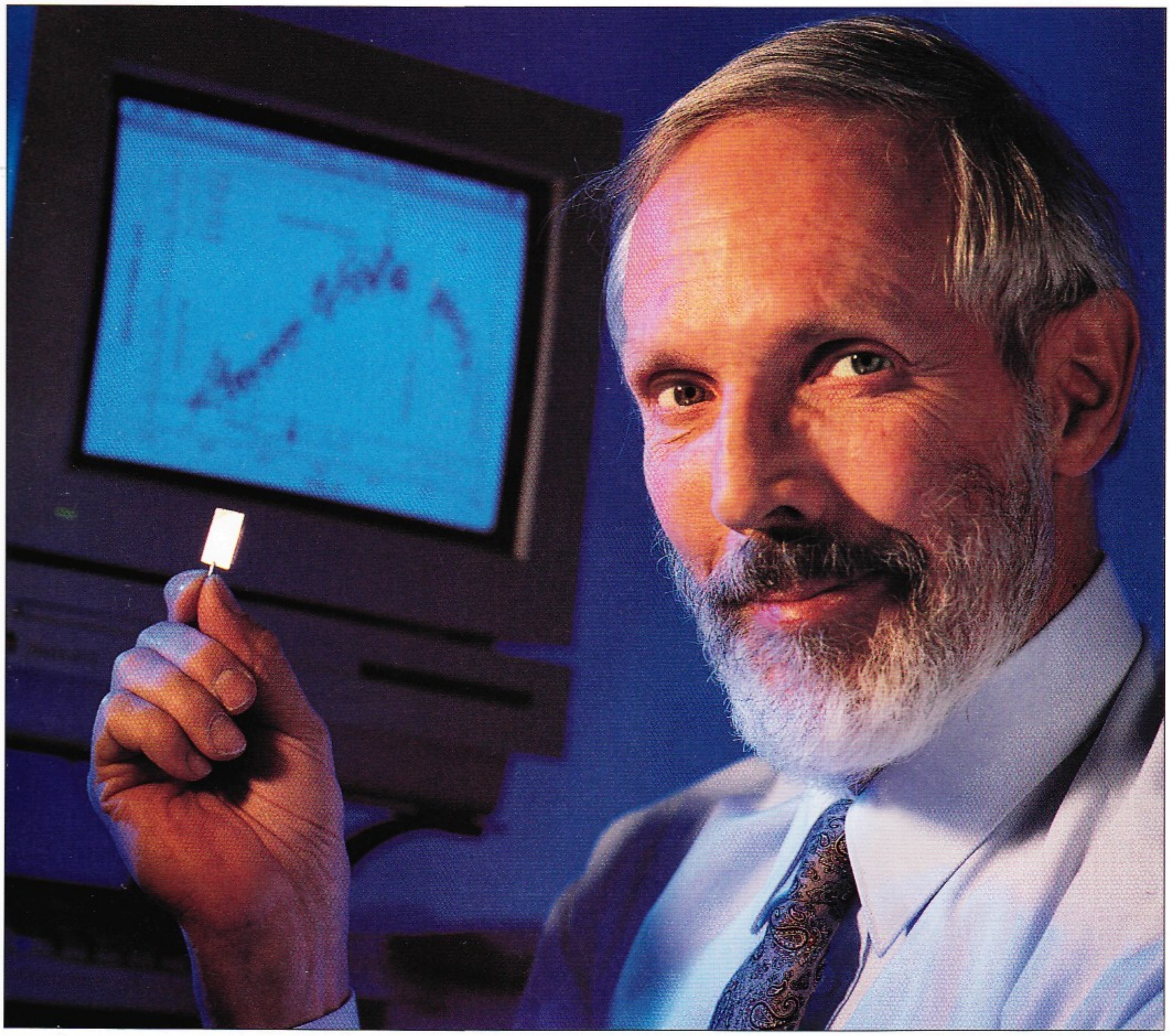
PREMIER ISSUE • Welcome to the Water-Fuel Age

"COLD FUSION"

A Wayne Green Publication

May 1994 Vol.1 No.1



After 34 Years at Los Alamos, Dr. Edmund Storms now pioneers cold fusion research. p. 43

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NUMBER 1 IN A SERIES BY NOVA RESOURCES GROUP, INC.

We've thought a lot about safety in cold fusion research. Perhaps you should, too.

SAFETY, of course, is the primary consideration. If research weren't pushing the boundaries of knowledge, if there were no unknowns, then there would be no need for experimentation. In developing this new phenomenon for commercial applications, one thing is certain: the operating temperature must increase well above the boiling point of water. This forces closing and pressurizing the cell. And therein lie the dangers. Hazards arise from several sources with varying degrees of severity.

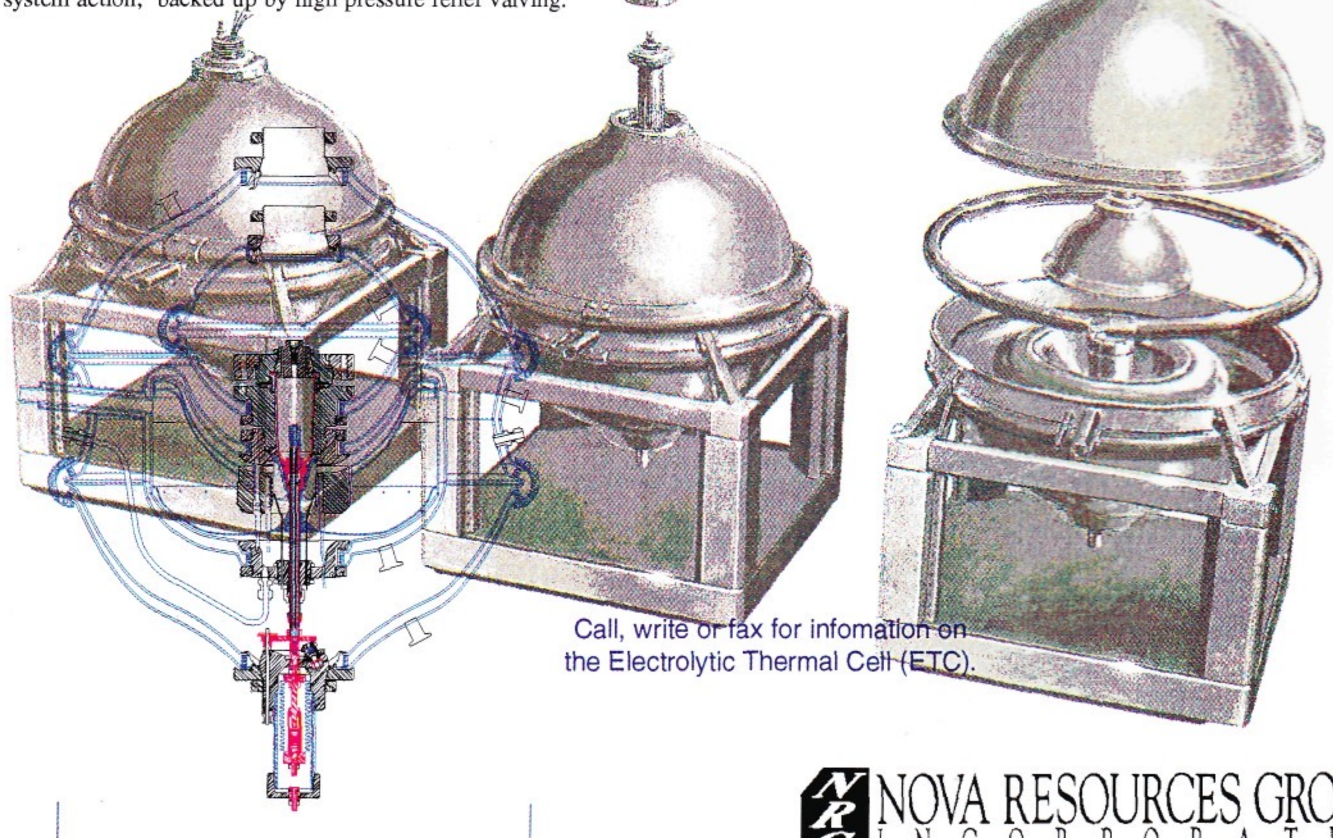
THERMAL
Water/steam heated in a closed vessel can reach pressures of nearly 250 atmospheres. Due to the positive temperature coefficient of reaction rate, at least in the palladium-deuterium system, thermal runaway reactions are possible, with resulting abrupt temperature and pressure excursions. This points to the requirement for exceptionally strong walls and high peak cooling capacity to control cathode temperature.

NUCLEAR
These reactions are now known to be nuclear, in some form. There are many confirmations of low levels of neutrons, tritium and gamma rays. Increasing amounts of these dangerous products will likely be generated as experiments are scaled up. Adequate shielding and appropriate handling precautions are necessary.

ELECTROCHEMICAL
Failure of the catalyst to recombine the electrolytically generated gases could produce unlimited pressures, though with a much slower rise-time than in the case of steam. This requires control system action, backed up by high pressure relief valving.

CHEMICAL
Accidental ignition of the unrecombined hydrogen and oxygen in the cell could create explosive pressures up to ten times higher than the pre-ignition conditions. An event of this nature certainly should be avoided, but it should be expected as well, so that its occurrence does not cause harm. Additionally, the electrolyte, usually a strong caustic, must be handled with care.

