The Fisher/Oriani Collaboration

Marianne Macy

Dr. John C. Fisher

Dr. Richard Oriani

John Fisher's interest in cold fusion dates back to the start of the field. His theoretical work on neutron clusters has resulted in the polyneutron theory, which he believes offers explanations for the various phenomena observed in cold fusion experiments, including excess energy production, energetic particles and particle showers, and nuclear transmutations. Fisher has shared a longtime collaboration with Richard Oriani, who Fisher met when they both worked at the General Electric (GE) Research Laboratory in Schenectady, New York.

With Fisher's theoretical approach and Oriani's experimental technique, the two scientists have made a serious and inspiring contribution, work which continues to this day. Both men are 90 years old now and use their experience and tenacity to advance understanding of the field. This article is drawn from oral histories they have both conducted for the New Energy Foundation Cold Fusion Oral History Project, a collaborative effort with the University of Utah.

JOHN FISHER'S BACKGROUND

Dr. John Fisher earned an AB degree in mathematics from Ohio State University. He took a year off and worked as a lab technician at Battelle Memorial Institute in Columbus, Ohio. He wanted to become a physicist, but found upon interviewing with colleges that he was not qualified to enter their physics graduate programs because he hadn't had any physics as an undergraduate. But, he got a teaching job in the mechanical engineering department at MIT; every time he taught a course that he hadn't had, MIT gave him credit for it and thus he got his Sc.D. degree in mechanical engineering. Contacts he made there resulted in getting a job with GE's Research Laboratory in Schenectady. "There I learned physics from the people I was working with. I learned some quantum mechanics and various other aspects of physics, and I became a manager of a group of physicists," he relates. "I hired a bunch of them and they did pretty good work, working on their own. Some of them ended up at the National Academy. A lot of them ended up as professors teaching physics at various universities."

In the 1970s, GE assigned Fisher to research and write his book, *Energy Crises in Perspective*. He spent six months traveling around the world. "I went to France to look at their solar power. I went to Russia to look at their breeder reactor and to look at their gas business. I got a helicopter ride out into the North Sea to see how the British were tapping the North Sea for oil. I went to Alaska to look at the North Slope."

Fisher's conclusion was in contradiction to the news headlines of that time. "The world was not running out of energy. There was plenty of it around," he recalled. "There was coal for several hundred years and oil for 100 years and natural gas for 100 years, and there was always nuclear power to back it up. And the idea was that we were going to run out in ten years. . .and I tried to point out that that was just not so." Fisher still believes that even today the basic trajectory of the book is sound. But in spite of his interest in energy issues, the intellectual challenges posed by cold fusion would come to absorb him.

John Fisher's career at GE continued at the company's in-house think tank at Santa Barbara, California, where his main job evolved into studying the computer business for GE. Along the way he was mentor and coworker with many people, among them Ivar Giaever, who shared a Nobel Prize with Leo Esaki and Brian Josephson. John Fisher relates the story of their work:

> I had a good idea and I needed somebody to work with; it turned out there was a Norwegian guy who worked at Canadian GE and then in the General Engineering Laboratory. And a guy I knew in the General Engineering Laboratory said, "John, this guy is a real smart guy. He really ought to be working at the research lab." And my boss said, "Okay, why doesn't he come and work with John." So Ivar came. I had the idea that you could make a thin film of an insulator and sandwich it between metal sheets and so on and you'd get tunneling current through there and by manipulating voltages and whatnot, you could make an electronic device. So I told this guy that he was going to work on thin films, so he bought a book on *photography* and read up on it!
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> First we thought we'd use Blodgett film, soap films on glass. So we tried that and they kept breaking down

and then we decided that we'd evaporate metal onto a glass sheet and oxidize it. The oxide makes an insulating film. Maybe that would work. I didn't know how to do that. He didn't either, but he asked people who did, they would help him, and we made a lot of films and we did tunneling experiments. *Physical Review* published our results.

Ivar decided he would like to become a physicist, so he went to Rensselaer Polytechnic Institute and took a course in solid state physics and his professor told him about recent work of Bardeen, Cooper and Schrieffer, who had a theory of superconductivity and that there's an energy gap in the superconductive material. He says, "I'm doing thin films, maybe I can see an energy gap if I try to pass a current through that." And then he came back and said, "I've got this idea and I'd like to do that." And it didn't seem right to me; we asked some of the theoreticians around there and they said, "No, that's not going to work." He said, "Oh, I'd like to do it." And I said, "Okay, Ivar, it's your baby, you do it." So he learned how to do that; he had to do it at liquid helium temperature, so we had some guys who do liquid helium and he got all set up, and he did that experiment. He found the energy gap, and he won the Nobel Prize.

