Texas, and submit it from Texas, and they put their names first on the paper. Bush and Lagowski, and then my name.

¹⁴ Bush, B.F., et al., *Helium production during the electrolysis of D2O in cold fusion experiments*. J. Electroanal. Chem., 1991. **304**: p. 271. <https://lenr-canr.org/acrobat/BushBFheliumprod.pdf>



I didn't like that, but I don't know if I even knew they did that, put that order, but that's the way it wound up. If you look in some of these books here, you may not see my name, you look under Bush, you'll find the reference to helium-4.

The head of research later told me that I should never have done that. He said the work was done in China Lake, it should have been submitted through China Lake. And I got into a little bit of trouble, but he was one of the supporters of cold fusion so I think he probably would have helped me get it through if I had done it the right way.

But that's the way it wound up, as a short preliminary note, and it was accepted right away. We were published within a month after we sent it in by the Journal of Electroanalytical Chemistry.

Our helium results also got published in the newspapers, too. I don't know how that happened. I didn't do it. I think Ben Bush talked to some newspaper and that's how it got published. China Lake was not too happy about that. It was not supposed to be released directly to the newspaper. But that was not me that did that. But it got reported in newspapers right away as well.

The preliminary note was sent to the Journal of Electroanalytical Chemistry and they published it right away, as they often do with preliminary notes. We sent it in probably February, and it was published in March, I think, in 1991. It was published before the ICCF2 Como conference.

The Journal of Electroanalytical Chemistry is where Fleischmann and Pons published their first paper. And the editors there knew Fleischmann quite well because they'd worked with him and happened to have seen a lot of his work so they really believed, knew that Fleischmann was a good scientist and that's why they published cold fusion quite readily. It passed review right away and got published.

The first presentation on the initial helium work was published in the ICCF2 Proceedings. About a year or two later, Ben and I worked up the full paper, and it was published in the Journal of Electroanalytical Chemistry with all the details, as well as the follow-up work, even looking at the diffusion of helium forth through the glass. We answered that question as well in that publication.

I think this 1991 publication on helium was when my work first got noticed, because I started getting calls from people in the field. I got a call from John Bockris who wanted to come and visit, a call from Preparata, who happened to be in the United States at the time, and he wanted to come visit, too. They both came and spent a couple of hours at China Lake and looked at my set-up after the report of helium-4.

Maybe we should mention that, in the paper, the liquid nitrogen boil-off was a good test that we were keeping any helium-4 in the air out. I don't think we made a big point of that. We should have at the time. We actually did this work in, towards the end of 1990 and it got published in 1991, I think. So that was fairly quick as far as science goes that it got published.

That was the first helium study, and then after that, Ben Bush got a postdoc appointment at China Lake, and he came to China Lake to do his postdoc by the following year in January 1991.



30 NO HELIUM FROM THE AIR

Of course, the only complaint that anybody ever had about our study was that the helium might have come up from the atmosphere. But our work to keep atmospheric air out went very well, so helium contamination from the air was not a problem.

Schwinger's idea didn't really pan out, and it's too bad. If we had found helium-3 it would not have been as controversial. There is so little of helium-3 in the atmosphere, it would not be a factor. The reason most people just threw out my report is they said, "Well helium-4 is in the atmosphere and helium-4 can diffuse through glass. And that explains it" and that was why it was not accepted.

It wasn't until later that I did studies of helium diffusion through glass and found that, yes, it does diffuse through glass, but at a rate so slow, it would not have made a difference in my results.

But at that time, nobody knew the rate of how fast helium diffuses through glass. There were equations where you could calculate it. The result depends on the glass thickness and the surface area of the glass, and that varies with the kind of glass, meaning, different glasses have different rates of diffusion. We didn't know the thickness exactly of the glass, but we later found that out.

I'd already postulated in my talk at ICCF2 that deuterium will diffuse through glass from the inside, towards the outside. And it might be going through the same sites as any helium coming inside from the outside. With the outward flow and the inward flow, the deuterium would slow down the helium coming in.

And when we did accurate diffusion measurements later, we found that yes, having either deuterium or hydrogen in the flask does indeed give you a significantly lower diffusion rate of helium coming into the flask. So that part was true. It wasn't a magnitude ten like I hoped, but it was a magnitude of like, two or three, difference. And so that also helped dispel that argument from contamination by helium from the air.

We submerged the flask in water to find the volume of the glass flask. And then you put water into the glass flask, at a certain temperature, and find the weight of that. And you can calculate the volume of the water and the volume displaced by the outer surface of the glass, and from that difference you can calculate the thickness. If it was a perfect sphere, it would be $4\pi r^3$ vs. $4\pi (r + \Delta)^3$ where Δ is the thickness.

We also calculated and found the actual thickness of the glass. I think 1.0 millimeters is about right. You could calculate it either theoretically, or measure it experimentally as we did. And both came out pretty close.

I suppose you could also break the flask and directly measure the glass thickness. That's another way to do it. Well, that might even be a more accurate way to do it. We didn't want to break our glass flasks yet, so we just did it that another way.

Later when I used metal flasks, I could actually measure how much, when I had no excess heat, how much helium-4 was getting in. The amount I measured was too large to be coming from the glass. I found that I did have sections of glass tubing connected by rubber vacuum tubing. and the rubber vacuum tubing diffuses helium much faster. Helium diffuses through the rubber much faster. I didn't



have much of it, but my calculation showed that was the major pathway for the small amount of helium getting into our system.

When I did studies in metal flasks, no helium comes through the metal, but we still found a background amount of helium, and that's due to the amount that got in when there's no excess heat, there's still a little bit that got in, and that was due to diffusion through the heavy rubber vacuum tubing. It came out about the right amount from what I would calculate.

The helium work should have been a major breakthrough, but by then, most scientists just passed it off as an air contamination problem. Cold fusion had been dismissed as worthy of attention within six months when Caltech, MIT, Harwell all produced negative results. I think most scientists didn't know who to believe. In reality, most scientists didn't really look into it. It was all just unfortunate.

There are still a lot of skeptics. I've heard that even a lot of people in cold fusion never did accept my helium-4, and it is even discussed today whether it's correct or not. I mean, everybody has different opinions. Tom Passel was one. He was thinking it was maybe some other reaction. So, I don't know that 100% of cold fusion people accept helium-4, but I accept it 100%.

We found helium-4 within less than two years after the announcement. I don't know why other people weren't looking for it but it is a difficult experiment because you have to be good at keeping air out and Ben was the top person for that. And you also have to have large excess heat. If you don't have large excess heat it wouldn't do any good to look for helium-4 anyway. Both Caltech and MIT did helium-4 studies, but they didn't report any excess heat so of course they wouldn't find any helium-4. If they did not see excess heat, why even bother.

The cathodes for my 1990 studies were both Johnson-Matthey materials. I worked on cold fusion I guess unfunded from 1989 to 1991, I think we didn't get official funding for it until 1992, but I worked on it for the Navy from 1989 "on the back burner" until we finally had Navy funding, which then lasted for only three years. I think those three years were 1992 to 1995.

31 SRI CONFIRMS HELIUM PRODUCTION

I would say that, within the cold fusion field, the helium work we did still stands as a seminal and very important work for the field in establishing the reality of the phenomenon. I think Mike McKubre's work also really helped that a lot, because one of his programs at SRI was to verify our heat and helium work. He spent several years working on it and came up with measurements like the energy per helium, for example, close to what one would expect theoretically. You should get 23.8 MeV per helium-4 formation. McKubre got pretty close to that value. But this value can vary widely because we don't know how much helium-4 stays in the palladium. If only 50% is released outside, then that value would be changed by a factor of two. And it would become, rather than 24, it would become 48 MeV per helium atom. So, to get close to 23.8 you have to be sure you're getting all the helium produced reaching the outside of the palladium and getting into the gas sample.

I don't take that 23.8 too seriously because it can vary over such a wide range, depending on how much helium-4 actually gets outside the palladium. According to Ed Storms, if the fusion reaction is near the surface where it likely might be, then half the helium would recoil into the palladium and half would recoil out. So, he expected it to be about 50% of the helium-4 produced getting outside



the palladium. And, on average, that's close to what I found. About 20 to 50% of what you would expect gets outside the palladium.

McKubre did some work to try to force the helium out to get a better correlation by baking the palladium and measuring helium after it had been forced out of the matrix, and he got 31 MeV per helium, versus the theoretical 24. That was closer than most of my work showed. My work was usually a little bit higher than that. But McKubre pinned it down more closely in that test and I think that's what got this field and helium-4 more widely accepted.

32 THE REACTION BY EYRING RATE THEORY

Some they say you can't write D plus D going to helium-4? Well, I say you can, because you're just saying what you start with and what you end up with. That's all I'm saying. I'm saying, inside the palladium you have D, you don't have D₂. You have mostly D, maybe D⁺. Somehow it combines, maybe using screening by electrons to minimize the coulomb barrier.

I applied the Eyring rate theory to my cold fusion results, and it shows that the rate of helium production is likely controlled just by diffusion of deuterons in palladium. Storms has measured this experimentally and come to the same conclusion. The rate is controlled by the diffusion of deuterons through palladium into a nuclear active environment.

In almost every physical chemistry textbook, you'll find a page or so on the Eyring Rate Theory. It is still controversial, not everybody agrees with it, but they'll present it as a one of the prominent theories on reaction rates. Henry Eyring passed away some years ago, but he had a son with a PhD. I got part of my PhD thesis with Henry Eyring, and most of it with his son Ted Eyring. I sent him a copy of this Eyring Rate Theory paper that I wrote. He's retired now and he wrote back right away, saying his dad would have been thrilled to see that paper. He said it was a very good application of his dad's theory to the cold fusion area.

As a graduate student at the University of Utah, I took a full, year-long course on the Eyring Rate Theory using the textbook they had written, Eyring and two other people. We covered all aspects of the Eyring Rate Theory, chemical reactions as well as electrochemical reactions, diffusion, viscosity. Anything that involves any rate process over time can be treated by the Eyring Rate Theory. When I sent this paper to the Journal of Condensed Matter and Nuclear Science, whoever the referee was, he tried to argue with me about the Eyring Theory. He has never taken a class, you know like I did, but he was arguing that I was not right on the Eyring Rate Theory, that all Eyring Rate Theory was, was just one simple equation. It's not just an equation, it's a whole book. There is a most useful equation that I used, but there are hundreds of other equations involved in the Eyring Rate Theory.

The Eyring Theory paper has never been published. I tried to publish it in a journal, but I haven't succeeded yet. I sent it to Journal of Condensed Matter Nuclear Science, but the referee was so critical, I said, "Okay, I'm going to take it somewhere else." I'm meaning to go back to that, but I'm trying to publish it somewhere else right now.

Eyring Rate Theory indicates, as well as Storms results, the coulomb barrier doesn't really exist wherever the reaction takes place. Wherever the reaction takes place, the coulomb barrier has been



neutralized. It's not really a factor. It's just the diffusion of deuterons into that active region that controls the rate of helium production.

This reaction might go through a number of deuterons combining to form beryllium-8, like Akito Takahashi likes to argue. Then the beryllium-8 breaks up into helium-4. I don't know what the mechanism is, and I've always taught, and I've had it stated in textbooks, you can never prove a mechanism is correct. You can just argue that it could be a mechanism.

You know what? Thermodynamics is only based on the beginning reactants and the final product. The beginning reactant in my experiments is D, or if you want to write it as D₂, you could. Going back further it starts off as D₂. So, it's D plus D going to helium-4. That's the net reaction from reactants to products. How it gets there, maybe Takahashi's mechanism and maybe any other mechanism, but I find that for every two deuterons, there's one helium-4. That's the ratio of products to reactants two-to-one.

If you know the reaction rate, and on the average, we know at usual current densities, the fusion reaction gives about one watt per cubic-centimeter palladium, and that gives your reaction rate. And one watt means there's so many helium atoms per second producing one watt, so you know the reaction rate in atoms per second. Based on one watt, that's 2.1×10^{11} helium atoms per second per watt. But a watt-second is a joule, so another way to say it is that many helium atoms per joule.

Using the typical rate constant, you can calculate the free energy of activation, the enthalpy of activation, and the entropy of activation, and I do all this. The enthalpy of activation is pretty close to what they call the activation energy by Arrhenius theory. If you compare the result I get with what has been reported for deuterium diffusion through palladium, they come out very close. Just using the basic cold fusion reaction rate, you wind up with an activation energy that is about equal to the activation energy for the diffusion of deuterons in palladium. Therefore, it must be the diffusion that's determining the rate, not the coulomb barrier. Somehow, the coulomb barrier is neutralized.

This was published as an abstract at the meeting at ICCF 20 in Xiamen, China, as well as an abstract and poster at ICCF 20 in Japan. And so, in that way it's been published and, since Dr. Li was a co-chairman though he wasn't able to attend the meeting in China. He had to write back and said that the Eyring theory application was very important because that correlated with his work he's doing in China. That there is no real Coulomb barrier for activation. So, it just depends on diffusion. Because some of their work had shown something similar, I think that's what he wound up saying.

I'm trying to posit how this may happen. Ed Storms argues for cracks. I think it might be reacting near some electrical double layer. On the table there you have, if you're doing a cathodic reaction on the palladium then you would have a buildup of electrons on the cathode surface and positive lithium ions outside the double layer.

That'd be the double layer, negative charges on the electrode and positive ions in the solution phase. So that'd be an area of very high electron concentration. There's ways, even like Hagelstein argues, for defects in palladium. Even there you could have a layer of a lot of electrons and a lot of deuterons opposing the electrons. So you can find situations, small regions, where you could get a lot of deuterons and a lot of electrons. And this might be the nuclear active environment for this fusion



reaction, and the reaction rate depends on how fast the deuterons can come into that active area of the palladium. Whichever way it turns out, I don't know whether Ed Storms is right, or Hagelstein is right, or if McKubre's right. Personally, I favor grain boundaries that are always present. I do think there is an active region and that the fusion reaction doesn't take place everywhere in the palladium. Active regions probably come and go, and they're probably very rich in electrons.

And electrons tend to address or affect your deuterons enough that the activation barrier is not there any longer, the Coulomb barrier is then not a factor. And after, I think I did put the abstract on the discussion group one time and Bill Collis came right back and answered that that's what he thinks is happening. There's no Coulomb barrier inside the palladium or wherever it takes place. It's not limited by the Coulombic barriers, but by the diffusion of deuterons. Whatever the theory, Bill Collis right away agreed with my application of the Eyring Rate Theory. I'd still like to publish the full paper sometime, but there is another paper I'm trying to publish first and that's about the NRL palladium-boron cathodes, and so after I publish one then I will submit the other. So that's about where that stands. Both are in progress.

33 HEAT IS DIRECTLY PROPORTIONAL TO HELIUM

Well, since then, I found out by the chapter I wrote in the Elsevier book, it was 2020, that you can show theoretically, that the amount of helium is directly proportional to the excess power.¹⁵ It's not proportionately, a factor of 10 times excess power, it's just directly proportional to excess power. In other words, if you have a peak three times as big, then you have three times as much helium, not 10 times as much. And you have a peak twice bigger than that, you don't have two powers of 10 larger helium amounts, you only have maybe, a factor of six or seven times larger. And that put us more in the range of what we found later about the net amount of helium atoms that we were seeing.

Ben Bush did run a control and a little bit of helium mixed in but how accurate that was, he wasn't sure, but he thought his detection limit was around 10^9 . But I think actually, it was more like a few parts per billion, which is around 10^{10} or 10^{11} . In the Elsevier book, I got that all straightened out.

The amount of helium is directly proportional to the excess power, and inversely proportional to the current, because the more current you have, the more other gasses you're generating, and that dilutes the helium. It's both directly proportional to the excess power, and inversely proportional to the current. And you can show that theoretically, which I later worked out. At the time, we didn't know quite how to work that out.

The mass spec we used in Texas could also measure helium-3, but helium-3 was at such a low level, it was undetectable. We never saw any evidence of helium-3 in any of the spectra. When Ben Bush came to China Lake, I had a chance to look at those mass spec spectra. Actually, I wish I had made a copy or kept them, but it showed the normal background, and it showed the **D2** peak that came out later, and it showed the helium-4 peak. It showed a little bump which we called the low peak, and it

¹⁵ *Cold Fusion Advances in Condensed Matter Nuclear Science*, ed. J.P. Biberian. 2020: Elsevier <https://www.elsevier.com/books/cold-fusion/gromov/978-0-12-815944-6>.



showed about a 3 times larger peak for medium. And then the much higher peak, which we called large.

Well, this is another error that Ben Bush made. I didn't know enough about mass spectrometry, but he thought that these peaks differed by a factor of ten. One he called 10^{10} while the other he called 10^{11} and the largest 10^{12} , for the number of helium atoms. I now know that these helium peaks differed by only about a factor of 3 or 4 based on the peak sizes.

I think some people didn't believe our results, maybe, because we made that mistake about powers of 10. I think that the difference in the peaks, from what I saw, was sometimes no peak or there was a peak, just a little tiny bump. There's another peak, maybe three or four times larger. Then there's another peak, maybe seven or eight times larger than that little bump, so that agrees with my recent theoretical calculations

The peaks didn't show any helium-3. I don't know for sure Schwinger was wrong, but I don't think we had enough hydrogen in the system. We didn't have a big enough source of protium. Martin Fleischmann had told me that the separation factor is pretty large between deuterium and hydrogen. If you have 10% hydrogen in your system, it'll wind up that the palladium contains almost pure hydrogen and this hydrogen is a poison, I think. You don't get excess heat with any hydrogen present.

That's the early way they used to separate the H and D, they'd just do electrolysis and they get a richer amount of deuterium left behind. Then they'd do another electrolysis, and the deuterium would get richer and richer. And in successive stages of electrolysis, you wind up mostly with just deuterium left in the flask. Because hydrogen reacts electrochemically, about 10 times more favorably than does deuterium.

That's one reason Martin Fleischmann went to an open system. He knew that even if you had a little bit of contamination of hydrogen, and you probably always do have a little bit, it's never 100% D, and D₂O is 99.9% at the very best. You always have 0.1% hydrogen, no matter how good the D₂O is.

The gas is coming out with something, might be a little bit of back flow of H₂O in the atmosphere. And that's why I think a desert laboratory, like China Lake, is a lot better that way because you don't have a lot of humidity. A lot of places that didn't get any results, if you look at it, it was mostly places with high humidity. I think hydrogen is a very important impurity to avoid, and that's the only reason I think I had success. I was very careful.

Every step I took was not to allow hydrogen, or water, to get into the system. I had thought ahead at every stage, how to prevent that. And if you don't take that kind of care, you might ruin your chance to see the excess heat. You're not going to likely see excess heat anyway because this depends mostly on the cathode used. With Schwinger's idea, you'd have to get the right amounts into the palladium, and maybe there's some way to do it, but normally you don't have the right amounts of H and D present to produce helium-3.

There's probably close to 99% deuterium in there, and if you're careless, and 10% H₂O gets into your system, you have almost pure hydrogen in the palladium. There's just a fine range between where you can get the 50/50 mixture you might need of H and D. That's kind of hard to find exactly where that is.



I think you'd have to use a D2 sample with just a very small contamination of H₂O, and you might be able to prove or disprove Schwinger. But, I can't disprove it, and I think that still remains a possibility. Some future research program may answer this.

34 I LIKE MATH

You know another experiment at China Lake I was doing?

I think this was a Johnson Matthey palladium wire that gave excess heat, and I did it in Ridgecrest. My current was then turned off, and I let it deload. I wanted to measure the deloading process.

Of course, the excess heat will go away when you deload it. You turn the current off and it out-gasses, and all the D2 comes out. Then I turned it back on late at night and went to bed. When I came the next morning, I measured the excess heat, and calculated like I usually do, and I came out with a -41 milliwatts. And I said, "What in the world, it shouldn't be negative."

And I went back and thought, maybe I wasn't considering the work term and the gas term, either one, I said, "Maybe that's why it's negative." I went back and calculated the work term, plus the gas term, which equaled -41 milliwatts exactly.

I wasn't seeing any excess, but I was at least able to bring it back to the excess power of zero. But because I wasn't often calculating the gas term, which is a headache, and the work term which is not hard to calculate. I usually don't do that when I do normal calculations. Later, if I think it's important, I'll do that and add it on. That will add on to what you measure for excess heat, because they're negative on the side where the excess heat terms and therefore when you put them on the other side, P_x equals this, they're positive terms. It came out exactly what I measured, minus 41 and those two terms were minus 41. The excess power was zero.

This was in late 1990 and I reported on this in late June in 1991.

35 ACS MEETING IN ONTARIO, CALIFORNIA

There was a very controversial ACS American Chemical Society meeting in 1999 that we had in Ontario, California. China Lake people were in charge of this meeting, and they asked me to organize a cold fusion session. A lot of influential people tried to keep our cold fusion session out of this meeting - and some even threatened to resign from the ACS. Our cold fusion session was a one-day meeting, and when we finally got to hold the event, we were sent off to a remote place in the same building, and our session could not even be advertised anywhere. Fleischmann had even suggested at one point that we should hold our cold fusion meeting in some church as Joule had done many years ago when his ideas were not accepted by others,

When we finally got going, it was a really great meeting. We had a lot of key people there. Martin Fleischmann came from England, Ben Bush from Texas, Michael McKubre came to that meeting, and Robert T. Bush, a physicist from CalPoly Pomona was there, and lots of other people too.



36 ICCF2 The 2nd International Conference on Cold Fusion JUL 1991

ICCF2 the 2nd International Conference on Cold Fusion was in Como, Italy in July 1991 and I went and reported on the helium results there.¹⁶

Ben Bush had just started at China Lake in January 1991 and we didn't have enough funding for him to go, too. At that time, we didn't have any Navy funding for cold fusion at all, so I was the one that presented our excess heat and helium-4 correlation at Como. It was mostly my work and my thing, anyway.

It was that meeting when we started using the term ICCF2 and this conference was totally different in Italy. It was on Como Lake in a very scenic area. The lake to the north ran into the rocky end of the Alps. If you'd drive 30 miles north, you would be in the southern part of Switzerland. They said it was the only part of Switzerland where you have palm trees, for example. And I saw them there just across the border. And that would be the Italian part of Switzerland that speaks that language. It was a very scenic area and a very quiet meeting, no press was there, no critics were there. Steve Jones did not even attend.