OUR SOLUTION
The Electrolytic Thermal Cell (ETC) has been in development since mid-1989 to accommodate these safety concerns within a versatile cold fusion research vessel. Now entering the final stages of production, certification testing of the ETC will begin this quarter. NRG is targeting deliveries of production units for the third quarter of 1994. Sophisticated design and efficiencies of mass production allow a price lower than you might expect.



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About our cover

Free-lance photographer Eric Swanson of Santa Fe, New Mexico, shot this portrait of Dr. Edmund K. Storms who is holding one of the palladium cathodes that were used in his successful cold fusion experiments. Dr. Storms carried out his cold fusion work at the Los Alamos National Laboratory, and continues to be a pioneer in the field.

"COLD FUSION" Magazine

Volume 1, No.1, May 1994

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EUGENE F. MALLOVE
Sc.D.

Why "*Cold Fusion*"?

Dennis Cravens, working with equipment that cost him less than $5,000, much of it from Radio Shack and the local hardware store, has been doing what lots of well-known scientists claim is totally impossible—he's producing "cold fusion" excess heat, and he's been doing it in a lab that he set up in his garage in Vernon, Texas.

Dennis is a professor of chemistry and physics at Vernon Regional Junior College, and he's by no means alone in his success with this new technology. Hundreds of researchers around the world—at universities, national laboratories, and some of the largest corporations in the world—have confirmed the claims of scientists Martin Fleischmann and Stanley Pons. On March 23, 1989 at the University of Utah, they said that their research had shown something very mysterious was happening when an electrical current was passed between a palladium and a platinum electrode in a solution of heavy water and lithium salt. They were getting more power out as heat than they were putting in as electricity.

When they added up all the excess energy their cell had produced, the amount was so large that it could not be explained by a mere chemical reaction—of any kind. The process became known as "cold fusion," for the lack of a better explanation, and because Fleischmann and Pons were using the very same deuterium form of hydrogen that scientists with billion-dollar machines were using to try to create practical *hot* nuclear fusion at temperatures of millions of degrees.

Dennis Cravens doesn't have a $30 million dollar a year budget like the hot fusion laboratory at MIT in Cambridge, Massachusetts, the Plasma Fusion Center, or the Princeton Plasma Physics Laboratory in New Jersey, where new results often capture headlines—even though the hot fusioneers have never produced *a single watt* of excess energy. (If and when the hot fusioneers do achieve excess energy, it will be with a blast of lethal neutron radiation that will have to be tamed.) Professor Cravens' work is completely and deliberately ignored by the U.S. government, even though major Japanese corporations have embraced this upstart technology.

Cravens has already accomplished what the hot fusioneers have never done and will never do with their $500 million a year program. He gets more heat energy out of his tiny liquid cells with palladium or nickel electrodes than he puts in as electrical energy—and he gets it with no radiation. You can read about his experiments in this issue, and—if you are then bubbling over with enthusiasm about "cold fusion"—you may want to try them yourself. He'll point the way.

Something new under the sun

Water as fuel? Pollution-free power? How can unlimited power from water be real, when scientific officialdom supposedly investigated the Fleischmann and Pons claims and found them without merit back in 1989? More to the point, why are some scientists ridiculing anyone who even *suggests* investigating "cold fusion?" Where does all this excess heat in the opposition to cold fusion come from? Is it intellectual arrogance on the part of some scientists and fear of pack journalists that they have to eat crow?

Any student of history will tell you that following many major breakthroughs in science and technology, pioneers were ridiculed by the establishment, often with the enthusiastic support of the news media. Nothing has changed. Did the media cover last December's Fourth International Conference on Cold Fusion on Maui, which was sponsored by the Electric Power Research Institute? If they had, you would have seen cover stories in *Newsweek* and *Time*.

The Maui conference showed, as had the previous international conference in Nagoya, Japan in October, 1992, that hundreds of researchers around the world are achieving the same astonishing results in an increasing variety of reproducible, solid experiments. Unlike the hot fusion people, "cold fusion" researchers aren't trying to mimic the nuclear reactions inside stars, which occur at millions of degrees. So what is this "cold fusion?" Is it a new form of nuclear energy or something else even more remarkable that *seems* like nuclear energy, because it gives out so much continuous power—far more, apparently, than can be explained by chemistry. Yes, there is something new under the Sun, only it doesn't work like the Sun!

If this technology develops as the pioneers in the field expect, within a few years we will be seeing the beginning of the end to our dependence on oil, coal, and natural gas—and the end of much environmental pollution. This new low-cost power source may change the world far more than the automobile, the airplane, the telephone, or the computer. The predictions are that this new process—in all its variants—will provide lower cost power than hydroelectric generators, photovoltaics, wind-driven generators, and nuclear power plants. The generators should be small, light, and inexpensive enough to power cars, homes, and aircraft. Formidable industries stand to lose if they are unable to adapt to the expected rapid development of cold fusion energy.

Water as fuel?

It sounds crazy until you stop to think about it. Sure, for thousands of years we knew we could get energy out of matter through chemical reactions—ordinary fire does that with wood and fossil fuels. Then in the 1930s and 40s we learned how to split the nuclei of uranium atoms to get fission power and the atomic bomb. Soon thereafter, scientists developed the even more powerful hydrogen bomb, which gets energy out of matter by uniting—fusing—forms of hydrogen nuclei. Now science and technology are confronted with what is indisputably another way of getting energy out of matter, a gentler way of tapping it: "cold fusion." Though its detailed mechanism remains unexplained, there is simply no longer any doubt that cold fusion works. To deny the scientific evidence for cold fusion—as many have attempted—is to stand science on its head: to suggest that past "accepted theory" can legitimately falsify thousands of experiments that appear to contradict that theory.

Because of "cold fusion", the world as we know it is about to end.

This is very good news. The fossil fuel age, the Oil Age, will in all probability begin to end during the waning years of the 20th Century, and we intend to be the herald of this process. After five years of controversy and neglect, an astonishing discovery—"water as fuel"—has begun to blossom around the world. "Cold fusion," Utah's "miracle or mistake" of the spring of 1989, turned out to be a real phenomenon after all. Hence this magazine: the world's first devoted exclusively to "cold fusion" and possibly the world's first to have mysterious quotation marks in its name! We do know what we are talking about—excess energy; we just don't know exactly what causes it! Our cold fusion theorist friends have plenty of ideas, and they will be telling you about them in our pages.

Cold fusion has now reached a critical stage in which improved communications will play a key role. The field is in ferment and expanding explosively. In one of history's classic ironies, the 1989 announcement of "excess energy from water" in a relatively simple table-top experiment—possibly by a heretofore unknown form of nuclear energy—occurred less than 12 hours before the Exxon Valdez caused a massive oil spill into the waters off the coast of Alaska. There was an initial media hoopla over the cold fusion story, but the press then lost interest as it became more difficult to discern the truth amid claims and counter-claims of angry chemists and physicists. With few exceptions, journalists bought the notion that cold fusion was nothing but hot air. "Pathological science" became the common insult, as few noticed that *pathological skepticism* about a new phenomenon was the real problem. Contrary to the media's perception, cold fusion never died and was certainly never disproved; it simply went underground as groups of courageous scientists in over a dozen countries mounted a concerted effort to understand and reproduce the mysterious phenomenon. Thanks to their hard work, it has survived.

Where does all this excess heat in the opposition to cold fusion come from? Is it intellectual arrogance on the part of some scientists and the fear of pack journalists that they may have to eat crow?

Scientists in laboratories around the world are closing in on an explanation. Some cold fusion researchers suggest that the nuclei of hydrogen isotopes participate in heretofore unknown nuclear reactions within the confines of metal atomic lattice structures—leading to virtually radiationless nuclear energy. Others say that cold fusion manifests as very faint low-level nuclear reactions, but that its more important aspect for technology—the prodigious heat evolution, which is far beyond known chemistry—comes from a new type of "super-chemistry," which affects not the nucleus but the outermost part of an atom, its electron "cloud."

Whatever nature's long-hidden secret that allows us to use water as fuel, cold fusion phenomena are real beyond any reasonable doubt. Excess power production and low-level nuclear effects have been convincingly demonstrated and made substantially reproducible with a remarkable variety of techniques. Moreover, laboratory cold fusion experiments have begun to exhibit astonishingly high levels of power intensity, surpassing in small volumes the powers found even in fission nuclear reactors—many kilowatts per cubic centimeter, but without associated radiation.

Is cold fusion about to be commercialized?

Yes, indeed! The Japanese Ministry of International Trade and Industry (MITI) has launched a massive "New Hydrogen Energy" program to do just that. It is conservatively estimated that Japanese corporations are already spending $90 million/year on cold fusion, a figure sure to increase dramatically as practical prototype devices emerge in the next few years—or months. In the U.S., where anti-cold fusion sentiment has been particularly intense, the Electric Power Research Institute, the $500-million/year research arm of the electric utility industry, continues its cold fusion R&D program. Last December it organized the Fourth International Conference on Cold Fusion, which was held on Maui. Other entrepreneurial cold fusion companies are springing up in the U.S. This corporate involvement is perhaps the most important reason we decided that the time was ripe for *"Cold Fusion."*

Wayne Green, our New Hampshire publisher, whose magazines helped accelerate the personal computer and other technology revolutions, says that a publication in a new technology area serves three purposes: "It speeds up technical development by providing faster and better communications between the researchers and developers in the new field; it not only helps attract new people to the field, it enables them to get up to speed much faster than they could waiting for books to be published; and, probably of even greater importance, a publication makes it possible for entrepreneurs to provide products to help the new field grow. It makes a new industry develop faster."