In 1980, John Fisher retired from GE. Though retired, he went to work for InterMagnetics, a company started by one of his GE colleagues that made magnets, including for magnetic resonance imaging (MRI) used for medical diagnostics. Fisher was their chief scientist and remained there until other interests changed his path. The company recently sold for over a billion dollars.

Fisher and his colleagues from GE were impressed by the 1989 announcement of cold fusion. In Fisher's case, interested but skeptical:

I followed it with very great interest, and so did my friend Tom Paine—he was a GE employee when I was, but younger. He was a metallurgist and I was an engineer physicist. He rose to become pretty high in GE and after awhile he was my boss. He left GE and became administrator of NASA during the time the Apollo 11 went up. Then he retired. So we were both retired and we were talking about cold fusion, so I bet him about it. I said, "Look, this whole thing is a couple of graduate students who have looked at Fleischmann and Pons and felt sorry for them because they weren't getting anywhere. And so one night, they sneaked in and they scattered a little tritium around and they smashed the apparatus." I figured that was the most likely solution. Tom didn't think it was funny. He said, "No, John, I think maybe there's something to that." I said, "No. I'll tell you what, I'll bet you a million to one that there is nothing to that." So he took the bet.

Well, then I got to work studying it. I had all sorts of crazy ideas about what it might be. And I would go to meetings and try those out, and somebody from Los Alamos or somebody like that would carefully explain to me that my ideas about high-energy plasma moving through metals wasn't right. There were losses I wasn't considering. And I kept looking at the evidence while people were publishing things and thinking about it myself. I finally said, "Well, Tom, you've won." And he said, "Okay, John you are going to have to pay me off in coffee." So every time we had lunch, which we did regularly, I bought the coffee. He died before I was able to pay in full.

Fisher also consulted for EPRI. After his consultantship wound down as his colleagues retired, he worked on his own projects, pursuing his "real love. . . elementary particles." For this he went to Fermilab in Chicago and worked on analyzing data for seven years, as he was friendly with their chief physicist. Ironically, he relates, "It took me a long time to realize what I should have realized in about three months. The particle I was looking for was not in their data and, furthermore, that the little signal I saw that I was hopeful was my particle was what they call an 'echo.' Their database contains parameters for pairs of mesons. Software identifies which of the pair could be a K and which a pi. Then software reconstructs a parent particle that decayed into the K and pi. They were looking for a D meson. I was trying to reconstruct a different particle. Sometimes they got the K and pi identifications both wrong, thought the K was a pi and vice versa. Then you get a nonsense reconstruction called an echo. That's what I found—a weak signal close to the D mass, but distinct from it. When I finally realized what I had, then I set that attempt aside. And then that was sort of coincident with the time that cold fusion came up. And I've worked on cold fusion ever since."

RICHARD ORIANI'S BACKGROUND

Dr. Richard Oriani emigrated to the U.S. from El Salvador in 1929 when he was nine years old. His grandfather was born in Italy and his own father had never pursued El Salvadoran citizenship; to Oriani's surprise, his own passport was Italian. So in 1943 when Oriani graduated from the College of the City of New York with a degree in chemical engineering, he was considered an "enemy alien" (a person who was a native or citizen of a country at war with the U.S.) and was not able to get a defense-related job. He was fortunate that an administrator at his school took a special interest in him because he was at the top of his engineering class. Strings were pulled and Oriani was given a job at the Bakelite Corporation Research Laboratory, working on the study of adhesion and on the development of a military adhesive for which he has a patent. This work kept him from induction into the Army. In 1948 he earned his Ph.D. in physical chemistry from Princeton University. This is where his story begins to intersect with John Fisher's.

Oriani was employed at the GE Research Laboratory in Schenectady, working in physical metallurgy, including some work on the diamond synthesis problem. He spent ten years at GE, then moved on to U.S. Steel Research Laboratory, where he served as Assistant Director for Fundamental Research. He notes, "I did a great deal of work on hydrogen in metals. That is what made me think I would be able to contribute to the problem of the nuclear energy generation during electrolysis reported by Fleischmann and Pons in 1989. . .When I got my Ph.D. I was not an electrochemist. My Ph.D. thesis was on molecules that have internal rotation in the gas phase, for example ethane dichloride, which has two carbon atoms that are bound together and one can rotate in respect to the other. I was measuring dipole moments in the gas phase under Professor Charles P. Smyth. I didn't know anything at all about electrochemistry, nor did I do electrochemistry at the GE Research Laboratory. I did physical metallurgy, thermodynamics, kinetics, phase transformations, and things of that sort. I had to learn electrochemistry afterwards."

What was Oriani's first impression of the Fleischmann-Pons effect? "I did not believe that assertion at first. I thought it must have been wrong," he replied. "It seemed to contravene the accepted separation of nuclear phenomena from chemical phenomena. But they are very good people, especially Fleischmann, so I thought I should investigate. For the first three or four months I did not have any success."

