

# Martin Fleischmann's Historic Impact

Compiled by Christy L. Frazier, with assistance from Michael McKubre and Marianne Macy

Electrochemist and cold fusion pioneer Dr. Martin Fleischmann passed away on August 3 in the comfort of his home in Salisbury, England, with his family by his side. He was 85.

Fleischmann was born March 29, 1927 in Karlovy Vary, Czechoslovakia to a Jewish father and Catholic mother. In a 1996 interview with Chris Tinsley in *IE* #11 (<http://www.infinite-energy.com/iemagazine/issue11/fleishmann.html>), Fleischmann related a harrowing story about his family's escape from Nazi-occupied Czechoslovakia in 1938: "I always tell people I had the unique and unpleasurable experience of being arrested by the Gestapo at the age of 11...[M]y father was very badly beaten up by the Nazis. However, we got out. We were driven across the border by a First World War comrade-in-arms of my father...At that time, my parents also got permission to come to England, and we all got on the train in Prague and came to the Dutch border and the Germans cleared the train of all refugees and we were in the last coach and my father said, 'No, sit tight, don't get off the train,' and the train pulled out of the station. So that's how we got away the second time, and arrived at Liverpool Street Station with 27 shillings and sixpence between the four of us."

Fleischmann's father died soon after the family emigrated to England, as a result of his mistreatment at the hands of Nazis. His mother showed great ingenuity and fortitude by starting and successfully running Old Cottage Toys, a doll company. Martin's sister Susi later joined her mother in the business. The dolls are now collector's items (see *A Collector's Guide to Old Cottage Toys: The Dolls of Margaret & Susi Fleischmann*, Terry and Christine Summers, 2003).

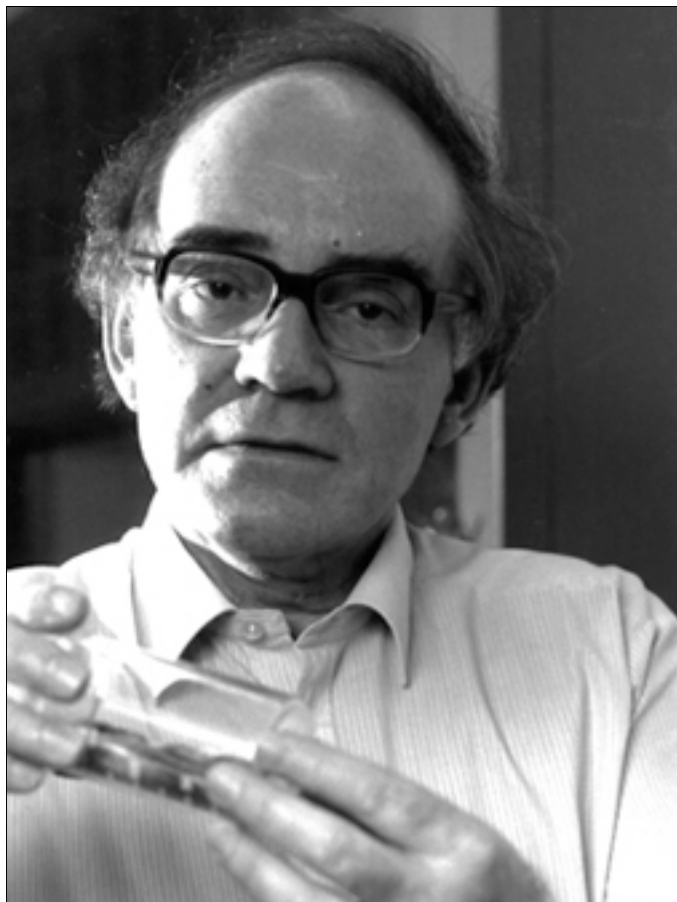
Fleischmann was educated at Imperial College (London). Gene Mallove wrote in *Fire from Ice*: "In a curious foreshadowing of his later thinking, Fleischmann's doctoral thesis focused on how hydrogen diffused through platinum. He used platinum membranes only 0.1 to 0.2 mm thick to study the electrochemical transport of the hydrogen."

In 1950, Fleischmann married Sheila Flinn. He began his teaching career at King's College, Durham University (now the University of Newcastle-upon-Tyne).

At the young age of 40 in 1967, Fleischmann was named the professorial Chair in Electrochemistry at the University of Southampton (England). His charge was to build a world class electrochemistry group, and two names well-known to the field would later study there as a result—Stanley Pons and Michael McKubre.

Fleischmann was honored to serve as president of the International Society of Electrochemistry from 1970 to 1972.

Mallove wrote in *Fire from Ice* of a 1972 paper Fleischmann co-authored with his graduate student B. Dandapani ("Electrolytic Separation Factors of Palladium," *Journal of Electroanalytical Chemistry*, 39, 323-332) that "lay the seeds of future pondering of the mysteries that lurked within a palladium lattice laced with deuterium." Mallove wrote, "In the April 1989 Fleischmann-Pons-Hawkins paper, the authors



say that one of the features revealed in that early 1970s study prompted their later cold fusion work. Fleischmann may have begun to wonder whether some of the peculiar behavior of the deuterium ions ( $D^+$ ) in palladium would make the lattice suitable for near collisions and possible conventional fusion reactions."

In 1974, Fleischmann played an important role in the discovery of the surface enhanced Raman spectroscopy effect [see colleague memorials below for more about this discovery]. He was awarded the Royal Society of London's medal for Electrochemistry and Thermodynamics in 1979.

Fleischmann retired from teaching in 1982, and was given an honorary professorship at Southampton. Fittingly, he was awarded the Palladium Medal by the U.S. Electrochemical Society in 1985—four years before he and Stanley Pons' work on cold fusion with palladium was announced at the University of Utah. In 1986, Fleischmann was named a Fellow of the Royal Society, an honor given to only the most distinguished British scientists. He has also received the Bruno Meyer Medal from the Royal Australian Chemical Institute (1988) and the Toyoda Medal from the International Society for Condensed Matter Nuclear Science (2009).

In the early 1980s, Fleischmann began visiting and work-

ing as a part-time research professor with Dr. Stanley Pons, then an associate professor at the University of Utah (later, Chairman of the Chemistry Department). The two met in the late 1970s, when Pons attended the University of Southampton to achieve a Ph.D. in chemistry. Fleischmann and Pons had similar interests, including hiking and cooking, and some of their first conversations about cold fusion-related effects took place during hikes in Millcreek Canyon and in the kitchen preparing dinner. Before 1989, the two chemists published numerous papers together on various topics.

Fleischmann and Pons spent over five years and \$100,000 of their own money on cold fusion research prior to 1989. They conducted experiments in Pons' laboratory in the Henry Eyring Chemistry Building at the University of Utah.

It wasn't until late 1988 that the University fully learned about the groundbreaking work being done. Chase Peterson, in his autobiography *The Guardian Poplar*, explained how he learned of the duo's work through an outside review of the chemistry department by Dr. Richard Bernstein (UCLA): Bernstein said to Peterson, "[T]here is one additional matter you should know about that I am reluctant to put in the written report. You have what could be an extremely important project under way in the department that will be hard to keep confidential." And, just a few months later, on March 23, 1989, the entire world knew about this "extremely important project." See Mallove's *Fire from Ice* and Charles Beaudette's *Excess Heat* for very in-depth coverage of the early years of the Fleischmann-Pons collaboration and the firestorm that began in 1989. Both books provide critical details about the ways in which the furor that followed the March 23, 1989 press conference served to taint the reputation of a potentially world-changing science for many years to come.

Fleischmann, in a 1998 interview for the New Energy Foundation's documentary "Cold Fusion: Fire from Water," commented on the unfortunate behavior of journalists following the March 1989 announcement: "I think what I find distressing about the name-calling is not so much that as just that it is such sheer bad journalism. I think there has been a terrible decay in journalistic standards. In the old days you had to present the facts and then you had to have an editorial comment. Facts and editorial comments are totally mixed now in such a way that in the end the reader finds it very difficult to establish what the facts really were. If the subject had been reported in a factual way and then with a very negative editorial comment, that would be different." He also said of scientists and journalists in the 1996 *IE* #11 interview, "I don't take kindly to being accused of unethical doings by people who clearly have been involved in unethical activities themselves."