There you have it, our mission: to accelerate the "cold fusion" revolution by disseminating the truth about scientific and technological developments in what will surely be one of the most significant technology upheavals in history. We will publish the latest discoveries and findings in a manner that can be understood by a broad spectrum of people. Our intended audience is not restricted to scientists and engineers, though we will certainly aim to provide these experts with timely and challenging material that will help them in their work. "Cold Fusion" will also explore the spectacular changes in store for civilization in the coming energy revolution—technological, as well as economic, social, and political.

We will also expose the strange politics of opposition to cold fusion, both past and present, which has so hamstrung research on the phenomenon. Part of that role will be to comment on how cold fusion is or is not being treated in the news media. We promise that our magazine will expose the numerous instances in which the media have ignored the facts, disparaged honest research, and stood science on its head.

Since the parallel is so striking, it is worth recalling what happened to two American inventors whose initial success occurred just over 90 years ago. On December 17, 1903, Wilbur and Orville Wright realized an age-old dream when they launched the world's first successful heavier-than-air flying machine. For five years, their millennial accomplishment went largely ignored by the scientific establishment and the major media, even though the brothers Wright made no secret of their invention. For years leading up to a dramatic demonstration at Fort Myer, Virginia, they tested their aircraft in full view of commuters on an interurban railroad near Dayton, Ohio. Yet for five years the Wrights were considered cranks by U.S. government bureaucrats who refused to take them seriously! So, in search of support the Wrights took their invention to France.

Formidable industries stand to lose if they are unable to adapt to the expected rapid development of cold fusion energy.

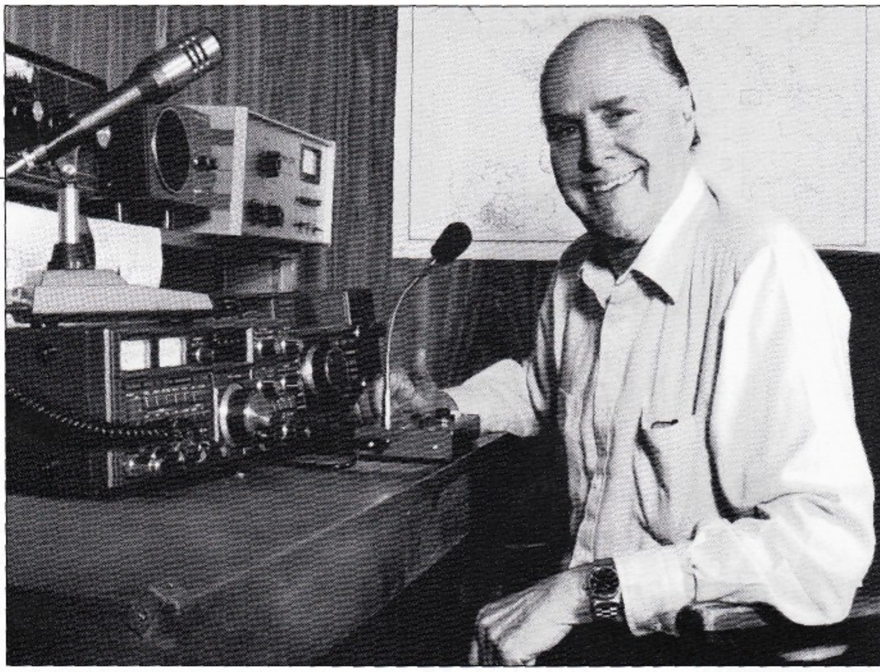
A few years ago, two other scientist-inventors, one American and one British, took another millennial invention to France: "cold fusion." Drs. Pons and Fleischmann are now working on cold fusion energy technology in the well-equipped Japanese-financed IMRA Europe S.A. laboratory near Nice, France. They left behind the scientific bigotry against their discovery that was unleashed in the U.S. The Japanese consortium of industrial giants has given them research funding. Meanwhile, in hundreds of other laboratories the world over, researchers explore an astonishing array of physical phenomena that stem from the original discovery of the cold fusion pioneers. It has been five years since the announcement in Utah, and the "Fort Myers of cold fusion" approaches—the demonstration of prototype technology.

Like flight, which we take for granted today, "cold fusion" will some day be taken for granted. But only five years into the Cold Fusion Age, as we launch *"Cold Fusion"* Magazine, we can hardly imagine anything nearly as exciting and pregnant with virtually infinite possibilities.

Our pages will offer much more than theories on the frontiers of science. In the exciting months to come, "Cold Fusion" will feature some of the most knowledgeable people in the world writing about what the cold fusion revolution is likely to mean for the world. How will cold fusion energy begin to replace the existing energy infrastructure? What will cold fusion automobiles be like, and the "cold fusion home"? What about the impact of water-fuel energy on agriculture, financial markets, geopolitics, and the environment? These will be a continuing focus of this magazine, in addition to detailed reports about the ongoing science, technology, and business of cold fusion. We expect that you will be thrilled with what future issues bring to you.

About the editor ...

"Cold Fusion" Editor Dr. Eugene F. Mallove brings to the magazine broad experience in high technology engineering with Hughes Research Laboratories, TASC (The Analytical Science Corporation), Jaycor Systems Division, Northrop Precision Products Division, and MIT Lincoln Laboratory. Since 1991, Dr. Mallove has worked as a consultant to U.S. corporations conducting and planning R&D in cold fusion. He is the author of three science books for the general public, including the Pulitzer-nominated book on cold fusion, "Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor" (John Wiley & Sons, 1991). He has taught science journalism at MIT and at Boston University; he was Chief Science Writer at the MIT News Office when cold fusion erupted. Prior to that, he was a top science writer and broadcaster with the Voice of America in Washington, DC, and also wrote science and technology articles for magazines and newspapers, including MIT *Technology Review* and *The Washington Post*. Dr. Mallove holds a Doctoral Degree (Sc.D.) in Environmental Health Sciences (Air Pollution Control Engineering) from Harvard University, and a Master of Science Degree (SM, 1970) and Bachelor of Science Degree (SB, 1969) in Aeronautical and Astronautical Engineering from the Massachusetts Institute of Technology.



WAYNE GREEN, PUBLISHER

But, is it real?

One human trait that seldom seems to survive the dumbing down process of the American school system is curiosity. Of course, the system was designed on the assembly-line approach of turning out as identical a product as possible for use as workers in the mass-production factories of the late 19th and early 20th centuries. The school system teaches us to do what we're told and not to ask questions.

The system obviously failed in my case. I am still asking questions. And my betters are still telling me to shut up. This independent streak has gotten me into endless trouble throughout my life. I remember my college physics teacher getting furious with me when I asked him what gravity was. I couldn't even get him to understand that the formula for the gravity force wasn't the force itself. Of course, now I know that no one really knows what gravity is, or how a gravity field acts on things. I got into the same fix when I tried to find out about electricity.

History is packed with wonderful examples of the scientific establishment denying new discoveries and ideas. I keep Max Planck's quote on the subject on my office wall. It's a pity that otherwise reasonably intelligent scientists haven't considered Max's take on them: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it."

A hundred years ago many scientists were firmly convinced that everything of importance had been discovered. Apparently little has happened in the interim to convince many of today's scientists that, despite everything that has been discovered in the last hundred years, there are still many important revolutionary discoveries to be made.

But think about where we are now as compared to 1894. And think about how fast science has been moving. Accelerating. If I were somehow able to transport my wrist watch back just 20 years, the resources of the entire world wouldn't have been able to replicate it.

So here we are in 1994. We all "know" that time travel is and always will be impossible. We all "know" that it will never be possible to visit other solar systems, much less other galaxies. Any scientist who admits to even being *skeptically* interested in researching death, reincarnation, psi, out-of-body experiences, and so on is ridiculed. UFO's? Har-de-har. Contactees? Give me a break! Why even waste time bothering to read books on these ridiculous subjects?

The naïve belief that there's something to this "cold fusion" phenomenon has been getting the same treatment. But this time the "skeptics" are treading on very thin ice. Cold fusion phenomena have become eminently reproducible at some of the world's leading research laboratories. And never mind the concept of being innocent until proven guilty, a concept which seems unacceptable to our media, in the case of cold fusion.

I enjoy experiencing and hearing about anomalies. When I come across an anomaly I see it as an opportunity to open a new door of understanding. Are all of the scientists who have been producing massive excess heat from their cold fusion experiments suffering from delusions? Are all of their measurements faulty? I see what they are calling anomalous heat being generated with palladium-deuterium, nickel-water, molten salts, gas discharge, ceramic proton conductors, hydrosonic pumps, and so on.

I hope you'll spend the $5 and get a copy of Michael Crichton's "Travels," and at least read the last chapter. Oh, you'll enjoy the whole book, but the last chapter is particularly pertinent to smug scientists who ridicule things they don't understand. And don't miss reading "Impure Science," by Robert Bell (1992-Wiley). It's subtitled, "Fraud , Compromise and Political Influence in Scientific Research."

One of the better-known scientists researching the cold fusion effect has recently been publicly ridiculed by his fellow scientists because he is interested in looking into the low-energy transmutation of elements. Shades of alchemy! Do they know there is considerable published scientific evidence for just that? Perhaps they should do their homework before they step out on a limb and hand their target a power saw.

How far are we from turning a bunch of cold fusion research projects into an industry? Lacking an understanding of what's actually happening — where the excess power is really coming from — most of the research so far has been empirical. Hey, let's try this! Okay, that worked, so now let's try it this way. When someone or a group manages to develop enough output power to make a practical product, you're going to see the birth of what looks like it could well be one of the largest industries in the world. Visit the British Museum in London some time and look at the first steam engines and the first internal combustion engines. Well, we're not quite that far along with cold fusion yet. But one thing we know for sure is that many of the high-tech products in everyday use in 2014 probably could not be replicated with the world's resources transported back to us now.