At that point in his life, Oriani had been at the University of Minnesota since 1980, after retirement from U.S. Steel. At the university, Oriani was a professor and the first director of the newly-established Corrosion Research Center. He reflects on those early days in 1989:

> I was working at my desk trying to design a calorimeter for the purpose of investigating the Fleischmann-Pons effect when the phone rang. I picked up the phone and a little voice came through and said, "Hello, Dr. Oriani. If you need a calorimeter, I have one for you." It turns out that this man was a pediatrician whose name is Rolf Engel. He had heard about my interest through the glass blowing department at the University of Minnesota. Engel had built a Seebeck-type calorimeter which was about 3 feet in length and 12 inches in diameter. He had used it on babies. He was interested in the effect of age on resistance to anoxia. In other words, young animals and young humans can resist the absence of oxygen far better than adult people. He was interested in this problem and how to understand it. So he used the calorimeter to study the metabolism of children in various conditions. He put a baby inside his calorimeter and dunked the whole thing in a great big tank of water that served as a thermal reservoir. And he would make measurements. I don't know the whole story but just what he learned. That was the calorimeter he offered me. I went to his home and there it was in the garage under a tremendous pile of junk. I took it home and cleaned it. It was a wonderful calorimeter. So that is what I used.

Although Oriani's previous experience with calorimetry was slight, he found it a short learning curve.

> It was obvious what to do. It had to be calibrated so I did that. For the first few months I kept repeating the electrolysis experiment with palladium and heavy water. That did not work at all. The thermal output fell right on the calibration line. But one day, by golly it worked! It worked in such a way that there was no doubting. I made two successful runs. The second run was terminated prematurely by an explosion. The explosion occurred because the graduate student who was helping me with the project picked up a flask, outside of the calorimeter, which was receiving the produced oxygen and hydrogen to be recombined by a catalyst. When he did this, the whole thing blew up. He got a gash in his arm. Luckily he wasn't killed. But there was glass all over the laboratory. I deduced that what happened was that the catalyst that I was using was very fibrous. Apparently one of these fibers had dried so that it was able to ignite the hydrogen-oxygen mixture. It should not have happened but it did. The catalyst consisted of small particles of platinum on asbestos. It was a catalyst that was being used by the people in NASA, if I remember correctly. That ruined a perfectly beautiful calorimeter. That put me off calorimetry for awhile.

Undeterred, Oriani's experimentation continued:

> I soon devised and built a flow calorimeter, and afterwards a Seebeck calorimeter designed to be used at temperatures as high as 400°C. I tried a variety of other methods that people had claimed were successful for them. One of these was Liebert's method with molten solutions of lithium deuteride, but that was unsuccessful. I tried a technique which I did not know at the time was similar to one developed by Stan Szpak. I wound palladium wire around my anode of platinum, which meant that the palladium would go into solution anodically, and then it would be deposited cathodically where I would also deposit heavy hydrogen—deuterium. The idea was to obtain fine particles of palladium with a large surface area, hoping to increase the loading of the deuterium into the palladium. Whether that was the reason for the success that I got, I do not know.

Initially Oriani found the University of Minnesota to be enthusiastic about following up on cold fusion; the University even committed some funding to the work. He had essentially retired by 1989 but kept on working with graduate students on corrosion problems. Cold fusion work became "something on the side." He noted, "The work went on because I had a lot of the equipment gotten for my other research. So I didn't have to spend much money except to buy heavy water, which is kind of expensive, and palladium. Except for that, I had what I needed."

Oriani's background in metals and hydrogen would help him in his approach to tackling the area of low-energy nuclear reactions in the metals. He elaborated on his prior work and the process:

> Part of my research interest was about trying to understand the problem of hydrogen embrittlement of steel. If a high strength steel absorbs a small amount of hydrogen, extremely small amounts, it becomes extremely brittle and a mechanical shock will rupture it. Hydrogen is very deleterious to steel, hence I took it upon myself to try to understand this, to know what to do about it. I succeeded.
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> What happens is that the hydrogen enters the lattice of the iron and interacts electronically with the iron atoms so their cohesion is reduced. As a result, the hydrogen is able to cause the parting of the iron atomic bonds so that a crack will form and propagate

very easily. It was used to conclude that what you need to do is clean up the steel very much to avoid what we call inclusions, thereby avoiding stress concentration factors in the steel. So in that sense it has been used.