I've heard the organizer of this conference, and other people say even today, that it was the best cold fusion conference ever held, and I think I would tend to agree with that. In my opinion, that was one of the best cold fusion meetings we ever had, in part, because we were left alone by the press. In Salt Lake City, the press was a nuisance, and here at ICCF2, we didn't have any, and that helped. We just concentrated on science.

Everybody working in cold fusion got to know each other pretty well at that conference. A lot of important things were brought forth there, and some of these were Navy related work. The press left us alone, the critics weren't there and it was great - many important advancements happened at that meeting.

I got to meet Martin Fleischmann and Stan Pons personally. That's when I first officially met Martin Fleischmann. I had seen him before at ICCF1, but I didn't get a chance to talk to him. But I got to know Martin Fleischmann at ICCF2, and it was there that I first reported on the helium-4 production.

Also, that was the meeting attended by the person that I did my post-doc with in Germany, Dr. Heinz Gerischer. He was in Munich when I did my post-doc at the Technical University of Munich, and now he had an even higher scientific position at an institute, I forget the name of it, in Berlin, Germany, one of the top institutions in science. He gave me my introduction and knowledge about electrochemistry and came down from Germany for the conference.

He did a lot of research in electrochemistry when I was there in Munich, Technical University. In German, it'd be Technische Hochschule, but they don't use that term anymore because it is confused with high school. It's Technische Universität, or technical university, it would be the English name, in Munich, Germany.

¹⁶ SIF. *The Science of Cold Fusion*. in *Second Annual Conference on Cold Fusion, "The Science of Cold Fusion"*. 1991. Como, Italy: Societa Italiana di Fisica, Bologna, Italy. <https://lenr-canr.org/acrobat/SIFthescience.pdf>



Dr. Gerischer was there for the whole conference, and he even wrote a write-up when he went back to Germany, that cold fusion is something that should be looked at by German scientists. He gave a very good report about the meeting in Como. In fact, he's about the only one of the very few scientists that ever attended cold fusion meetings from that country. Germany is still pretty lacking in cold fusion scientists. I don't know any cold fusion scientists in Germany. I don't know, but I'm not aware of any cold fusion experiments that Dr. Gerischer did. I think he was more in an administrative position by that time.

He was very impressed with the meeting, and he later went back to his high position in Berlin. He was, of course, a very accomplished electrochemist. At one time, he was, I think, president of the Electrochemical Society, an international society.

Maybe I should mention a little story. I took my teenage daughter, I think she was about 15, with me to that meeting in Como, because I wanted to travel around Europe, and I wanted somebody to travel with me, and she came with me to that meeting. At that meeting, Heinz Gerischer invited me to visit him in Berlin, Germany, and I gave him a date when I thought I'd be coming back to Berlin, Germany.

I think he said that time, "That's fine", and I was supposed to meet with him. I regret that that didn't happen, because we got delayed a little bit. We went up to Poland and Czechoslovakia at the time, and I don't know what caused the delay. Poland is a bigger country than I thought it was, for one thing.

We had problems one night in what was then Czechoslovakia before we got to Poland. I had some people telling me that unless I gave them some money, my car would be gone in the morning, and things like that. You don't sleep very well because you're afraid about losing your car. I talked to the people in the motel and they said, "Well, I think it'll be okay." And I just said, "Ignore it." But things are different in different countries.

We were about a day late getting me back to Berlin, and I called Gerischer, but he apologized, said he couldn't meet me that day. He would have liked to talk to me if I had been there the previous day. I think it was kind of bad that I missed that appointment that I set up with him.

It's too bad that Germany never got really involved, like Italy. Germany would have been a big asset if their scientists had really looked at this, but as far as I know, none of them did.

That was mostly what I remember from the meeting in Como, Italy. It was a nice friendly meeting, no conflict, no arguments with anybody, and just cold fusion people were mainly there. Michael McKubre reported on his flow calorimetry measuring excess heat.

Of course, Fleischmann and Pons reported on their work. Then, a Utah State University person, Wilford Hanson, had done an independent study of the Fleischmann and Pons report, and his analysis determined that it was real excess heat, and that was the independent study authorized by the State of Utah.

The State of Utah had given 5 million to cold fusion for the Cold Fusion Institute. They wanted somebody independently to analyze the data and that was Hanson that did that. This Institute didn't last more than a year or so at the University of Utah.



That's where I really got to know Dave Nagel, Scott Chubb, and Talbot Chubb at ICCF-2, the key NRL people that became my good friends.

37 NAVY COLD FUSION AT ICCF2A

None of the work I did on cold fusion from the announcement in 1989 until Como, Italy, 1991 was officially funded by the Navy. Bob Nowak knew about it, but it wasn't an official program. He said they had too many enemies of cold fusion at the Office of Naval Research (ONR) and other places, it would be hard to defend funding it. He still thought it was okay to work on it, though, and he was quite pleased with the work that I had done showing the correlation between excess heat and helium-4, and with the other Navy researchers presenting, too.

Pam Boss and Stan Szpak reported about their work on co-deposition, a new method to produce the effect. They didn't do calorimetry, but they saw other effects that seem to be reproducible every time. That was their first time they reported that at a cold fusion meeting. Frank Gordon, and all these Navy people were there at this meeting, and that's when we decided we should try to get a funded Navy program.

Dave Nagel was there. That's when I really got to know Dave Nagel and people from NRL, like Talbot Chubb and Scott Chubb. We went out to dinner one night and talked about setting up a Navy program. It was that Como meeting where Bob Nowak decided, all with what Navy cold fusion people were doing, mainly Stan Szpak and Pam Boss in San Diego at a Navy laboratory, and with what I was doing at China Lake at a Navy laboratory, that he would try to get a Navy program set up. Dave Nagel supported that idea also, and he was already in a pretty high position. At that time, I think Dave Nagel was a superintendent at NRL, which is a supervisor over many different groups at the NRL, so Dave Nagel pushed it, and Bob Nowak pushed it, and they got Fred Saalfeld to fund it.

And this, we actually did. We had a follow-up meeting in late 1991, and then the cold fusion program was set up, starting about January 1992 and lasting until June 1995.

It should have lasted longer in my opinion, because we were just getting the results that we wanted, finally, when it stopped.

The Como meeting led to the Navy program, and the work that Stan Szpak did at San Diego and what I did at China Lake. This led to Navy Final Reports by the three groups.

38 US NAVY COLD FUSION PROGRAM JAN 1992

The Navy cold fusion program started with a meeting that took place at the Naval Research Lab, NRL, back in Washington DC. All the cold fusion people in that area were there. Scott and Talbot Chubb, Dr. Imam, Dave Nagel, Pam Boss and Stan Szpak from SPAWAR, I think even Deborah Rolison, was there. I was the only person there from China Lake, at that time.

David Nagel played a big role in this program. In fact, he basically wrote up the program proposal of what the Navy program would be. It was written by David J. Nagel.



Three Navy laboratories were to work on cold fusion, the Naval Research Laboratory in Washington DC, the China Lake lab and my work. Ben Bush was working with me at China Lake then, too. That was the beginning of the Navy program. Of course, the key feature of the program proposal was, we wanted to make in-house Navy materials and produce the Cold Fusion effect. We wanted reproduction by multiple laboratories like NRL. Those were the main goals of the program.

Bob Nowak was one of a number of program managers there and he said a lot of program managers didn't like ONR getting into cold fusion. There was too much negativity there, even among his fellow workers. Of course, most people hardly know anything about it.

I think even Fred Saalfeld was there. He was the Director of the Office of Naval Research (ONR). He was quite enthused about it. He's the one that made it happen.

Without his approval that program would never have happened and he said he would fund it.

I remember him telling us how important this program was, and that we couldn't call it cold fusion. We had to call it Anomalous Effects In Deuterated Materials. He said there were too many bad things going on about cold fusion, and it would cause problems if it got out that the Navy was funding cold fusion research.

Politicians might even cut their funding off for other Naval research programs if that got out. The idea developed that we should try to form a Navy program despite the existing opposition. He said we could still write publications and so on, as far as that goes.

I think even Frank Gordon was there, by the way, if I remember right. He's the one doing most of the funding of Stan Szpak and Pam Boss. They think they got shorted by this program and didn't get their fair share of money, so Frank had to keep funding their program. But I think ONR's reasoning was, if Frank Gordon would do it, why should they?

It was mainly Stan Szpak and Pam Boss at SPAWAR. It had a different name at that time when we started. It changed names several times, but today it's known as SPAWAR, the Navy laboratory in San Diego. And it was myself and Ben Bush at China Lake, because the other postdocs had left. However, then towards the end it was myself and Kendall Johnson, who was very good, and he built a new, much more accurate, calorimeter that could measure within a couple milliwatts. He spent a year doing it. Almost his whole time at China Lake was spent building the calorimeter. And he only got to run a few experiments before the program ended, actually. And so that was interesting, but it didn't lead to very much future work, using that calorimetry. He got another position at China Lake. However, he was from up in Northern Utah he eventually wound up somewhere further north, where it's cooler. He didn't like the heat of the desert, I think.

The program got funded in January 1992 and ONR kept our China Lake lab funded, as well as funding the work at NRL. They really wanted more of the breakthrough to come from NRL, because that was the Navy's prime laboratory, and they wanted us to get things to NRL that they could then reproduce. But the things we gave them, they could not reproduce. In fact, they didn't actually reproduce what we did, they changed major aspects of the experiment. That's the tendency of every lab, of course. That's what happened at New Hydrogen Energy NHE in Japan, too. People don't follow what others do. If they didn't understand Martin Fleischmann's equations, they made up their own and used their own methods.



I think Bob Nowak wanted to do too many things at once. He wanted to do major loading studies along with excess heat. That works fine for a flow calorimeter, because size of the cell doesn't affect the calorimeter that much. However, if you do it in an isothermal isoperibolic calorimeter, that makes a big difference, because you make everything a lot bigger, it gets less sensitive. That's because the excess heat being produced is spread out over a larger amount of material and surface area, and also, there's more heat loss out the top and other things.

My average experimental error when measuring excess heat, which was too large in my opinion at that time was ± 20 milliwatts. The NRL error became ± 200 milliwatts. They put that in the final report as their calorimetric error. I only saw 600 milliwatts in one experimental run. If I had been using that calorimetric error band, I would never have seen any excess heat either. My excess power effects would have all been within the three sigma of 600 mW.

I found my error to be 20 milliwatts, and I can't express too much satisfaction with anything less than 60 milliwatts, or maybe even 50 milliwatts for excess heat. I wouldn't even call it a real effect then. It would be borderline. NRL would have had to have a very large effect outside the 3 sigma error range of 200 milliwatts. It would have to be larger than anything I'd ever seen in order to be detectable with their calorimeter.

39 MEETING BEN BUSH 1991

Later that year in 1991, Ben Bush did get a postdoctoral appointment at China Lake for one year funded out of a different program, a program for postdocs. It could be renewed for a second year, which it did. I'd never met him before, and I had no idea of what to picture him as. I was kind of a little bit surprised, he was different from what I thought. People often are not what you sort of put in your mind.

First, one thing, he was older. He had been a postdoc for quite a long time. He was not a youngster, like I thought he would be.

Ben Bush and I got along well, except for one problem, and Mike McKubre had the same problem when Ben Bush went to work for Mike McKubre. He is very fixed on what he wants to do. He doesn't want to do what, say, a group wants to do. He wants to do his own thing. And in short, you can't control him. One thing, he would come to work at three in the afternoon, and I would go home at six. We only had a couple of hours where we overlapped. I don't know for sure how late he worked, but he'd work until about two or three in the morning. I feel most of the time he was on his own, and doing his own thing.

He arrived around the time right before the Navy program got started. He had his own ideas, what the Navy program should do. At one point, he even wrote a letter to the head of NRL, telling him what should be done. Bob Nowak was very upset, when I told him that Ben Bush had shown me the letter, copied the letter, and he sent it off already, and Bob Nowak had to make sure he intercepted it somehow. And he did intercept it. But this caused me problems, and Bob Nowak and other, my supervisor problems as well, on things that he did.

McKubre said the same thing about him. He wanted to do his own experiments, in what he felt was correct, what needed to be done, and he didn't care what the group thought.



All I can say is, in the two years he was there, I never knew one time that he had a real, a good excess heat effect. And he was trying different things. Like one time I looked at an experiment running and I said, "What's that gunk floating around in there?" He said, "Well, I thought glass might be a bad thing for cold fusion, and I put a polymer coating inside the cell, and it came loose." He was doing his own things, and I didn't know what he was doing.

We wanted to do more heat and helium, but we had a hard time ever reproducing the excess heat. And one reason is the two cathodes that worked previously in every experiment, but I didn't realize those were so very precious. At that time, the expert in the country for doing the helium-4 measurements, Brian Oliver, came to China Lake and talked to Ben Bush, and wanted him to cut up those cathodes so they could examine them for helium-4 being present. I wasn't fully informed of that, and before I knew it, those cathodes were cut up, and they were gone.

And I didn't realize it, but it would be at least a year before we saw excess heat again. And if you don't see excess heat, how can you do helium-4 or anything else?

But Ben did find a government laboratory for helium-4 measurements in Amarillo, Texas. He was good at finding places like that for me, that would do helium-4 analysis. We did find a place that could do the helium analysis, if and when we had excess heat again.

40 THREE HELIUM STUDIES

I divide my helium results into three groups. The first was with Ben Bush at the University of Texas.

The second group was samples sent to Brian Oliver at Rockwell International, and that became the best results that I've ever seen as far as correlating the heat and helium, despite the fact that Brian Oliver would probably not accept that.

And then the third group was when we were trying to convince people it was not contaminated by helium diffusion through glass so we made metal flasks instead. We put the gas sample in metal flasks. And Ben Bush was good at building things. He actually built these metal flasks, and I still have them out in the garage actually, at least a couple of them, I may have given one of them away. It just had an inlet and outlet and it has a special valve that it makes when you close it, it's a metal to metal seal. There's not any plastic involved, to keep helium out. Make sure even the way you close the flask has metal to metal, it doesn't allow any helium diffusion. Ben Bush left before we actually got to use these.

Ben Bush went on to SRI in 1993, so later, I finally got some gas samples of excess heat along with a lot of samples that didn't.

There were three phases. Phase one: University of Texas helium measurements, Phase two: Rockwell international, Brian Oliver, and then Phase three: a Department of Interior laboratory in Amarillo with metal flasks, and they all agreed. All three different studies, different times, different labs, and we got the same conclusion: Excess Heat produces helium-4.

I was getting the same ratio of helium to heat, and they all still corresponded about the same way.



This is another paper I'm preparing for the ICCF22 meeting. Ruby already gave it at MIT but I'm going to submit a similar abstract to ICCF22.

With the help of a scientist at the University of Utah on statistics, in my final reply, I will do a statistical analysis. The chances of getting those experiments to agree, 30 out of 33 times, if you calculate exactly by statistics, then this chance is only 1 out of about 500,000. The details of these statistical calculations are given in the back of my China Lake Final Report.¹⁷ What odds would result if the fact that the correct levels of Helium-4 were also observed?

That was just for random errors. The chance would be, and that's even including the three exceptions that was explained it still comes out one in 500,000. It's in my China Lake **final** report, I might add it when I write a paper for ICCF22. I might put it in there.

41 2nd HELIUM EXPERIMENTS BRIAN OLIVER

At that time, this group in the LA area with Brian Oliver wanted us to collect samples in our flasks when we did get excess heat, and send it to him, so he could measure it. Brian Oliver was from Rockwell International and he was recognized as the top person in the world for measuring helium-4.

The reason we collected this was because Rockwell International and Brian Oliver wanted us to send samples to them so they could measure it and we did that. He did the measurements and he gave me the best correlation to the theory that I ever got, whether he wants to admit it or not.

We couldn't go back to the old palladium rods because Ben Bush had cut them up. Ben Bush worked with me for two years. Like I said before, he was kind of hard to manage, he did things his own way. He worked mostly late at night, so I didn't know what he was doing. I don't know if I gave him permission to cut the rods up or not, I don't remember.

I don't think I said he could do that, but he did it, and sent it off to Rockwell International to be examined for helium-4. Well, for one thing, we'd already run another experiment. I mean, an H₂O study. So I think the H₂O study had been run in-between and we'd loaded it and de-loaded it with hydrogen, so not likely much helium would have remained by then.

Well, because those good electrodes got destroyed, we really had a hard time ever seeing excess heat again. I don't know of any experiment that Ben Bush set up that ever showed excess heat for example. We went almost that entire rest of the year 1991 to not see any excess heat. Toward the end of the year we saw a small excess heat effect. It was another Johnson Matthey electrode that was just a small wire, 1 mm diameter. Based on the volume you would expect only a small effect, and that's what we saw. I didn't know about the volume effect then, but looking back at it, the size of that electrode being only 1 mm thick you would not get the big effect we had seen earlier. That's what we saw, it was, it was large enough I could be pretty certain about it. I mean it was **100 mW**, well above the error of my calorimetry. Another sample was **50 mW** and a third sample was 20 mW, and that would have been right near the borderline of what I could measure.

¹⁷ Miles, M., B.F. Bush, and K.B. Johnson, *Anomalous Effects in Deuterated Systems, Final Report*. 1996, Naval Air Warfare Center Weapons Division. <http://lenr-canr.org/acrobat/MilesManomalousea.pdf>



There were exactly three measurements for the second phase helium experiments because they were all samples collected from the same experiment, just at different times, when I had different amounts of heat. Those three samples in that second phase were the most accurate helium-4 measurements I ever had done. It was not the most accurate excess heat, because we're pressing too close to the limit, especially the 20 mW.

In fact, in this new paper I'm preparing for ICCF22, which I actually wrote first as a book chapter, Biberian has gotten a company (Elsevier) that is going to publish a book on cold fusion and he had different people submit chapters. I submitted a chapter on my helium-4 results, and that's the basis of the MIT talk and the other ICCF22 presentation, as well.

I was doing the calorimetry a lot quicker then, and when we finally got this one experiment where we collected samples. I thought the error was maybe plus or minus 10. I thought that my 100 milliwatts and 50 milliwatts samples were definitely excess heat, but we also sent a flask measured at 20 milliwatts, which I knew was questionable and borderline excess heat.

We made this a double-blind experiment where we would not know their measurement results of helium, and they would not know our heat measurements for the flask until we both reported back to Dr. Lagowski at the University of Texas, independently, so it was double blind.

I reported my 100 milliwatts, 50 milliwatts and 20 milliwatts, and Brian Oliver reported their helium results. They got the same order. The highest helium was our highest excess heat. The one in the middle was our middle excess heat and the smallest one was our smallest excess heat. I did a calculation, these were small amounts. They had measured our samples very accurately, to 0.1 part per billion. They measured over several days.

Now there is some diffusion of helium from air through the glass, so another thing Brian Oliver did was measure the rate of heat diffusion through the glass. They observed it over about 100 days and could see the helium, kind of in a linear fashion, increasing with time. They could see the atmospheric helium gradually coming into the flask, it kept going up slowly, because of the glass diffusion.

And they could extrapolate the straight line back to zero when the sample was collected, and that's why they reported how much helium there was initially in the flask by this extrapolation.

Their experiments gave nice straight lines and good extrapolations to zero, and it fit exactly with what we measured excess heat in the order. In fact, the rate of helium diffusion was pretty close to what we calculated theoretically based on the thickness of the glass flask. That gave us a measurement of the rate of helium diffusion from glass and we found out, in that first preliminary report, that would not have been significant with what we had seen. The amount of helium from diffusion would be too small to affect our results.

They didn't know the amount of excess heat that I had measured, and it came out close to just the right amount, so that seems strange that would happen that way to me. I thought it was quite significant, and some of the, you know they say you should get, based on helium it should be 23.8 MeV per helium, well it came out quite close to that. The 100 mW came out the best, it was the most accurate as far as the calorimetry was concerned.



When I did a calculation, I came out very close to the theoretical amount, the closest I ever came of the amount heat expected theoretically, almost matched to what they had measured. They measured helium, we didn't. But I think a given amount of excess heat would produce so much helium per second.

I think it's 2.1×10^{12} helium atoms per second per Watt, but I would have to look it up. But when I calculated the amount of helium being generated, that number was certainly within experimental error of the theoretical amount.

That certainly confirmed it, though Brian Oliver was not fond of cold fusion. Brian Oliver was an honest researcher, but he was not convinced at all in cold fusion. Of course, there's physical differences in the flask, they're a little bit different, but you extrapolate to zero time, those results differed, and this is not due to the glass. It shouldn't be anyway.

I don't know if he was convinced of our results, but I was pretty convinced.

And this turned out to be some of the best results in correlating with the predicted amount of helium-4. It came very close to what is predicted and of course the 100 mW sample showed the largest amount of helium and the 50 mW showed a medium value and the 20 mW showed the lowest value. It was right in the right order. And I don't think Brian Oliver was convinced it could have just been from the extrapolation but he probably says in the book by Hoffman. I have that book. They reported it like it doesn't mean anything, but to me it did mean something.

But Brian Oliver and the Rockwell lab that did helium-4 in the LA area, they wanted us to send samples from the two electrodes used in our first study so they could analyze it for helium-4, but by then we'd already run controls in H₂O, so I think any helium-4 that was there would have been probably removed by the loading with hydrogen and deloading again, but anyway Ben Bush cut up those two electrodes and that was the end of those two electrodes. And I don't know whether I gave him permission, if I had I shouldn't have because if you find electrodes that give excess heat you really should not let them out of your sight, because that a very rare thing to have, is an electrode that will give you excess heat in every experiment.

This was done about a year after Ben arrived. The end of that year, I think it was December. December 1991.

42 3rd HELIUM EXPERIMENTS - AMARILLO

In the third phase, there's about five controls and about five or six with excess heat.