Scientists tend to be smug. But it doesn't take much reading of history to learn that most of the major technological breakthroughs have been made by amateurs. I was surprised and pleased when I went to Rensselaer Polytechnic Institute to find that many of my classmates were avid "Astounding Science Fiction" readers. Later I got to be good friends with John Campbell, the editor (and a ham operator - W2ZGU). I took him as my editorial role model when I started my first publication in 1952 (*Amateur Radio Frontiers*). By the way, you could do worse than subscribe to *Analog*, the current title for the old *Astounding*. The science fact articles alone are worth many times the subscription price.

A new industry

In 1969 I got the idea of promoting amateur radio VHF repeaters as a way to revive what was a then slowly dying hobby. I started by publishing hundreds of articles on the subject in *73*, my ham magazine. Then I started the *Repeater Bulletin*. Within two years, repeaters became the single largest amateur radio interest, quickly spreading all around the world. Hams, with little handy-talkies, were able to talk for hundreds of miles and make phone calls from almost anywhere in the country. I could call home while skiing down a mountain in Aspen!

It didn't take long before the hams working at Motorola and G.E. figured out that this could be a big new service for the public. It's now known as the cellular telephone and is patterned after the ham repeater system set up in Chicago. And that directly resulted from my magazines and the books I published on the subject. No one else was pushing repeaters.

It was my success in building a whole new industry with my publications that got me to start *Byte* magazine when the first microcomputer was put on the market. I wanted to do it again. After starting *Byte*, I went on to start *Kilobaud,Microcomputing*, *80 Micro*, *InCider*, *Micro Marketing*, *Desktop Computing*, *Hot CoCo*, *Run*, *Pico*, dozens of computer books, Instant Software, Evergreen Software, and a chain of 58 Software Centers.

When the compact disc was introduced I started *Digital Audio* to help CDs be accepted. This turned out to be the fastest growing new consumer electronics industry in history.

What I'd discovered is that a dedicated publication speeds up technical developments by providing fast communications between the researchers in the field. It also helps attract newcomers and brings them up to speed quickly. And perhaps most important, it provides a means for entrepreneurs to provide products for the new field and reach their potential customers inexpensively. A publication helps turn a new technology into a new industry.

When 8mm video came along I tried to interest the major players in supporting a dedicated magazine. I failed. Maybe you've noticed that while 8mm is still around, it hasn't really gone anywhere. I believe that a magazine would have helped 8mm knock out Matsushita and their VHS-C format, and put it in common use for a wide variety of businesses and services. Hi-8 could have been the standard for most smaller TV stations. It might even have replaced the VHS VCRs, just as VHS replaced the Beta format. Billions of dollars lost.

How far are we from turning a bunch of cold fusion research projects into an industry?

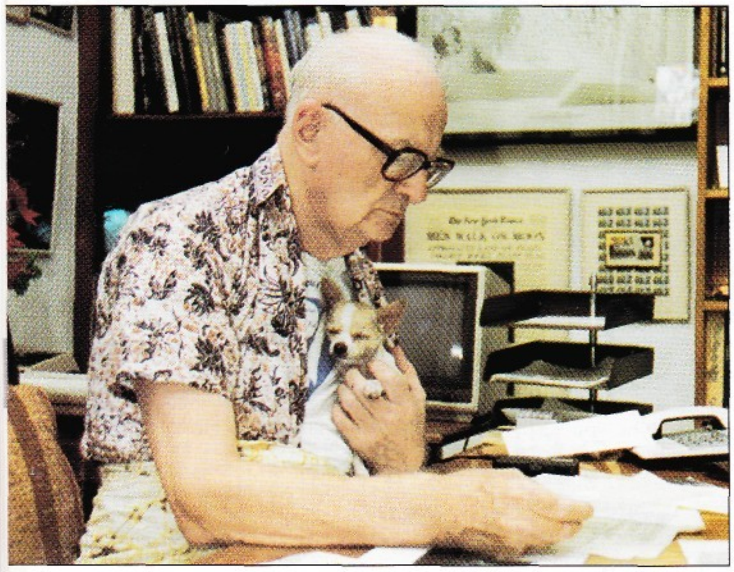
Cold fusion

So you can see why I got excited when I found that the Pons and Fleischmann experiments had been confirmed and even surpassed by new ways of generating excess heat, and that whatever the phenomenon was, it looked promising. I'll let the scientists argue over where the power is coming from. Has it something to do with two deuterium nuclei merging to form helium, with the left over energy being converted to heat instead of radiation? Some cold fusion theorists think so, but others have even more exciting explanations. We'll reserve judgment until one theory fits the data perfectly.

How accurate are laboratory claims of several kilowatts per cubic centimeter of palladium or even nickel? Can the process be scaled up and still work? I'll leave it for museums to dig up artifacts of the past. I'm interested in artifacts for the future. Like helium, anomalous heat, and practical working devices.

"It is beyond serious dispute any more that anomalous amounts of energy are being produced from hydrogen by some unknown reaction...Now, please fasten your seat-belts. After these modest daydreams, I want to really stretch your imaginations."

2001: The Coming Age Of Hydrogen Power...



And the Dawn of a New Era

by Arthur C. Clarke

Fellow of King's College, London
Chancellor, International Space University
Chancellor, University of Moratuwa

What follows is Arthur C. Clarke's memorable address at the Pacific Area Senior Officer Logistics Seminar (PASOLS) in March, 1993 at the Hilton in Colombo, Sri Lanka. Among the audience were Adm. Larson, Commander In Chief of the Pacific Fleet, Lt. Gen. Stackpole of the Marines, and leading officers of the military forces from many other countries, including Australia, India, Japan, Korea, Russia, the Philippines, Sri Lanka, and others.

Admiral Larson, Lieutenant General Stackpole, Major General Abayaratna, distinguished guests, I'm very happy to be here today, even though I should really be in Washington this week. All my friends will be gathered in the Uptown Theatre to celebrate the 25th anniversary—I can't believe it—of 2001: A Space Odyssey.

Now, that movie provides a very good example of how difficult it is to predict the future. You may recall that in the film we showed the Bell system and PAN-AM—well, they've both gone, long before 2001. But I'm happy to see that the Hilton, which we also showed in 2001, is still here, though not yet in orbit!

This proves how impossible it is to predict social and political developments: Who could have imagined what's happened in Europe during the last few years? However we can, to some extent, anticipate technological developments, by observing what's going on in science and engineering. But the problem there is predicting when things will happen, even though one can be quite certain that they will.

A good example is provided by my 1945 paper on communications satellites, which I imagined would be large, manned space-stations. When I wrote that, World War II was still in progress, and I was working on Ground Controlled Approach Radar, which had the then enormous number of something like a thousand vacuum tubes in it, at least one of which would blow everyday.

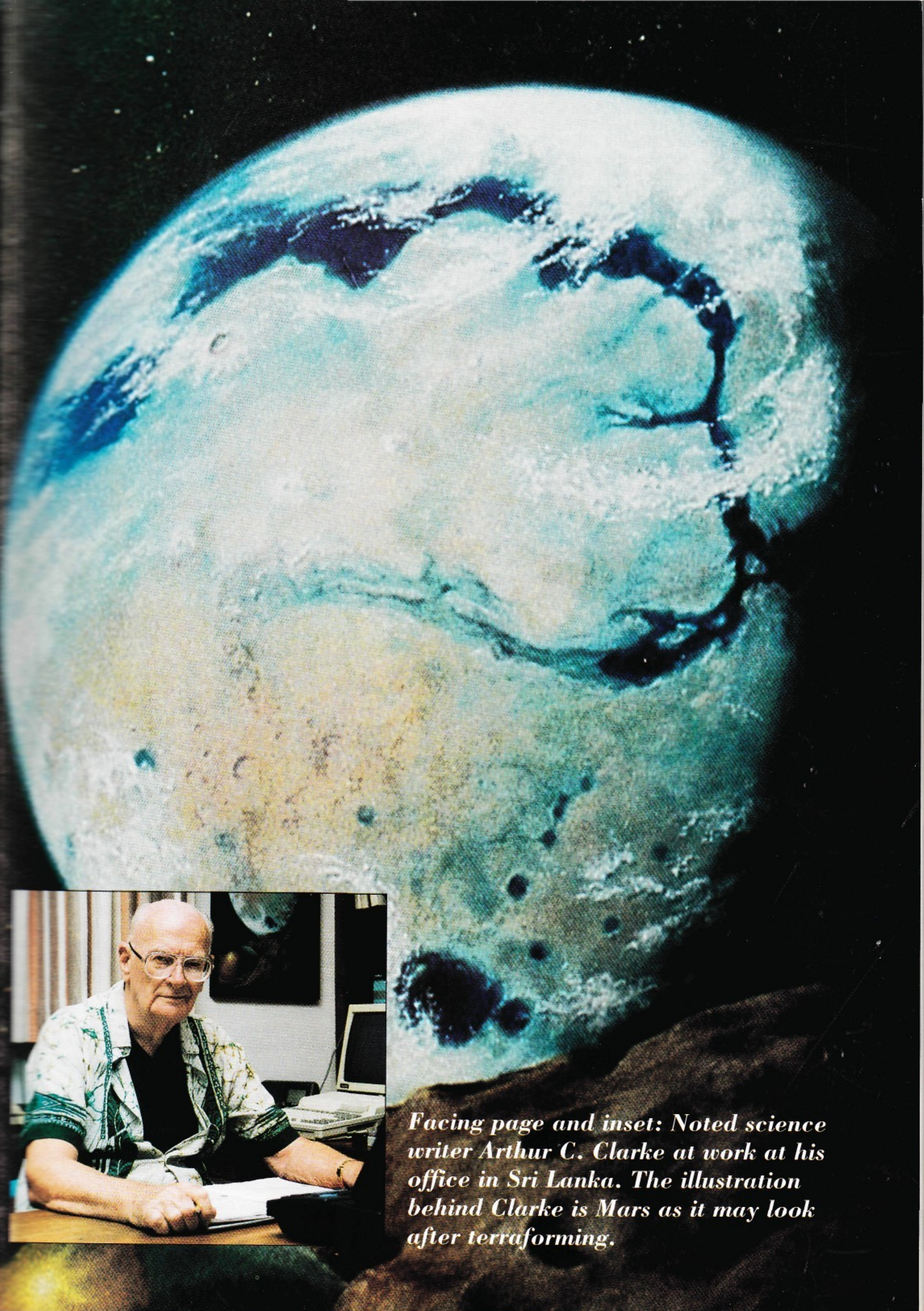
So it was impossible to believe, back in 1945, that TV relay stations could operate without a staff of engineers changing tubes and checking circuits. But of course, the transistor and the solid-state revolution came along within a few years, and what I'd assumed would have to be done by large manned stations could be achieved by satellites the size of oil drums. So everything I imagined would be done around the end of the century happened decades in advance.