I knew how to handle hydrogen and how to make measurements, so I thought it was very easy for me to adapt that knowledge to the problem of the Pons-Fleischmann phenomenon. I had lots of unsuccessful attempts over the years, *i.e.*, no excess energy produced. There was one that was particularly interesting, using a palladium specimen from Takahashi in Japan and also his technique, which he calls the ramp electrolysis technique. I did not succeed in getting any excess thermal power from that experiment, but I did succeed in getting an intriguing anomalous distribution of the isotopes of palladium. There are six naturally occurring isotopes of palladium. Everyone knows what they are and how they are distributed, the percentages of each isotope. But I found by secondary ion mass spectroscopy (SIMS) that the distribution was completely different in the outer 150 angstroms in from the surface. This was very exciting, so I decided to have this checked by the Stanford Research Institute (SRI) people. Michael McKubre's group was very excited to check it out for me. So I sent him a specimen. However, I made the foolish mistake of first cleaning it up. The experiment with the Takahashi technique left the specimen rather not good looking. The surface was no longer highly polished as it had been before, so I thought that I would clean it up. . .I decided to use hydrochloric acid. That removed the interesting portions apparently. That killed the possible verification of the anomalous distribution of isotopes. After that I got involved with trying to replicate Mizuno's work, which is completely different of course. It involves a solid state proton conductor with which I had not had any experience whatsoever. By that time I had built myself another calorimeter, a high-temperature calorimeter that I was able to use to pursue Mizuno's work. It was able to operate at 400°C. It was a Seebeck-type calorimeter.

Calorimetry is an area of cold fusion research that is frequently scrutinized. The complexity of the different types of calorimetry, their applications and what they offer, are hugely varied and illustrate the painstaking methods of experimental researchers working in the field, as Oriani's story illustrates.

The Seebeck-type calorimeter is an enclosure whose walls are composed of as many thermocouples as can possibly be crowded. The one that Rolf Engel had built had over 3,000 thermocouples, all with AB junctions on the outside surface, and with BA junctions on the inside surface, so that each thermocouple signal added to the next thermocouple signal. Hence, the entire set of thermocouples produce a large EMF. That EMF is a measure of the temperature difference between the inner and the outer walls, averaged or integrated over the entire surface of the calorimeter. A Seebeck calorimeter is extremely good because it is almost insensitive to the spatial distribution of heat-generating sources inside the calorimeter. That's not the case with most other calorimeters. I built mine with machinable ceramic. It was difficult to build, a homemade job with almost 400 thermocouple junctions which had to be hand produced, one by one.

The Seebeck calorimeter is a type that has been used for many years, but the application to cold fusion was my idea. After all, the Rolf Engel calorimeter also was a Seebeck calorimeter. I applied it to electrolysis and I succeeded in verifying the production of thermal power from electrolysis. But the idea of using a calorimeter for Mizuno's work was original. But I had to build a calorimeter first because I had to operate at high temperature and that was very difficult. But at any rate it succeeded. After a great deal of effort I was able to find that, yes, Mizuno was correct, but mine was not a consistent success. I would guess that 10% of the experiments were successful and all the rest were unsuccessful in terms of obtaining excess thermal power. We don't know why. We still don't know why some were successful and some were not.

Oriani traveled to Japan and made presentations of his work at Hokkaido University, where Tadahiko Mizuno welcomed the illustration of Oriani's success, as it was supportive of his own cold fusion efforts. The work was published in *Fusion Technology*, edited by George Miley.

Mizuno wrote an honest and exciting book about his work, *Nuclear Transmutation: The Reality of Cold Fusion*, translated by Jed Rothwell. In it he praises the rigor of Oriani's experiments, "His experimental technique was flawless," and he was tenacious in working on replication of others' results, among them Mizuno's hydrogen proton conductor experiment. Mizuno relates how Oriani used the Seebeck-type calorimeter with a calibration heater turned on at all times. "This eliminates major sources of error, and allows an absolute measure of heat. His preparations took more than a year. . .[It was] January 1995 that Oriani was able to. . .perform the main experiment. It was not until a year after that, on March 20, 1996, that he contacted me to say that he had observed excess heat from some of the sample proton conductors I had sent him." Mizuno covers the continued shared experimentation he and Oriani performed and relates the levels to which Oriani would continue to test: "Oriani had sent back to Sapporo a mixed selection of used conductor samples, some of which had produced heat in Oriani's lab, some of which had not. He sent along instructions asking me to run them again in my own calorimeter. In short, it was a blind test in which I did not know which samples had previously produced heat." Oriani and Mizuno would also compare results on reaction products and shifts of isotopes in their experiments. He depicts Oriani as a painstakingly thorough experimenter who will check and recheck, in one instance relating how "the ever-cautious Oriani was not satisfied with the results" and so Mizuno invited him to do the final check of an experiment together.

COLLABORATION

Oriani's collaboration with Mizuno is just one of many he has had in the cold fusion field. Perhaps one of his greatest and longest lasting collaborations is with John Fisher. Fisher's theoretical work has been tested experimentally by Oriani. Both give each other credit for the direction of their work and resultant breakthroughs.