In 1992, Fleischmann and Pons left the University of Utah to work for Technova at IMRA Europe (France), funded by Minoru Toyoda (of the Toyota company). He said in the *IE* #11 interview, "I had actually thought of dropping out of

this field in 1991 and just waiting to see what other people would make of it in order to go back into it in 1993 myself, but I was persuaded to go to France." Fleischmann retired from IMRA-Europe in 1995.

In his interview for "Cold Fusion: Fire from Water," Fleischmann reflected on his initial and continued interest in cold fusion research: "There's obviously some very interesting science here; for me I was always interested in the wider implications of looking at systems from the point of view of quantum field theory, but I think there is a technological angle to this whole project and that is that if it can be engineered then it can prove to be a very important energy source."

Fleischmann published over 200 scientific papers and a number of portions of textbooks. For our special issue #24 in 1999, celebrating ten years of cold fusion research, Fleischmann wrote "Nuclear Reactions in the Pd/D System: The Pre-History and History of Our Early Research" (<http://www.infinite-energy.com/images/pdfs/fleischmannie24.pdf>).

In the *IE* #11 interview, Fleischmann humbly projected his hopes for the success of the cold fusion field: "Scientists are really very conscious of the fact that they stand on the shoulders of an enormous tree of preceding workers and that their own contribution is not so enormous. What I've always said about cold fusion is that 'everything I can say about cold fusion can be condensed onto about half a page now and I will know the subject has arrived when it is a footnote.'" While it did not happen in Fleischmann's lifetime, the field will hopefully overcome its negative "footnote" status and become a scientific footnote in the sense that Fleischmann meant—that the research and its resultant technologies will become commonplace.

Fleischmann is survived by his loving wife Sheila, two children, Nick and Vanessa, and numerous grandchildren. His daughter Charlotte Fleischmann passed away earlier this year.

Dr. Martin Fleischmann's impact on the new energy field, and science as a whole, is immeasurable. The Fleischmann-Pons discovery and announcement interested so many scientists around the world; the majority of experimentalists and theorists in the LENR field today began work in the field in 1989, some within days of the press conference. A great number of Fleischmann's colleagues and friends have offered remembrances of the great man, which we share below.

## Dr. Michael McKubre

(Senior Staff Scientist and Director,  
Energy Research Center, SRI International)

Martin was a man of joy. In all the time I spent with him I never heard him complain about his physical ailments—which were many—or of the treatment at the hands of textbook physicists. I miss Martin as a scientist, as a teacher and



Martin and Sheila Fleischmann at River Barn (Tisbury, England), one of their favorite places, May 2010.



Martin Fleischmann and Mike McKubre at ICCF15, October 2009.

as a friend. What I miss most about him is his ready smile and deep belly laugh. He was a man of wit and charm. Kind, gentle, and fun to be with.

He was also the most highly innovative person I ever knew. Without Martin this community very likely would not exist. What Martin excelled at was effective creativity. Ignorance is an aid to creativity, if you don't know what has gone before, but it is not constructive and makes no progress. You need deep wisdom, acquired through study and training to know what already is. You also need a polished skill set to advance your intuition. Martin had both.

Martin was also very efficient—the Pablo Picasso of physical chemistry. Modern, clean lines, not a stroke wasted. The constructs were always much deeper than they seemed. He never made a problem more complicated than it needed to be. This allowed him to see further, faster, than anyone I have known. Most often he let others fill in the details, to apply the shadings and add dimension to his Picasso's. One example is surface enhanced Raman spectroscopy (SERS) or the giant Raman effect first identified in 1973 by Martin with Pat Hendra and my countryman Jim McQuillan at Southampton. Like cold fusion this was so far advanced, so “outside the box,” that it was ridiculed and disdained until proven true (and of huge significance) by others.

At a high enough level creativity is indistinguishable from precognition. Martin was never troubled by the gnats or Nates. He knew, and the data bore him out, eventually. This is one thing that his critics could never forgive him for, and the discourse became personal. Max Planck observed that scientists will only look at a field objectively once the original proponents have died. Perhaps now we can expect a higher level of objectivity.

I spent a lot of time with Martin in the summer of 2009. Martin's health had subsided to the point that he was “not Martin.” Some of his friends, and particularly Mike Melich and Marianne Macy, organized to bring Martin to New York on the Queen Mary (he could not fly), and then New Jersey into the care of Irv Dardik, Alison Godfrey and their team. The results were spectacular. With physical and intellectual stimulation Martin recovered to the point that I saw him jogging in the corridors of Irv and Alison's home, and I was able to talk science with Martin, experience his wit and hear him laugh again. The timing also proved fortuitous for his introductory talk in Rome at ICCF15, and for that beautiful evening organized by Vittorio Violante at Castel Sant'Angelo where Martin received the only Minoru Toyoda Medal courtesy of Akito Takahashi and the ISCMNS.

I first met Martin when I went to Southampton in 1976 to do my post-doctorate with Graham Hills on a topic of Martin's (concerning packed and fluidized bed electrochemical reactors). I went to Southampton because it was the number one school of electrochemistry in the English speaking world at that time. This was true largely because Martin was there. So Martin was instrumental in the first part of my career that lasted from 1976 to 1989. Martin was responsible directly for the second part of my career in cold fusion from 1989 until now. And I expect that Martin will be responsible for the third (and final) stage of my career starting essentially today—with acknowledgment to Dr. James Truchard—in the industrialization of quantum energy.

## Dr. Michael Melich

(Professor, Naval Postgraduate School)

Once Martin and Stan became public figures in 1989 the torrent of requests by letter, fax, personal entreaties and telephone competed for their attention that they so desperately wanted to devote to the actual scientific work that they knew and wanted to complete. I was abroad in late March 1989 and when I returned home I called my parents in Salt Lake City. Dad asked: “What do you think of the big story?” I said: “What big story?” He said “cold fusion” and I said ridiculous. Within weeks I was in Salt Lake learning from Jim Brophy, the University of Utah VP for Research and a Physics Professor, something about this new discovery and the near impossibility of meeting with Pons or Fleischmann. Over the next few months that changed as my father was appointed by Governor Bangerter to sit on the Fusion Energy Advisory Council, whose task was to oversee the National Cold Fusion Institute and its expenditure of \$5M to pursue cold fusion research and development. My father was a lawyer and told me that I was to read the technical material he received as Council member and to interpret and explain it to him. Also on the Council was physics Prof. Wilford Hansen from Utah State University and eventually it became Wilf's job to independently analyze data from FP cells. Wilf's report was delivered to the board in early 1991 and he then presented it at ICCF2 (Como, Italy, 1991). It was at Como that I began to get to know Martin and Stan.

The general good reception of Wilf's analysis prompted Martin to suggest that we undertake a similar analysis of 1989 data from the Harwell experiments supervised by David Williams, published in *Nature*, and the experiments reported in *Science* supervised by Nathan Lewis of CalTech. This was the first time that I embarked upon a scientific project with Martin and was to learn much from it. Attempts to obtain access to the CalTech data failed. However, Martin arranged a meeting with the Technical Director of Harwell, Ron Bullough, and David Williams to request access to their data. They generously cooperated and within a few months we had the digital data on floppy disks, notebooks, and also had employed Anthony Kucernak, who had run Harwell's electrolytic cells. Anthony moved to Valbonne, France to work with Martin on the data and Wilf and I worked on it in the U.S. Wilf and I presented in Nagoya, Japan in Fall 1992 at ICCF3 a very conservative assessment of what we found, namely, that in one cell there seemed to be good evidence of a burst of excess heat of about 10% of input power for about 250 minutes, 100-200 mW. There were 10 such “bursts” of



power in this one cell with deuterated electrolyte, while the light water control cell showed no effect. Anthony and Martin had gotten similar results in their reanalysis. Martin showed his mastery of extracting information from data sets, although he had no personal facility operating computers and writing their programs. It was during this time that I heard him repeat the phrase: "Finally, it is about the data analysis." We were able to show that despite the problems with the Harwell data, that a calorimetry accuracy of about 1% could be obtained. This was a considerable improvement over the 5-10% error estimate that the Harwell team had estimated, which they interpreted to mean that they had not observed heat of interest. In fact, the majority of the Harwell effort was devoted to finding standard nuclear products of fusion, *i.e.* neutrons, gamma rays, energetic charged particles on experiments where no heat was being measured. Wilf and I concluded from this effort that Harwell simply didn't have time to learn by themselves how to do these, at that time, poorly described FP experiments. It would be another year, in 1990, when the first complete paper from FP was published.