I'm going to say very little about communications satellites and the communications revolution, because you are all very familiar with what's happened here. Essentially anything we want to do in this area can now be done. And satellites have not only transformed communications, but meteorology and navigation. You all know what the GPS (Global Positioning System) did during Desert Storm. However, the satellites I have always been particularly interested in are what I call "Peacesats," the reconnaissance satellites largely responsible for the Cold War never becoming a hot one, ones which created a transparent world, and vastly reduced the threshold of uncertainty. But I won't say any more about satellites, because (if I may be allowed a commercial) I've just written a whole book about them, How The World Was One.

So now I want to change the subject completely, to something perhaps even more important than the communications revolution. But first I'd like to mention a bit of forgotten history.

In December, 1903, Orville and Wilbur staggered off the ground in North Carolina, and made the first controlled flight in a heavier-than-air machine. As a result, the North Carolina state motto is "First in Flight," which you military men may well think a rather unfortunate choice of words.

Yet for five years, Washington didn't believe that the Wright brothers had actually flown—because everybody knew it was impossible. Leading scientists were then still writing papers proving it couldn't be done. Not until the Wrights went to France and started giving public demonstrations did the boys in the War Department say, "My goodness, these things really can fly. Perhaps they may even be useful for reconnaissance. We'd better look into it." And they did—five years late.



Facing page and inset: Noted science writer Arthur C. Clarke at work at his office in Sri Lanka. The illustration behind Clarke is Mars as it may look after terraforming.

Well, history has just repeated itself with what's been (perhaps inaccurately) named "cold fusion."

You all know, of course, that the Sun is powered by the fusion of hydrogen atoms, when they combine to make helium. Tremendous efforts have been made to reproduce this reaction on Earth and produce virtually unlimited amounts of energy. The only successful attempt to do this so far is the Hydrogen Bomb. Literally billions of dollars have been spent in efforts to reach the multi-million degree temperatures in the heart of the Sun, where this reaction occurs. One day these experiments will succeed, but so far only a few percent of the input energy has been obtained, for very short periods of time.

In the Year 2002...

However, just four years ago, scientists Stanley Pons and Martin Fleischmann claimed to have achieved "cold fusion" at room temperature in certain metals saturated with deuterium, the heavy isotope of hydrogen. these conditions, they reported that they were getting more out than they put into the system. This, of course, created a worldwide sensation, and many laboratories tried to repeat the experiments. They all failed, and Pons and Fleischmann were laughed out of court. That was the last anyone heard of them for a couple of years.

Twenty years ago, when OPEC quadrupled oil prices, I remarked "The age of cheap power is over — the age of free power is still fifty years ahead." I may have been slightly too pessimistic.

Meanwhile, there was an underground movement of scientists who believed there might be something in all this business, and started experiments of their own—often in defiance of their employers.

Pons and Fleischmann went to France—just like the Wright brothers—and are now working in a laboratory near Nice, financed by the Japanese consortium Technova. Even more significant, Japan's Ministry of International Trade and Industry (MITI) is investing millions of dollars in an effort to commercialize the new technology.

The laboratories of NTT, the Japanese telecommunications organization, recently announced positive results, and just before last Christmas, NTT started selling do-it-yourself cold fusion kits for $565,000. I don't know how many of them were snapped up, but that price sounds like a bargain for a discovery that could change the world.

In October, 1992, the Third International Cold Fusion Conference took place in Nagoya, Japan, and was attended by over 300 scientists. The conference highlights have been summarized in a 34-page report by Professor Peter Hagelstein, of MIT's Research Laboratory for Electronics. Other reports confirming positive results have been issued by the U.S. Navy Air Weapons Center, the U.S. Army Research Office in Japan, SRI International, and many others.

It is beyond serious dispute any more that anomalous amounts of energy are being produced from hydrogen by some unknown reaction. The term "cold fusion," "C/F," has stuck because no one can think of anything better. However, the skeptics who originally pooh-poohed the whole thing did have a very good point. If it really were fusion, the experimenters should be dead! Where were the neutrons and gamma rays and tritium and helium—the lethal "ashes" such a reaction should produce?

They have been detected, but in quantities far too small to account for the energy liberated. The theoretical basis of C/F therefore still is a major mystery, as was the energy produced by radioactivity and uranium fission when they were first discovered.

What are the implications of this? I'd like to give several scenarios.

- There's a conspiracy of hundreds of scientists in dozens of countries. They're either totally incompetent, or they're superbly organized, and out to make a killing in oil and coal shares.

Slightly more probable:

- C/F is a laboratory curiosity, of great theoretical interest but no practical importance. Frankly, I doubt this. Anything so novel indicates a breakthrough of some kind. The energy produced by the first uranium fission experiments was trivial—but everyone with any imagination knew what it would lead to. Cold fusion can be scaled up to moderate levels, say 100—1000 kilowatts. Even that could be revolutionary, if cheap and safe units can be manufactured. It would make possible the completely self-contained home that Buckminster Fuller envisaged, because the electric grid would no longer be necessary for domestic distribution. And it would be the end of the gas-fueled car—none too soon. Automobiles could, quite literally, run on water, though perhaps only heavy water.

- The third possibility is that there are no upper limits. In that case, the Fossil Fuel Age has ended, along with CO_2 buildup, acid rain, and air pollution.

Twenty years ago, when OPEC quadrupled oil prices, I remarked "The age of cheap power is over—the age of free power is still fifty years ahead." I may have been slightly too pessimistic.

However, coal and oil will always be essential raw materials for an unlimited range of products—chemicals, plastics, even synthetic foods. Oil is much too valuable to burn: We should eat it.

Five, four, three, two . . .

Now, please fasten your seat-belts. After these modest daydreams, I want to really stretch your imaginations.

Back in 1982, I published 2010: Odyssey II and dedicated it to my friend cosmonaut Alexei Leonov, and to academician Andrei Sakharov, then in exile in Gorky. I knew that Sakharov had worked on low-temperature nuclear fusion (as well as on the H-bomb) and in the novel I suggested that, in his enforced solitude, he'd invented a spaceship engine based on these principles.

He didn't, of course, so that's a piece of fictitious history. However, three Russian scientists who have indeed been working on nuclear propulsion for rockets have gotten into the cold fusion act, and have recently published some startling results in Physics Letters A, one of the world's leading scientific journals.

The three Russian scientists are obtaining about five times their energy input in gas mixtures, not solids, at temperatures of up to 1800 C. This is not exactly "cold" fusion—but it's certainly ice-cold compared with the tens of millions of degrees the hot-fusioneers are talking about.

And it's very interesting, indeed, from the point of view of rocket propulsion. If a plasma-fusion rocket could be developed, it would open up the solar system, just as the airplane opened up this

References:

1. *"The Third International Conference on Cold Fusion": Drs. Victor Rehn and Iqbal Ahmad (U.S. Office of Naval Research, Japan)*
2. *"Anomalous Nuclear Reactions in Condensed Matter: A report on the Third International Meeting on Cold Fusion": Dr. Iqbal Ahmad (U.S. Army Research Office [AMC] Far East)*
3. *"Summary of the Third International Conference on Cold Fusion in Nagoya": Peter L. Hagelstein, Massachusetts Institute of Technology—Research Laboratory for Electronics*
4. *"Nuclear Product Ratio for Glow Discharge in Deuterium": A.B. Karabut, Ya R. Kucherov and I.B. Savvatimova.* Physics Letters A *170,265 (1992)*
5. *"Deuterated Metals Research at SRI International": 4 Mar 1993*

It's not generally realized that the energy cost of going to the Moon is less than a hundred dollars in terms of kilowatt-hours of electricity. The fact that the Apollo roundtrip tickets cost about two billion dollars per passenger is a measure of the chemically-fuelled rocket's inefficiency. Well, back to Earth. I'd like to read you a letter which I sent to Vice-President Gore last week—it should have reached him by now.