I think the total if you combine everything that there's a total of about 33 experiments and there's a correlation between the excess heat and helium-4 in 30 out of those 33. The three exceptions all had something wrong with them. There was a calorimetric error that was in the first set. We found that the D₂O level had gone too low, and if the level gets too low then you can see a false excess heat. There's a false excess heat and there was no helium for that sample.

The other two exceptions were Palladium-Cerium electrodes that Martin Fleischmann gave me. Somehow, these gave excess heat, but when we measured helium, we didn't find it. In that case, you could argue that it is a different experiment. It's not really palladium, but maybe somehow Cerium



keeps the helium from coming out of the electrode or something and if so, it's probably still in there. I had no money to test that.

Then after that, Ben Bush, and I give Ben credit for this, he made a metal flask to collect the samples and send them to the DOI, the Department of the Interior lab in Amarillo, Texas where they could measure to plus or minus one or two parts per billion. That wasn't as good as Brian Oliver, who did ± 0.1 . Brian Oliver was the best I guess, but this Amarillo laboratory was very good.

This was a government laboratory, Amarillo, Texas. Now I used to teach in Middle Tennessee State University and traveled on I-40 regularly. Amarillo is right on the way. There's a big sign saying Amarillo, the Helium Capital of the World. I don't think it's there anymore. They had large deposits of helium. But I think it's tapered off a lot, and helium-4 is in short supply these days.

I had the helium measures done for our first two sets, and while Ben Bush was there at China Lake, he had located a person in Amarillo, Texas that worked at this Department of Interior laboratory there. I sent samples there for the third set of helium measurements; some that did not have excess heat, along with some that did. The five or six that did not have excess heat reported pretty close to the same background. Now, I finally knew what the background was.

I kept my equipment exactly the same, on purpose, except whether we collected in glass or whether in metal. I wanted to make changes, but I thought we could make the system better like the gas lines and so on, but I knew if I made changes, I would change everything, so I kept everything the same. That's a period where everything stayed the same, and when we went to metal flasks, I finally had the background. I knew what the background was. It was not very large. Then when I had excess heat, it was quite a bit larger than that, and I could subtract the background.

We're talking about parts per billion though, like the helium background was 4.5 parts per billion, and I was getting measurements up to 10 or so parts per billion. We had to do subtraction. Now, you could do it without the subtraction, but I found it agreed better with theory with the subtraction. Take the excess heat and helium-4, subtract the background off, and you got even closer to the theoretical amount.

You have to be pretty sure the background is correct. I think I did five studies and got a mean value which was 4.5 ± 0.5 ppb. It was that accurate, ± 0.5 ppb. I was getting results back to a tenth of a part per billion. I guess Amarillo reported ± 1 part per billion, that was kind of typical. Sometimes it was a little larger, I don't know why it varied. But it was never less than ± 1 sometimes ± 1.5 parts per billion.

You know one thing people don't understand, and I don't think Ben Bush even understood this when we first talked about measuring helium, I'd already done some calculations. I knew the amount of gas you generate by electrolysis, because you can calculate that, and that comes out to fill a 500 mL flask. That's 0.5 L, and if you calculate the number of atoms in there, it comes to like 10^{22} molecules of D₂ and O₂. Then, you calculate how much helium you get per certain excess power, and I could see it was going to be in parts per billion. This computation showed me pretty much at the beginning, and that's why I questioned Ben about how accurately he could do the helium-4 measurements.



43 CO-DEPOSITION 1992-1993

When the research started out on the Navy program, they wanted us to look at the co-deposition method that started with Stan Szpak and Pam Boss from the SPAWAR lab in San Diego. So Ben Bush and I started working on that. We had to change things somewhat because of the size of the cell, so it would fit it into our calorimeter.

We started on co-deposition and ran experiment after experiment, and never could measure any excess heat. Sometimes we thought we saw excess heat, but the palladium particle deposit would sometimes float around in solution, get to the top, and dry out, and then cause recombination. In fact, one time it even caused an explosion, kind of like a loud ricochet, because of the sudden explosion of D2 plus O2 mixture.

We had to measure the amount of gas coming off to be sure any excess heat was not due to recombination, and we did that in every experiment. There were a couple of experiments that did show some excess heat, but I think only about three out of 30 gave excess heat. In about 3 out of 30, we measured excess heat. That was a lot of experiments we ran with poor reproducibility. It was kind of a negative period we spent.

I later realized when I wrote the final report for the Navy at China Lake that this was probably mainly due to the fact that the amount of palladium was so small. ¹⁸ Fleischmann explained that in normal conditions you'd get about one-watt per cubic-centimeter of palladium. We were depositing such a small amount we would have to get a lot larger than one-watt, maybe 100-watts per cubic-centimeter, before we'd see anything. So that's not surprising after the fact that we didn't see excess heat because the amount of palladium was so small. That amount of heat would not have been measurable. Also, that could be the reason the deposition just does not deposit a large amount of palladium onto copper substrate.

We had a Geiger Muller tube nearby, and it normally was just normal. However, whenever we started a co-deposition experiment, it would shoot up, indicating some amount of radiation. Even though we weren't seeing excess heat, the Geiger tube was picking up some radiation. It had some very large effects. In terms of standard deviation, I think it was about 27 standard deviations above background, but that was hard to trace down. There was a radiation effect, but only when the cells were running, though not every time.

A Geiger Muller is not sensitive to the type of radiation, whether its alpha, beta, or gamma, so you didn't really know what type of radiation it was producing. Because of the radiation effect, Dave Nagel arranged for somebody from NRL to come out and measure it with instruments that could detect the type of energy. He was an expert for radiation at NRL that Dave Nagel knew. In fact, I think he worked at NRL for Dave Nagel actually.

He came out twice, and we set up experiments and ran them, one after another, real quick, and every single time, he didn't pick up anything, and my Geiger meter didn't pick up anything either. Neither

¹⁸ Miles, M., B.F. Bush, and K.B. Johnson, *Anomalous Effects in Deuterated Systems, Final Report*. 1996, Naval Air Warfare Center Weapons Division. <http://lenr-canr.org/acrobat/MilesManomalousea.pdf>



one of our counters picked up anything. We called it “the NRL effect”. Whenever they were there, this radiation didn't show up.

We also examined the way we were using the Geiger Muller tube, and he said that what we were doing was correct, where you set the voltage on a certain plateau. But even the Geiger meter didn't respond when we did these tests while NRL was there.

I later realized what happened. We were running experiments too quickly without letting the lines clear out. In the procedure of co-deposition, as the solution becomes acidic, and it generates chlorine. Chlorine was left over in the lines. If we didn't let it sit, and let that dissipate, we wouldn't get the effect. The failure to see radiation when the NRL was testing seems to correlate to the fact we didn't allow the chlorine to escape from the lines we were using. Apparently, if you have chlorine present anywhere, even traces of it, it kills the effect. That's the way I explain it now.

I reported this later at ICCF7 at the meeting in Vancouver, Canada. I said cold fusion radiation?, with a question mark as the title.

44 NOTHING WORKS

One of the major goals of the Navy program was to make our own palladium material. We knew by then that some palladium worked well, but most palladium did not work at all. We just didn't have the right material, like the Johnson Matthey palladium. Generally, it would not produce the effect. We knew right at the start of the program that the palladium material was the key problem for cold fusion.

We didn't want to have that be an uncontrolled variable anymore. We wanted to get control of that material, find material we could make ourselves, and make material that would be active for cold fusion. Also, we were supposed to pass on what we did at one lab to another and see if they could reproduce it, and ultimately see if NRL could reproduce it.

The Navy Research Laboratory made palladium materials. When Dr. Imam started making palladium materials, I remember first he made pure palladium. They sent us both palladium and palladium-silver alloys, but every time we ran an experiment with those samples, we never saw any excess heat.

1992 was a year of frustration. I think we went almost the whole year without finding any excess heat effects. We started cold fusion and we couldn't find excess heat though. Actually, we went about almost a two-year period where we did not ever see excess heat. We saw radiation. We got more immersed in radiation, because that's the only thing we could see. Most of '92 and '93 were fairly negative as far as finding anything except for radiation effects.

Palladium, silver, other palladium materials, and I would run tests on them and one after another there was no excess heat, two years almost no excess heat. The only thing is in between, Fleischmann sent me a palladium sample that worked for him and that also worked for me. And that's the only thing I saw. The only positive excess heat in about a two-year period was this palladium, I mean Fleischmann said it was palladium silver alloy. And I sent it, I sent a sample down to Bush. He also measured excess heat with it. He did an analysis of it and said it was palladium cerium, not palladium



silver. I don't know. Either Fleischmann was wrong or Bush was wrong, but after that I called it palladium cerium.

I took the same electrode with me to Japan and it gave excess heat there. It was an experiment run in cell A-3, A-2 was palladium boron, that's the one in the middle. And the one on the end was the palladium cerium from Fleischmann and again gave excess heat in Japan, and the position A-1 was a NRL material made by Dr. Iman, was palladium boron cerium. The cerium in that case didn't help out, it didn't give any real excess heat, or, very little if any. So that was the one experiment they did that did not give excess heat in Japan. But because it didn't give excess heat, the very early cell constant I calculated was pretty close to the true cell constant. I calculated about 0.83, I think Fleischmann later found out the true cell constant was 0.85 something.

The one that looked almost identical to me, the three cells, they looked very identical. I would assume they would have about the same cell constant. Well the one giving no excess heat, it looked like it was a good control, and I measured 0.83 with it. The other measurement was just palladium boron, there was excess heat present, it gave your lower value. It comes, since you don't know the excess heat, you calculate that assuming it's zero, and that's the Fleischmann method of getting a lower bound constant, and so you just run it. You just get a lower bound constant and take the highest value it ever hits, and use that for the true constant, and that's what I did in Japan.

I base everything eventually on that lower bound value. That, and the highest one I measured. Not exact, but it was a pretty good approximation there. A lot better than NHE. They did a calibration on the third day. It was already producing excess heat on the third day, and they came out with a constant of like 0.78. That's a big difference than 0.85 and this is presented in the NRL report. Half the time they're way below zero and half the time they're way above zero. It just moves to the zero line. This just moves the zero line to about half-way between all the peaks.

The other electrode that worked was a sample that Fleischmann loaned me. I was invited to give a seminar at the University of Utah sometime in that period, and Fleischmann was there and I remember Mike Melich was there. Anyway, unlike my seminar I had given at BYU, they were quite respectful and treated me well. It was just a normal seminar and I got compliments on it. The seminar was on my heat- helium results. And then, I met with Fleischmann and Mike Melich later, and I told them I wasn't seeing any excess heat. Fleischmann offered to give me two electrodes that he had gotten excess heat with. I took these and tested these at China Lake, and sure enough they gave excess heat. In fact, they gave a pretty good excess heat effect. I took one of them with me to Japan. It gave excess heat again. I know in the notebooks they wanted me to return them and I think I did. But I don't have them anymore. But they did produce excess heat. I assume Johnson-Matthey made them.

If you have the right electrodes, you see the effect. If you have other electrodes, for some reason they don't work.

I think it was about twice a year, we'd have meetings back in Washington DC and Fred Saalfeld had a lot of interest. He would be at all these meetings, and Stan Szpak would present his results usually, from San Diego. I'd present my results and NRL, I guess



Dawn Dominguez would report, or maybe Dr. Imam would report, about making materials too, sometimes. I think Dr. Imam was excellent in making materials. I gave him a lot of credit for that.

I liked cold fusion and I worked as much time as I could on the cold fusion. In fact, I'd work weekends. I put in a lot of extra time on it. If you don't get excess heat, you can't really test any variables. You just sort of, you're just stalled. You can't really make progress, so there was not a lot of progress during this time.

Because of this, the sponsor at ONR started talking about ending the program. From month to month, we never knew how long the program would continue from about 1993 on.

45 BYE BEN BUSH 1993

After about two years at China Lake, Ben Bush went to SRI, about 1993. The NRL had been sending us material for almost two years and the postdoc appointments are usually for a year, and then, you're maybe renewed for another year. Before Ben Bush came, David Stillwell had moved on to another position back east, I think, working for a State Laboratory.

I first met Ben Bush after the paper was already pretty well completed, and I'd never met him before. You know when you talk to somebody on a telephone, and you have a picture of what they're like and when you meet them, sometimes it's not quite the same as what I thought.

One thing he was older, he had been around for quite a while, I think he's already in his thirties. We got along fine, except he was kind of hard to work with because he always had his own ideas and if what you wanted to do didn't agree, he would do it his own way and he wouldn't do it your way.

That got me in trouble with people sponsoring in a Navy program later because he would not follow the program and he'd just go his own way. He was hard to supervise because he would come into work at three in the afternoon. He'd work until maybe three AM at night, after midnight. And so he is, most times he's there on his own and he would not, I didn't know exactly what he's doing and most of the time, one time he had a cell running and I looked at it and I said, what's that gunk in there? And he said, well, I put a polymer coating on the glass, it's on the inside of glass, and the polymer coating come loose. Well, I never knew he did that.

We've since learned that, if you use a polymer coating, you don't get an effect. Somehow the silicates in the glass may be important in creating the effect and explains that you need to build up a somewhat inert silicon layer on the cathode so you get high current densities through just little small regions of the cathode. That gives you very high over voltages and that gives you a lot higher pressure thermodynamically of deuterium in the palladium. And so anyways, it's been argued that as far as I know anyway, nobody has found cold fusion effects without using a glass cell.

Ben just got me in trouble several times. He wrote a letter to the head officer of Naval research telling them how the program should be run. Bob Nowak, my direct supervisor, was quite upset. He said he had to intercept that letter and we would have been in real trouble had that got to Fred Saalfeld, you know, somebody down below telling them how to run, how the program should be done.



The head of research talked to Ben Bush one day and Ben Bush, I don't know what Ben Bush was telling him that he later told me, you said that. He said, you know, that guy can be dangerous. He could have maybe been hard to terminate, except his personality was such that some people thought he could be dangerous in the Navy, and the way he would talk without permission, what's called a "loose cannon".

Mike McKubre will tell you about the same thing. Ben Bush was skilled in a lot of ways, but you couldn't get him to do what you wanted. He did what he wanted.

I remember Ben Bush and I went to ICCF3 together in Nagoya, Japan. We ran into a problem right away, because we thought we could share a room, and when we went to register, they said, "No. Two males cannot share a room." I don't know how we worked it out, but we worked it out somehow. I know they have a homosexual problem, but I never thought there'd be that kind of rule.

Ben wound up going back to the University of Texas until Dr. Lagowski retired. Last time I saw him was at this March meeting of 2003, which was held in Austin, Texas of the APS, American Physical Society. I told Ben Bush about the meeting and so he was already in Austin, so he was at that meeting. I don't think he presented anything, but he was at the meeting.

Later, Linda and I spent the evening going to his lab at the university in Austin and talking with him. He pointed out a tower, there were some gunman Charles Whitman shot a lot of students at one point and that's the last time I saw Ben Bush. He had a second job working at an electronics manufacturing company. He was in quality control, I think.

46 NRL Palladium-Boron Success 1994

Anyway, we lost the two palladium rods that we used before that worked every time, and we could not recover from that position. We still had a little bit of that palladium we used, but other sections we cut off did not produce the effect. Nothing that the NRL sent us gave excess heat. The Stan Szpak co-deposition did not work out well for excess heat. We had been stuck in limbo in that situation almost from 1992, the beginning of the program, until 1994, when Dr. Iman with NRL finally made the palladium-boron samples. Dr. Iman sent me a palladium boron rod he just made. The palladium boron he sent me was just as good as the Johnson-Matthey.

When we started looking at palladium-boron, almost every sample he sent us gave us an excess heat effect. Suddenly, we had a material that seemed to work every time made by the Naval Research Laboratory and Dr. Iman. It gave excess heat almost every time.

When I first reported this, it got the sponsor at ONR quite interested. And when I gave a presentation, we had periodic presentations of our results at NRL, or else back in Washington, D.C. And the head of ONR Fred Saalfeld was always there. He was the one that was giving money to the program and he was quite interested in having the Navy solve this cold fusion puzzle. He funded us for about two-and-a-half years, I think it was.

When we reported results on palladium boron, there was quite a lot of interest in that. I went out to dinner that night. My sponsor said that probably saved the program, at least for a while. Navy cold fusion research continued another year into 1995.



47 NRL Verification Fail 1995

I continued to get success with palladium boron. But ONR wanted the NRL to reproduce it.

Now we had to get the NRL to verify this result, but their calorimetry had a large error, such that they would not have seen it. So, they worked on getting a Seebeck calorimeter set-up from Hart that would be more accurate.

It wasn't until January in 1995 that I went back there to help them. I helped set up the two experiments and Roger Hart was there. He helped calibrate everything, so we knew the calorimetry was working well. He was there while I was there making sure everything was calibrated.

I set up experiments and I stayed there for the first week or so. I didn't see how they set the cell in the Seebeck calorimeter. Otherwise, I would have commented. You need the gas to escape directly upward and out, and they had the long line running parallel, which eventually would get clogged with **D2O** vapor that comes out as well and it condenses. So the line would eventually get clogged up.

One time I went out to dinner with Roger Hart and he gave me his opinion of the NRL work, which was not good. He said they had been "collecting data for about several months", and "it was all useless". He couldn't tell "where the zero point was".

It turns out they'd never done the calibrations for the calorimeter, and you did not know where zero-mark was. It was stacks of paper, months of data that he couldn't make heads or tails out of. But he did make sure the system was working.

When I went back to NRL in January, 1995, I set up two palladium boron experiments there in their new Seebeck calorimetry. In one of my papers I'm trying to get published right now, it did give early excess heat, but the NRL didn't recognize it. It was because they had the gas outlet lines going horizontal. I didn't know that. I helped get everything ready, but I didn't see how they assembled the gas line. If you have a horizontal gas line, the gas comes out saturated with D2O vapor, and when it hits a cool area, it all goes back to liquid, and blocks the line. The tube for the gas exit must be short and vertical, never horizontal.

These lines get clogged with liquid unless it goes straight up. It goes straight up, it will cool, but then it will flow back down into the cell. I had the exit line, the glass exit line like this, and so did Fleischman and Pons. I saw the F-P gas exit lines at NHE, and they were about the same as what I usually used. You'll see condensing, but it condenses the liquid and it goes back down.

If you connect a Teflon line that goes horizontal, it'll get clogged. That's what happened later in their experiment, they saw big oscillations in the excess power. Well, you're seeing big oscillations in the cell pressure. The cell pressure builds up until it gets big enough to have a little burp, and then the pressure goes back down. It goes up and down, up and down, and pressure's one of the variables. I mean the work the cell does is equal to the negative of the external pressure times the change in volume, and you're having pressure build up and go back down and you're seeing that work term fluctuates all over.



I could see problems with the cold fusion program at NRL. Dawn Dominguez is a nice person and a good chemist, but she's a polymer chemist and you need training in electrochemistry and physical chemistry for cold fusion. That's not what a polymer chemist has training in. She was outside her area and didn't have the training needed. I probably wouldn't have done well working in polymer chemistry either.

Another problem I encountered when I went to NRL to work for a few weeks one time, in 1995 January, just before the program ended actually, is that nobody wanted to help Dawn Dominguez, because they didn't want to be associated with cold fusion. Pat Hagan was supposed to be working with her, and I hardly ever saw him when I was there. He went and worked on something else. Even Deborah Rolison, who knows electrochemistry, hardly ever came around. She got very little help from other people at NRL, that could have helped her. I don't blame her, it's just that they picked the wrong person to be doing this kind of work. It's just like if they had picked me to do polymer chemistry, they would have picked the wrong person. I admit it. But that's just the way it turned out.

One of the big goals as the Navy program was to make your own material and get excess heat. Well we actually did that right here at the end, but because the NRL could not repeat it, it was not believed, so the program ended.

I guess the main goal for NRL was to make materials, and I was to test these materials. I think that was the main thing. We wanted to make our own materials and have them produce excess heat. I got a lot of new materials and I did a lot of testing, but that was part of the two-year period where nothing gave excess heat. That's why the program was in jeopardy by the third year. The third year was when palladium-boron was produced, that last and final year.

But it wasn't until we hit the palladium boron that we started to see success. No success with anything to speak of until palladium boron. I think if NRL had reproduced the palladium-boron excess heat, the program would have continued, but NRL could never reproduce anything, and I think that's because of the people they had working on this at the time.

48 NAVY PROGRAM ENDS JUNE 1995

We had known the program was in jeopardy. In fact, it was hard to even plan during that last year or two. I keep getting the word that the program is "going to end".

We had a review meeting back at Washington DC in December of 1994, and Fred Saalfeld was there. He always came to these review meetings and was very interested in this program. He wanted us to join together and write a very defining publication in a major journal that would come out of this work. That's when I first presented the palladium-boron results using the Dr. Imam's cathodes made at NRL was at that meeting. We went out to dinner that night and Bob Nowak told me, "The palladium-boron work saved the program, I think." He said we need to continue to look at that. In January of 1995, I spent two weeks at NRL setting up two Pd-B experiments for testing with the new Hart Seebeck calorimeters.

So that was my first reported palladium-boron was at the December 1994 meeting at Washington DC. Then I did more work to follow it up, and everything I did produced excess heat. But when NRL could not reproduce it, or not in time anyway, that was the end of the Navy's cold fusion



program. The first funding was in January 1992, and it ended in June 1995. I remember distinctly how it ended.

We had a meeting at China Lake. Bob Nowak came, and Deborah Rolison, and Dr. Imam, and Dawn Dominguez were there from NRL. Even Mike McKubre was invited because he was getting some funding from Bob Nowak at that time. We met for the first day or two and suddenly the third day, it seemed like overnight Bob Nowak had changed his mind.

It was such an abrupt ending. Bob Nowak just briefly came in without Deborah and just said the program's over. He said, "Well, I'm just going to end my support of this research. This is the end of the program." and then he suddenly left us all just sitting there. We never know what happened overnight. Everybody there wondered what happened between the day before when we talked to him, and that morning. He seemed to be positively inclined until this final day. Interesting. Maybe he and Deborah talked and had some big disagreement. I don't know who talked or what happened overnight, but Bob Nowak came in with a very different attitude on the last day. It was startling, disappointing, and devastating. It seemed to me that Deborah Rolison and Bob Nowak had some disagreement overnight because they then went their separate ways the next day. I don't know what relationship they had, but neither was married and they often traveled together. I later learned that Deborah got a ride with Mike McKubre back to San Francisco for her flight back to NRL. Whatever the reason, this was a very unprofessional way to abruptly end the Navy Cold Fusion research program. In addition, it proved to be the end of my productive Navy research career. Without funding, the research ends. At this time I had more research publications than anyone else at China Lake.