I also learned that Martin's relationship with Harwell had over the years been close because of his expertise in the properties of Pd. Further, he had given David Williams and Harwell, in either February or early March 1989, some understanding of what he and Stan Pons had been finding out in their electrolysis experiments on PdD. Martin considered this essential under the terms of British law, since he feared that any energy process with nuclear power and energy densities might be of great importance—just as conventional nuclear processes are.

Ron Bullough, knowing that Harwell was to be transformed out of existence by the Thatcher government, saw the Fleischmann-Pons Effect (FPE) as an opportunity and took his last "blue sky" budget, 600,000 pounds sterling, and told David Williams to organize the effort. Their project was initiated prior to the March 23, 1989 press conference and included both a FP type electrolytic experiment involving 16 cells using a matrix design to see difference in electrolyte, H and D, and electrode geometry. An improved calorimeter was also built to overcome many shortcomings of the first generation calorimeters. Finally realizing that they were not seeing large heat effects, Williams employed a highly sensi-

tive calorimeter normally used to measure the heat of radioactive decay of plutonium to see if he could detect a weak heat signature. Their heat experiments showed no excess heat, using the methods of data analysis employed.

After this experience Martin and I enjoyed a friendship that would endure and deepen until his death. There are two other more recent events that have a special place in my memories of Martin and Sheila. During the planning with Dave Nagel for ICCF14 (August 2008, Washington, DC), I had stopped in Tisbury to see if we might get Martin to come to the meeting. Although he was suffering the early stages of Parkinson's disease, it did not limit his ability to interact in the vibrant, fully throated way that was his nature. It became clear that for Martin to attend ICCF15 in 2009 (Rome), the 20<sup>th</sup> anniversary of the announcement, a special effort to address his health problems would be necessary. We were successful at arranging for Martin and Sheila to board the Queen Mary in Southampton and sail to New York in June 2009. For the next three months Irv Dardik and Alison Godfrey Dardik and their staff had the Fleischmann's as resident friends at their New Jersey country facility. It was a glorious time for Martin and Sheila, even if their lurcher, Mouse, was not with them. Martin in June was limited in his ability to speak and walk. He left New Jersey with Ryan Freilino and Marianne Macy and I accompanying him back to Tisbury at the end of September 2009 and after a week at home proceeded on to Rome for ICCF15. The first morning he stood, as the Martin that we all had remembered, and addressed the conferees, standing throughout his talk in full voice. The warmth of his reception by the assembled cold fusion research community gave him a great boost and prompted him to respond that he hoped to get back in the research game again.

On the evening he received the Toyoda Medal at Castel Sant'Angelo, his daughter Charlotte, who was most like her father of his children, gave an enthusiastic talk that valued Martin's discoveries and his presence on this occasion. He was pleased, but not until we were standing on the roof looking toward the Vatican on that starry night did I appreciate how his sense of the wonder of civilization's work and cumulative beauty that lay before was a treasure that was deep in his soul. We would not see him again on this public stage. Last December, Marianne and I stopped by Tisbury to



Photo courtesy of Mike McKubre

Martin Fleischmann addresses the crowd at ICCF15 in Rome, as Francesco Scaramuzzi listens, October 2009.



Photo by Dave Nagel

Martin Fleischmann proudly views the Toyoda Medal presented to him by the ISCMNS at ICCF15, October 2009.

visit with Sheila and Martin. He now was confined to the downstairs bedroom with a great view of the English countryside. Although no longer able to speak, he was able to express himself and lying on his lap were two copies of *New Scientist* that he had been reading. We spent an hour together as I brought him news of the continuing flow of results that he and Stan had unleashed. He would laugh, grimace and otherwise communicate and I believe that he knew that his probing of the contradictions of our models of physical systems, including the biological ones, had probably produced a discovery that is likely to be the most important one that physical science has offered the world—the discovery that nuclear systems can be manipulated by chemical systems. I will miss him for all the reasons that we miss a good friend, but we will thank him for the gift of expanding our curiosity and showing us how, in the face of our critics, to maintain a sense of humor and balance and make progress.

**Fred Jaeger**  
(Former CEO, ENECO)

I first met Martin in the picturesque Ville de Valbonne in southern France where the Japanese entity Technova sponsored research for him and Stan Pons in nearby Sophia Antipolis after they left Utah. As CEO of ENECO, I made numerous trips to Valbonne to secure Martin and Stan's permission to license their original patent rights from the University of Utah and to cross-license any Technova improvements; so commercial entities would have convenient one-stop access to combined technologies, should they become scalable.

Those were happy times for Martin and Stan, where they were once again able to quietly work together away from the glaring distractions of press and critics. Work days were often followed by long and enjoyable dinners where the three of us would engage in discussing every conceivable topic. Martin had the unique intellect and ability to open several different topics, discuss each in random rotation, and bring it all to a sensible conclusion. His wry sense of humor and endless suspicion infused every topic with his unforgettable twist.

In later years, Martin returned to his Tisbury home to work through prior calorimetric data and review theoretical aspects, yet he always had an experimentalist urge. I fondly remember driving around deserted Tisbury breweries with him to seek out suitable quarters to setup a small lab to continue certain experiments he was keen to perform. As an overnight houseguest, I got to know Martin and Sheila on a more personal basis and to observe how comfortably and lovingly they interacted with each other during his golden semi-retirement years. They were truly a handsome and dignified couple.

I have one particular recollection of Martin sipping a cocktail in his living room easy chair while vigorously discussing the usual concurrent multiple topics when a fly landed on his arm. Without missing a syllable or change in voice inflection, he calmly stood up and escorted the insect safely back outdoors through an open window. Yes, this gentle man wouldn't even hurt a fly.

His early Southampton work involving the unique hydrogen storage capacity in Johnson-Matthey Pd samples undoubtedly focused his attention and curiosity on experimental design at the University of Utah. Martin was a fear-

less and inquisitive scientist. His bold, pioneering work paved the way for other scientists to approach topics in new ways and to discuss new perspectives. The work of many great scientists, artists and writers often is not adequately recognized during their lifetimes. Martin's scientific contribution will evolve in coming years and provide him the degree of immortality that comes with lasting works of beauty. My heartfelt condolences go out to Sheila and his family. We have lost a dear friend, and the world has lost a wonderful human being.

**Dr. Irving Dardik and Alison Godfrey**  
(Retired Vascular Surgeon, Founding Chairman U.S. Olympic Committee Sports Medicine Council / LifeWaves International)

Martin and Sheila Fleischmann lived with us from the end of June to the end of September in 2009. When they first arrived, Martin wasn't able to participate in conversations, but remaining true to his unflinching commitment to reawakening his abilities, he grabbed life by the horns and made grand changes. In September, we hosted a wedding for close friends. Martin strolled in, having dressed himself in a suit, and joined in the celebration with full gusto. By this time he was able to engage in scientific discourse and battles with close friends, lighting up the house with his brilliance and "cut to the quick" comebacks.

Martin always sat at the head of the kitchen table. Most of the time we would think that he wasn't listening to the banter at all. Then like a firecracker, Martin would burst into the conversation with a spot-on, very British quip, making everyone dissolve into laughter. This is the Martin that we hope you knew: observant, witty and a pleasure to be with.

An artist, and childhood friend, who also stayed with us that summer, was able to encourage Sheila to paint again. Every day Sheila would present us with another watercolor which would create fierce negotiations over who was to become the proud owner. Sheila was fiercely devoted to Martin, giving encouragement and support day in and day out.

The most meaningful moment came during ICCF15 in



Mike McKubre, Irv Dardik, Niamh Sullivan and Martin Fleischmann, Summer 2009.

Rome when Martin elegantly presented at the conference and graciously received the prestigious Minoru Toyoda Gold Medal at Castel Sant'Angelo. His daughter, Charlotte, was in attendance, and spoke about the changes that had come about for Martin over the summer, thanking us for giving her back her father.

We will be forever grateful to Mike Melich and Marianne Macy for giving us the gift of living with Martin and to Mike McKubre who spent so much time with us helping Martin come back.