"COLD FUSION"

Dear Mr. Gore,

I am happy to learn that you are being briefed on the above—perhaps misnamed—subject, as it is impossible to imagine anything of greater potential importance from both the economic and geopolitical points of view.

After initial skepticism, I have now seen so many positive reports from highly respected organizations (e.g. NTT—which is already marketing experimental kits in Japan!—ONR, U.S. Army Research Office, SRI, MIT), that there can be no further doubt that excess energy is being produced by some previously unknown process, not essentially nuclear. I am sure that your staff has already seen much of this material, and I also refer you to Representative Swett's statement in the Congressional Record for 16 February, 1993.

Whatever the source of the energy—which I am sure will be elucidated in the fairly near future—the sixty-four trillion dollar question is: (1) is this merely a laboratory curiosity of no practical importance, or (2) can it be scaled up for industrial and perhaps even domestic use?

If No. 2 is correct, the consequences are immeasurable. It would mean essentially the end of the "Fossil Fuel Age," and an era of cheap, clean power. The environmental benefits would be overwhelming; at the very least, concern with CO_2 build-up and acid rain would vanish.

Clearly, no effort should be spared to resolve this matter speedily, by supporting scientists who are obtaining results (and, perhaps, discouraging those who have been obstructing them.) One witness you might call is my friend Dr. George Keyworth II, President Reagan's Science Advisor and an expert on fusion physics, who remarked in a recent letter to me: "The conventional path we've been pursuing is trying to build a bridge across the seas instead of inventing a boat." Perhaps "Cold Fusion" may give us the lifeboats Spaceship Earth so badly needs!

Respectfully,

Arthur C. Clarke

And as Stop Press, I should mention that Representative Dick Swett has just made the same point in a statement to the House Committee on Energy (March 26, 1993). Let's see if it produces more energy than went into it.

In conclusion: With monotonous regularity, all throughout history, religious crackpots have predicted the imminent end of the world. I have about 90 percent confidence that I'm now doing something very similar...And this time, it's good news.

Thank you.

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COLD FUSION: DOES IT HAVE A FUTURE?

Look to the rising sun

by Julian Schwinger

Abstract: Dr. Julian Schwinger outlines the case against the reality of cold fusion. It is based on preconceptions inherited from experience with hot fusion. That cold fusion refers to a different regime is emphasized. The new regime is characterized by intermittency in the production of excess heat, tritium and neutrons. A scenario is sketched, based upon the hypothesis that small segments of the lattice can absorb released nuclear energy. Julian Schwinger developed the theoretical basis for quantum electrodynamics, for which he shared the 1965 Nobel Prize in Physics with Richard Feynman and Shin'ichiro Tomonaga. This lecture was given in Tokyo near December 7, 1990.

A totally unexpected phenomenon has been discovered in a certain field of science. It could have significant implications for the future of mankind, and especially for the Japanese. The overwhelming reaction of the experts in the field is rejection, based on the absence of other effects that are considered to be necessary companions of this new phenomenon.

To quote one expert: "We know a lot about what happens... We no longer have the latitude to say 'Well, some strange event occurred and generated those things.'" Nevertheless, this new possibility seems to have enough validity that one skeptic said, "It's hard to believe it. But there seems to be something to this. It should not be necessary, however, to understand the mechanism before embracing the concept. If a proven track record can be established... you have to believe it."

Reproducibility is often cited as a canon of science. And so it is, in established areas. But, early in a study of a new phenomenon that involves an ill-understood macroscopic control of a microscopic mechanism, irreproducibility is not unknown.

To which scientific field does all this refer? In view of the title of my lecture, the question may seem surprising. In fact, the object is *seismology*. The new phenomenon is the occurrence of electromagnetic effects just prior to the onset of an earthquake. The most striking event happened on October 17, 1989. An apparatus set up by a team of radio detection specialists in the Santa Cruz mountains of California received an unprecedented blast of radio power. The strong signal continued for several hours, and then stopped, to be followed by the Loma Prieta earthquake that, last year, wreaked severe damage in the San Francisco area.

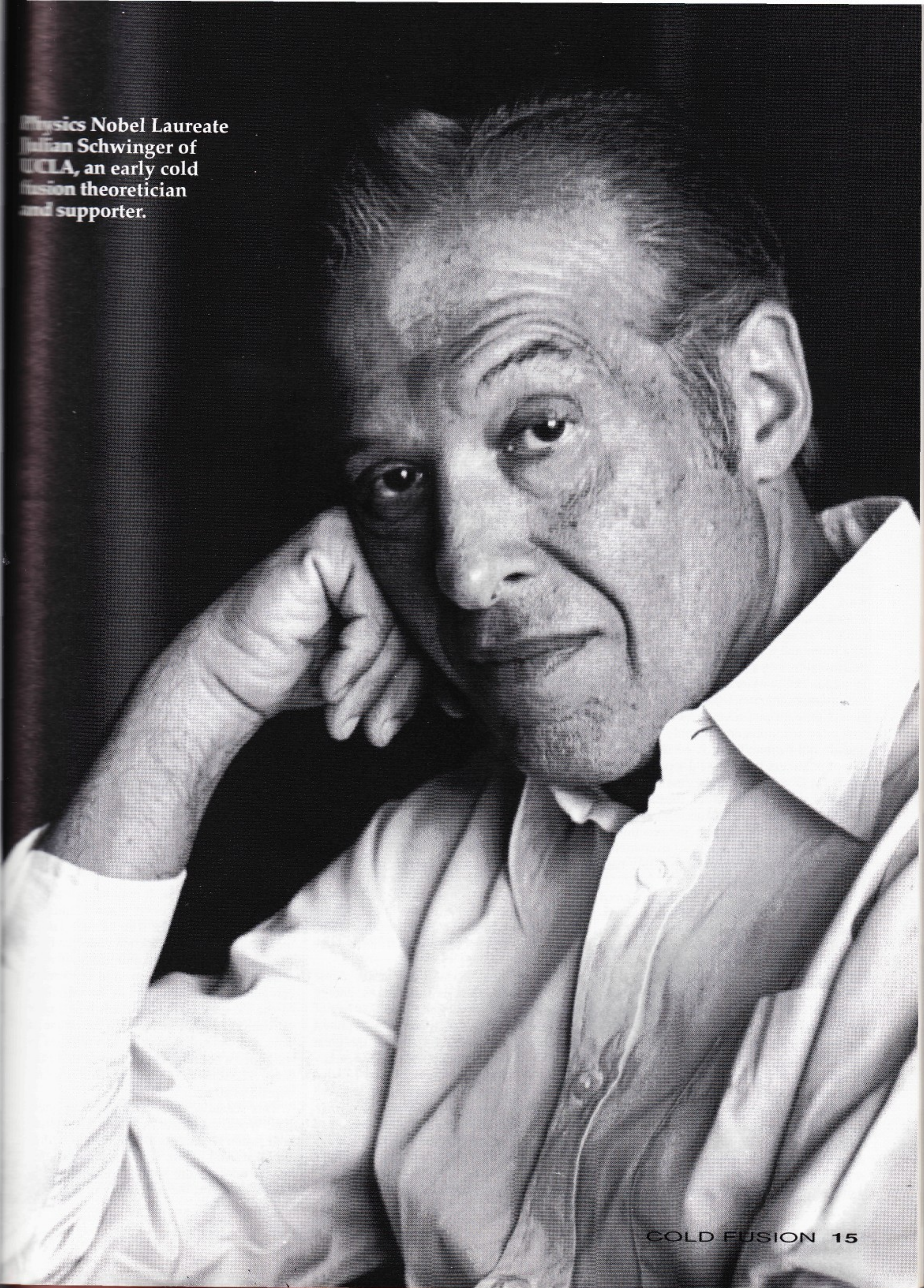
As a principle, the above also applies to the phenomenon of cold fusion.

It is astonishing that there was an early precursor of the claim to have achieved cold fusion. Dated at the beginning of the Showa era, the German title of the paper is translated as "On the transformation of hydrogen into helium." At that time, neither the existence of the heavier isotopes of hydrogen, nor of the lighter isotope of helium, was recognized. If, indeed, they did produce helium, was it ^4He (helium-4), or was it ^3He (helium-3)? Incidentally, at just that time, Nishina Yoshio was at Niels Bohr's Institute in Copenhagen. One can only wonder how he reacted to the bizarre claim.

On 23 March, 1989, the University of Utah, at Salt Lake City, threw a press party. Its purpose was to establish priority for patents on a new source of energy. The impetus was supplied by what seemed to be a rival group, down the road at Provo, Utah. The patent lawyers needn't have worried. The Provo people were investigating a very weak source of neutrons, which is only of academic interest. But, science filtered by patent attorneys is no longer science. Isn't it possible to establish a track record without reference to the initial claimants?

The National Cold Fusion Institute has provided a clearing-house for reports that bear on the reality of cold fusion. As of August, 1990, 78 other groups from all over the globe have reported positive evidence, as conveyed by the detection of one or more of these indicators: Excess heat, tritium, neutrons, γ-rays, ^3He. The standard response to such a list is: "Yes, but what about the much larger number of failures?" Does anyone really think that the scientific judgment is like an election, in which the majority carries the day?

The characteristics that seem to be common to all successful cold fusion experiments are: (1) Intermittency—the production of heat, of tritium, of neutrons, coming in bursts, switching on and off at random. (2) Irreproducibility—seemingly identical cells vary widely in their ability to "turn on." It may not be too much of an exaggeration to say that, early in April, 1989, everyone—including those who, like myself, had to look up the meaning of enthalpy—had thrown together an electrolysis apparatus and was waiting for dividends. After a few weeks, with no reward, they quit in disgust, and denounced it all as incompetence, or fraud. Their votes are irrelevant.