We even had people from the University of Utah that were involved in the program. Also, Utah State, Wilford Hansen was there, two people from the University of Utah that were getting some funding. I don't know how far they got with their work, but they were funded. One of them was my former teacher, Haven Bergeson, when I first went to the University of Utah and took Modern Physics. He was very interested in this field and was working on it.

NRL later wrote up their Final Cold Fusion report where they did not ever report either any excess heat, or any other measurement of cold fusion significance.

I never saw any of the NRL data for my Pd-B cells except what I had in my notebook until years later. I had to return to China Lake just as these January 1995 experiments started. In about 2018, Steve Krivit did a Freedom of Information Act and got NRL and ONR to release these internal reports that show this data. Looking at this Pd-B data, I can see there was a small, but significant early excess heat in these experiments. Even though they did reproduce excess heat for Pd-B, they did not realize it. Although I had set up these two Pd-B cells at NRL they never showed me the data. It was a small and early excess heat effect as found later at NHE for Pd-B and also for my later Ridgecrest Pd-B experiments. It was real- NRL had finally an excess heat effect from two cold fusion experiments, but they failed to recognize this important result. Worst of all, I was never given this 1995 data despite my setting up these experiments. We had reached a major goal of the Navy program: Excess heat from our Navy material that was reproduced at NRL.



Unfortunately, if we'd known this earlier about this 1995 experiment, this might have saved the Navy program. It was because NRL did not recognize this palladium-boron excess heat at NRL that the Navy program ended in June of that same year 1995.

That's when my funding for cold fusion ended as well. I had a period at China Lake where I had no research funding, and it was very difficult to go through that.

49 STEVE JONES

I blame Steve Jones for a lot of the bad beginning for cold fusion. In 1989, Martin Fleischmann did not want to go public for at least 18 months or longer. He said he didn't want to give a press conference, but Jones was going to present results that weren't even accurate, claiming he did the work first. He would have wiped out any claim to patents at the University of Utah. Pons and Fleischmann were put in a real bind by Steve Jones.

To this day, nobody has ever reproduced Steve Jones' work. He was just jumping in there, trying to get credit. He caused the whole mess, in my opinion. I know he's a member of my church, but I don't care. He reviewed the proposal from Fleischmann and Pons. And he had done something somewhat related and then he wanted to jump right back into what he was doing and claim he'd been working on it for three years continuously. But there was a big gap in between. The gap only started up again when Jones read the Fleischmann-Pons DOE proposal.

Steve Jones wanted to claim that he'd been working on this for years and that he had evidence for neutrons and he talked about a new way to do fusion in metals and condensed matter. All this came out after the Fleischmann-Pons proposal. So naturally they were suspicious, they wanted to see what he had done and they saw little tiny baby food jars for electrochemical cells that he was doing experiments in. Fleischmann-Pons said "That can't represent three years of work." Stupid little jars! Even other people around Jones commented on it, that their work, what they were doing at BYU looked so primitive compared to Fleischmann-Pons' demonstration cells they brought down. Jones was nowhere close to what they were doing, but he wanted to claim that he did something that involved palladium and deuterium and he wanted to claim prior credit for cold fusion by stating that he found neutrons. He wanted to present that in a meeting that would've been prior work for any patent.

He caused the whole thing to get off to a bad start. Fleischmann and Pons knew what Jones was doing and they knew he was trying to claim prior credit for patents and they had to protect what they did. But they didn't want to do it.

This goes back to another long story when Steve Jones invited me to give a seminar at BYU and I did that. A friend of mine was teaching there and working a little with Steve Jones. This was Doug Bennion. I'm on vacation here in St. George, and he flew his plane down to St George airport and landed, it was on the Black Hill, and I went with him up to the meeting. We flew up to Provo, and I gave that seminar and he flew me back down.

I thought I was just going to be a regular 50-minute seminar, and that's the way it started out. Steve Jones and his group were there. His group interrupted so much, one-hour went by, and I was only



one-third through. Some people came to hear the whole story, they had to leave, they couldn't stay another hour, and it even went on to a third hour.

He or the people in his group tried to think of every question where there could have been a mistake. That was his reason for inviting me. I didn't realize that until afterwards. I should have stopped him right away and said, after I finish my talk, I'll answer your questions. I should have done that and regret I didn't.

I said I had nothing to hide, I'll answer everything you ask, so that's what I did. After that Jones started compiling his critique of my work. I've never heard of inviting somebody to a university for a presentation in order to write a critique of their work. I've never heard of that happening before. That only happened with Steve Jones. I don't know of anybody else having that experience ever.

But it affected Bob Nowak. During that June 1995 meeting, the paper of Jones' critique had just come out a month earlier, and Bob was quite unhappy. He made it clear to me, he was quite unhappy that Steve Jones had published all this critique about my work. And of course, Steve Jones' negative paper could very likely have a big effect on his Navy program.

He was quite unhappy because Steve Jones' paper against my work had been published in a major journal -the Journal of Physical Chemistry, and it had got a lot of publicity. It more or less made me look bad, with a lot of what I thought were false things that he had stated. He tried to bring up every argument he could think of apparently, where mistakes could have been made.

I blame Steve Jones a lot for this. I think that's because of what happened to me, I have never been fond of Steve Jones. I wasn't fond of him in the beginning and I wasn't fond of him afterwards.

He was in the news again a few years later, with a solar cooker for how to use the sun's energy to cook food in third world countries. Got back into the news. He has to be in the news somehow. Then he jumped into the twin towers issue, that it was an inside job thing, and the government brought those buildings down, not airplanes. That's when BYU said to him, we have to part ways. He just had to be in the news somehow.

I mean Steve Jones is dishonest, even though he's Mormon. And Fleischmann-Pons are honest even though they're not members of my church. Hmmm, that's just the way it works out.

50 ANOMALOUS EFFECTS IN DEUTERATED MATERIALS

After the Navy program ended, I had a little bit of money to finish some things up, and I was supposed to write a report, but pretty soon, I found myself unfunded. That was the first time I had ever been in that predicament in China Lake. Up until cold fusion, I always had more funding than I could use, and even had other people working with me, too. After cold fusion, nobody would fund me, within China Lake or at ONR.

When Bob Nowak's successor Richard Carlin came in, he was upset that I worked on cold fusion. He'd tell people at China Lake that I ruined my reputation by working on cold fusion and reporting positive results from experiments. He did not want to fund me, and he was the one that should have been funding me. He would instead fund people around me in electrochemistry at China Lake, and then these people would then come and have me help with the program. I had to help them do the



electrochemistry, but I was not funded directly. That's the kind of situation I got into. And it went on for a year or two. I didn't even have funding to write a report.

Each laboratory wrote their own report. NRL wrote their report, which I have a copy of. Stan Szpak and Pam Boss were a little put-out about having to write a report, because they didn't get their share of the money. Frank Gordon was funding them, and I guess they didn't see any need to write a report for ONR at the time. They might have written something, I don't remember.

But I didn't have any funding to do a report on the China Lake effort. When Dave Nagel found out about this, he sent me the money to cover the report. Nagel had influence. They knew he was pretty high up at NRL and he was known and respected at China Lake. They even had a day when he was a visitor at China Lake, and he was highlighted on the visitor board. I didn't see him that day, but if he gave me money, they thought that was great. China Lake couldn't then complain that I was spending time writing my Navy report about cold fusion.

If it weren't for Nagel, the China Lake Anomalous Heat program Report would never have been written. He funded me for several months to do it and I was really happy that I could go back and work on writing up all my cold fusion research.

But because of that gap in funding, the final report wasn't completed and published until about 1996.¹⁹ I didn't start until after the program ended and it took me quite a bit of time. By then, new people had taken over China Lake. Robin Nissan became chairman and he did not like my working on cold fusion from day one. Some people were very negative. They thought it was hurting the reputation of the China Lake Laboratory to even report anything positive about cold fusion.

That report got me in trouble with some people, because in my final conclusion I wrote about what NRL did and I said, "NRL reported zero experiments that produced excess heat and they had zero experiments that could have measured the excess heat that I measured and therefore their results are zero over zero, which is indeterminate." That was my final sentence. They tried calorimetry, but they had zero experiments that could have seen the excess heat except for the Pd-B experiment that I set up at NRL in 1995 using their new Hart calorimeter.

51 FLEISCHMANN AND PONS

I think there was conflict between the National Cold Fusion Institute on the one hand, and Fleischmann and Pons on the other hand, at the University of Utah. I think they wanted to know all the secrets of the National Cold Fusion Institute, and Fleischmann and Pons wanted the patents to be finished first. They weren't as helpful as they could have been, but they had their reasons. They were trying to find a way out from where they were, too, at least Pons was.

Then Fleischmann and Pons left and the National Cold Fusion Institute came to an end after one year. It was replaced by a commercial outfit called ENECO. Fritz Will went from NCFI over to ENECO. He was the head of ENECO. But then, at some point, Fleischmann and Pons left the University of Utah and went to France.

¹⁹ Miles, M., B.F. Bush, and K.B. Johnson, *Anomalous Effects in Deuterated Systems, Final Report*. 1996, Naval Air Warfare Center Weapons Division. <http://lenr-canr.org/acrobat/MilesManomalousea.pdf>



I don't remember exactly when they left. I was kind of curious about that. I think Pons must have left after the next school year. I think by September of 1990 Pons was gone to France. I don't think he stuck around for another full year. He was Chairman of the Chemistry Department and he wanted out of the United States and he didn't want to ever come back.

He had a church connection I think, in North Carolina that he picked up when he went to France. His relatives were from a small Protestant group in Italy, not part of the Catholic church, so they were persecuted, and the Pons group went to France. I think that by going back to France, he reconnected with that group.

In some of his emails, once in a while, he doesn't write to me often, maybe one in five emails he'll answer, but he will even tell me sometimes, he's going to go to Italy to check on family property there. He also told me the stories about him having immense wealth are all false. He doesn't have immense wealth from family connections. Maybe there's some wealth there, but he said they're exaggerated and they're false.

The two of them went together to France and went to work for this Japanese outfit, IMRA and Toyota.

52 TECHNOVA

Technova started in France. It was funded by Toyota or Toyoda family, but IMRA was some kind of an industrial association, and they were in there somewhere, too. Fleischmann and Pons went over to France and apparently were successful at Technova, until Johnson Matthey started making materials differently. They no longer used the cracked ammonia atmosphere, which Fleischmann thought was critical. I thought it was too, because that's a nitrogen and hydrogen atmosphere.

You ought to note that any oxygen in the palladium will react with the hydrogen and form water vapor and remove oxygen from the palladium. Same thing that boron does. Like the Imam material, it's called a getter. It's a getter for oxygen, and JM stopped using that method. I know Fleischmann told me in a letter or something that they lost ground for a while but then they regained it. I thought they had success in France, basically.

I think it was kept secret. I think they never were allowed to say too much, but Fleischmann still presented papers up till about 1995, even with Pons' name on them. After that, they wrote their own papers. Charles Beaudette told me that you could see the difference between when Pons wrote a paper and when Fleischmann wrote a paper. He said, based on that, Fleischmann wrote all the original papers that were published. It was Fleischmann writing the papers. Pons, he said, doesn't have the style that Martin had. He said he liked Martin's style a lot better.

That's what I heard too, was that the whole organization did not want there to be success in the France lab, because of some kind of professional jealousy or something between the home organization in Japan and the part of the organization that was in France.

NHE was somewhat connected. Pons came and visited and brought the cells that they were using, and helped them set up the calorimetric system. The three cold fusion cells that I used were made by Fleischmann and Pons.



To make those cells you need to find a really good glass blower who knows how to make a good vacuum. I don't know where you'd find it anymore. It's getting harder and harder to find people good at glassblowing.

Jean-Paul Biberian might have more insight than I do about what happened in France, because he is more in contact with Pons than I am. I only get an email once in a while if I write one to him. Even then, I don't often get a reply. I don't care if he doesn't want to discuss cold fusion. That's fine, and apparently, he doesn't.

I don't know if he'd want me to say this, and I don't know if he's written it up or not, but Pons told me directly on the phone one time, after the cold fusion program ended in France, he started going back to working in electrochemistry with some group. But, he said he always wrote under an assumed name. I'd like to know what name that was and he said he would get good reviews on his electrochemical papers and they wanted him to come give a presentation. He couldn't do it of course, without revealing who he really was. He was successful in working for a company in France and using an assumed name in publications.

Another thing I probably don't know I should say, is that Fleischmann's critique of Pons was always quite mild. He got to a point where he didn't quite trust Stan anymore for some reason, but I talked to Pons once on the telephone about this, and his critique of Fleischmann was much more brutal. I just don't know what happened between them, but Fleischmann never said anything that bad about Pons. He thought it somehow connected with the French secret service, and he wasn't sure whether he trusted him. I think the French secret service were trying to find out more about what they're doing and he thought they had something on Pons, or Pons had to tell them some things.

I don't know how much of that is true, but that was all Fleischmann ever said.

53 FUNDING BLACKLIST 1995 - 1997

I had no funding from the NRL and Bob Nowak never funded me again. I was told, more or less, to not do anything.

And then Bob's successor came in, trying to think of his name it was Richard Carlin, but he was an anti-cold fusion guy and I remember when I first got back from Japan, I went to an electrochemical meeting in San Diego and I was supposed to meet him and talk about maybe future funding.

When I met him, before I even started, he said, "I don't want to hear anything about cold fusion." That was off the table. He did send some funding, but I think he sent it to somebody else at the time, to do some work on corrosion, polymer coatings to resist corrosion which the Navy is interested in – corrosion and how to prevent it.

But if I got it, I was no longer directly funded, but the person at NRL or ONR I mean, started funding people around me, he funded electrochemistry. He would fund them and they would have come to have me do some of the work. They'd make the polymer of course, and I would study the coating by electro-chemical methods.

John Stenger-Smith was the polymer chemist that was funded. The best method to measure that is an electrochemical method called electrochemical EIS, it's a way to do a sort of spectroscopy, with



an electrochemical instrument. Another instrument is added, so okay, it's an electrochemical impedance study, something like that.

That's the way I got by for a few years, working on somebody else's project as electrochemist. They were funded by the person who should be funding electrochemistry at ONR. I didn't get direct funding again from the ONR. It was indirect, but at least I had some work I could do.

I didn't really have any choice. Without funding, you really can't do anything. And I was told to find other funding. I spent a lot of time just trying to find funding on something else.

I didn't really do cold fusion research at China Lake. During those years, I was looking for another place to go to continue cold fusion research.

I was talking to John Dash and we were exploring the possibility of going to work with him at Portland State University. I was discussing some options with George Miley, perhaps to go and work in his laboratory. I even got an offer from Preparata in Italy. In February 1997, they paid my airfare to go to Italy.

54 THE MILAN MEETING FEB 1997

Not having funding at China Lake, I was put in a very bad position that I had never been in before.

During that time, I was getting letters from Martin Fleischmann, and he told me that Preparata was trying to get funding to set up a laboratory in Milan, Italy. He asked if I would come and be the director of that laboratory on cold fusion research - if they could get the funding. I jumped at the chance, and said I would really like to do that. They arranged for me to fly to Milan, Italy to discuss it. Fleischmann came from England the next day. The three other people involved were Emilio Del Giudice, Giuliano Preparata, and Martin Fleischmann.

Martin made trips to Milan, and he and Preparata got along pretty good, so they wanted to get a group together. Martin wanted me to be the head of that group with the technician, and other people, del Giudice and Preparata were the theoretical people. Martin Fleischmann would come and visit and help with the program.

They thought they had a funding source from Pirelli Tire, a big company in Milan, Italy. A meeting was arranged for us to give presentations to a representative there, and we thought they would probably fund us. They also wanted to get enough funding to get set up in a laboratory somewhere else, like on an island off Italy. I don't know, but that would be quite a lot of money.

I thought at the time, that was my ticket out of town, because I would have funding. They did pay for a flight out of LA, and I went. That's the one time I got a little scared of flying, because we took off from LA and got about 50 miles away, kind of near the mountains, and a loud bang was heard from outside the airplane. The pilot came on and said we have an emergency, we have to go back. I don't know exactly what happened, engine failure or something. It was a Pan American flight and we had to go back to LA.

I wasn't too worried, because the plane seemed to be okay except for those loud noises we heard and we came back and then I got a little worried because all these emergency trucks were there to meet



us. Fleischmann later told me that he thinks that Pan Am, if you sit on the runway on a hot day, it wasn't a hot day, but if you run the air conditioning too long, you get vapors build up in the fuel tank. You use up fuel, you get vapors build up and you can have a fuel air explosion happen. That's what happened with the Pan Am that crashed off the east coast.

That might have been what happened, except it was February, and it hadn't been a hot day. But Martin sent me a letter explaining how that crash can happen. That's one letter he sent me.

Anyway, I got another flight with an Italian line. I was late, and we didn't have email back then, or at least, I didn't know how to use it. I didn't have it. Nobody took laptops with them. When I arrived, I had to find my way to the hotel, which I did with a taxi. Then, we had a meeting in Preparata's lab. Then we went over to Pirelli.

55 PIRELLI TIRE FEB 1997

I really liked Preparata, but he had a kind of fiery temper. He was constantly yelling. The morning we were supposed to go to Pirelli, we were supposed to be there at a certain time, but he got into shouting matches with his technician. Fleischmann took me into a side room and said, "Well, this happens often." But he said, "Can't do anything. We just have to wait until he gets through." He has to yell and yell. I guess it's an Italian trait, I don't know. We just waited in another room until he calmed down. It made us late for the meeting, and I was kind of bothered that this happened, but Fleischmann told me it's just something you have to put up with.

However, I saw the lab that Preparata had set up in Italy and the technician that was doing his work. And Emilio del Giudice, who was more in theoretical work, as was Preparata. Fleischmann thought that would be the ideal group. He said that would be the best group that he could ever hope to work for. Better than when he was working in France with Japanese funding.

Anyway, when we went to Pirelli, it was almost like a city within Milan. It was like going into a Navy base. You had to check in at the gate, you had to get a pass and check everything before you could get in.

You could get lost in there. There were a lot of buildings and in fact, Emilio got lost trying to find the right place we were supposed to go. We were supposed to meet at what used to be a very rich person's hunting lodge. It was a very nice, old building that we had met in. I thought it was strange seeing such a large place where Pirelli was located on the outskirts of Milan.

We spent all that morning giving our presentations to Pirelli. I gave a presentation, Martin gave a presentation and so on. I talked about my heat and helium-4 correlation, and Fleischmann gave a talk, Preparata gave a talk. Then we went out to lunch.

I thought I'd settle in Italy, but then Pirelli changed their mind. They said, you know cold fusion has - I heard the reason - cold fusion has a bad reputation, so we should maybe just do this in-house at first. And then the Pirelli people started turning up at ICCF meetings after that. We never got the funding. I lost my way out of China Lake and that's when I said I'll try to go to Japan.



56 FILM CREW AT PREPARATA'S

We probably spent three or four days together. It was Fleischmann, Preparata and Emilio del Giudice. Preparata had a nice laboratory and was working with Emilio del Giudice. There was a technician working on cold fusion and he was running some experiments and they wanted me to take a look and help with those.

They would say they would pick me up at 9:00 and I'd go down and wait. They might show up at 10:00! They got started late every day. We'd wind up going out to eat around maybe 9:00 or 10:00 at night, and not getting done until midnight. I guess that's sort of the way they do things in Italy.

While I was there, a TV film crew came in and Fleischmann and Preparata told me they were going to do a good story on cold fusion and I should talk to them. I guess they convinced Fleischmann and Preparata to do it. They interviewed me and they did things I didn't like. They wanted to pretend I was back in California!

They said, "Let's pretend, and make it look like you're in California" and then they took things off the wall to make it look like I was in California. And that's not really truthful, I thought. You know, trying to interview me and claim I was in California. "This doesn't look like California and this doesn't look like a California lab!" I didn't like even pretending I was in China Lake, but they insisted it'd be okay to do it. They said it'd be okay, you know? So they talked to me like I was in California.

You know I had already arrived and checked into the hotel the day or two before, but they wanted to record a video of my arriving in Italy at the hotel, driving around the corner, and then getting dropped off and going in the hotel. We did that several times. Then they talked to Fleischmann and Preparata and Emilio.

They keep telling me it's going to be a positive program on cold fusion. After I got back home, I kept asking Emilio del Giudice about it, and finally, he said they told a terrible story, showing us in a bad light. Everything was very negative, attacking cold fusion. They tried to paint us as crooks. They might have even said that I was lying about being in California, who knows? I told Emilio to send it to me, but I never got to see it so I don't know how it wound up, but Emilio said it was terrible.

I was very disappointed that anything we thought was going to be favorable turned out so bad. I don't even know what happened to it. It was broadcast on national television in Italy. That might have killed the Pirelli deal. That's funny, really though.

57 PIRELLI BACKS OUT

I guess we thought it was pretty sure we were going to get a lot of funding from Pirelli, so I kept waiting. I went back to China Lake and kept hoping to hear from them, and I kept calling Emilio del Giudice to see if he knew what was going on, and he'd say, "we're still trying". I really thought they were going to get the funding, but finally Pirelli canceled out. What I heard from Emilio was they thought it would hurt their reputation to get involved in cold fusion. Rather than funding us, they would do work internally, and look out for themselves.

I thought I was going to go to Italy, we were going to be funded by Pirelli, that's part of the reason Linda and I got married, we were not married at that point. We got married in April 1997, because



we were going to go to Italy, we wanted everything to be legal and so on. Had these books to learn the Italian language. Because I thought it was going to be a long stay in Italy.