When Oriani left GE, he and Fisher stayed in touch. Remarkably, both men independently began inquiries into cold fusion after the March 1989 announcement. In about 1990, Fisher was in the audience as Oriani presented his calorimetric results (using the Engel calorimeter). Fisher was excited about Oriani's results and approached him about working together. Oriani notes, "He would feed me some theoretical concepts and I would feed him some experimental results. That's what we have been successfully doing since 1990."

How do you develop a theory to explain cold fusion? John Fisher said his technique was to go to conferences and listen to what was said and to try to work out a theory. "I gradually came to believe that it couldn't be fusion, because charged particles with low energy cannot get together and fuse with sufficient frequency. I came to believe that if there was not a neutral particle involved to do the shuttling of neutrons back and forth, it wouldn't work. So it had to be some kind of neutral particle. Edward Teller said it would have to be a meshuganon. It had to be neutral, and I figured maybe it was a dineutron so I worked on dineutrons."

Fisher discusses how he worked with trineutrons, quadraneutrons and so on. "I figured I was getting into a realm of unreasonableness to think that that many neutrons would stick together. But, I overcame that mental barrier as I have overcome others, and came to believe that lots of neutrons would stick together. So I have been trying to develop and improve and strengthen that theory and check it against experiment."

"The possibility for neutron-rich carbon during electrolysis is a consequence of some of John Fisher's ideas," Richard Oriani declares. He explains what Fisher's theory postulates and how he set out to test it:

> Fisher's theory involves the decay and growth of polyneutrons. Depending upon how they decay, they can produce alpha particles, and finally in chain reactions neutron-rich species. It occurred to me that it would be very nice if I could verify that hypothesis. So what I did, and this is published by the way, is to set up a gaseous circulating system such that in one region of the circulating system I was able to heat up material to about 1200°C and in another section I was able to freeze things down to a liquid nitrogen temperature. The idea was to use the palladium which had been successful during electrolysis to have shown excess power. Then I would oxidize the specimen, realizing that the only oxides that would be volatile at 1200° would be carbon dioxide, or oxides of nitrogen. Heating a successful palladium in oxygen would produce oxides that would remain in the hot region of the apparatus, except CO_2 and NO_2, and these would be frozen out in the side tube kept at liquid nitrogen temperature. I circulated the gas mixture for about 12 hours. Then I would remove the cold finger from the apparatus and I would take it to a mass spectrometer. An individual in the chemistry department would help me with the determination of the various masses that were present in the gas phase. The observed masses were then compared with those found by a similar experiment with palladium that had never seen electrolysis. Did we see differences? Yes, we did. We saw several differences and then I took hours and hours to study the collection of data in the *Handbook of Chemistry and Physics* to see what possible materials have vapor pressures large enough to have been able to reach the cold finger. There were a few. Some compounds of ruthenium, for example, which is a precious metal, extremely rare.
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> Ruthenium has some oxides which are fairly volatile. But I would also have found those masses in the controls if ruthenium were an impurity in the metal. So after going through all the possible volatile oxides, I had to conclude that I had a neutron-rich carbon with a mass of 240, whereas normal carbon's mass is about 12. This was published as an indication that Fisher's theory has some value.

John Fisher adds, "We collaborated on some publications where we both did a share of the work. One of the best things we did was. . .He was using these little CR-39 plastic chips to record energetic particles in the electrolyte. I said, 'Why not put it in the vapor, because the theory allows reaction in vapor too?'"

"That is something I would not have done were it not for John Fisher's ideas," Oriani says. He expounded on the uniqueness of the idea:

> Nobody in the world had done such an experiment. What people have thought, and many of the theorists still think, is that one needs crystalline palladium to cause a cold fusion reaction. Therefore, one would have to put a CR-39 detector very close to the palladium to observe anything, if that were the case. But Fisher's idea is that polyneutrons can be wafted up into the vapor phase during electrolysis, where they would decay to produce charged nuclear particles. Anyone who disbelieves Fisher's theory would never think of putting any detector chips anywhere except next to the palladium during electrolysis.
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> My idea was that if Fisher's idea is correct, the polyneutrons would be reacting to produce alpha particles or other nuclear projectiles in the vapor phase above the electrolyte during electrolysis. And that proved to be correct. In addition to that, if I put a chip inside the liquid, between the anode and the cathode, I would also get many nuclear tracks. That would be impossible according to people's thinking about the problem at that time. So this was a great step forward in showing that Fisher was not all crazy in postulating polyneutrons. There may be other ways of accomplishing the generation of alpha particles at the vapor phase above the electrolyte, but nobody has ever thought of anything like that except John.

Fisher elaborated, "So we did that, and he got them. And one day he called up or emailed and said, 'I got one chip here, it's got too many pits on it to count.' And I said, 'I'd like to see that one,' so he mailed it to me and I looked at it and I figured, geez, somebody's got to count this. So I bought a microscope and got to work, and I took something like 300 photographs of that one little chip, overlapping photographs."