Physics Nobel Laureate Julian Schwinger of UCLA, an early cold fusion theoretician and supporter.

The defense is simply stated: The circumstances of cold fusion are not those of hot fusion.

Reproducibility is often cited as a canon of science. And so it is, in established areas. But, early in a study of a new phenomenon that involves an ill-understood macroscopic control of a microscopic mechanism, irreproducibility is not unknown. That was so at the onset of microchip studies. It also appeared in the initial phase of the discovery of high-temperature superconductivity, which, by the way, is a prime example of "embracing the concept" without having "to understand the mechanism."

What is it about cold fusion that seems to enrage a substantial number of physicists? The people who have spent a lot of money on hot fusion would doubtless echo: "We know a lot about what happens. We no longer have the latitude to say, 'Well, some strange event occurred and generated those things.'" To be specific, this is how their preconceptions work: (1) In hot fusion, the union of two deuterons, to form ^{3}He and a neutron, proceeds at about the same rate as the formation of a triton and a proton. But the emission of neutrons from palladium electrodes immersed in heavy water occurs at a rate around the insignificant background level. Conclusion: No neutrons—no cold fusion. (2) The two cited reactions are the only important ones in hot fusion. There is no independent source of excess heat. Conclusion: Incompetence. (3) Given the essential absence of neutrons, what of the claims for substantial tritium production? Conclusion: Fraud. (4) At the low energy of cold fusion, the penetrability of the Coulomb barrier is so overwhelmingly small that nothing could possibly happen. Conclusion: Stupidity.

The next item of the hot-fusioneer's creed are responses to suggested cold-fusion mechanisms: (5) Very soon after March 23, 1989, it was proposed that excess heat is produced by the formation of ground state ^{4}He in the DD fusion process. Response: Where is the accompanying γ-ray of roughly 20 million electron volts? (6) Then came the recognition that excess heat might be dominated by HD, rather than the DD reaction. Heavy water unavoidably contains some fraction of a percent of light water. The fusion of a proton with a deuteron produces ^{3}He. Response: Where is the accompanying γ-ray of roughly five million electron volts? (7) The HD reaction is a source of heat and of ^{3}He, but not of neutrons or tritium. The latter must come from the DD reaction. What happens if two fusing deuterons populate, not in the ground state, but in the first excited state of ^{4}He? That excited state is unstable against decay into a triton and a proton. It is *stable*, however, for decay into a neutron and ^{3}He. Here then, is a mechanism to account for the great disparity between neutron and triton production—the ratio is about one in a hundred-million—that seems to be characteristic of cold fusion. Response: Where is the accompanying γ-ray of about four million electron volts?

So stands the indictment of cold fusion. The defense is simply stated: The circumstances of cold fusion are not those of hot fusion.

It is a standard operational procedure, in hot fusion work, to represent the reaction rate as the product of two factors: The barrier penetration probability, which involves only the Coulomb repulsion; and, the intrinsic reaction rate, which is dominated by nuclear forces. But, at the very low energy of cold fusion, one is dealing, essentially, with a single wavefunction, which does not permit such factorization. The effect of Coulomb repulsion cannot be completely isolated from the effect of the strongly attractive nuclear forces. This is a whole new ballgame. It is, so to speak,a sumo tournament restricted to the *maku-no-uchi*, indeed, to the *yokuzuna*.

The wavefunctions for a low energy proton and deuteron, and for a low energy pair of deuterons, are effectively dominated by zero relative angular momentum. They are states of even orbital parity. The intrinsic parities of all relevant particles—neutron, proton, deuteron, triton, ^{3}He, ground state, and first excited state of ^{4}He—are also positive. So, the normally dominant process of electric dipole radiation is forbidden; it requires a parity change.

If the γ-rays demanded by the hot-fusioneers are greatly suppressed, what agency does carry off the excess energy in the various reactions? One must look for something that is characteristic of cold fusion, something that does not exist in the plasma regime of hot fusion. The obvious answer is: The lattice in which the deuterium is confined.

Imagine, then, a small but macroscopic piece of the lattice absorbs the excess energy of the HD or DD reaction. Please—I beg of you—do not rise in high dudgeon to protest that this is impossible because of the great disparity between atomic and nuclear energy scales. That is a primitive reaction to what may be a very sophisticated mechanism. And do not forget the failure of theory to predict, and then to account for the phenomenon of high temperature superconductivity. I advance the idea of the lattice playing a vital role as a *hypothesis*.

Past experience dictates that I remind you that a hypothesis is not something to be proved mathematically. Rather, it is a basis for correlating data and for proposing new tests, which, by their success or failure, support or discredit the validity of the hypothesis. It is the essence of the scientific method.

Intermittency is the hallmark of cold fusion. It incorporates irreproducibility as a circumstance in which the time intervals between bursts significantly exceed the duration of the observations. Intermittency is the ultimate rebuttal to the charges of fraud in the tritium production. Externally introduced tritium maintains an essentially constant counting rate. There is no resemblance to the switching on and off of the observed bursts. Does the lattice hypothesis have a natural explanation for intermittency?

One needs information about the lattice structure of deuterided palladium. The experts say that, "We know a lot… ," but that knowledge does not include what happens in the important regime of heavy deuteron loading. There is, however, a theoretical suggestion that, in the circumstance of heavy loading, a pair of new equilibrium sites comes into existence within each lattice cell. The equilibrium separation for that pair is significantly smaller than any other such distance in the cell.

It would seem that a close approach to saturation loading is then required for effective fusion to take place. but, surely, the loading of deuterium into the palladium lattice does not occur with perfect spatial uniformity.

The replacement of impartial reviewing by censorship will be the death of science.

There are fluctuations. It may happen that a microscopically large—if macroscopically small—region attains a state of such lattice uniformity that it can function collectively in absorbing the excess nuclear energy that is released in an act of fusion. And that energy can initiate a chain reaction as the vibrations of the excited ions bring them into closer proximity. So begins a burst. In the course of time, the increasing number of vacancies in the lattice will bring about a shut-down of the burst. The start-up of the next burst is an independent affair.

This scenario raises an interesting question: Would the efficacy of room temperature cold fusion be enhanced significantly by further lowering of the ambient temperature? Lower temperature would presumably decrease somewhat the probability of the initial

Cold fusion and sonoluminescence: A postscript

Physicist Julian Schwinger's keen mind continues to probe the enigma of cold fusion, which he maintains can be explained by metal atomic lattice-induced nuclear reactions. He has published his theoretical works on cold fusion in, among other journals, the *Proceedings of the National Academy of Sciences*.

Ever up to the challenge of trying to fathom the physics behind seemingly "impossible" phenomena, Schwinger has in recent years become fascinated with "sonoluminescence." Like cold fusion, sonoluminescence "should not exist," but it does. This now well-established phenomenon occurs when ultrasonic sound, beamed into a liquid, causes bubbles to oscillate stably—to expand and contract regularly—and also to emit regular pulses of light.

Since the 1930s, it has been known that under the right conditions light can emerge from so-called "cavitation" bubbles in fluids exposed to ultrasonic sound or undergoing turbulence. In 1990, a group under Professor Seth Putterman at UCLA discovered that the light from a *single* acoustically-driven bubble could come out in extremely brief pulses of 100,000 light photons—a pulse duration of less than 50 pico-seconds, about 10^{-11} seconds—this, when the oscillation period of the emitting bubble was much larger, or 10^{-4} seconds. Schwinger has drawn parallels between cold fusion and sonoluminescence in his continuing technical publication on both topics. He has also developed an impressive theory to explain sonoluminescence.

Schwinger remarks in one of his commentaries in 1991: "The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different. But they both depend significantly on non-linear effects. Put in that light, the failures of naive intuition are understandable."

For our more technically inclined readers we offer the following list of Schwinger's publications in this area these past five years. *"Cold Fusion"* Magazine will publish more of Julian Schwinger's eloquence, and his perceptions of the frontiers of physics. —Gene Mallove

Works on cold fusion and sonoluminescence by Nobel Laureate Julian Schwinger

"Nuclear Energy in an Atomic Lattice." *Proceedings of the First Annual Conference on Cold Fusion, March 28-31, 1990, Salt Lake City, pp. 130-136.*

"Cold Fusion: A Hypothesis," Zeitschrift Für Naturforschung, *Vol, 45, No. 5, May, 1990, p. 756.*

"Cold Fusion: Does it Have a Future?" in Evolutional Trends of Physical Sciences, *Springer Verlag, 1991. (From a talk delivered in Tokyo, 1990)*

"Phonon Representations," Proc. Natl. Acad. Sci., *Vol 87, September 1990, pp. 6983-6984.*

"Phonon Dynamics" Proc. Natl. Acad. Sci., *Vol. 87, November 1990, pp. 8370-8372.*

"Nuclear Energy in an Atomic Lattice—Casual Order", Prog. Theor. Phys., *Vol. 85, No. 4, April 1991, pp. 711-712.*

"A Progress Report: Energy Transfer in Cold Fusion and Sonoluminescence," a lecture delivered at MIT and at the University of Pennsylvania, autumn 1991.