Fleischmann had told me when I was there that he thought this was a really great group, and that we could really do something. He said "He never quite had the right group before." I thought somehow, he didn't think working with Pons and the Japanese in France was ideal. I guess there were problems there. He thought with the three of us, we'd have the right people together. He was really anxious that it would happen, but it never did. He was not too happy, as I found out in letters and so on. I guess he wanted to do more basic research. They wanted development into a product. And that's often the case. They want to skip the research and go to development.

They set up some work in house. And I saw Pirelli people at the next few conferences. There were always some Pirelli people there that were doing work in house.

You know, I ate dinner with them and I thought it would likely happen. But they changed their mind at the last minute I think, and just thought there was too much bad publicity about cold fusion going on.

The Pirelli meeting was in February, and I think I finally found out several months later, around April or May, that they weren't going to fund us. During that time, the head of the chemistry department started sending me emails telling me what I could do, and what I couldn't do anymore. He said since I wasn't funded I was to report to the stockroom person each morning at eight o'clock to work on inventory of stockroom chemicals.

58 STOCKROOM DUTY APR 1997

If you don't have funding, you're in a bad position. You're on down time, and you charge overhead, and nobody likes you doing that. You can also charge to somebody else's job order, JO, and they don't like you doing that either, so it's not enjoyable at all. I didn't like it.

I had people angry with me because I didn't have funding and would have to use their JO, they didn't like that. I had a lot of funding at one point and somebody who was actually the acting branch head at my branch, he used up all that JO and never did anything. I had seen that happen to other people when I had funding, either that or you put it on overhead. But they usually made it up with somebody else's JO. You're not a likable person when you get to that stage and I was trying to find a way out.

I did get some indirect funding to do corrosion work, electrochemistry involving corrosion, and some other types of work.

This was in 1997, April, a couple months after I returned from Italy. That's when I'd been given the memo to report to the stockroom by the head of chemistry. Since I wasn't funded, I was to report to the stockroom person each morning at eight o'clock to work on inventory and stock room chemicals. That really told me, he wanted to get rid of me.²⁰

²⁰ Fleischmann, M. and M. Miles, *Letters from Martin Fleischmann to Melvin Miles*, R. Carter, M.C.H. McKubre, and J. Rothwell, Editors. 2018, LENR-CANR.org. <https://lenr-canr.org/acrobat/Fleischmanlettersfroa.pdf> See pages 154 - 161, especially p. 158.



I still had a few friends at China Lake. When the head of research got wind of this, he talked to me, and I think he also talked to the head of chemistry, and I think he more or less told him, "this is no way to treat top scientists". They backed down a bit, but I could tell, they didn't want me to stay there.

I did a little bit of the assigned work. I said "I'll start with my own lab." I did inventory in my lab, and I worked slowly on it. They couldn't say I wasn't working on it. They wanted to waste money so I thought "Okay with me." I could have been doing something useful.

Instead, I just did a slow inventory of chemicals, and I never ever reported to the stock room clerk. That would have been an insult. She had a high school degree, and I wasn't going to go and report to her for work every morning.

I had been really kind of frantic to get to Italy at that point, but the funding never came through. I decided to approach the New Hydrogen Energy Lab in Japan, and I got a job offer to go there.

59 NEW HYDROGEN ENERGY OCT 1997 – MAR 1998

The job offer was about a little more than half the salary of China Lake, but it was a chance to get back into cold fusion. I took a leave of absence from China Lake and suddenly, Linda and I were at the New Hydrogen Energy Laboratory in Sapporo, Hokkaido, Japan.

It was October 1997. We had returned to the winter weather, and Linda didn't like it. Snow was on the ground almost all the time. You had to watch your step or you might slip and fall, and you couldn't walk under buildings because big icicles from up there could fall down. They found us an apartment up on the fourth floor. There was no elevator, you had to walk upstairs. We were young enough so it wasn't a problem. It was a good workout for both of us, climbing those stairs up and down.

The Japanese NHE program was funded by NEDO, New Energy Development Organization. It's like what we call the DOE Department of Energy here. The lab itself was called NHE New Hydrogen Energy. Toyota was one of the big sponsors.

Technova was at Toyota, I think. We had one floor of the building and the Japanese fuel cell people had the floor below us.

It was about the best equipped lab I'd ever seen for cold fusion. They had the calorimetry research in a separate room where the temperature control was very accurate. People coming in-and-out would kind of disturb that somewhat, but the room temperature stayed pretty constant. They ran three Fleischmann-Pons calorimetry cells at a time in a water bath. The water bath would be about this long and about this deep, and you could see what was going on in each cell through the glass and the water.

They already had the cells on hand. Pons had made a trip in 1993 to the NHE in Hokkaido, and he helped them set them up. In fact, when Pons actually visited the laboratory, he brought the cells to the lab, and then helped set-up their equipment exactly. I think they even had the calorimeters made and gave them to NHE. The Fleischmann-Pons computer-controlled equipment was duplicated at



NHE. NHE was operating with actual Fleischmann-Pons cells that they themselves had made, as well as their actual computer program.

They even had a book of instructions, about 80 pages. It had lots of math written by Martin Fleischmann and how to do experiments exactly. The NHE ignored most of it. It wasn't in Japanese. The manual was in English and they didn't want to read the English. I've got probably the only copy available in my room back there.²¹

These cells were silvered at the top, and it was a very good calorimeter, best I've ever used probably. I came home doing very accurate calorimetry I'm sure, but NHE did not like to read all the instructions I guess, and they just developed their own method of analysis. Their big mistake was they would use the first heating pulse to calibrate. They assumed there was no excess heat. Well almost every experiment I did had early excess heat - palladium boron gives a very early excess heat effect, and that threw their calibration way off. When I did co-deposition, I had an early excess heat, and that threw it off even worse, because it's a bigger effect. Calibrating only by the third day threw everything off, but that's what they did.

I pretty much ran my own experiments. I had taken some samples from the China Lake experiments. I took palladium-boron electrodes that I'd never run before. I took another electrode Dr. Imam made of palladium-cerium-boron and ran that. I took electrodes that Martin Fleischmann sent me, of palladium-cerium. I took with me electrodes I had that previously had worked. I also took a palladium-boron sample that I know from the dimensions and composition, it was one I had never tested. I said, "This would be a chance to test it." I ran those three electrodes in Fleischmann-Pons cells and that was the main part of my research.

I really enjoyed that year because the NHE lab was very well equipped and the people were nice. It was really a good group of mostly engineers. They were trying to develop a product more than basic research. The main technician I worked with was Mr. Sumi. The head of the group was Dr. Asami, who's since passed away. Dr. Asami and Mr. Sumi, he was an engineer I think, not a Ph.D., they weren't research scientists, they were trying to engineer a useful device. Dr. Asami was very nice. He was a chain smoker, he was always out in the lobby smoking, but they never did smoke where other people were.

That was one of the best periods of my cold fusion research, because we had a good group, and Pauli Tripodi of Italy was there. I became pretty good friends with him. Lipson from Russia, and the Japanese people and we all got along. In Japanese culture, you do everything together. You don't just come to work, you go bowling together, you go on a skiing trip together, I'd never been skiing before, but I went. Fell down over and over again.

Asami was a nice person to work for, even though he contributed to some papers that I don't agree with. He was critical of Fleischmann, asking, "was Fleischmann telling the truth?". Read my letter to Nature. I bring this up. It should've never been discussed as an answer to energy until we knew

²¹ Pons, S. and M. Fleischmann, *The ICARUS Systems, Isoperibolic Calorimetry: Acquisition, Research, and Utilities System*. 1995, Tokyo, Japan: Technova, Inc.



more about it. I mean unless we understand it, you can't know what it's going to be like, or whether it's ever going to be useful.

But they had already decided, the people above had already decided the program was going to end at the end of March in 1998. They were just winding down. They were in the last six months of the program, but they invited several foreign scientists to come. I don't think they even wanted any excess heat. Myself and Dr. Lipson from Russia were there, and another person from Italy was there. I don't think they even wanted me to find anything.

In fact, I wrote the report, and reported on the palladium and boron, and everything. I got Jed Rothwell to translate the Japanese version and they more or less ignored my results, or said that I never saw anything significant. It was clear the Japanese had a political game they were playing. That justified closing it down.

When I got back, Martin told me that he didn't even get a copy of his own report when he left France. NHE had this report in Japan. He had me make a copy and send it to him. It was a very detailed study of how to do the experiments.

60 ANALYZING DATA FROM JAPAN

The calorimetry was good, but the NHE analysis was very bad. That was what I got out of the NHE experience.

I didn't follow Martin's book because it's kind of complicated, and I did my own analysis. I knew then that there was, what Martin Fleischmann called, a lower bound constant. You first assume there's no excess heat, and that's a really good starting point. Then, you just calculate the cell constant. The cell constant is your unknown, and if there is excess heat, the cell constant is lower than it should be. If you have no excess heat, then it's higher and gives a more correct value. You run the experiment, and the highest point it reaches is getting close to the correct constant.

When there's a small amount of excess heat, or none, that calculation will give you almost the correct constant. That's the method I used, and it worked quite well. The constant was still a bit too low, but it was close enough that I could get pretty much the correct results and see the correct peaks.

When Martin did his analysis, he came up with a little larger constant and little larger effects, but still the same peaks were present. In co-deposition, you see excess heat right away. The only thing I didn't know at that time was the effect of my changing the composition from that used by Stan Szpak and Pam Boss at San Diego. They used lithium chloride and palladium chloride as the solutes, and then they plated out the palladium.

I read that you can get a pure metallic plating deposited if you use an ammonia solution. So, I used deuterated ammonium hydroxide and D₂O, and a little bit of the platinum, the palladium chloride. What I didn't know at that time was that when you plate palladium out, the positive palladium ions are replaced by hydrogen ions, or D⁺ ions, so the solution might start off at pH7, and wind up around pH 1 or 2. It becomes acidic, and then you can start to get chlorine evolution, because of the chloride in the system. I didn't know this at the time in Japan, but I found out later that the chlorine going through an ammonium hydroxide solution will react and produce nitrogen trichloride, which is a



little different smell than chlorine. I noticed when I was running the experiment in Japan people complained about the odor.

There was the nitrogen trichloride odor, it wasn't quite the chlorine odor. It was a little bit different, and as long as you have that reaction going on, there is an extra, excess heat peak that appears very early, when the Japanese tried to calibrate it. It was a chemical effect, the excess heat was chemically generated, but it was gone within a day because when you involve chloride ions, it has to be replaced by negative ions, which is OH^- , or OD^- , and so the pH goes back up and then the chlorine evolution stops. The problem is it's only for one day and then it clears up. After that, any excess heat is okay.

This is a different story, but a few years ago when NRL tried to reproduce my co-deposition result, they did see this initial peak, which is a chemical effect, and at that time I knew it was a chemical effect and I told them that. Then they wrote everything off as chemical excess heat after that. The certain thing is that this initial peak is due to chemistry.

Because I knew what to do, and what cathodes to use, almost everything I did produced excess heat. The only thing that was new for me was the co-deposition process producing excess heat. Instead of 18 milliliters of solution as before, I had 90 milliliters of solution. That gave me a much larger palladium deposit than before. That made a big difference. That'd be five times the deposited palladium. In other words, the same concentration of solution gave me five times the palladium deposit. Well, with a four-five times larger deposit, you see a bigger effect.

Fleischmann and Pons' work in France funded by Technova was still going on at that time. The two programs were going on simultaneously and they both ended on the same date in March of 1998.

61 END OF NHE PROGRAM MAR 1998

Well I would say I was treated quite well there, but I think they had a political agenda. They had to follow what people had already decided, and that the program was going to end. They didn't want anything disrupting it. The last three weeks of March NHE was being dismantled. The last two weeks I remember were quite sad, all this very nice equipped laboratory being torn apart, put in boxes, and different universities and other places coming and taking equipment. Over a two-week period, it went from a well-equipped laboratory to empty rooms. We were working in the office area, and we didn't need to go in there. I got pictures of how that lab got dismantled.

I was writing my final report during that time, because we had to cease all experiments by the first week in March 1998.

Before we left, we all wrote our own reports. In fact, I got the report for myself that I wrote. I got the reports somewhere for Lipson's report. I got somewhere the report for Paolo Tripodi. I got all three reports. I have a copy in here.

Almost every experiment showed excess heat and I wrote it all up. I even brought my China Lake calorimetry and before I ran the Fleischmann-Pons I did my own calorimetry and my China Lake calorimetry with the two cells in the water bath. I brought the Johnson Matthey one-millimeter diameter palladium material that I had a lot, quite a lot of it, and I still have some of that left. I ran those cathodes in two cells, and I got excess heat in one, but not in the other.



When I got back, you know I couldn't do this on Navy time, I wrote a paper on weekends. I even went up to my cabin in Oregon, spending a couple weeks writing that paper. Well, I was mostly kind of analyzing the data. You have to come up with a good analysis of the data. This was published in my last reviewed journal publication, the Journal of Electroanalytical Chemistry in the year 2000. It was published.

I later heard from Martin Fleischmann that David Williams was one of the reviewers. He said all the reviewers were very impressed with that paper, and David Williams even said "This is about the best paper I've seen that shows the early excess heat effect." I was plotting the cell temperature over time, I had a graph that did that. It showed that just because the cell voltage is going down, the temperature would be going down. Then it would start going up, even though the voltage was still going down. Why is the temperature going up when the input power is going down? You would see that right on the chart paper.

The voltage keeps going down, but the temperature should track the voltage, but it goes up. I got that paper in there, and that's why I think they liked it because you can very clearly see the cell voltage and the temperature. The temperature's not behaving like it should.

When I left, I asked if I could keep the manual that Martin Fleischmann made for them, and they told me yes. I asked if I could take the Fleischmann and Pons cells with me, and they said, no. They thought about it though. I had asked Dr. Asami, and he said, "No, I better keep them here." But about the manual, he said, "Yes, go ahead." They didn't use it anyway. Why not?

62 RETURNING TO CHINA LAKE APR 1998

I enjoyed it, the NHE work. That was one of the high points in my scientific career. And when it was time to go back, I just tried to get along in the Navy doing whatever I could.

It was early April 1998 and I didn't have any cold fusion funding, so I couldn't do any cold fusion research there. I paid my own way and went to the ICCF7 meeting in Vancouver. I just used annual leave and my own money to go to any conference after that. I had my old cells, but I didn't do anything with them. They were just sitting on a shelf.

I continued work on different things at China Lake, mainly corrosion research and research on materials for supercapacitors. Corrosion is a big issue with the Navy. The corrosion of ships and so on. They had a polymer group developing polymers to protect against corrosion and we were investigating polymer coatings that would retard corrosion.

I used a special electrochemical technique that was really gaining favor at that time, and I already had it in my laboratory. It was called EIS, or electrochemical impedance spectroscopy and it was sort of just catching on. In a quick experiment, you can get results that are quite predictive of how well something will stand up to corrosion over years of time. It was a new way of doing corrosion research, but I had already been using this for other electrochemistry, so I knew how to use it. It got even more popular later, and a lot of work was done, but this was very early work in that area.

Supercapacitors was another area of research that they were interested in in the Navy, and it's still an active area now. That's where you store a lot of charge, store energy, electric energy, and then



release it later. Like in automobiles, you can store the energy in a super capacitor, and then, when you need to go uphill, you don't need to just use the battery, super capacitors can contribute to the extra energy needed.

For example, if you run Ruthenium as an anode in water, it will form an RuO_2 , a ruthenium oxide coating. Ruthenium oxide stores charge very well apparently, so it's a good supercapacitor material. Just by doing cyclic voltammetry, which is one of my main electrochemical techniques, you can judge how effective different things are for storing charge, by just seeing the capacitances present.

During this time, I never touched anything related to cold fusion. In fact, I wasn't allowed to work on cold fusion. I just did whatever I could, you know, to have funding.

I didn't get any more funding from Bob Nowak after the Navy cold fusion program ended. Bob Nowak went to DARPA and from there, he funded Mike McKubre for cold fusion research for quite a few years. I talked to Richard Carlin, who took over for Bob Nowak at ONR, and he said he was going to fund me. Then, they found out I went to Japan to work on cold fusion, and suddenly, he refused to fund me. When I came back from Japan, I met him, and was supposed to have a meeting with him after lunch and he said, "Meet me at a certain place at 1 o'clock." I went there at 1 o'clock and waited. Finally, he showed up about 20 minutes late and, he says, really abruptly, he "didn't want to hear a word about cold fusion".

So, I never really got any more funding from Richard Carlin or China Lake. But, he would fund people at China Lake on electrochemistry. Now I was really the only electrochemist there but, he would fund other people in an electrochemical area then, and I would have to work for them on the electrochemistry part. That's how my career went in the Navy.

My main source of funding has always been internal funding. It's called IR, internal research, and also, funding came from the Office of Naval Research. I heard that Bob Nowak thought that since I didn't find an answer to cold fusion, that therefore I didn't do my job, and he didn't want to fund me anymore. Of course, nobody today has even found a 100% reproducible experiment. What he wanted me to do was still not even accomplished yet today by anybody.

New, younger people took over China Lake. They liked to push the older people out. That's typical of what happens when you become older and the supervisors become younger. And so, I wasn't given any of the internal funding that I had for years and years there, so those two sources were gone. I spent a lot of time trying to get new funding. But it was very difficult to get. In thermal battery research, I tried to bring back some of that funding and also with electrochromic materials, corrosion research, and supercapacitor research. Those were the main areas I worked in during that period.

I would still go to cold fusion meetings, paying my own way. At the time, Scott Chubb was sponsoring a lot of meetings associated with the American Physical Society that we'd have a session at. I remember going to them in various places like Minnesota, and there was one in New Orleans. That was the first time I met Thomas Grimshaw, in New Orleans. I was funded for that meeting because of the electrochemical work I was doing.

I did write a paper on my Japan work, but I just did that on my own time. I have a cabin in Oregon, and Linda was busy working. I just took off after Christmas and spent a week or two at the cabin in



Oregon, and wrote most of that paper up there. And I had to go through all the data and analyze it. A lot of time spent analyzing the data and then you have to do that, and then you write it up. It was published in the Journal of Electroanalytical Chemistry in the year 2000.

63 MARTIN FLEISCHMANN ON COLD FUSION WEAPONIZATION 2000-2001

Martin Fleischmann talked to me several times about the dangers of a cold fusion weapon. He was very concerned that cold fusion could be weaponized.

There's a video recording that Linda made in Italy with Martin Fleischmann. His words were that this should've never seen the light of day. Those were his words. It should've been confined to a government laboratory until we knew more about it. He thought it could be weaponized and he wanted me to record what he said about it and give it to Navy Intelligence, which I did. But what they did with it, I have no idea.

I was invited to give a presentation at a meeting at Navy Intelligence in Washington, D.C. They invited some people from the CIA to come, too. I kind of went over what Martin said about the possible use of cold fusion as a weapon. You would load the palladium with deuterium to produce the weapon, then, subject to stress like a high temperature or explosive stress or something, it would explode. At the least, I gave them what he wanted me to give them.

Afterwards, a CIA person came up to me and said, "This is off the record." I think that means I can tell somebody, I guess, I don't know. But he said, when cold fusion came out, the Russians were already experimenting with deuterium in metals and subjecting it to an explosion, and they were getting a lot more energy back than they expected. At that time, they didn't know about cold fusion, and they said, hey, this might be how that works. I don't know any more about that, other than that.

People at China Lake came in and said you need to get a big bunch of material and load it up deuterium, and then give a big current pulse through it and see what happens. I never did that but that was his idea. They had a lot of explosive experts there at China Lake.

In an interview, Malcolm Fowler talks about somebody at Los Alamos during the hey-day in the weeks and months after the 1989 announcement, just about everybody at Los Alamos was after this phenomenon, he said somebody was using C-4 on it, doing explosive compression. So, somebody did look at that.

Martin Fleischmann urged me to try and get the Navy to look at cold fusion and weapon research and he even gave me some ideas to get funding for this at China Lake. In fact, during my last year or two at China Lake, I wrote up an IR proposal. That means Internal Research, which they later changed it to ILIR, Internal Laboratory Internal Research, because we joined another lab in California.

We put in an internal research proposal for the study of cold fusion weapons in 1999 or 2000. With the help of Martin, I wrote up the things we wanted to try, and Martin even wrote comments on the proposal I had. We set up some proposals on how things should be tried, and how to stress these materials. The people I was working with, they had ways of stressing the materials by mechanical means.



We had to find a remote location to even do that kind of testing. If you do calculations, you get one cubic-centimeter of palladium loaded one-to-one with deuterium, and you got all of the energy out suddenly at once, it would be equivalent to 31 tons of TNT. A lot of people don't recognize that fact. That's what got me thinking about the weaponization of cold fusion. When I told the people that we had to find a safe place to do the testing, because even a small amount, if we suddenly released all the energy, that would wipe out the laboratory. It could take out a big part of a city!

Well we got funding for it, and we were about ready to do testing for it in remote areas at China Lake when suddenly, the funding was pulled away. We were right on the way to getting the test ordered when the funding was withdrawn. That had never happened to me before. I thought it was funny, I mean strange. They gave out the internal funding and then they took it away. I never got an explanation for it.

Maybe the military already knew about a cold fusion weapon and they tried to downplay cold fusion because of it; they already knew about it. They didn't want anyone getting interested in doing it. Maybe, it may have been a black program. Nobody was supposed to know it except those working on it.

I had to wonder, because if you're doing research and interfering with some black program, that's when they'll pull the funding from you. A Navy scientist told me that had happened to him. If it's a black program, and they're already doing work in that area, they'll tell you to stop, or they'll stop the funding. That could have been a reason our project was pulled, I don't know, but I was quite disappointed when they did that.

I sometimes wonder what if Mike Melich knew about it because he was quite evasive about exactly what he was doing with the Navy. I never got a straight answer from him exactly what he did. One time he told me, his job was to "know as many people as possible and bring the right people together". He had a home in Florida, often worked in Washington D.C. and, sometimes in Monterey, California. It was an unusual situation. I knew he went to every cold fusion meeting and conference, until he wasn't able to go, but he was always attending the meetings.