Fisher provided more detail about the meticulous process involved:

> One way of detecting energetic charged particles, particularly alpha particles which are energetic nuclei of helium atoms, is when they zing along and hit a piece of Lexan, which is a kind of plastic. They go in a little way—not very far, but a little way—and they sort of wreck up the plastic where they go in, and they leave a little trail of damage, sort of like a contrail behind an airplane, only you can't see it. It's just in there, making garbage out of the structure of the plastic. Then, if you want to see that track, you can take the piece of plastic and put it in some caustic soda, which is sort of like lye, and leave it in there for awhile, and that eats away the plastic, but it eats away faster where that track is, because the damaged stuff dissolves faster. Then you take it out and rinse it off and look at it under the microscope, and you see that little pit where the etching went into the plastic. And some of those have a nice, sharp point at the bottom when the etching didn't get as far as the length of the track, still it's running along the track. If it reaches the end of the track, then the bottom of the etch pit sort of rounds out and you can tell that some tracks didn't get that far and other tracks got farther than that. Most tracks rounded out, but a few had sharp points.
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> So I started counting etch pits because they were not uniformly distributed over the chip. They were all bunched up in one corner, and then the density died away as you moved away. And it was perfectly clear that there had been some kind of little explosion in the vapor near the chip that had spewed out something like 150,000 energetic particles. Alpha particles is what they turned out to be.
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> We didn't catch them all in the chip because half of them went in the wrong direction, and some of them missed the edges, so there were only about 30,000 on the chip. And then I began worrying about, "Well, I've got two kinds of things going on here." And it occurred to me that one of these families has an energy big enough to make long tracks with sharp-pointed etch pits and the other one had only an energy to make short tracks with rounded-bottom etch pits. Must be two kinds of things. And by looking on the back side of the chip where the shower didn't get, I see there are pits back there. What are they? They're all radon impurity, and they were all the ones with sharp pits. So I knew now that I had radon, which went in far, and I had these other ones that didn't go in so far, so they didn't have as much energy. And by a little studying and using formulas that people had got, including Lipson from Russia, who has calibrated energy versus track depth, I could deduce that the alpha particles had an energy of about two million electron volts compared to radon, which is more like six.

In any good collaboration, there should be debate and disagreement. Oriani remembers this part differently:

> These are chips that were held in the vapor phase above the electrolyte. There is no chance of any radon getting in there at all. These were tracks that were produced by the Fisher reaction, as I will call it, on the rear surface of the detector. The detector was just simply hanging in the vapor phase. There was no preference for this side or that side at all. It's just that one side got more reaction than the other side, but that was all happenstance.
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> If he is referring to the experiment he refers to as the Oriani showers, it's a confusion. Those showers were obtained in 2002. The significance of course is that it verified Fisher's ideas and it showed that one could get extremely large number of nuclear particles. However, we do not know even now whether the generation of alpha particles and protons that manifest themselves on the CR-39 is related in any way to the generation of thermal power. We do not know that. It could be they are parasitic reactions that take away from what you want to have, namely, more thermal power generated than the power that you put in electrically. The additional significance is that for the first time we have a map of a tremendous reaction. In fact, John was able to actually trace the path of the reaction. By looking at the various angles of impingement by the particles, the angles of these pits, he was able to show that over a few seconds the reactions were drifting in the vapor phase and depositing the alpha particles and protons on the CR-39. It is very interesting.

Fisher recalls what happened next. "Well, we wrote this up, and tried to publish it in a regular physics journal. We did not claim that this was caused by electrolysis. We did mention that we did it in conjunction with an electrolysis experiment, because you have to explain what your apparatus was, and that was it. We had just wanted to say, 'Look, we noticed this shower. It's very unusual, has these properties.' We know how many particles there were in there. We know how the shower formed, it wasn't just 'blip and it's over.' It was more like a firework display, where it's a bright flash and then you've got sparks coming out. And you could see as it drifted with convection currents how it died away."

Fisher described the exciting discovery: "By studying the directions of the tracks you could tell that the shower was moving in the direction it should be to fade out. All of that stuff—and identifying the energy of the particle—there is no known particle decay that gives 2 MeV alphas. This was brand new stuff." He pauses and wryly adds, "And the paper was rejected. We were not trying to make the case that electrolysis had anything to with it. We were trying to say, 'Look, we found this previously unknown, inexplicable shower with these remarkable properties. There's new physics in there and people ought to know about it.'"

What did the reviewer say? Fisher explains, "He said that it was impossible for electrolysis to do that. We didn't claim it had. So I appealed, and the appeal referee backed him up. I did not go any further with it. We published it in one of the ICCF publications."