### **Dr. Stanislaw Szpak**

(Retired, Space and Naval Warfare Systems Center, SPAWAR)

I met Prof. Fleischmann in the mid-1970s. The occasion was a review of ONR sponsored research in Boston, MA. It was a Sunday afternoon. My friend, Ted Katan, and I after arriving from California, decided to go to the hotel bar to see who else had arrived. The bar was empty. We ordered drinks and talked "shop." A little later, a person walks in, looks around, orders a drink and sits not too far from us. After awhile, he said something like, "I am Martin. I am here for the same reason as you are." We started to talk; my "strange" English aroused his curiosity. I told him that I grew up in Poland and he told us about his Czech connection. I noted that we were having a Slavic "meeting" because Ted had Russian roots. We talked about what is common to all Slavs and what separates them. I could not agree with all his arguments but I was impressed with his assessment and understanding of the past and present.

The chat with Prof. Fleischmann ended when more people came down. The next day, the scheduled review ended. Listening to his presentation and comments, Ted and I, painful as it was, concluded that we were not playing in the same league. From that time, as well as today, as I read his papers, I ask myself: "Why has he undertaken this particular research?" During the ICCF1 meeting, he provided an answer: "We, for our part, would not have started this investigation, if we had accepted the view that nuclear reaction in a host lattice could not have been affected by coherent processes." Key words—"if we had accepted the view."

The March 23 announcement was not a great surprise because I knew that Prof. Fleischmann was involved in research on a potentially inexhaustible energy source. What I did not know was that it involved nuclear reaction. For the next month I followed the work of others, professionals and amateurs, and could not accept the view that there is only one way to proceed. Following the well established procedure, I prepared cathodes by Pd+D co-deposition. The results were more than satisfying. I visited Prof. Fleischmann to present the results and get his opinion. Discussing the merits of this approach, he noted that this is "an interesting new variant" worth further study. We discussed the pluses and minuses of the procedure. It is gratifying that he used some of the results in his lectures (X-ray emission and hot spots).

During the time period from May 1989 to October 2000, I met with Prof. Fleischmann a number of times. As I think of them today, the meetings were very satisfying, both from scientific and personal points of view. The last time I saw Prof. Fleischmann was in October 2000. Frank Gordon scheduled a meeting to assess: Where are we and where are we going? Instead of participating in this meeting as origi-

nally planned, I had emergency open heart surgery. A few days later, he and Frank visited to wish me a speedy recovery. Prof. Fleischmann was a gentleman and an extremely gifted scientist.

### **Dr. John O'M. Bockris**

(Retired, Distinguished Professor of Chemistry,  
Texas A&M University)

Martin Fleischmann attended my twenty lectures in electrochemistry as an undergraduate and applied to me to accept him as a graduate student for the Ph.D. I was only in my third year on the faculty of the Imperial College of Science and Technology—Britain's MIT and a part of London University. I already had ten graduate students so I recommended Martin to work with J.F. Heringshaw, one of my colleagues who was open to students showing an interest in electrochemistry. Fleischmann worked for his Ph.D. on the diffusion of hydrogen in palladium which, of course, set him up for the contribution he made in collaboration with Stanley Pons some 42 years later.

My lively group of ten (which included both Roger Parsons and Brian Conway) attracted Martin during his Ph.D. studies and we saw a lot of him. He visited my group not only to discuss his work but upon at least one memorable occasion, came with us on one of our social gatherings.

Fleischmann contributed a distinguished career in electrochemical research, mostly at the University of Newcastle where an active group in electrochemistry, under the direction of the eminent electrochemist Reginald Thirsk, already existed.

The first contribution to the literature bearing Fleischmann's name came in the early 1960s about the first step in metal deposition of identical metals. Microelectrodes were just coming into use in the mid-1960s and Fleischmann made some distinct contributions here and wrote a book on the subject. It was very much fuel cell theory time (1967) and the interplay of electron transfer concentration of radiants and IR drops in the pores of fuel cell electrodes gave opportunity for Fleischmann to show skill with which he could handle complex electrochemical situations. I used some of the equations he originated in these systems later. Fleischmann came along with attention to optical method for looking at surfaces covered with solution.

Largely on the basis of the papers he had authored at Newcastle, Martin Fleischmann became Professor of Electrochemistry at the University of Southampton. Martin was also honored by the Royal Society of London with a medal for electrochemistry and thermodynamics.

But time was getting on and Martin had already met Stanley Pons, head of the Chemistry Department at the University of Utah. No one except the retired Pons knows how the scientific collaboration began. But, just after they had announced their amazing claim that they had discovered an unknown nuclear reaction to be accompanying the electrolysis of deuterium oxide, I was with them on two occasions and heard about long walks around Salt Lake, and that the idea that there must be a nuclear component evolved during those walks.

The reaction to the claim was—as we all remember very painfully—a huge explosion of rejection, ridicule and nastiness the likes of which no one I met in the U.S. at that time

could remember. With his position on the committee and charge of a Max Planck Society in Germany, Martin now could have justified being called “famous” in February 1989. But by the end of March, he and Stan Pons were instead called “infamous” by U.S. physicists. “Anyone who dares...”

Well, I had been with him in his thesis days and although we were not “friends” as many thought because they heard we were of the same alma mater, I still felt I had to do what I could. It happened that at the time I had 23 collaborators in my team so I was in a position to see if...

Many of our first attempts failed but then in August 1989, Nigel Packham, a member in my group, came back from taking one of his samples to a technician in the nuclear group at Texas A&M who could analyze for tritium. “This one is full of tritium,” Packham reported. It meant a lot to all of us and to Fleischmann in particular.

I tried to call Martin but he was not in. However, Mrs. Fleischmann was there and she repeated what she thought were my words, saying, “You’ve got chickens?” “That’s about it,” I said.

It was a horrible period in our lives and I think in the life of science in America. I recall how my wife and I suffered. Most know that Mr. Toyoda, of the company Toyota, rescued Martin and Stan and set them up in a laboratory at IMRA (part of Technova Corporation, a subsidiary of Toyota) in France. Free from the controversy and attacks, in idyllic surroundings, Martin and Stan might well have been expected to have really made progress and quelled the doubts. At first, it seemed to go well. Fleischmann was keeping quiet but Stan Pons believed in publicity and kept the good news flowing.

But then the good news ceased. “What caused the break-up?” people asked and I can only say that it seemed all the good results they had got before could not now be repeated.

Fleischmann retired and returned to England. Pons, with support from a wealthy family, stayed in France.

But Martin Fleischmann was not the kind of fellow who creeps away, beaten. He boldly pushed on in Bury Lodge in Tisbury soon after leaving France and he commenced a collaboration with Preparata, an Italian physicist who told me he was the greatest physicist in Europe! He worked theoretically on “cold fusion” and applied some thoughts which he said explained the earlier Pons and Fleischmann results. Fleischmann and Preparata saw very much together and had often spoken of quantum electrodynamics (QED). He, too, had a new theory which interpreted the earlier results. (It is unclear to me to what extent it was a new theory or was it following Preparata?)

Fleischmann continued to appear at the yearly meetings on cold fusion, and made useful contributions, even at one event after it became known he was suffering from Parkinson’s disease.

So when the bad news came, it was no surprise. He was controversial indeed, a brave and honest plow into the future. I have nothing to say about him that is not good.

### **Dr. Melvin Miles**

**(Retired, Naval Air Warfare Center Weapons Division  
at China Lake / Adjunct Faculty, University of La Verne)**

I first heard the 1989 Fleischmann-Pons announcement of cold fusion on the radio as I was driving to my work at the Naval Weapons Center at China Lake. I was already using

palladium/hydrogen as a reference electrode in my research work as an electrochemist. My first reaction was how great this sounded and why had I not thought to do such experiments. Everything I needed for these experiments was already available at China Lake, and I started my own experiments that same weekend. I quickly realized that this was not a simple high school experiment done in a mason jar, as critics liked to describe it, but instead was very challenging. The major problem was to find an accurate method to do the calorimetry. David Stilwell, a post-doc student at China Lake, worked with me as we tried various calorimetric approaches. Within three months, we had found no excess power for either Pd/H<sub>2</sub>O or Pd/D<sub>2</sub>O, *i.e.* 0±4% excess power for each. I was getting telephone calls from a DOE representative about my experiments, and I reported no excess heat effects. After about six months, we tried a new palladium rod from Johnson-Matthey and found our first excess heat effect. I reported this to DOE, but I was still listed in the November 1989 DOE Report with groups that found no excess heat or power.

I first saw Martin Fleischmann and Stan Pons at the May 1989 Electrochemical Society meeting in Los Angeles. David Stilwell and I arrived early to get a first row seat, but then they cleared the large conference room. We wound up stuck at the entry door as a huge crowd was pushing forward. This could have been a fatal situation, but then the doors were opened and we found our original front row seats. I was impressed by Martin’s handling of questions and criticisms and by his video that showed how quickly red coloration added to the cell becomes mixed in response to stirring issues.