Even Edward Teller's protégé Lowell Woods was at a lot of cold fusion meetings, sort of under the radar. I met him once at a cold fusion meeting and he had been working with Edward Teller for a long time. Edward Teller's protégé, is what Martin Fleischmann used to call him. That was in the summer of 2001. I did work on that aspect of cold fusion right towards the end. That's when, within a month or two, I was suddenly out of funding again.

Within a month after that, I got a call from the head of chemistry at Middle Tennessee State University that their teacher of physical chemistry had suddenly taken another job and left. He wanted to know if I'd come out for a one year appointment. I of course said yes.

And so, I got another leave of absence from China Lake. And if you don't have funding, they're glad to give you a leave of absence. And Linda and I moved to Tennessee. We didn't really move anything. We put what we could in the car and rented an apartment and then bought everything we actually needed when we got there.



64 MIDDLE TENNESSEE STATE UNIVERSITY AUG 2001 – 2003

Back before I worked at China Lake, I worked at Middle Tennessee State from 1969 to 1978, and they had all my information from then. When this one teacher suddenly left, they called me and I went back there and taught for one year.

When I got a chance to go back to Tennessee to teach for one year, I was quite happy to leave China Lake. I took a leave of absence for one year, but I came back and left for good in August of 2002.

The University of Tennessee in Knoxville, Tennessee, is the largest University in Tennessee. Middle Tennessee State University is named because the campus is fully in the geographical middle of Tennessee from North to South and East to West, the middle point of which is on the campus of MTSU.

Also, back in the 60's, Middle Tennessee State University had to open up for blacks to attend. It couldn't be all white. MTSU had to let blacks in during the 1960's. Middle Tennessee State University had 10% black students and about 90% white students. I don't know how it is today. But, that is how it was then.

And, for the first time they let blacks play on the basketball team when I was there. Of course, they excelled at the sport. They were the best at it. I don't mind blacks at all. A lot of them are quite friendly when they talk. But, when I think back, I never really had a really good black student. The best they ever did, maybe they got a "B". I don't think they grow up with proper training on how to study and things like that. It holds them back somewhat. But, they were nice kids. I enjoyed teaching them.

I had just then started teaching at MTSU in September when 9/11 2001 happened. And for a day or two all the students wanted to do was talk about the attack on New York, so I remember that quite vividly. That was the day I was, in fact, I watched it on TV as I was getting ready to go to work. I heard that one plane had crashed into the tower, and I turned on the TV and before long, the second plane on TV crashed in. That affected the students while I was teaching. In fact, we couldn't hardly teach. We just had to discuss what happened in New York, and why, for a day or so.

Teaching loads are pretty heavy, but I did do a little bit of research in cold fusion at the University. I was teaching a physical chemistry lab. And, Martin Fleischmann once remarked to me; the boiling point of D₂O containing LiOD changes as the D₂O boils away. He had, sometime before that, asked me if I could measure how the boiling point of D₂O increases as the concentration of LiOD increases. Because when you boil off the D₂O, the LiOD concentration is increasing and the boiling point changes. He did that in his analysis of the boiling point of the solution itself, as it gets more and more concentrated. I thought that was an interesting thing. I even thought about that before I left to go to MTSU, so I took the equipment with me to do that experiment, you know, the thermistors and so on. Also, the D₂O to do it with, I had from a physical chemistry laboratory experiment. We took about two weeks working on it. We determined how the concentration of LiOD affects the boiling point of the electrolyte solution.

The LiOD doesn't boil away so the LiOD gets more concentrated, so the boiling point goes up. That change is needed in some of Martin's calculations.



Towards the end of the semester, I had the students do the experiment of seeing how the boiling point is affected by the LiOD concentration. We got a paper prepared from that. I wrote the paper but put all the student's names on it who were in the class. I presented this study at ICCF 9 in China, and all the students in my physical chemistry class at Middle Tennessee State were included as authors. This is in that proceedings. I put all the students' names on the paper.²²

But, it shows how the boiling point goes up with the concentration. There is a theoretical way it goes up but, the actual rate it goes up is actually a little less than that and we compared the theoretical and experimental rates. That was one of the publications I have on cold fusion with ICCF9, with the students' help.

I taught one year there and I went back to China Lake and actually retired after working a couple of weeks during Christmas break. Retired at the end of January in 2002. I never gave much else to China Lake after that.

Then they hired me to work during the summer break in 2002, so I went there for about eight weeks in the summer and did some research that actually wound up in an interesting paper. A paper that was on the use of hydroxide as a molten salt, LiOH and KOH and molten salt electrochemistry. I thought it was very interesting.

Then, I applied for the Faculty job there but, the person in charge, he had a friend he wanted to bring in and I finished second. So, I got good recommendations from the department head. He sat in on one of my classes. He really liked it. But, I taught him physical chemistry. And, he said he would be glad to write a recommendation. He gave me a very good recommendation and so, there was an opening at Bates College in Maine. And, with his recommendation, I got interviewed and got a one-year position in Maine. Soon as I got back to Bates in Maine, I wrote that up as that LiOH and KOH and molten salt electrochemistry paper and it was published.

65 BATES COLLEGE MAINE 2002 - 2003

At Bates College, I was in the city of Lewiston, Maine. There's a river that goes through the town, and one side is called Lewiston, and the other side is called Auburn. Auburn, Maine and Lewiston, Maine, are really just one city, but they are separated by a river and have different governments.

It was a private college. As I recall, it was almost all white students there, but they were great as students. I never thought about it. They are quite selective on who they let in. They take the top students to go there.

It was a nice one year there. The winter was quite cold. I remember walking to school and it was - 20°F. You have to keep your ears protected and things like that, quite different from the Southwest where I grew up. Here, you worry about getting stranded in the heat of the summer. There, you get worried about getting stranded in the cold of the winter. That could be dangerous.

²² Miles, M., et al. *The Elevation of Boiling Points in H₂O and D₂O Electrolytes*. in *The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science*. 2002. Beijing, China: Tsinghua University.: Tsinghua Univ. Press. <https://lenr-canr.org/acrobat/MilesMtheelevati.pdf>



Unless you get into a major university, it's always a heavy teaching schedule. You teach three classes and that takes up most of your time. Well, I taught a class on Electrochemistry, and I also did an Electrochemistry Intersession class, which is a short class. We did an Introduction to Electrochemistry and laboratory things, and we went into cold fusion. I think students were quite understanding and enjoyed hearing about it. Someone thought that I would become famous someday.

I also gave a seminar on cold fusion there, both at MTSU and at Bates College, in Maine. They were both pretty well received, except there was one person at Middle Tennessee State University, I can't remember his name, but he had been at Texas Tech University when Bockris was having all that trouble, and he was very much against cold fusion. Even though he didn't bring it up during the presentation, I heard a lot of talk that he was attacking my work, and going around telling other people it was all wrong.

But Lewiston, I never had any feedback like that. In fact, I was asked to teach an intersession class related to cold fusion, which I did. And we also brought in electrochemistry and did some laboratory work on electrochemistry. I actually developed experiments for the college there for electrochemical experiments. And those were pretty good experiments. I later heard that one student got a job in some battery company there because he knew about electrochemistry.

I was really wanting to see what Maine was like and I spent one year there. But it is a cold winter, and my wife didn't like it because of the snow and ice. She had spent her whole life in California and it was quite a different climate. That summer, I started looking for other jobs.

66 UNIVERSITY OF LAVERNE 2003 - 2006

I talked with Dr. Iraj Parchamazad. I'd met him previously when he came to China Lake trying to get people to collaborate with him on a Fuel Cell program. There was an opening at University of Laverne where he was Chairman of Chemistry. I wound up going to the L.A. area that next year for a one-year teaching appointment. They kept renewing my appointment, so I ended up being there for three years in the California city called Laverne.

The smog was bad at times, but the Santa Ana winds don't hit Laverne very hard; they come down more through further south in the valley.

I have been out running in the mountain areas and when you see a dust storm, it blinds you with dust all the way to the L.A. basin. But up in the Laverne area, there is no dust. It protected them.

There are so many cities in that area. Pomona is to the south, Claremont to the east, and mountains are to the north. Some cities keep graffiti out, but in Pomona, it's all over. In Laverne, you didn't see any graffiti. The different police departments kept that kind of thing out of the city there. But, you see just across the border, Pomona you would run into it.

Dr. Iraj Parchamazad fled Iran when the religious Mullahs took over. He and his wife said they were going to a meeting in France, and then they just stayed and never went back, eventually winding-up in Laverne, California.



He was at the cold fusion conference in Italy. But I got him interested in cold fusion again. He always thought there was probably something to it. But some people aren't just skeptics. See, he thought it really sounded reasonable, and I talked to him about it.

He was an expert in zeolites, so he set up a zeolite experiment where you would deposit palladium inside zeolite cavities. He wanted to try to put nano-sized palladium inside the zeolites, not the normal way most people do it, but by making, or buying, an organic metallic compound with palladium that would dissolve in the organic solvent where it would flow into the zeolite cavities.

You can get zeolite pores of different diameter cages around ten angstroms wide, and you can tailor them to only fit certain molecules if you want. Like most organic chemists, I already knew about zeolites because organic chemists at China Lake used them to remove water from organic solvents. You would put zeolite in an organic solvent and any water would get into the pores and get stuck there. And then you'd have the rest of the solvent free of H₂O. And so that was already known to me as a way to get rid of water in an organic solvent area, substance.

He would take the zeolite and heat it up, drying the solvent out, and the palladium would then become palladium metal inside the zeolite.

It was done in a way that he put the zeolite in organic solvent and he used an organometallic palladium compound, and it was dissolved in the solvent. Anywhere there are cavities, the solvent would flow in and so would the palladium complex. All the pores got filled with the electrolyte and palladium complex. Then, he would evaporate off the solvent and then, heat it up. That would decompose everything inside the cavity except the palladium metal.

Then you would feed D₂ gas into it and measure the temperature. Well it got a spike upwards in temperature with the D₂ gas. When he did it with H₂, it just stayed at normal temperature. That's what got him into the zeolite as a possible way of doing cold fusion experiments.

For a small amount of palladium, there was a big thermal response. The temperature rise we saw was only a few degrees, but the amount of palladium he used was so small, it might have been kilowatts of power generated, relatively speaking. That was my estimate anyway.

Iraj was always quite reluctant to ever write anything out. He doesn't like to publish things unless it is protected by a patent. He actually went to ICCF 14 in Washington, D.C. and gave a presentation on the zeolite work. The only thing he reported was the abstract. You can find his graphs on the internet. I think Steve Krivit has them, or used to have them, on his site.

I remember that meeting, somebody started talking about transmutation, and Iraj got really excited. He sat next to me and he said, "You know, I wonder how I got these other metals mixed into this material."

When he examined the zeolite material, he was finding things like nickel, zinc, and other elements showing up. And he said, "How did they get there?" And that was the only explanation that he had. It must have been a transmutation. Because he had instruments that did careful analysis of what was actually in the zeolite, and he had these other metals, transition metals, showing up in the analysis that were never in any starting material. You shouldn't get any other metals into the palladium,



He thinks he even has a theory to explain the effect of electric fields inside a zeolite cavity where to see inflated metal, because he is an expert on mass spectrometry.

Iraj also said that unpaired electrons are important. "If you have unpaired electrons, you can have effects." Unpaired electrons affect the nucleus and then, affect the spectra of nuclei. He called it the Dynamic Nuclear Polarization Effect. You can look it up, but unpaired electrons affect the nuclear magnetic resonance spectra. So, electrons do affect the nucleus, and, somehow that's related.

I wish Iraj had published more. He did a lot of work that's probably never going to be published but he's about ready to retire now. I think he even has work on nickel in zeolite, and he has some interesting results he's told me about, but he hasn't ever published them.

I still correspond with him and talk with him on the telephone. I am trying to get him to send me something, so I can at least start writing it up. Maybe he will, but his eyesight isn't that good. He had to more or less give up his teaching position because he couldn't grade papers anymore.

67 2nd DOE Review 2004

The DOE re-did the ERAB Report in 2004 with Peter Hagelstein and David J. Nagel, and others. They didn't consult with me on that.

I thought helium-4 would have been a big plus to try and present but, I don't think they were convinced enough of it, and I don't think it was really presented as a major argument. I don't agree with Abd Lomax on a lot of things, but at least he did recognize the importance of helium-4 and he has written a paper on it.

68 CHINA LAKE CONTRACTOR 2006 - 2011

After 3 years in Laverne, Linda and I were getting tired of living in an apartment. We owned a house in Ridgecrest so we moved back to the house we lived in when I was at China Lake.

After we moved back, I was contacted by a fellow saying that the Navy wanted to get a program going on thermal batteries. The lab had received an Army contract to do thermal battery research and he wanted to know if I would come up to China Lake and work as a contractor. He wasn't in my chemistry area, but he was into thermal batteries. Of course, I could have done this very work when I was put on stockroom duty a decade previous, nevertheless, I said I was interested. In 2006, I found myself working on thermal batteries for the Navy as a contractor.

This same fellow got me involved with a group called Millennium Engineering and Integration MEI. A person that I'd known from China Lake was a member of that group and he'd been able to get another job in Washington, D.C. He heard me give a seminar on thermal batteries at one time, and thought it was very interesting. He got MEI to get into the thermal battery area, and they finally wound up being the source of funding for the contractor work. I was actually an employee of MEI, but worked through a contractor at China Lake in this laboratory. He tried to find a place at China Lake where I could work.

My main nemesis in chemistry, Robin Nissan, would not allow me to come back and work in chemistry. I don't know why, but he wouldn't allow it. The same department chairman was there and



didn't like my work on cold fusion. Even to come back to thermal battery work, he didn't want me in chemistry. The person that was in charge of this company MEI Millennium Engineering & Integration, had quite a bit of political power. They didn't try to force that issue. Still, they got me a lab in another area which was actually better.

A Navy friend had added another, separate, laboratory, kind of away from everything, and said "There's a room over there" where I could set up a laboratory. That room was made into a laboratory, and I was the only one using it for the next couple of years.

It was a brand new laboratory, and very nice. MEI bought all the equipment I needed. They bought me an electrochemical instrument like I had at China Lake, an even newer version that could do any type of electrochemistry I wanted, and vacuum ovens, everything you would need in the laboratory.

I thought there'd be other people working there, but I wound up being the only one that used it for the first couple of years. There was a small room I had to myself for an office. When the project got the funding from MEI, I had a little bit of help when one of the students from Laverne came up and we worked for the summer on the program.

That lasted about three years and then we did what they called SBIR Small Business Innovative Research. They went to the Army and got an SBIR contract that funded us for about another two-year period. It was Army funding that came through the Navy.

I went to this company first, and then came to the Navy to work as a contractor. Then that program ended after about two, three years because another person that was also hired by MEI had taken another job. He was a battery engineer, and he was the one that really knew how to build batteries, and so they didn't have the expertise anymore.

They were going to take my research and start building batteries. But they had trouble doing it, so the program ended. That was work that was done more back in the Washington DC area. But the two people had never worked on batteries before and they ran into a lot of trouble trying to get everything to work.

We finished the SBIR. I even went back and gave a presentation to the Army. I think they were quite happy with it. It was hard to get a battery company to pick it up. You needed an actual battery company to go forward with it, which they didn't get that quite accomplished. I think it would still work. You just need people, expertise in building batteries to follow-up on the research.

69 CO-DEPOSITION EXPERIMENTS

I spent 3 or 4 years back working at China Lake working on different things.

At one point, I even got a grant from the Defense Threat Reduction Agency DTRA to work on cold fusion for one year. LENR is what they called it, I think. So, even though I was an unfunded Navy worker, they hired me back as a contractor to do things that I could have done back then.

By then, Mike Melich had come by, and he had a contact at a Navy program, probably the one where he took funding from Frank Gordon. I don't know. And basically, they did produce excess heat.



However, the co-deposition only worked with palladium, and I think this is where Mike Melich came in.

He got funding from a Navy person and he wanted to reproduce the co-deposition work I did in Japan. In Japan, co-deposition did produce excess heat in a Fleischmann Pons cell. It was a bigger cell, where you get a larger deposit of palladium. All three experiments gave excess heat, and I revised the electrolytes somewhat, so it wasn't just lithium chloride, it was an ammonia solution. Ammonium chloride, ammonium hydroxide, that was deuterated. I got some large effects which have been published in several places.

When Stan Szpak found out about it, right away he wanted to publish it, and he even published Fleischmann's analysis. I think I told you before, both Fleischmann and Stan Szpak wanted to write this paper. There were two papers that came out in journal publications, Journal of Fusion Technology and Electroanalytica Acta. They both reported on this co-deposition I did at NHE in Japan.

But anyway, Mike Melich wanted to reproduce that experiment and also have it reproduced at NRL. This work was funded by the Navy office that was called the Defense Threat Reduction Agency, DTRA, and I think somebody by the last name of Wilson was in charge of that. I was asked to write a proposal. It was kind of nice. Your sponsor asked you to write the proposal and of course, you get funded that way. That doesn't happen very often, but it happened then.

At one point, I was supposed to be doing both, with funding for both battery research and cold fusion research, and that was kind of hard to do both. I know there's a period that I was actually trying to cover both battery and cold fusion research.

I worked at China Lake as a contractor and as I recall it was mostly codeposition I was doing. Oh, I developed the new copper calorimeter that I'd use. I adopted the new calorimeter that I have right now. I tested the new calorimeter. It was used for my experiments at that time. But I don't know whether I got any large effect, but I saw a lot of small effects, and I found that the co-deposition of other transition metals did not work very well.

One thing I did right at the end that I'd never had a chance to go back and repeat, was something that Peter Hagelstein came up with the idea that co-deposition, if you do it with a higher current, you create a lot more vacancies. He thought that doing that might give bigger effects. I just started thinking about that, and I think that was true. I even did one study with nickel, doing a fast deposit and I only did one experiment but it seemed like he was right, I did get some excess heat.

I only got to do one experiment on that, but it looked like maybe Peter was right. If you deposit a higher current, you get more defects, you get a bigger result, bigger excess heat effect, but I never got to follow-up on that.

I was working on co-deposition, except I was looking at a lot of other metals other than palladium, like the deposition of nickel and ruthenium, rhenium and different transition metals, to see if you could create any effect. And, I could only get excess heat with the palladium.

Peter said Stan Szpak and Pam Boss had gotten away from that, and gone to lower currents, and Hagelstein said, try deposition of the higher current. It would give you more vacancies when you



deposit palladium at a higher current. Peter had a theory about Stan Szpak and Pam Boss' work, early on, but co-deposition at a larger current, where you deposit using a large current, turns out that the deposit is not as good.

Near the end of that research at China Lake, I deposited nickel at a high current in deuterium D2O and I saw an excess heat effect with the nickel. But, I was never able to follow up on it. That was the very end, and I didn't have time to reproduce that experiment. The nickel deposited at a high current and it looks like it did give some excess heat but it's never been published.

You don't get a good deposit. You get more dendrite formations but, by doing it at a high current, you create a lot of vacancies in the nickel, but the problem is getting the deuterium to soak in. The deposited material may fall off, but you produce a lot more defects that way.

This work was "on the side", yes, because I had everything there and so I tried to do more co-deposition studies, also to study other depositions of other metals. I think during that period is when I did co-deposition of other transition metals like ruthenium and other metals, including nickel. I might have written a paper on that. I'd have to look for it. But co-deposition usually did not produce excess heat with most other metals I looked at.

This all led to several publications, at least in Journal of Condensed Matter Nuclear Science.

I had my own little lab set up in China Lake, and I probably worked there from 2006 to about 2011, maybe five or six years there. Then the DTRA funding ended, and the MEI funding ended, and suddenly, I didn't have any funding at all. It was lucky the one in charge there, Don Thompson, was a supporter of cold fusion, so he allowed me to keep working, even though I wasn't funded, which is kind of breaking the rules. Without funding, you're not supposed to be working. But I probably continued to work on through about 2011 or so.

They just kind of let me stay there. You know, you're not allowed, if you don't have funding. I worked another 2 years on cold fusion without funding until a new department head came in and he said it was not legal to be here without funding. When he came in, that's when I left.

Nothing much happened after that regarding cold fusion until I decided to set up experiments in my Ridgecrest home.

70 KITCHEN COLD FUSION 2017

The Navy has a fence around the area called China Lake. Next door is Ridgecrest, where I lived. They're side by side. We had a home over in Bakersfield across the mountains. and Linda was there most of the time so I had the kitchen to myself when I was over there in Ridgecrest. I was still getting funding coming in from the private sponsor and so I wanted to do something. I had to move everything out of the lab at China Lake, so I set up a lab in the Ridgecrest kitchen, and ran another Johnson Matthey wire that I had. I still have some of that wire that gave the second set of excess heat results.

My first study in Ridgecrest was the Johnson Matthey palladium wire, and this produced excess heat again, and this was reported. I forget what I had accomplished before that. But I was still doing codeposition, and I'd like to have something new to report.



Besides the equipment the sponsors had bought, which I had to leave there, I had my own equipment, which was actually better in a lot of ways. It was analog and so you turn it on and you can run day after day. The newer, computer-controlled instrument, you have to program it, and they don't allow you to go for long periods of time, because it then shuts itself down.

The analog equipment, which I still have, turns out better for these types of work. You can set it up, and it continues and continues day after day. You don't have to keep adjusting using the computer.

I did several experiments in the Ridgecrest kitchen. Linda was in Bakersfield and I had the kitchen to run experiments in. I usually only ran it for about 3 weeks but you see that paper I gave you, That's part of what I presented in Colorado.²³ The Johnson- Matthey wire was probably the 2016 experiment. I think I did one in 2016 and another in 2017. I haven't done any since I got here.

In 2017, there was interest in palladium-boron again. Then at ICCF-20 in China, Dave Nagel and Steve Katinsky talked with me. They wanted to know what I would recommend for research in the cold fusion area. They were thinking of co-deposition, but I told them, "Well, co-deposition had a lot of problems." One thing is, it generates chlorine. You need to have a laboratory to do it, with a fume hood. And I said, really to me, the biggest advantage of cold fusion not followed-up on, is palladium-boron and I said, "This is a Navy material that gives excess heat nearly every time." That's where we should focus."