Fisher and Oriani were nearly seventy when cold fusion entered their lives. One thing that they both communicate is their deep interest, even joy, in the intellectual challenge the work poses. What they have proven with their dedication is the extent to which they are willing to work. In his book, Mizuno writes about his reaction to seeing Oriani's lab in the U.S.: "Oriani had put together his own calorimeter to test the proton conductors, which I found surprising, because after all, he is more than 70 years. In Japan it would be unthinkable for a retired professor to work as hard as a full time professor."

Fisher's description of getting good data on their experiment further illustrates the detailed accuracy that both men strive for:

> To count those particles, I've got to be sure that I count every one and that I only count it once. And that's why I had these photographs. I would print out a photograph on my computer, get out my pen and I would go through, checking off etch pits as I counted them—one, two, three, four, five. . .ten—I don't want to count too high. And then I go over on the side and I write down ten. I continue counting and recording in this way and I can tell that I've got every one and also that none have been counted twice or missed. Tedious. Then of course, since I knew where the photograph came from, it was possible to reconstruct the pit density as a function of position on the detector chip. I didn't have that capability. But my son Mark did, and he got interested in that. He said, "Dad, let me see what I can do with plotting out that data." He does mathematical modeling, works with the Federal Reserve as a research economist. And so he said, "I'd like to try this out." So he did, and he got a nice contour plot. So that's what we published.

Fisher adds, with no rancor, that at the time they published the data, the work received little attention. "Most people, in those days particularly, were focused on trying to find out how to increase the power output. Because unless you can get a lot more power out than you put in, it just has no practical use for power. You've got to get, I would estimate, ten times out what you put in to begin to think about making it commercial. And getting 50% more out than what you put in won't do it. It's where they were all putting their effort. And the idea of studying this complicated paper by guys who were looking at a few thousand etch pits that came out of the vapor of one of these things, was not useful to them. It wouldn't do them any good to know that."

In a field that has been as replete with savage attacks as cold fusion, the courtly manners coupled with astounding intellect, dogged determination and ingenuity of Richard Oriani and John Fisher is an enlightened gift to the community, if all could learn to function as they do. Fisher elaborates on the theoretical debates that exist:

> The point is that no theory has been very successful. And when you think about an unsuccessful theory long enough, then you cast around, and say, "Well, let me think about another one, maybe it will be better." And [my colleagues] are sort of, I think, in that stage now; they still don't think my theory is right. But we've looked at the other ones so thoroughly and. . .they haven't had the utility. A theory that can only explain things that have been done is not useful. You've got to have a theory that explains and forecasts things that haven't been done. That theory doesn't have to be right, but it could be useful if it encourages people to do things they would not have otherwise done. They might find something. In the case of my theory, it has encouraged Oriani to do things that weren't done before, things that did turn out. So he's found it very useful.
>
> There's always been great difficulty in getting a reaction going. My theory says the reason is that what you need to start one of these reactions going is a very exotic particle that's floating around in the air, but there aren't very many of them. But one of them has to decay in your apparatus and release a polyneutron. You've got to have a polyneutron get in there. They don't occur in nature because they're radioactive, but there are particles that when they decay, will produce a polyneutron. If one of those decays in your apparatus, you're off and running. Many people have tried and failed and quit. Others who were more lucky or more persistent waited and theirs got going. And then the experiment could produce a lot of exotic particles that contaminate their apparatus. Now a radioactive piece of your equipment emits polyneutrons. Just mail that to somebody and he drops that in his cell, maybe it'll go. . .I tried to do Oriani's experiment several years ago. I got no luck. So he says, "Well, let me send you one of my O-rings, John." So he sent it to me and I put it in my apparatus and wham-o! Went right off.

Fisher added, "I subsequently did some careful radon experiments that showed I had not properly corrected for alpha particles from radon contamination. So I still have not been able to duplicate Oriani's work (or Lipson's or SPAWAR's or any other etch pit work). Oriani and I are currently working to understand and resolve this problem."

Fisher and Oriani found that interest and activity grew

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among researchers looking into particles in LENR. Fisher explained:

> It used to be just Lipson and Roussetski and Oriani and maybe one or two others; I think Miley did a little. But now others, who also are interested in the fundamentals of what's going on, are studying it because I think increasingly people are coming to feel that by studying these very tremendously low-energy processes. . .I should point out how low-energy it was. I computed the total energy that was released in this big shower I talked about. It is 10^{-8} Joule. A Joule is a watt second. This is like having one watt of power, for one one-hundredth millionth of a second, and that doesn't interest anybody who wants to build a power plant. But they're coming to feel, some of them, that if they only understood how that worked, maybe they could apply this physics to the setup they want to use to make power. And that's my hope in this.