I next saw Martin Fleischmann at the first cold fusion conference (ICCF1) at Salt Lake City in 1990. I still only knew Martin from a distance, but I was again impressed by his skill in answering questions from critics who were obviously trying to ridicule cold fusion at this conference. I gave my first cold fusion presentation in Salt Lake that showed ample excess heat for the Johnson-Matthey palladium. I recall that Stan Pons quickly came over to congratulate me on my presentation. I later realized that my results were in excellent agreement with the F-P experiments. Several people commented that China Lake was the only group listed in the DOE Report for no excess heat that later reported excess heat effects. This was simply being honest and reporting what was found in my experiments.

I came to know Martin better at ICCF2 in Como, Italy. There were no distractions from the news media or from critics at this conference. The reporting of important new results made this one of the best cold fusion conferences.

In the following years, Martin began writing me long letters about calorimetry, such as errors in the Caltech, MIT and Harwell work. I incorporated some of this in my calorimetric paper published in the *Journal of Physical Chemistry*.

I recall one long letter where Martin responded almost line by line to the NHE paper published in the Monaco ICCF5 proceedings that was critical of the F-P calorimetry. Although NHE used the F-P Dewar calorimeters, many mistakes were made in the NHE analysis of the data. In 1997-1998, I worked at NHE and used these same Dewar calorimeters. It became obvious to me that large mistakes were made by the NHE methods of analysis.

In 1998 at the Vancouver ICCF7 meeting, Martin and I decided to work together on the analysis of the NHE data

that I had obtained in Japan using the F-P Dewar calorimeter. The first analysis was on the study of the Pd-B experiment. This led to Navy publications at both the NRL and SPAWAR laboratories. I was amazed at Martin's analysis of the data and the information that he extracted from the data. In my opinion, no one else in the world could have matched Martin's data analysis methods. Many scientists probably would not even understand his mathematical methods. Although I had used a much simpler method of analysis that was less accurate, Martin and I were in good agreement with the various rises and falls of the excess power effect. Martin later completed his analysis for one of the three co-deposition experiments done at NHE, and this also agreed with my simpler analysis. This was published in *Thermochimica Acta* with Stan Szpak and Pam Boss of SPAWAR.

At ICCF8 (Italy) in 2000, Martin met with me to discuss his concerns about a possible cold fusion weapon. In Martin's words, "this should have never seen the light of day" but instead should have been classified and kept secret. My wife, Linda, videotaped this conversation with Martin. Martin wanted me to report his concerns about weapons to the Navy. I did this upon my return to China Lake, but I never heard anything back. However, I was invited to a meeting at the CIA back East and reported on Martin's concerns. Off the record, one CIA person told me that the Russians had been loading wires with deuterium and then exploding them. He stated that the 1989 announcement by Fleischmann and Pons caused one Russian to comment: "Maybe this is how it works." I hope cold fusion weapons do not work because they would make the world much less safe.

This reminds me of a story that Martin told me more than once. After the 1989 press conference in Salt Lake City, Martin had a flight back to his home in England. However, this flight was diverted to San Francisco and then cancelled. The passengers were taken to a hotel for the night. Soon after Martin entered his hotel room, the telephone rang. Martin answered and was told, "This is Dr. Edward Teller and I need to talk with you." Within a few minutes Dr. Teller was in Martin's room for a discussion about cold fusion. In the U.S., Dr. Teller is known as "the father of the H-bomb." It should also be noted that Dr. Lowell Wood, who worked with Dr. Teller, attended many of the cold fusion conferences.

Martin would write papers during the 2000-2005 years and add my name to them. Often, I did not do much except for proofreadings. Martin wanted me to get them published in major journals such as the *Journal of Physical Chemistry*. I kept trying but often could not even get past the editors. If they were sent out for review, it was obvious that only arch-critics of cold fusion were being selected as reviewers. The end result was that these papers were never published.

I am grateful that I had the opportunity to meet and work with Martin Fleischmann. He will be greatly missed both as a great scientist and a good person.

### **Dr. Thomas O. Passell** (TOP Consulting)

The announcement of March 23, 1989 by Fleischmann and Pons hit me and my colleagues at Electric Power Research Institute like a steam locomotive. Everyone was scrambling to understand the implications if it were con-

firmed. Many if not most of the department heads assigned a staff person to follow the developments. Many such managers were obviously hoping that the possible disturbance to the status quo would dissipate—which it did after the ERAB Panel Report of November 1989. There was an implied threat to each department's status if fusion were to be a reality. People in the Exploratory Research Division were excited about the possibilities. Individual project managers with relevant technology for proving or disproving the claims immediately pressed their contractors to set up tests to check the claims. I was a project manager of research projects on ways of reducing corrosion in nuclear (fission) power plants in the Nuclear Power Division. As such I was one of those project managers with a headstart on testing the claims. As you might imagine, the immediate promise or threat would be expressed most quickly in adjustments in our research budgets, so it had everyone's attention among our ~300 technical staff members.

My project with SRI International was particularly well positioned to try reproducing the claims. First it was a project designed to measure the level of dissolved hydrogen gas in a high temperature, high pressure loop of a pressurized water reactor. (Hydrogen gas was there for two purposes: the first to suppress oxidative corrosion of piping and core internals and the second to suppress the radiolytic decomposition of water by gamma radiation.) They were using a "dip stick" of a coil of palladium metal wire in the reactor coolant water. Palladium's electrical resistance was subject to changes as a function of hydrogen gas dissolved in the palladium metal matrix. Also, hydrogen gas dissolved in the reactor water was able to spontaneously enter the palladium metal wire. Those resistance changes could be easily measured as a dissolved hydrogen gas continuous on-line probe. So, for me, all the technology planets were lined up in a row! But there was a final factor. The project manager at SRI was Michael McKubre, who was personally acquainted with Professor Martin Fleischmann from his post-doctorate fellowship years at the University of Southampton in the United Kingdom. His Ph.D. thesis was on the subject of electrochemistry!

My first act was to request permission to switch the objective of the SRI project to attempt an observation of the claimed excess heat episodes in deuterium-loaded palladium. McKubre's high respect for Fleischmann was such that he was confident he was not dealing with either a fraud or a foolish mistake. So his efforts were infused with confidence that he would succeed. As it turned out, his persistence and patience paid off with the appearance of excess heat episodes which eluded many less confident investigators. This was especially true of outside investigators whose institutions were fearful that the claims were true, so they knew the correct answer for their institution was a negative result. In electrochemistry it is very easy to get a negative result. With Joseph Santucci's leadership and support from EPRI upper management, the SRI project expanded rapidly until a fatal accident in the SRI laboratory on January 2, 1992. Recovery from that accident and rebuilding a new, safer laboratory required almost one year, after which more results were obtained of a nature that encouraged continuation. The repetition of excess heat episodes was still not straightforward or simple, so the project finally terminated at the end of 1994. However, other funding agencies kept the extremely well-equipped laboratory at SRI alive, even to the present day. I



have always remained proud of my role in EPRI's \$6.5 million investment in the SRI contract which succeeded in confirming the claims of Fleischmann and Pons but were unable to take them to a commercially useful level of reliability.

After retirement in 1997, I have retained a strong interest in resolving the controversy over cold fusion. There are numerous indications, in the many research efforts spawned by the Fleischmann and Pons 1989 announcement, of other nuclear reactions besides that of the fusion of two deuterons to helium-4. I am currently intensely reviewing the literature at lenr-canr.org for evidence supporting even more surprising nuclear reactions with potentially equal or greater significance than simple deuterium fusion.

Did the Fleischmann-Pons March 23, 1989 announcement change my life? You bet it did! For which I am extremely grateful. I now have a laboratory of my own to pursue the glow discharge method of loading deuterium into palladium, titanium and other hydrogen-absorbing metals. The matrix of possible combinations of metals, gas pressures, voltages and current protocols is extremely large. It is a pleasure to have something game-changing to do beyond playing bridge with my aging friends! The list of experiments is so large that it should last as long as I do even if I live to 110, as one man was reported recently to do.