They agreed with that, and that's what Dave Nagel and Steve Katinsky are doing right now. They're trying to have a material made by another company, with Dr. Imam's help, and then, they'll be able to develop a new calorimetry and they'll test it at George Washington University. I'll help them as much as I can and especially by email, which I spend a lot of time even now with emails back and forth.

Initially, I told them I didn't even know where the palladium boron electrodes were, if I even had any. I just didn't know where the palladium-boron electrode I used in Japan was. I couldn't find it.

Steve Katinsky lived in the LA area, so he came up and we spent a day going through my garage looking for the materials that I had. Steve found the palladium-boron electrode. It was in a little glass vial sitting on a shelf in my garage, the same electrode that I used in Japan. It was the same dimensions, the same size and I had it marked. There it was, sitting in the garage in a little vial, all by itself. It was labeled palladium Pd-0.5B. And then NHE was written on it, too, I think, and it was the palladium rod I ran in Japan.

That was around 2016. Steve Katinsky when found it. I think I ran it in 2017 and I reported that in 2018 at ICCF20 in Japan and China, both.

Then last year, before I moved, I ran a palladium-boron in my kitchen again. Just like before, it gave us an early excess heat effect, which I'm quite sure is correct. It still gave excess heat, but not as big as it did in Japan, the very same palladium rod.

²³ Miles, M. Excess power measurements for palladium-boron cathodes (PowerPoint slides). in The 21st International Conference for Condensed Matter Nuclear Science ICCF-21. 2018. Fort Collins, CO. <https://lenr-canr.org/acrobat/BiberianJPjcondensedzb.pdf#page=21>



We had a problem. One problem with it is, it started having more bubbles than normal. This had a lot of bubbles forming on the cathode. They block the surface, and when the bubbles start blocking the surface, the voltage goes up, and when they disengage the voltage goes down so it's hard to get a good voltage reading. That causes an error. So that was probably what I'll try to do next, is repeat that.

I had trouble with bubbles forming at the bottom of the electrode. We had this electrode positioned on a Teflon support, and somehow bubbles would form. This is probably due to the D2O used. Different bottles of D2O will cause different bubble problems. Some bottles of D2O will give a lot more bubbles than others. But, if you have bubbles that collect on the electrode or other surfaces such as Teflon, it blocks the surface so the voltage goes up and down a lot more. It's harder to make your voltage steady, because you see it go up and then bubbles are disengaged and it'll go down. You try to get an average voltage, but that is difficult.

Also, usually, you can't see inside the cells so, you have to sort of estimate where the level is or else you have to get a syringe system and pull up on the syringe. The liquid amount in the syringe helps to determine the liquid level. And, normally in an experiment, you can calculate how much you lose by electrolysis. But, some of it evaporates and that is hard to figure. Typically, you add 5% more for evaporation and this is usually pretty close. But the gas coming out still has vapor in it, because it is a higher temperature. And when it goes straight up the little glass tube, the room temperature is where it starts to condense, and some goes back down. Not all of it that actually evaporated down below, actually winds up going out.

In most other experiments where the top of the glass tube was at room temperature, the D2O vapor goes up with the D2O and O2 gas. Some of that will condense back, and you don't lose as much, but when you insulate the cell top, you can lose even more of the D2O.

This experiment I did at home, when I kept the cell top warmer by insulation, there was more D2O escaping.

You know, I did a lot of work and tried to make a correction for the electrolyte level, but thought it will probably come out the same. So, I just left it the same.

This refluxing goes on, all the way to that glass tip. If that glass tube is warmer, you get more D2O vapor getting out the top. I just found that out in the last experiment. It makes a difference if the top of the glass tube is at room temperature or if it is closer to the higher cell temperature.

I was at University of Laverne when I started thinking about making a new cell and I kind of diagrammed how to do it and I did a lot of calculations to get a cell constant that you'd like to get. How much insulation do you use? A lot of thought goes into how to make a cell, a good cell, and then I thought how to do it using copper tubes.

It was sort of a scale up of what the postdoc had done earlier on. He used a small cell. I just came up with a bigger one, basically. I worked with him on his cell and it's the same time the inner copper and outer copper and they don't contact each other. They're separated by installation all around and at the bottom. They're totally out of contact. Now heat gets transferred, but copper's a very good conductor.



Then all you need is a heat transfer fluid between your glass cell and the inner copper wall. Then that gets transferred to the installation to the bath.

That was Kendall Johnson. That was at the very end. He worked at China Lake in 1994 and 1995 and he spent almost the whole year building the cell. And that cell stayed there. I should've taken it with me when I left, I guess, but I never did. It was only used in a couple of experiments. He measured excess heat in one. He saw exactly where it turned on and then it turned off. It's in my final report at China Lake.

It shows that when he increased the current, then the excess heat kicked in and when he changed something else, it fell right back down again. We could have done a lot of interesting experiments with that sensitive cell, about temperature effects and why the excess heat changes and a lot of things related to this, but this was 1995, and the Navy program ended. I later just made it a bigger version of this copper cell, and it works quite well.

The only problem is you can't see into the cell. That's true of almost every cell, except Fleischmann-Pons cell, where you could actually see in and watch the electrolysis, but the electrolyte level is still hidden up in the silvered region. You can compensate theoretically, but if that level gets off, that's the main reason the calorimetry gets off. This electrolyte level could still get out of control and you might be incorrect from what you think it is. Again at NHE, I used the syringe method to avoid such problems. The major problem with the electrolyte level is that it can change the cell constant.

Dave Nagel tried to get around by making a cell out of clear plastic that you can see through, but plastic is a much better heat conductor than an insulator. And the way he built it, he comes out with a cell constant about twice as large as mine. Instead of 0.140 W/K, his cell constant is more like 0.28 W/K. I knew that when he started. I said, "You're going to have to have that thickness pretty large to get down to where my constant is." But he didn't want to make it that big. You need quite a thick piece of plastic to block the heat as much as the insulation in my copper cell.

Insulation blocks it pretty readily.

It was 2017 and we were getting ready to move to Utah.

71 PUBLISHING IN JOURNALS

For every experiment I've ever run, even before cold fusion, I have a notebook record. You can do an inventory and it'd be a lot of copying to scan all those pages. The first thing is to get an inventory, and then maybe later we can do the scanning.

Then I have my own notebook data, and even the NHE computer data is somewhere. I don't know where it is right now, but it was sent out to several people. Some of them are not living so I don't know what happened to this NHE data.

I have a lot of notebooks of calculations on cold fusion, but they're not really for publication. They're just how to calculate certain things. I have a lot of things like that. I keep going back and using old data for doing new calculations, for example. And those are not published.



There are some experiments more important than others. Like, the experiment I did in Japan on palladium-boron is very important because it was documented by Fleischmann in his analysis in the NRL Report.

There's a paper by Martin Fleischmann, and he wanted me to help him on it, and try to get it published. I went to the Journal of Physical Chemistry with that paper and they sent it out for review. Two-out-of-three reviews were good, but still the editor squashed it. The Editor didn't want to publish it, so that remains unpublished. I've got documents on that including the Editor's rejection letter.

I didn't even try until the last year or so to publish in major journals. I'm trying to do that right now, to see what their attitude is. I have now found that such efforts are a big waste of time.

72 STAN SZPAK AND PAM BOSS

CR-39 came later, with Pam Boss mainly, I think, to measure neutrons. I think that came after Stan Szpak retired.

One of the big things Stan and Pam worked on was they found tritium production. Also, they saw signs of x-rays and measured for acoustic effects, sound, and he found that he could get sound effects coming right off the electrode. They got a thermal-imaging counter, a camera, and they could see these bright spots indicating higher temperatures coming and going all over the surface of their palladium cathode.

These active sites would show up and then disappear. The image was always changing. They were always going on, and they never went away completely, they were just changing locations. When we had a meeting at San Diego, Martin Fleischmann thought these might be due to micro-fusion events going on, fusion events on a small scale.

I went down a few times and visited their lab. Szpak didn't travel much. I think he only came to China Lake once.

But the problem is to do things using the calorimetry. When I did co-deposition, I had to change things from what they were doing. I think they were using more like a beaker, and they weren't really doing calorimetry. They never got into calorimetry. They did measure temperature change. They used a larger cathode that I couldn't even fit in my cell and they used the back side of the square cell and they put a thermistor on the back side, and they could measure the fact that they'd record temperatures, I don't know whether it's the back side or front side, but it was right close to the cathode, and it was a large cathode. They would register a higher temperature at the cathode than what was measured in the solution. This indicates excess heat.

That showed that heat was coming from the cathode, that's the only way you could explain that. I would say they should have done a H₂O control or a platinum cathode control, but they didn't do the controls I thought were needed. However, they did report a higher cell temperature which was consistent with excess heat effect.

Anyway, we tried to do calorimetry, but we had to use a smaller cell for the codeposition. Our cell at that time only contained only 20 milliliters, so that's a kind of a small test tube. That made the



amount of platinum deposit much too small. Our cell at that time could not readily detect less than about 20 milliwatts. Assuming an effect of one-watt per cubic-centimeter, that is not sensitive enough for co-deposition.

We saw radiation in almost every experiment, which I think was real. The only time we didn't see radiation when running a co-deposition experiment, was when we did not flush the system out, or did not let the cell sit for a few days, because every experiment generates chlorine and several groups have found that chlorine poisons the effect. I didn't know that at the time, but I thought that might be true. When I wrote my final Navy report, I finally realized that this was probably true. Anytime we flushed the system out, we got the radiation effect, otherwise we didn't get any radiation effect if chlorine could linger in there.

Dave Nagel sent NRL people with much better equipment to measure the energy of the radiation. But every time they came, not knowing the chlorine effect at the time, we would rush experiments, and we skipped the flushing out procedure. We didn't know how important this was at the time. Twice NRL came out because we thought we could get radiation every time using co-deposition, but when they came, we didn't flush out the chlorine and we didn't ever see the radiation. My Geiger-Muller counter did not show any excess radiation and their equipment didn't show it either.

I realized there's a connection to the chlorine effect too late, when I wrote my final report. Pam Boss and Larry Forsley and I are working on a paper that is soon to be published. It's mostly Pam Boss' work, and she discusses other cases where chlorine seems to be a poison. Professor Xing Zhong Li's work in China, for one example that I remember, showed the same thing. The presence of chlorine in a system prevents any cold fusion effects.

73 BEGINNING AND END PRODUCTS ARE THE IMPORTANT THING

There are different opinions on how to write a nuclear reaction. When I write a reaction, I am not stating a mechanism. I am only saying what you start with and what you wind up with. Now, you may go through different stages to get there, but in thermodynamics, all you need is the initial stage and the final stage. When we write a chemical reaction, we are not saying we know the mechanism. We are just saying these reactants are what you start with and then these are the products you end up with. That is all. You are not saying how it gets there.

Whenever you bring out $D + D = \text{Helium-4} + 23.4 \text{ MeV}$. It really gets Takahashi and Abd arguing against that. How are you supposed to write it then?

If we are talking about Takahashi's reaction, it would be $4D$ to Beryllium-8 going into helium-4. He thinks that Be-8 is the intermediate, but I don't care. The ratio is still 2 Deuterium to 1 Helium or 4 Deuterium to 2 Helium.

You know in chemistry, we say that you never prove a mechanism. They are interesting to talk about, but you can't absolutely prove a mechanism because it goes through a state that is a transition state. It's not around very long unless you get better instruments to measure this. It happens in a very short time period. You don't really know what the real mechanism is. You can only theorize about it.



It's interesting, I have had this same sort of discussion from Tom Claytor regarding the Feynman Diagrams. You know the beginning, and you know the end, but you have no idea what the intermediates are on a nuclear basis. It goes to a virtual state, and then it goes to some final state.

You can't call it chemistry. What is that transition state? This is the Eyring Rate Theory. Eyring often used an example of a valley, like Salt Lake City. He was in Salt Lake and supposed he wanted to go to Provo. You would then go over the mountain and you would drop down to the lower valley in Provo. And, then the intermediate state would be the lowest energy state over the mountain that will take you to the valley.

You don't go up the highest way and back down. You look for the lowest elevation pass. You don't take the highest elevation pass. And, chemical reactions are the same way. They follow the pathway that gives the lowest energy.

Even a catalyst can be a new pathway. That's what they do. They give you a new pathway of lower energy. They might find a secret tunnel or something through the mountain pass. So, a catalyst is just a quicker way to get there. Cold fusion is all about some catalytic pathway.

It doesn't change the thermodynamics any, but it changes how you get there. The beginning and the end are still the same.

74 WHAT'S GOING ON WITH THE REACTION?

I think there has to be some special reasons for a lot of electrons to accumulate, and that's what is necessary for cold fusion to take place. With enough electrons, I think you can avoid the Coulomb barrier, and then, two deuterons can get together and fuse and form helium-4.

I'm not sure where this happens. I wouldn't rule out Ed Storm's cracks. I would maybe be more in favor of Peter Hagelstein's vacancies. I think the double-layer region is also a possibility. In fact, Heinz Gerischer, when he went to the meeting in Italy, ICCF 2, he wrote up something about it, and he thought that the electrochemical double-layers were very strong electric fields, and that might be where cold fusion takes place.

I just think it's an electron-rich area that is somehow created in a vacancy or in a crack or even in grain boundaries that create double layer regions where you get electron concentrations so high, and, of course, positive deuterons are attracted there and the positive charge on some deuterons probably gets neutralized by the electrons. There are so many electrons there that the Coulomb barrier doesn't exist. This is shown by Ed Storm's experiments, that the activation energy is very close to what you'd expect for the deuteron diffusion in palladium. The deuterons are diffusing into a special area where they react.

It's likely that in a lot of these little areas, the rate of fusion is determined by how fast deuterons can diffuse in, and maybe, how fast the helium can get out. The process could end if helium builds up too high. That might cause it to end in one spot, but of course the co-deposition results seem to show that these little hot-spots tend to move around a lot. I don't think it just stays in one spot. It varies, changes, and is undergoing changing all the time in the experiment.



Self-destruction of the NAE, in other words, the deuterons get together in an electron-rich area where the Coulomb barrier is negated or is neutralized, and then fusion takes place. Then, a lot of energy is given off, so maybe the NAE is destroyed as soon as it's formed.

That's a big question, how all this energy dissipates. I don't really know. I think if I remember right, Hagelstein thinks it's dissipated by phonons, the elementary particles of sound and vibration, where the energy gets dissipated to neighboring palladium.

The energy goes into the lattice as heat as the palladium atoms vibrate and so the heat is dissipated back into the palladium lattice.

As far as the rate being controlled by diffusion, I think Ed Storms would certainly agree on that. Nearly everyone has found that the rate depends upon the temperature. The higher the temperature, the greater the rate, and Ed would say it's because of the greater mobility of the deuterons that the higher temperature allows them to diffuse quicker.

Diffusion is known to be temperature dependent and gets faster at higher temperatures. That's how you determine the activation energy. You need at least measurements of two different temperatures and if you have even more, you can do a plot of the plot of the reaction rate versus one over temperature and get the activation energy that way. The rate of diffusion plotted versus $1/T$, and the slope will be related to the activation energy divided by gas constant R . You can get it that way as well.

If you could neutralize this Coulomb barrier and achieve fusion through electron clouds, very highly-concentrated electron clouds, with a huge negative energy localized, then that would eliminate one of the major issues that Ed Storms has with his nanocrack and hydroton hypothesis, and that is, that he has to have the deuterons approaching each other and a little bit in a vibrating situation where the deuterons are suspended by the negative walls of the crack, suspended in these linear structures he calls hydrotons, and then the hydroton vibrates. Then, as they approach each other in this negative environment then they don't fuse, but they approach each other, and some of the matter of the deuteron is given off, in this case, as photons, that then go out into the lattice and heat the lattice.

But it's an incomplete fusion, and then it goes to the next cycle and two other ones approach each other and a part of the mass is converted to energy into photons, and so through enough cycles of the vibration of this hydroton, the mass is finally consumed. The conversion by $E=mc^2$ is not instantaneous, but over a period of time, under his theory. I think he's running into some major resistance to this idea of partial conversion from atom to energy in that manner.

Peter actually is very good as a theoretician and he has something that seems to verify his theories on superabundant vacancies. Certainly, if you take these pockets of extremely high negative charge that constitute the NAE to overcome the Coulomb barrier, you don't have the problem that Storms does, cause the fusion takes place and maybe it self-destructs the NAE by the energy given off, but it certainly seems a lot simpler. I don't know who's right, I'll put it that way.

I think that there is something happening inside the palladium, I've always thought that. Otherwise, why does it seem to be related to volume? The more volume, the more heat you get generally. And it can't be surface because co-deposition produces a very large surface. In fact, my co-deposition experiment shows that the surface may be a million times larger, i.e., 10^6 larger than the surface of



the substrate, but you don't see that multiplication of excess heat. Excess heat still seems dependent on the volume, or how much palladium you have there. Initially F-P claimed it was a volume effect, so they thought. I don't think Martin ever changed his mind on that. He didn't know for sure, I think he thought it could be a surface effect, but he didn't know for sure. Electrochemistry seems to favor that the NAE might be a surface involving some double-layer, I think.

Just look at the double layer of electrons. When you put high charge on a cathode in an electrolyte, you're pushing extra electrons there, and tend to gather the surface electrode and the negative charge to attract with, positive ions from the solution and so they might not be having on the electrode surface you have negative electrons and opposing that you have positive ions. Then it's gradually less and less positive once you go further away. That's called the double layer. This layer of opposite charges and it happens almost on any surface, not just in electrochemistry chemistry. In very dry conditions in the winter, you walk across the carpet and touch a door knob, you'd build up a surface charge and it discharges. It's quite common in everything that you tend to get a surface charge on things.

If you believe the NAE is a highly concentrated region of electrons, it could be right within the double-layer. The negative electrons on the surface and the positive deuterons are attracted, so there you have a region of very high concentration of electrons and charges opposed by deuterons. Then inside the metal you have these electrons on the surface and the D^+ is mobile in the palladium. It may come from inside of the palladium near this negative charge on the opposite side, the NAE as a double-layer, it could be inside the palladium in that sense. Also, if you were to form a vacancy, the cathode charge would build up on the surface of the vacancy and deuterons would be attracted to that negative charge, so that might be where the action takes place. Maybe it's not right in the vacancy, it might be the electrons are on the surface of the vacancy and deuterons attracted to them and they're still in the metal but not in the vacancy then it might take place right on the surface of the vacancy.

I found it interesting that Martin once stated that he never would have never tried cold fusion experiments if the deuterons in palladium were NOT mobile.

75 STORMS EXCESS HEAT

Back to Storms and Pd-B. I sent a NRL Pd-B sample to him, and Ed later said he didn't get excess heat, but he forgot a previous paper where he reported excess heat for the NRL Pd-B.

I then showed him the paper he wrote. It was never published, but he reported excess heat.

76 THE FLEISCHMANN LETTERS

After the Second International Conference on Cold Fusion (ICCF2) in 1991, and the Navy cold fusion program started, one of my first correspondences was to Martin Fleischmann. I heard he was going to be visiting Bob Nowak, the funding officer at ONR, and I sent a letter to Bob Nowak, to give to Martin Fleischmann. In it, I had a lot of questions, like "Why was it so hard to get excess heat?", and whether you had to do certain things to the palladium to make it work. I wrote these questions down and I got an answer back, directly from Fleischmann; these were good questions



that I had. That's one of the first, earliest Fleischmann/Miles letters, and that started the correspondence, that letter I sent to Martin through Bob Nowak when he visited ONR.

Some of the things he said, he hadn't tried yet, and just he didn't know about, like using different palladium alloys, for example, and what he did in pre-treating the palladium. Did he heat it in an oven, and things like that? For many questions, he wouldn't give much information. He answered all my questions, but he just didn't know the answer to many of my questions.

I was surprised Martin kept writing to me, even without my writing to him. He started writing me things, and so I'd write back, and we started the correspondence.^{24, 25}

I don't know how he was getting along with Pons at that time, but about 1994, a couple of years later in France, they kind of broke up. By 1995, they were pretty well separated. I know he was corresponding to others too. I think Jed Rothwell got quite a few letters from Martin, as well.

I don't know exactly why he picked me to do all this correspondence with, but I think he knew I understood mathematics better than most people did. Also, he told me several times that he knew I was a "very honest person", and he wanted somebody like that to correspond with.

I have whole stacks of letters. He would write a lot longer letters than I wrote in response, but I think his wife typed a lot of them. Some of his letters were in longhand, which I had to study a little bit to be able to read. His handwriting was sometimes a little hard to figure out.

In his early letters, Martin described how he wanted to set up a program with a committee of experts in electrochemistry and science to evaluate the different calorimetry methods that are being used, including his own. He knew, if anybody really seriously knew what they're doing, that they would come out correct, and others would be shown to have errors. I believe that if he could have done that, it would have been very helpful. The more and more I studied his equations, I learned that he had it all worked out, and he was correct. I haven't found any case where he was incorrect.

That would have been the scientific conclusion, if a committee had really, really looked into this, that the Fleischmann-Pons calorimetry, their equation, their modeling, was absolutely correct. Martin has always been known for being very good at modeling any kind of system, and that was really his expertise. And by that, I mean a mathematical description, using equations. Martin's mathematics was outstanding. He could do that better than anybody I've ever met.

I remember in one letter he sent, he was taking the basic calorimetry equation, and made a couple of assumptions, and attempted to derive how it behaved, how the temperature was changing over a long time period from his equations. It starts out linearly, and it goes up, and it curves around, and if you don't make the assumption that some things are constant, then it never levels out completely. It just keeps kind of fairly straight, but it's not exactly level, it's not exactly asymptotic.