Scott Chubb discussed the progression of the polyneutron theory with John Fisher in Sochi, Russia at ICCF13. Chubb pointed out, "Your theory has matured in the sense that you used to talk about polyneutrons, and you didn't have a mechanism for how they got there. And now you do. You've got this particle. And then you have the attributes of the particle. And that's a major advance. It makes it very much more credible; you've gone from beginning to end now. And you have signatures of what to look for."

Fisher, as much of a stickler for theoretical structure as Oriani is for experimental, mused:

> The major complaint about polyneutron theory is that it is well known that two neutrons do not stick together. They know how much they miss by, maybe two-tenths of an MeV. They know that three don't stick together. And, by measuring the interaction of neutrons in ordinary matter, where ordinary matter has got about equal numbers of neutrons and protons, they study the dance that they make with each other. Pairing up proton with neutron and proton with proton and neutron with neutron enables them phenomenologically to deduce what the interaction of neutrons with each other is when there are protons around. And it isn't very strong. And so they take that strength, and they say, if that's all the strong that interaction is, a bunch of neutrons won't stick together. But the thing that caused me to wonder is, "Do I have to give up and abandon the theory on that account?" Well, I think the answer is no, because the dance that neutrons make with protons constrains them; they've got to spend time with partners of protons and they don't get a chance to show off with each other the way they would if the protons weren't there. Take neutrons all alone, then some long range attractive forces, like you have in superconductivity, can come into play and bind a bunch. That was encouraging. And then there was a group at CERN who found that four neutrons do stick together and they had a lot of discussion with people about that who said, "Well no, maybe it's that they almost stick together rather than they do stick together, because your error bar is big enough that it would allow you to have discovered four that almost stick together but not quite."

Chubb suggests this is "like a resonance." Fisher's thoughts: "But a closer resonance than two. And that encouraged me, because that's on the right track. I would not have thought that so few (four) would hang together. But my theory in its present state is, I assume that four is close, and that six would hang together. But I have to tell you, and also as the years go by, I keep thinking of bigger and bigger polyneutrons. I think that the sizes of interest to cold fusion people are in the hundreds or thousands of neutrons." Not necessarily in even numbers and pairs always, he added:

> If there's an odd one, it still sticks, but not very well. And that matters, because in working out polyneutron reactions, it depends on whether the polyneutron has an even or an odd number. In the dance of neutrons, it makes a polyneutron stick together if they go in pairs. And if you have an odd neutron, it's got nobody to pair with. And it sort of tries to cut in here and there and that really encourages it to stick around, but it isn't very strongly bound.

Chubb points out that the pairs are bosons, just like Cooper pairs. Fisher agrees:

> I thought that a dineutron was pretty far to getting two neutrons to stick together, and now four or six, gee that's very far. People were having trouble believing two or four or six—they certainly wouldn't believe 100. They really wouldn't believe 1,000! I am led to that by trying to fit the experimental evidence. A 1,000 neutron polyneutron is five times as heavy as a uranium nucleus. It could catalyze reactions. I can tell you what polyneutrons mean in terms of energy. In active experiments, polyneutrons generally collect neutrons; whenever they interact with deuterium or oxygen-18 or some other fuel nucleus, the neutrons are more attracted to them than to the nucleus, so they keep growing. And there's a little energy released in that process, which turns out to be minor. The major energy is that polyneutrons are unstable. They decay by emission of alpha particles. Each alpha particle carries a couple of MeV so that the final energy production, and what I think is the major part of it, is decaying polyneutrons, where they're given back energy that they absorbed when they were growing.

Oriani also reflects on the direction the work took him in. "I was only interested in showing that, by golly, a nuclear reaction can accompany electrolysis and here is the proof, here is the verification. Not necessarily showing what they are. Now, what Fisher did was in part going in that direction to show that they had to be alphas and protons; that was his contribution from my experimental work."

Oriani considers what Fisher has said about hoping that his work can serve his role as a teacher, by enlightening people to the rewards of trial and error. "It would open up a new area of nuclear physics entirely. It would augment nuclear physics as we understand it today. Yes, a teacher in the best sense of the word because Fisher is opening up a new field of understanding."

Does Oriani feel that his supplementary experimentation has done that? Does he feel that he has made a very solid case for the fact that something different and unique is going on? "Yes, certainly," he declares. "I have also shown that of all current theoretical interpretations alone, Fisher's has a fighting chance of being right and nobody else has that. Let me make that more clear. For the generation of nuclear particles that we see in CR-39 detectors, only Fisher's theory has a possibility of being right. For the generation of thermal power, I don't know. These are very different branches, very different reactions. We just don't know. We are too darned ignorant."

Both John Fisher and Richard Oriani have conducted meaningful and successful collaborations with other researchers and theorists in the field. Additional material from both of their oral histories will likely be used in forthcoming pieces about selected topics.