### Dr. Jean-Paul Biberian

(Editor, *Journal of Condensed Matter Nuclear Science*)

The 1989 public announcement by Fleischmann and Pons had a dramatic impact on my life as a scientist, but also as a human being. This gave me the opportunity to work on the most challenging research there possibly is—a solution for the world energy problem. I did not start research on cold fusion right away, but did it in 1993, as soon as I realized that I could bring some contribution to the field. I have always been an enthusiastic scientist, but working in cold fusion made my life even more exciting. Thanks to Martin Fleischmann, I became addicted to this endeavor.

I met Martin Fleischmann at each international conference or workshop he went to, and I was always very impressed by the depth of his knowledge. He was a great scientist, and I was sad to feel that their discovery hurt him deeply. The last time I met Martin was at the Rome conference, and I had a picture taken with him. I felt it would be my last chance.



Jean-Paul Biberian and Martin Fleischmann at ICCF15.

I heard about Martin before cold fusion. I had been doing work in the field of surface science, and I remember when surface enhanced Raman spectroscopy was discovered and announced by him. It was an important discovery, quite unexplainable.

Martin and Stan were very helpful in supporting the research on cold fusion in France. My colleague Georges Lonchampt met them several times in their laboratory in the south of France, and Martin gave him two of their Icarus 2 cells, as well as all the details about how to be successful. I am therefore very grateful to him, as without his help there would be much less research on cold fusion in France.

### Dr. George Miley

(Professor Emeritus; Nuclear, Plasma and Radiological Engineering and Electrical/Computer Engineering Departments, University of Illinois at Urbana-Champaign)

I was introduced to cold fusion several days before the Fleischmann-Pons announcement when Steve Jones from BYU called me to inquire about submission of an article on it to *Fusion Technology*, which I was editor of. Steve said that he was not sure if I would think cold fusion was appropriate for inclusion in the journal. I asked him what cold fusion was, but did not have time to digest his comments because I had to catch a flight to Tokyo to attend a meeting. When I landed, my host from the University of Tokyo greeted me waving a copy of the *Wall Street Journal* which featured a main article on "fusion in a test tube." They thought I would be able to explain this to them, but unfortunately I had not had the time to discuss it with Steve. As is well known now, Steve Jones' research was one of the factors that caused the premature public announcement by Fleischmann-Pons. I never received the article from Steve, although I had told him to send it to me. It apparently ended up in *Nature*. When I returned home, a number of students in my department came to see me wanting to set up a duplication experiment. We initially did that, but later switched to different types of loading techniques based on use of plasmas which I knew more about than electrolysis. Needless to say, this experience and the initial excitement set me off on cold fusion research (which I now term low-energy nuclear reactions, or LENR, due to involvement of transmutation reactions). I was convinced that this area of nuclear reactions in solid state deserved basic study, so was not deterred when many claimed that the Pons/Fleischmann experiment did not work.

The first time I met Martin Fleischmann was actually in the initial congressional hearing held shortly after their announcement. The purpose of this hearing was to help Congress decide if this discovery warranted a national program to develop the technology. The congressional assistant who called me to serve on the hearing said they wanted someone recognized for innovative research and an open mind, not someone who was already involved in the area. I agreed. As it turned out, I was placed in the hearing between testimonies by Martin and Harold Furth. At the time Harold was head of the Princeton Plasma Physics Laboratory, and since I had been working on hot fusion I knew him reasonably well. Harold was dead set against cold fusion, having learned that duplication experiments were generally failing and fearing that work on it could take funds from the hot

fusion program. He and Martin were both outstanding speakers and debaters. One example that I recall well is that Martin claimed that cold fusion had already demonstrated energy breakeven, whereas hot fusion had years to go before it could claim that. Harold responded by saying that Martin's definition of breakeven was faulty, and that his experiments themselves were faulty. I tried to stay out of that debate, but in my talk I did inject some thoughts following up on Martin's testimony. One was to suggest that use of a deuterium-tritium electrolyte might produce copious 14 MeV neutrons, providing a cold fusion neutron source. During a break in the hearing, Martin approached me and said that it was an interesting question, but he was convinced that such a neutron source would not work. In any case, this first personal meeting with Martin turned out to be one of many. Our paths crossed at various International Conference on Cold Fusion (ICCF) meetings, and also at a series of meetings in Italy. Martin was an advisor to the cold fusion group at the Frascati INEA laboratory outside of Rome so he made frequent trips there. Martin is well known as a leader in electrolytic experiments and theory, but I found his overall knowledge of science was spectacular. In addition, he was always quite willing to discuss others' research. Thus I described aspects of my LENR work to him several different times and he was quite willing to listen and provide advice. Indeed his advice was often right on and I implemented some of his suggestions into my work.

There is no doubt that Martin's work with Pons has already had a great effect on the scientific community by bringing scientists from over the world to the field despite the turmoil that developed around the original public announcement. I am now convinced, as many others in the field are, that their experiment will be shown beyond any doubt to have been correct. Their work will be recognized as the start of this new field of condensed matter nuclear science, which appears destined to play a large part in society's future energy sources. All of the unfortunate attacks on Fleischmann and Pons will be forgotten while their tremendous contribution to society will be remembered in future histories of science.

### **Dr. Irina Savvatimova**

(Scientist, Federal State Unitary Enterprise LUCH)

Martin Fleischmann has influenced the work of every scientist involved in cold fusion research. There is no doubt that Fleischmann and Pons took a great risk in announcing this new direction of research. It is thanks to their courage in communicating with the press and the world scientific community that the new era of cold fusion research began.

Immediately with the March 23, 1989 announcement, many scientists remembered encountering their own unusual anomalous effects related to physical processes (plasma electrolysis, gas discharge, etc.). More than twenty years before, Russian scientist B.V. Bolotov registered neutron emission during electrolysis; A.S. Filimonenko suggested the device for changing radioactivity. But only Fleischmann publicly announced to the world that there was a possibility to synthesize elements using low-energy processes. It was a very bold statement at that time, but also boldness of thinking. Many researchers were inspired by his and Pons' public statement and immediately remembered their unusual

results and resumed the research in this field with their own background, experience, results and opportunities.

We (Kucherov, Karabut, Savvatimova) shared this fate. As soon as we heard the announcement, we started to actively study the plasma effect registration in the glow gas discharge. I had previously researched the damage effects of the first wall of a thermonuclear reactor by low-energy hydrogen and deuterium ions. I used the glow discharge as the source of low-energy hydrogen and deuterium ions. The parameters of the most intense damage effects had been identified. Therefore we had all the necessary equipment for this process, including palladium, hydrogen and deuterium, and were ready to begin deep study of the physical process accompanying cold fusion phenomena. And when Yan Kucherov asked to look at my installation, I understood that he would offer to involve in me in cold fusion research. He did and I said: "Yes." After the visit, they promised to find me a device for registration of neutrons and within a few days we registered neutrons after switching off the gas discharge. This was the effect which Fleischmann called very exactly "life after death." We were very excited and could not sleep (Kucherov and I experienced similar feelings). After a month, we sent the results for publication in the Russian journal *Uspekhi Fizicheskikh Nauk (Advance in Physical Science)*. Our paper was rejected and returned to us with the inscription, "Not approved for publication." After that we filed two applications for patents: on the application of glow discharge as a source of neutrons (we got approved) and on the use of glow discharge as a source of heat (we did not get approved). At first application we received an inventor's certificate.

This work did not bring me any advance in my career and no increase in my salary. However, thanks to cold fusion I met many scientists who did research in this direction, not only in Russia but around the world. I met an enormous number of scientific enthusiasts willing to spend their own funds for research on the study of cold fusion phenomena. I am so thankful for this fate of mine and of course to Fleischmann personally.

Absolutely the name of Martin Fleischmann will remain in the hearts and souls of the scientists who were inspired by him to work in cold fusion. Undoubtedly our positive results in this direction, including a 1992 publication in *Physics Letters*, were initiated by the Fleischmann-Pons activity.

### **Dr. Pamela Mosier-Boss**

(Chemist/Visiting Scientist, Massachusetts Institute of Technology)

The world has lost a great man of extraordinary vision, curiosity, humor and courage. Martin Fleischmann has impacted the work of many, including me, and he will be sorely missed. He discovered the giant Raman effect, which later became known as surface enhanced Raman spectroscopy (SERS). SERS holds the promise of detecting and identifying ultra low concentrations of molecular species. He pioneered the field of *in situ* spectroelectrochemistry, which enabled scientists to determine electrochemical reaction mechanisms by monitoring the spectral changes of the electroactive species on the electrode. He pioneered the use of ultramicroelectrodes (UMEs), which allowed us to probe electrochemical reactions in the gas phase, solids including ice, and in brain cells. He was the first to demonstrate the use of fiber optics to obtain Raman spectra.