That's what's in one of his letters - you read through 30 pages of mathematics. I can understand a lot of it, most of it, but not all of it. He uses things that are really quite advanced in mathematics. Like,

²⁴ leischmann, M. and M. Miles, *Letters from Martin Fleischmann to Melvin Miles*, R. Carter, M.C.H. McKubre, and J. Rothwell, Editors. 2018, LENR-CANR.org. <http://lenr-canr.org/acrobat/Fleischmanlettersfroa.pdf>

²⁵ Miles, M., *Excerpts from Martin Fleischmann Letters*. J. Condensed Matter Nucl. Sci., 2016. 19. <http://lenr-canr.org/acrobat/BiberianJPjcondensedr.pdf#page=218>



if you can't do this directly, you can represent it by a series, and you cut off the series, just using the first couple of terms, and that's pretty advanced mathematics when you do that, and you're solving a differential equation.

We corresponded about once a month. I reached the point where, if I didn't get a letter from Martin in a month or two, I got a little bit worried. What happened to him? Later I found out Martin had been ill.

A few times I didn't agree with Martin and I'd worry when he wouldn't write to me for a while, maybe he was angry. I wasn't expected to agree with him, though I think later, in some of his letters, I thought he was a little bit off on some things. Towards the end, I thought he was making more mistakes than normal and it's probably true of all of us.

For example, late in his letters, when he did the NRL Report using my results with palladium-boron in Japan, he'd come up with a cell constant of five significant figures, 0.650 something, I forget, but it was five significant figures, and I think that was correct. He'd worked out in the NRL Report the cell constant to five significant figures in units of watts per degree to the fourth power, where degrees are in Kelvin, because the big number is on the bottom, watts. It might be 0.5 watts, or, it might be 300 degrees to the fourth power. That's a pretty small number.

You can see there he worked out a mathematical method where you put a lot of things in common with the Y value, and everything else he groups into the X value and he plots, Y versus X, and if time equals zero, extrapolating that gives the cell constant, and that comes out very accurately.

When George Miley saw that, he said, "He can't have it that accurate, that's not right." But Martin Fleischmann was correct. He worked out a method where he could take all the data and extrapolate it to zero time, and that was the cell constant expressed as five significant figures.

Anyway, he one time told me, using his results, it shows for one day, day 61 of the NHE experiment in Japan, it shows a negative excess power on average for that whole day. It was -5 milliwatts. It turns out, he put it in joules, but I converted it to watts by dividing by the number of seconds per day, to get the average for the day, that it comes out to an average of a -5 milliwatts. That would not be important to a lot of people, but it was important to Martin, to ever have anything turn out negative. He thought we had to change the cell constant to make up for that.

I didn't agree with that because it would damage what we said before. If we said the cell constant was accurate to five significant figures, then we had to go back and change it. I didn't think we ought to make a change. Instead I started thinking what else could have caused that.

This is towards the end of our correspondences actually. I finally said, the gasses generated have to push against the atmosphere, they're doing work. I checked in my physical chemistry textbooks and that was correct. I worked out an equation for what the gasses did and when they were calibrated, the current was only 150 milliwatts and only a small amount of gasses were being generated and the work term would be maybe 5 milliwatts. But when this point showed up, we were at 1,000 mA or one amp. I mean 1,000 milliamps or one amp, we were generating a huge amount of gas, much larger than when we calibrated it.



I calculated the difference in this work term for the smaller current and for the larger current when the excess heat showed up as negative, and it came out there was about 15 milliwatts more work being done at the higher current, and the excess power instead of being a negative five, it came out at something like a plus 10 milliwatts.

This was near the end. Mike Melich was going to go visit Martin, and knew the equations quite well, I thought. I said, ask Martin about this. I told him that the work term explains the negative effect that he saw. Mike Melich did that, and reported back to me that Martin said I was right on that.

I figured out that F-P didn't change currents like I did, because I always liked to change currents. They liked to set it at one current and just run it for the whole time. However, then, you don't need to worry about the work, because you calibrated for it in the calibrations. But if you change the current, the work term goes up, and you haven't accounted for that.

If I'd used a constant current at NHE, this would not have been a problem, because the cell was calibrated by Martin when I was running at 150 milliamps and this negative excess power was measured at a 1,000 milliamps current. That made a big difference in the work term, and it showed up as a negative value. Negative five milliwatts for that day (Day 61), but the work term takes care of that.

I'm one of the few that took time to try to understand his mathematics. It was mostly physical chemistry. There's not a lot there in Martin's equation that you could not find in a physical chemistry textbook. Of course, that's what I majored in, and that's what I've taught, was physical chemistry, so that's what I'm really familiar with.

And the nuclear part of cold fusion, is kind of a completely separate category all in itself that's not really related much to chemistry. The chemistry brings it together just so that the nuclear reaction can happen. Of course, what actually happens is still a bit of a mystery.

That's what I say too, about Storms and his hydroton and cracks, it's all chemistry until you get where the nuclei, the hydrogen atoms, the deuterons and the protons, when they interact. That's the big mystery, that's where the Nobel Prize is.

77 ST GEORGE, UTAH NOV 2017

Near the end of November 2017, I moved here to St. George. I've been here about a year and a half and I haven't even found all the things I need to do an experiment. They're probably still in boxes in the garage. The main thing missing is the palladium **boron** rod that I used before. I found some of the other rods right off, but I can't find that one I used at NHE. I remember I said, "this is special. I need to do something different with it," so I didn't put it in with the other materials. I just should've. Now, I don't remember what I did with it.

In the last eight months or so, I was asked to write two chapters for an Elsevier book coming out. That took an awful lot of time to get written the way I wanted it.

Well, some people have told me I should write a book. It'd be a lot of time to write a book, but I could do it. I don't know if anybody's interested in reading a book about cold fusion unless it took off somehow and becomes accepted. That should eventually happen. There's a lot of books written



on cold fusion by other people. Or, I could look at certain things too, like a heat helium correlation, or the palladium boron-there are a lot of topics that are very positive. So those are at least three areas. Martin Fleischmann, the palladium-boron, and the heat-helium could all be mainstays of the book.

I certainly have an inside look at the Navy program. There's a lot of politics that go on. The NRL would not like what I would say. I think the real value of a book is if cold fusion takes off, or when it does, it's a documentation of the science that was going on during this 30-year period.

What I would actually like to do next is find the palladium-boron that I lost. I hope it's here somewhere. I will load it, and de-load it, load and deload; look at what happens in that early period and try to get a better handle on how to analyze that kind of data. Does excess heat really show up early in palladium-boron, which I think it does? It's hard to measure accurately, because when you're loading the deuterium into the palladium, you don't really know exactly what the thermoneutral potential is. You know what it is if all the deuterium goes into the palladium, you know what it is if none of the deuterium goes in, but, you don't know the in between, hardly at all.

Unless you had some accurate way to measure the amount of gas coming off, then you could get an accurate measurement, because the amount going in is the amount that should be coming off minus what you actually have coming off. That would be the amount going in. You could determine it that way theoretically, what should come out by Faraday's Law, and how much do you measure. Of course, oxygen is always coming off. It's the anode, so you're getting the other side of it.

Well, the amount of hydrogen is twice the amount of oxygen, the way the palladium works out. You get 2 H₂ from two waters and one O₂. It is a two to one ratio. That's one third of the amount in oxygen. So, if all went in, you'd have about one-third of the gas you'd expect normally.

78 MAIN GOAL NOW

My main goal now is I want to see cold fusion get accepted while I am still alive. I was trying to think of ways to get it accepted. One way is what I'm working on with Dave Nagel and Steve Katinsky, in getting more palladium-boron made, and have other labs look at palladium-boron in more detail, and test, and try to reproduce excess heat with it. That would help.

If there was unlimited money, I would try to work with David Nagel to do the palladium cube experiment reported by F-P. That would answer a lot of critics if they could see video evidence of that. Of course, they could say, well you just fake the video. I mean, they could always come up with an answer to counter whatever you do. I talked to Martin Fleischmann about that cube experiment. You have to put a counter electrode exactly symmetrically around all six faces of the cube, and that's the way they did it.

There is one publication where they repeated the cube excess heat experiment and found nothing, but they just used a cylinder as a counter electrode with a cube inside of it. So, you only have the sides, the top and bottom are open for deuterium to escape and that won't work. The counter electrode has to be a bigger cube, a screen or something that surrounds the smaller cube. When I discussed it with Martin, he said I was correct, you have to have good symmetry, because what you are trying to do is force deuterium in equally through all six faces of the cube.



That way you're pushing everything into the center point of the cube. If it's going in all six spaces for them to meet. It's just a point where they all meet, that's the center, the cube. If you use a rod with a counter electrode the deuterons all would meet at the center line.

If you use a plane, two planes, it forces everything in on equal sides, you have the plane of symmetry in the middle. And you get a smaller effect with the plane. Bigger effect with a rod and the big, big effect with a cube, or you can do it with a sphere. That experiment should be done. You have to have digital equipment. You have to be careful that it doesn't get too big of an explosion, I mean to boil-off all the water, and make a hole in the bench, and a hole in the cement. That's a lot of energy dissipated. I remember trying to back calculate what the energy was and what the power was. That would be pretty high.

I also want to get the publication of the palladium-boron results from the Navy lab in China Lake, the two labs in Japan, and my Ridgecrest experience. All in my papers, plus the NRL experiment to show that I have a graph for that. What I got from Steve Krivit, it shows that there is excess power there, and that if you think about it, let's see... one... two... three ... four different calorimeters right in that one paper. We measure similar facts, using four different calorimeters. You can't argue with the calorimeter.

Well see when you do electrolysis, you are going to go up here anyway, because hydrogen reacts more readily than deuterium. So as you electrolyze it away, your deuterium is going to be more and more concentrated as time goes on, because there is a certain amount of hydrogen in every sample of D₂O.

Why does palladium-boron almost always work? That's another question that needs answers. I have never had a palladium-boron electrode that didn't work to some degree. Some electrodes work more than others but there is one failure that we had because the palladium-boron was so hard, they had been trying to make it into a rod, but they created a crack in part of the rod. So, it was almost the full length of the rod. That would be like a real crack and you don't have electrochemistry going on in there. So you load deuterium into the rod, but it escapes from the crack.

But, if I was going to do anything, if I was going to run one experiment, I would look at the early excess heat effect from palladium-boron. The excess heat shows up within two or three minutes.

79 IF I KNEW THEN WHAT I KNOW NOW

How does the scientific community judge this area of cold fusion? You know it's very negative of people to say "Do I want to ruin my career with this?" You'd have to answer that first. I think it destroyed my Navy career. I think it really hurt Peter Hagelstein's career at MIT, and I think any younger person would have to think about that before getting into this area of research. If it were an accepted topic, then of course it would be very interesting to get into.

If I were not worried about hurting my career and if you're older you don't care, that's why a lot of older people got into this. Where would I start today? I'd probably start right where I am right now, with palladium-boron and co-deposition, that would be the two key areas I'd focus on.



It's very hard to design experiments that would tell you what you want to find out, unless the reaction becomes quite reproducible. If the reaction is not reproducible, it's very hard to study the variables. That's the problem we have right now. If you can get a very good, reproducible effect, then look how the variables effect it, and then maybe you can maybe answer that question. But right now, I don't know how you do it.

It needs a theory, and a reproducible experiment. Science has never functioned like it did with cold fusion, in saying, because of our theories it's not possible. That's not the way to conclude something. If the experiment is correct, your theory has to change to explain the experiment. You can't just throw out experiments because it doesn't fit a theory.

There is a little-noticed saying: Theories guide, experiments decide. Stan Szpak felt the same way, that theories guide, but experiments decide. That's the way it should've been, but they turned that around in cold fusion. Using theories to decide that experiments which don't fit with the theory are plain wrong is not correct science and this was a major cause of the early and vigorous criticism of cold fusion.

My Cold Fusion Dates and Places

Dr. Melvin H. Miles

1989 Pre-cold Fusion. I had three research programs and two postdocs. These programs were

- (1), Li/BrF₃ Battery (5 V per cell),
- (2) Electrochromic Materials and
- (3) Electrochemical Reduction of Carbon Dioxide (This used a Pd/H₂ Reference Electrode).

1989 Cold Fusion. No excess heat effects until September after receiving a Johnson- Matthey palladium cathode. November: Reported with groups not finding cold effects in DOE ERAB Report.

1990 March: Reported excess heat at ICCF1 in Salt Lake City, Utah.

November, December: Found Heat/Helium Correlation Results at China Lake with helium measurements done at the University of Texas.

1991 March: Preliminary Note on Heat/Helium published (JEAC) followed by visits from Bockris and Preparata.

March: Ben Bush starts work at China Lake.

June/July: Heat/Helium results reported at ICCF2 in Como. Italy. Discussions about a Navy program.

1992 January: Navy Program begins with NAWC, SPAWAR and NRL. No excess heat effects for most of that year. Small excess heat (100, 50, 20 mW) in December for a JM Pd 1.0 mm wire, but very accurate He-4 measurements done by Brian Oliver of Rockwell International.

1993 Frustrations. NRL electrodes are not producing excess heat results



.1994 March: Ben Bush's Post Doc ends and he leaves for SRI. First excess heat results with NRL Pd-B

Electrodes later that year. Nowak tells me that this saved the Navy Program.

1995 June: The Navy Program strangely cancelled on the morning of the third day of a review meeting at China Lake.

This meeting involved people from The University of Utah, Utah State University, ONR, NRL, SRI and NAWC. I have a list of all the names.

1996 September: Completed the China Lake Final Report on cold fusion thanks to funding from Dave Nagel. No research funding for anything else.

1997 February: Trip to Milan, Italy to meet with Fleischmann, Preparata, Del Giudice for a possible cold fusion program

to be funded by Pirelli. I was to be in charge of the research, but the Pirelli Funding fails.

April: Assigned to Stockroom duties at China Lake.

October: Start cold fusion research at NHE in Japan.

1998 April: NHE closes. Return to China Lake but forbidden to work on cold fusion (but still write papers on my own time).

2001 March: NRL Report published despite opposition by others. Written mainly by Fleischmann for my Pd-0.5 B results at NHE using F-P Dewar Calorimetry.

2001-2002 Teaching at MTSU in Tennessee. Retire from China Lake in January 2002. My MTSU Physical Chem lab students do a cold fusion related project for ICCF-9.

2002-2003 Teaching at Bates College in Maine. Teach a class on Electrochemistry and Cold Fusion during Spring Break.

2003-2006 Teaching at University of La Verne in California. Dr. Iraj Parchamazad tries Zeolites in a cold fusion experiment with positive results.

2007-2011 Work as a Contractor at China Lake on Thermal Batteries and Cold fusion (U.S. DTRA funding). Research on cold fusion relating to co-deposition using various transition metals. Only palladium gives excess heat results.

2017 November. Move to Utah.

2018- Present Visiting Professor at Dixie State University. Continue to write cold fusion manuscripts.

Appendix A. Timeline Spreadsheet Prepared from Dr. Miles' Interview Transcripts

N O	MONT H	STAR T	END	LOCATION	INSTITUTION	EVENT TYPE	NOTES
1		1971		Ridgecrest, CA	China Lake Lab	Research	Summer session research at China Lake while teaching at MTSU. Continued 1972, 1975, 1976, 1977
2		1973		Long Island, NY	Brookhaven National Lab	Research	Spent summers researching water electrolysis.
3		1974		Long Island, NY	Brookhaven National Lab	Research	Spent summers researching water electrolysis.
4	10-Oct	1957	1960	Germany		Personal	Missionary and basketball player.
5		1960	1962	Provo, Utah	BYU	Education	College student.
6		1962	1965	Salt Lake City, Utah	U of Utah	Education	PhD Graduate student.
7	10-Oct	1965	1966	Munich, Germany	Technical University	Education	Post-doc studies with Heinz Gerischer.
8		1966	1969	Corona, California	Navy Ordnance Lab	Research	First job with the Navy as a Battery Systems Researcher
9		1969	1978	Tennessee	Middle Tennessee State	Teaching	Physical chemistry teaching appointment.
10		1978		Ridgecrest, CA	China Lake Lab	Research	Moved to Ridgecrest to begin thermal battery research at China Lake Lab.
11	3-Mar	1989		Ridgecrest, CA	China Lake Lab	Research	Start cold fusion experiments, no success until September.
12	5-May	1989		Los Alamos, NM	LANL	Meeting	Santa Fe meeting where David Stillwell presented China Lake results.
13	7-Jul	1989		Ridgecrest, CA	China Lake Lab	Meeting	Bob Nowak says no official funding of cold fusion.
14	9-Sep	1989		Ridgecrest, CA	China Lake Lab	Research	Excess heat success with new Johnson Matthey palladium rod received.
15	3-Mar	1990		Salt Lake City, UT	University of Utah	Meeting	ICCF1 Reported excess heat at ICCF-1 in Salt Lake City, Utah.
16	10-Oct	1990	1991	Ridgecrest, CA	China Lake Lab	Research	1st set Heat/Helium experiments begin.
17	12-Dec	1990		Austin, TX	University of Texas	Research	Ben Bush analyzes heat-helium results at University of Texas
18	1-Jan	1991		Ridgecrest, CA	China Lake Lab	Research	Controls for heat/helium using protium are performed.
19	3-Mar	1991		Ridgecrest, CA	China Lake Lab	Ben Bush	Ben Bush starts work at China Lake.



20	3-Mar	1991			JEAC	Published	Preliminary Note on first Heat/Helium experiment published (JEAC) followed by Bockris & Preparata visits
21	7-Jul	1991		Como, Italy	ICCF2	Meeting	Heat/Helium results reported at ICCF-2 in Como. Italy. Discussions about a Navy program.
22		1991		Washington, D.C.	NRL	Meeting	Meeting to discuss a Navy cold fusion program.
23	12-Dec	1991	1992	Ridgecrest, CA	China Lake Lab	Research	2nd set Heat-Helium experiments begin.
24	1-Jan	1992		US	US Navy	Research	Navy Program begins with co-deposition, 3 out of 30 measure excess heat.
25		1992		Ridgecrest, CA	China Lake Lab	Research	Only real success were electrodes sent by Martin Fleischmann.
26		1993		Ridgecrest, CA	China Lake Lab	Research	Frustrations. NRL electrodes are not producing excess heat results
27		1993	1994	Ridgecrest, CA	China Lake Lab	Research	3rd set Heat-Helium experiments begin.
28	3-Mar	1993		Menlo Park, CA	SRI	Ben Bush	Ben Bush's Post Doc ends and he leaves for SRI. First excess heat results with NRL Pd-B
29		1994		Ridgecrest, CA	China Lake Lab	Research	Dr. Imam Pd-B electrodes provide success.
30	12-Dec	1994		Washington, D.C.		Meeting	Navy Review meeting; Pd-B success saves Navy program.
31	1-Jan	1995		Washington, D.C.	NRL	Research	Helped to set-up NRL verification apparatus, which failed.
32	6-Jun	1995		Ridgecrest, CA	China Lake Lab	Meeting	The Navy Program strangely canceled on the morning of the second day of a review meeting at China Lake. This meeting involved people from The University of Utah, Utah State University, ONR, NRL, SRI and NAWC.
33	9-Sep	1996		Ridgecrest, CA	China Lake Lab	Published	China Lake Final Report completed thanks to funding from Dave Nagel.
34	2-Feb	1997		Milan, Italy	Pirelli	Presentation	Trip to Milan, Italy to meet with Fleischmann, Preparata, Del Giudice for a possible cold fusion program funded by Pirelli.



35	4-Apr	1997		Ridgecrest, CA	China Lake Lab	Insult	Assigned to Stockroom duties at China Lake.
36	10-Oct	1997	1998	Hokkaido, Japan	NHE Lab	Research	Start cold fusion research at NHE in Japan.
37	6-Jun	1998		Ontario, CA	ACS meeting	Meeting	Well-attended controversial meeting.
38	4-Apr	1998		Ridgecrest, CA	China Lake Lab	Research	NHE closes. Return to China Lake but forbidden to work on cold fusion (but still write papers on my own time)..
39	4-Apr	1998		Vancouver, Canada	ICCF7	Meeting	International Conference on Cold Fusion #7
40		2000			JEAC	Published	NHE analysis published in Journal of Electroanalytical Chemistry
41		2000	2001	Ridgecrest, CA	China Lake Lab	Research	Work with Martin Fleischmann on cold fusion as a weapon, but funding pulled at the last minute.
42	3-Apr	2001		Washington, D.C.	NRL	Published	NRL Report published despite opposition by others.
43	1-Jan	2001	2002	Tennessee	MTSU	Teaching	Teaching position at MTSU in Tennessee; Physical Chem lab students do a cold fusion related project for ICCF-9.
44	1-Jan	2002		Ridgecrest, CA	China Lake Lab	Personal	Retired officially from China Lake in January 2002.
45	6-Jun	2002		Ridgecrest, CA	China Lake Lab	Contractor	Hired for summer session research at China Lake.
46		2002	2003	Lewiston, Maine	Bates College	Teaching	Gave seminar and taught intersession class on cold fusion during one-year teaching appointment.
47		2003	2006	LaVerne, CA	University of LaVerne	Teaching	Teaching position; Chemistry Chair Dr. Iraj Parchamazad tries Zeolites in a LENR experiment with positive results.
48		2006	2011	Ridgecrest, CA	China Lake Lab	Contractor	Work as a Contractor at China Lake on Thermal Batteries and Cold fusion (U.S. DTRA funding). Research on cold fusion relating to co-deposition using various transition metals. Only palladium gives excess heat results.
49		2011	2015	Ridgecrest, CA	China Lake Lab	Research	Working in the lab under the radar without any funding.



50		2014			Wiley	Published	Co-author with Michael McKubre on chapter about Heat-Helium correlation in "Developments in Electrochemistry".
51		2016	Youtube		Cold Fusion Now channel	Media	Video release of "Anomalous Effects in Deuterated Systems" by Ruby Carat detailing Heat-Helium correlation work.
52		2017		Ridgecrest, CA	Home kitchen	Research	Set up a cell in the Ridgecrest home kitchen.
53	11-Nov	2017		St. George, UT		Personal	Moved to Utah.
54		2018		Sendai, Japan	ICCF20	Meeting	Reported on cold fusion cells.
55		2018	present	St. George, UT	Dixie State University	Teaching	Present Visiting Professor at Dixie State University. Continue to write cold fusion manuscripts.
56		2020			Elsevier	Published	Two chapters authored in the book "Cold Fusion Advances in Condensed Matter Nuclear Science"