As for LENR, Martin Fleischmann and my colleague, Stan Szpak (another renowned electrochemist), knew one another. Stan knew of the experiments Martin and Stan Pons were doing with palladium and heavy water and knew about the long incubation times. To overcome the incubation issues, he developed Pd/D co-deposition that in turn led to an amazing 23 year odyssey of discovery and exploration. It wasn't always a smooth ride, but it was one we would not have undertaken if we didn't have confidence in Martin's findings.

## **Dr. Mitchell Swartz and Gayle Verner** (JET Energy and Cold Fusion Times)

Intellectually honest and always approachable, Dr. Martin Fleischmann is this century's most unsung hero.

His actions were those of a consummate seeker of the truth, as he was involved in two of the most important things in life: learning and teaching. He taught at King's College, Durham University (later, University of Newcastle-upon-Tyne) and the University of Southampton.

Martin was a brilliant scientist and the co-inventor of cold fusion. He discovered, explored and taught the scientific fields of surface enhanced Raman scattering effect and the successful achievement of lattice assisted high energy production using nuclear reaction enabled by a palladium lattice, electrochemically loaded (cold fusion).

In doing the earliest experiments and initial report, they created this new field in solid state nuclear physics which has not been like other scientific fields. The road to cold fusion has not been a smooth and easy path. At one point, experimentalists only had physical barriers to deal with, but Martin (and colleagues behind him) have had to deal with skeptics and third-rate thinkers who have sadly been in charge. Unnecessarily, they made life difficult for him and everyone else in the field that Martin had just created.

The question was, or should have been: How can we make fusion work at lower temperatures? Martin, graced with determination and acumen, forged ahead as the leader in what will be the world's never-ending quest to put cold fusion and clean energy production on the map. His actions brushed aside the naysayers, burly bureaucrats and their stale comments.

Martin attended the international conferences on cold fusion that followed his discovery, and paid close attention to its development. He had a well-deserved rock star status in his newly created field. His footprints were so deep that his questions and supportive comments were the things that kept us going through much travail that followed.

Fortunately, Martin lived to see the beginning of the "phase change" of social awareness to cold fusion, but not

yet its adoption or implementation.

Martin (and his two partners) were the first to achieve the purposeful attainment of cold fusion (fusion of deuterons to helium-4) using applied electric fields and a lattice and salty heavy water. It was not the most efficient way to do cold fusion, but as the cat whisker junction is to the Internet, Dr. Martin Fleischmann's contribution in cold fusion will be to enable space travel, fully powered artificial internal organs, and more.

We will never forget his contributions, and will miss his clear, reasoned thinking. The world is slowly, but inexorably, moving toward a better place because of Martin Fleischmann's transit through it.

## **Dr. Frank Gordon**

(Retired, Space and Naval Warfare Systems Center, SPAWAR)

While we are all saddened now, I remember Martin for his smile, quick wit, genius and determination. It is a rare individual who can withstand the personal attacks that he endured and continue to smile and keep going. I'm sure that everyone who knew him has good memories from their friendship. He modestly recognized his contribution with this quote: "Scientists are really very conscious of the fact that they stand on the shoulders of an enormous tree of preceding workers and that their own contribution is not so enormous." We are all standing on the shoulders of Martin who, I'm sure with a smile on his face and a twinkle in his eye, added his own limb to the tree of knowledge. We can all honor his memory by continuing the research that he started until his limb becomes recognized for the revolutionary scientific breakthrough that it represents.

## **Dr. Edmund Storms**

(Retired, Los Alamos National Laboratory)

Martin demonstrated that Nature has a diabolical plan. He and Stan were not the first to cause the LENR process but they were the first to attract attention. For that they paid the price Nature always extracts when a great discovery is made. They attempted the "impossible" based on a flawed model, using lucky material that most people could not duplicate, and stirred up a firestorm of antagonism from people who were their colleagues and friends. They were rejected for reasons both ignorant and self-serving by people who we all thought should know better. Sadly, Martin did not live long enough to say he told them so, and have the last laugh. Hopefully, the rest of us can complete the process and gain acceptance for what he and Stan paid such a dear price to make known. We will all miss the man who led all of us into this crazy field.

## **Lawrence Forsley**

(President, JWK International Corporation)

I had the opportunity to meet Martin Fleischmann for the first time at ICCF7 in Vancouver, British Columbia, though I'd become familiar with cold fusion soon after the announcement in 1989. Nearly half my time since then has been spent pursuing cold fusion in a variety of labs with peo-



Martin Fleischmann and Mitchell Swartz at ICCF7 in Vancouver, Canada, 1998.

ple around the world. One of my most memorable meetings was when Martin, his wife Sheila and I got together for lunch at the Forrester Inn near their home in Tisbury, UK, and they brought along their dog, Mouse. Mouse hovered throughout lunch, being well behaved. However, shortly after our just desserts arrived, our attention was drawn to one side. Out of the corner of my eye I saw, too late, Mouse. She had been eyeing our desserts, and with our eyes turned away, she darted, head level to the table, and snatched a pastry.

### Dr. Emilio Del Giudice/Dr. Antonella De Ninno (Retired Scientist / Scientist, ENEA)

Martin Fleischmann's life has been marked by an endless endeavour for unearthing the deep mysteries of the dynamics of Nature. In this endeavour he studied passionately the efforts of other pioneers; he has been an archaeologist of science, as he defined himself, and looked at himself as a component of a "coherence domain" of scientists tuned to each other and deeply devoted to understanding Nature. This has been always the task to which Martin dedicated his life, sacrificing to it his health, wellbeing, career, reputation. We are privileged for having been friends of Martin and members of his "coherence domain."

Martin's main achievement has been the conception of the cooperation among all the components of physical reality as the main feature of the dynamics of Nature. He realized that this conception is in opposition to the dominant paradigm where macroscopic phenomena are almost always "reduced" to the independent dynamics of microscopic components connected basically by pairwise interactions. The only exceptions allowed in this scheme occur at low temperature, the only physical range where quantum field theory is accepted to explain phenomena. Martin decided to overcome the difficulty searching for phenomena not explainable in the framework of the prevailing paradigm but understandable only in the context of the quantum dynamics of collective processes. He used to say that condensed matter, in special circumstances, is made up of plasmas of electric charges: "(an) extremely dense proton plasma is present in an electron concentration of 600-1000M...we are driven to the conclusion that a satisfactory explanation of the stable existence of the dense plasma must be based on an appropriate many-body model" [M. Fleischmann, S. Pons and G. Preparata, 1994. "Possible Theories on Cold Fusion," *Il Nuovo Cimento*, 107A, 143-156].

He searched for evidence confirming this intuition in many different fields. The list spans almost the whole physics of condensed matter.

In 1974, Fleischmann first studied the strange phenomenon known as SERS (Surface Enhanced Raman Scattering). Martin held the collective oscillation of the surface electrons of the metal responsible for the EM field whose coupling with the atoms of the metal produced the enhancement.

Another phenomenon which attracted his historical interest was the Cöhn effect, discovered by Alfred Cöhn in 1929 and almost forgotten in the following decades in spite of the opinion of Walter Nernst who, in a letter retrieved by Martin, considered the Cöhn experiment one of the most important in its century. Martin had a special ability in rediscovering findings neglected by modern science. The Cöhn discovery tells us that hydrogen (and its isotopes) dissolves



Larry Forsley, the Fleischmanns and Mouse the Dog enjoy dessert, Tisbury, England, October 2007.

in metals not as neutral molecules or atoms but in ionic form. Hence there must exist a lattice dynamics able not only to split hydrogen molecules but also to ionize hydrogen atoms. As an electrochemist, Martin was impressed by the analogy with the phenomenon of Arrhenius dissociation in electrolytes where neutral molecules in water split spontaneously in two interspersed plasmas of positive and negative ions. In a prominent article [B. Dandapani and M. Fleischmann, 1972. "Electrolytic Separation Factors in Palladium," *J. of Electroanal. Chem.*, 39, 323] he reported the hints of a sort of phase transition occurring in the metal hydride upon changing the concentration of hydrogen: at lower concentration, the oscillations of hydrogen atoms around their equilibrium positions are narrow but became much more wide whenever the concentration increases over a threshold, appearing as a plasma oscillating as a whole within the metal.

Here is the conceptual origin of cold fusion. Is it possible that an ensemble of like charges, which at low density obviously repeal each other according to the general laws of electrostatics, could give rise to a many-body attraction? This chance, namely that above a certain threshold of density "like likes like," has been proved in more recent years in other fields of science. In 1997 Larsen and Greir reported that ensembles of negatively charged beads of microspheres suspended in water form stable regular arrays known as colloidal crystals. The structure and dynamics of such crystals show the evidence for a strong long distance attraction non-compatible with the Coulomb repulsion law.

The assumption of a long range many-body attraction among like charges is in fact the theoretical basis for the intuition which led to the search for nuclear fusion at room temperature that Martin Fleischmann and Stanley Pons started in the 1980s. By using their electrochemical knowledge deriving from lengthy experience, they were able to reach the critical threshold required for the rise up of the phase where "like likes like" and, according to the envisaged picture, deuterons no longer repeal each other but come close enough together to start a nuclear reaction. The Coulomb repulsion which dominates at low deuteron concentration is therefore replaced by the expected long range many-body attraction among like charges. In the spring of 1989 the news was communicated to the public...and the quarrel started.

Since the discovery of Martin and Stan was totally unex-



pected in the generally accepted vision of science, many scientists didn't see the point of the importance of the concentration of deuterons; their attempts to replicate the experiment, using the well known electrochemical procedures, failed because they failed to reach the critical threshold. However, a search for the many-body attraction was going on, at the same time, in the frame of quantum field theory (QFT) and quantum electrodynamics (QED). A fundamental theorem has been proved: "An ensemble of a large number  $N$  of charged particles oscillating between two quantum states, becomes dynamically unstable when the temperature  $T$  is below a threshold and the density  $N/V$  exceeds a critical value. Under such conditions the minimum energy state of the system is a state where particles have the same phase (in a physical jargon are coherent) in tune with a non vanishing e.m. field trapped within the ensemble itself." [G. Preparata, ed. 1995. *QED in Condensed Matter*, World Scientific] The negative difference of energy between the initial non-coherent state and the final coherent state, labelled "energy gap," attracts more and more particles to join the coherent state thus realizing the long range attraction, which adds up to the short range electrostatic repulsion, just as envisaged by Martin. This theoretical vision, which corroborates Martin intuitions, was the reason for his collaboration with Giuliano Preparata and his colleagues, including us. The first product of such a collaboration appeared in the *Il Nuovo Cimento* issue noted above, where the possible interpretations of the astonishing experimental data have been discussed.

A further step has been the search for a more efficient technique to load deuterium inside the palladium lattice. The solution proposed by Preparata and his group takes advantage of the property of a quantum system having a defined quantum phase (then a coherent system) to add up (algebraically) an external EM potential to its own chemical potential. This allows, with a suitable choice, the decrease of the chemical potential of the deuterons inside the palladium, thus forcing a further uptake of deuterons from outside. The main results of this research work, realized mainly at ENEA labs in Frascati (Italy), are not well known in the scientific community, since the most important journals have refused to "give a look inside Galileo's telescope" and to accept the logical implications of QFT.

The idea of many-body long range attraction among charged particles does not belong only to the LENR field; it is, in fact, a general feature of Nature. A close cooperation with Martin went on for many years after the cold fusion parenthesis, before and after Giuliano Preparata's untimely death in 2000, widening in many areas of science.

One important result has been the explanation of the dynamics of Arrhenius dissociation of electrolytes. It has been shown that ions suspended in water always enter in a coherent state, thus making available an energy gap of  $\sim 3$  eV per univalent ion. A neutral molecule such as NaCl, having a binding energy between  $\text{Na}^+$  and  $\text{Cl}^-$  equal to 5.5 eV, finds its minimum energy level spontaneously splitting in two separate ions which add up to their respective ionic plasmas. The same dynamics, proposed to explain LENR, are also able to explain a household phenomenon such as electrolyte dissociation.

In the same conceptual framework, we discussed with Martin the appearance of the electrolyte deposition on metal electrodes. But in the last years biology has become more

and more an important field of interest in our collaboration with him and some preliminary reports have been published [E. Del Giudice, G. Preparata, M. Fleischmann and G. Talpo, 2002. "On the 'Unreasonable' Effects of ELF Magnetic Field upon a System of Ions," *Bioelectr.*, 23, 522-530; E. Del Giudice, A. De Ninno, M. Fleischmann, G. Mengoli, M. Milani, G. Talpo and G. Vitiello, 2005. "Coherent Quantum Electrodynamics in Living Matter," *Electromagnetic Biology and Medicine*, 24, 190-210].

Now Martin Fleischmann has left us. We can presume that he is continuing the former collaboration with Giuliano Preparata in a parallel universe, where the idea of coherence doesn't antagonize so much its inhabitants.

## Dr. Igor Goryachev

(Professor, Research Institute of Nuclear Instrumentation)

It was with deep regret that our group of Russian researchers learned about Dr. Martin Fleischmann's death on August 3. This sad news turned out to become a milestone in the short though bright trace of Martin's cold fusion discovery, which meant new perspectives for mankind in searching for future sources of energy.

For me personally it marked an abrupt turn in my life and career. After years of being engaged in R&D dedicated to defense problems, I moved to Salt Lake City, one of many international scientists who at once associated their further career with this brand new direction in modern physics. My further cooperation with the University of Utah, the National Cold Fusion Institute and then with the FEAT and ENECO companies in Salt Lake during the 1990s actually determined my involvement in the newly-formed scientific community in Russia and abroad.

It is clear from the involvement with Dr. Fleischmann and other scientists in the international cold fusion community that we Russian scientists had the luck to have, that this new physics has found many followers in Russia, which will surely result in decisive breakthroughs with new energy engineering. We are greatly thankful and feel strongly obliged to Martin Fleischmann for his outstanding discovery which should provide a brighter future for all mankind.

We will preserve our memory about Dr. Martin Fleischmann as one of the most famous electrochemists of our times.

## Roger Stringham

(First Gate Energy)

My first personal encounter with Martin was at ICCF4 on Maui. He wanted to know about bubbles and cavitation. I had advice from a close associate not to get into a discussion with Martin "because he will cut you to pieces." So when he sat down next to me in the conference hall I was very nervous, and was very careful with our conversation. He left saying something equivalent to "It is all yours." We had several other conversations, including one about our next door neighbors, The Gorilla Foundation, with three full grown gorillas; he, with a big smile, was also a bit skeptical about that.

The 1989 announcement changed the direction and the excitement level of my life.

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Photo by Mike McKubre

New Jersey, September 2009



Photo by Marianne Macy

England, May 2010



Photo by Gene Mallove

Sheila and Martin, England, October 1997



Photo by Alison Godfrey

New Jersey, September 2009



Photo by Alison Godfrey

New Jersey, September 2009



Electrochemists Manuel Baizer and Martin Fleischmann, early 1980s





Stanley Pons, Marvin Hawkins and Martin Fleischmann, University of Utah, 1989



Photo by Dave Nagel

Italy, October 2009

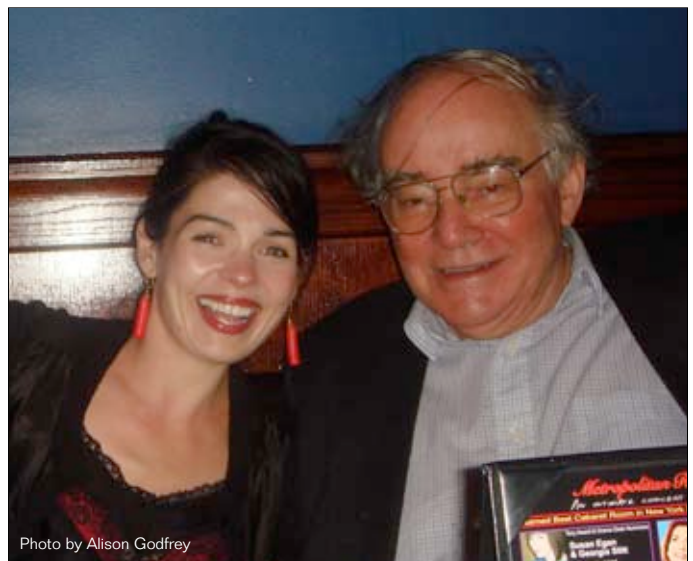


Photo by Alison Godfrey

New York, September 2009