"Casimir Energy for Dielectrics," Proceedings of the National Academy of Sciences, *Vol, 89, May 1992, pp. 4091-4093.*

"Casimir Energy for Dielectronics: Spherical Geometry," Proceedings of the National Academy of Sciences, *Vol. 89, December 1992, pp.1118-1120.*

"Casimir Light: Pieces of the Action," Proceedings of the National Academy of Sciences, *submitted, 1993.*

"Cold Fusion Theory: A Brief History of Mine," a talk for the Fourth International Conference on Cold Fusion, Maui, December 6-9, 1993 (spoken by Eugene Mallove, in Schwinger's absence).

fusion. But, it should increase the probability of forming and maintaining the lattice structure against the destructive onslaughts of thermal agitation. Experiment must supply the answer.

I find it both amusing and tragic that the members of a panel, investigating the charge of fraud in tritium production by cold fusion, dismissed the charge as "unlikely" and "much less probable than that of inadvertent contamination or other unexplained factors in the measurement." That the "unexplained factors" might be the reality of cold fusion was not admitted. Why? Because "critics" questioned the results, saying that the tritium was not accompanied by other fusion byproducts… " It is the old story. If a significant flux of neutrons is not observed, there cannot be any tritium, even though one finds tritium with a signature that differentiates it both from external and internal contamination.

The pressure for conformity is enormous. I have experienced it in editors' rejection of submitted papers, based on venomous criticism of anonymous referees. The replacement of impartial reviewing by censorship will be the death of science.

Does cold fusion have a future? I have little hope for it in Europe and the United States—the West. It is to the East, and, specifically to Nihon, that I turn. The willingness that the Japanese have displayed, of foregoing short-term rewards for greater long-term successes, should be a key ingredient in this endeavor.

Indulge me in a fantasy, not of the future, but of the past. I should like to think that, if cold fusion had been a burning topic a few years before 1951, as well it might, Nishina would have recognized that was a subject for open-minded research—not suppression. And, in view of the physico-chemical nature of this subject, that he would have thrown all the resources of the Institute of Physical and Chemical Research into the study and development of cold fusion. Dare one hope that a dream of the past also contains a glimpse of the future?

Domo arigato gozaimasu. Thank you very much.

Professor Emeritus Julian Schwinger is a member of the Department of Physics, UCLA.

The Solid State Alters Nuclear Behavior

Radioactive decay mystery

By Christopher Tinsley

Almost any scientific or technology periodical is not ordinarily good bed-time reading. *Physics Letters A.* is no exception. Well, usually. But open the issue for January 3, 1994, and just inside the front cover is a well-written, clear paper, titled, "Reduced radioactivity of tritium in small titanium particles," written by Otto Reifenschweiler, the retired Chief Physicist of Philips Research Laboratories in The Netherlands. It just may also be one of the most important papers in 20th century physics—though it may be a big mistake. It doesn't *look* like a mistake, it *looks* like a beautiful piece of experimental science.

Essentially the paper says that if tiny crystals of titanium are allowed to absorb tritium (in the ratio of one atom to about 285 titanium atoms) then the resultant material is less radioactive than the original tritium. [Tritium is the radioactive isotope of hydrogen, which has two neutrons plus a proton in its nucleus.] Worse, if the material is heated the radioactivity drops further. No problem. The radioactivity is being shielded somehow, and when you heat a metal the tritium will be driven out. Not so, it seems. Tritium is a beta-emitter, giving off an electron when it decays to ^3He (helium-3). These electrons can form an electric current which is measurable, tiny though it is. And the X-rays produced in the decay can be measured as well.

If it looks like a duck, walks like a duck…

Accompanying the live experiment (where any tritium released by the metal is pumped out at once) is a dummy, which measures the outgassing of an identical sample. The dummy is heated in the same way, and virtually no tritium is released below 400°C.The real jaw-dropper is that although the radioactivity drops by 28% between 115°C and 160°C, and more slowly by a further 12% at 275°C. It then rises again to slightly more than its original value by the time the metal has reached 360°C. After that, the radioactivity drops as the tritium leaves the metal at about 400°C.

That is absurd, because *the nucleus is inviolate*, except to very high energies. Although radioactive decay can be hidden or shielded, it cannot be altered (to any significant extent). Obviously something is very wrong here. Anybody reading the paper will start a hunt for errors, for loopholes.

Maybe there is a loophole, but all the obvious ones have been firmly plugged. The dummy experiment shows that virtually no tritium leaves the metal below 400°C, and any that does is pumped out immediately (the heating process takes about 10 hours).

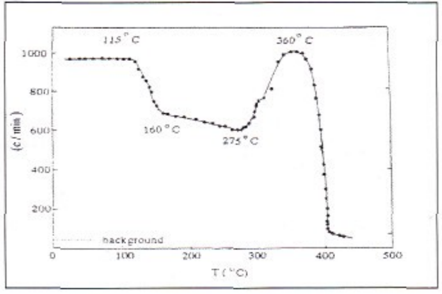
The rise in radioactivity between 275°C and 360°C shows that nothing has actually changed; the substance is giving off at least as much radiation as before. And the beta particles, the electrons, are not being diverted or hidden by some odd change in the metal. The X-rays and the current formed by the electrons are shown in a series of experiments to be in step with each other.

Perhaps a replication of this [experiment] by one of our great laboratories will be the first step to healing the ridiculous rift in science today.

"Various experiments established for both detection systems the linear relationship between the read-out and the activity."

It becomes clear from reading the paper that the author was very familiar with this material, and had done many experiments with it. This particular one was done several times, under different conditions, but with similar results. One experiment with 10 times as much tritium, and five times the rate of heating, did not show the effect.

Another, equally careful set of experiments at room temperature showed that "small accurately determined quantities of tritium" absorbed by the titanium did not emit radiation as might be expected. As more tritium was added, the emissions did not keep in step with the amount. At one point there was only half the radioactivity expected, but at double that amount of tritium the beta-decay had increased four times—back to about the expected level.



Count rate as a function of temperature in the pumped system.

The titanium used is interesting stuff. It's "a kind of soot." The titanium is vaporized in inert argon and condensed as particles, each being about 15nm in diameter. Titanium is quite a light metal and, if these particles are spherical, they will only contain about 100,000 atoms. The ball will be about 55 atoms across and have perhaps 1,000 atoms on its surface.

The question is whether tiny monocrystals are one big "grain boundary." With masterly understatement, Reifenschweiler suggests, "it seems justified to put forward a highly unorthodox hypothesis." He proposes a nuclear pair hypothesis, with pairs of tritium nuclei forming something more stable than a single one. On the face of it that sounds like an odd idea—especially since the effect is not seen with higher levels of tritium—but I have no doubt that Dr. Reifenschweiler has good reason for putting it forward.

What's in it for cold fusion?

If the effect is real, if it is confirmed, then perhaps it may be due to some interaction between the tritium and the titanium. The only (and very naive) comment I can put forward is that the neutron in deuterium is stable; in tritium it decays slowly; on its own it decays fast. Maybe that is an over-simplification, but it makes tritium look like a prime subject for some kind of process which might stabilize its neutrons. Assuming that there isn't a "trivial" explanation (Dr. Reifenschweiler is perhaps being unduly modest when he rates the chance of that at about 50%), what might be the implications for cold fusion? He makes it very clear that he has published this paper *because* of cold fusion.

It is astonishing to read that it previously existed only as an internal report within the Philips company—written in 1961! His first three references are to summaries of cold fusion progress by Storms and by Srinivasan, and the 1993 *Physics Letters A* paper of Pons and Fleischmann. And he makes suggestions for experiments which might connect his results with those of cold fusion workers.

What this paper seems to show is that solid state conditions *can modify* the behavior of atomic nuclei, which is what cold fusion scientists assert, and their opponents deny. There are none of the subtleties of electrochemistry or the complexities of "proton carrier" crystals—no calorimetry, to search for traces of nuclear "ash." Although the best experimentalists do seem not to realize how good they are or how difficult others may find it when trying to replicate their work, this does look to be a very straightforward matter to assay.

If the world's best nuclear labs are not trying this, then they should be. It is exactly the sort of experiment which they are best equipped to do, and which they are best able to investigate in depth. Perhaps a replication of this by one of our great laboratories will be the first step to healing the ridiculous rift in science today.

I fear this result will be regarded as too absurd to justify any attempt at replication, except perhaps by those whose brains have now been softened by the impact of all the cold fusion results. There may be a perfectly simple explanation, one which the reviewers have missed, but we can expect to see some interesting cases of "dismissal of findings," and of specious "explaining away."

Why was such a revolutionary finding not published? Was it because the originator knew it was too absurd, because he would be attacked for having done such work without finding the mistake which *had* to be there? Or was it because he knew it had no hope of passing peer review? There may be a more mundane reason. It may even be that Dr. Reifenschweiler just didn't believe what he saw at the time.

Minsk Cold Fusion Conference

Minsk, Republic of Belarus, will be the site of a cold fusion conference May 24—26. Fear not, English-only speakers: the conference will be bilingual. Corporate and academic laboratories will be presenting papers and displaying working cold fusion devices. There will be an exhibit hall displaying this hardware, and video presentations for equipment too delicate or too large to transport to the conference.

Economic conditions in the former Communist-controlled states have reduced opportunities for many research groups in these countries to meet with their peers in hard-currency nations. The Minsk conference will help to overcome that problem.

Some 70 papers accepted for the conference will be published in English and Russian editions of the proceedings, which will be provided to attendees at the beginning of the conference. Papers from scientists in Russian-speaking countries were submitted to the organizing committee in Minsk. Papers in English were submitted to Hal Fox of the Fusion Information Center in Salt Lake City.

The Minsk proceedings are expected to provide a tutorial overview of the new science of cold fusion for a multidisciplinary audience. Subjects that the conference will encompass include:

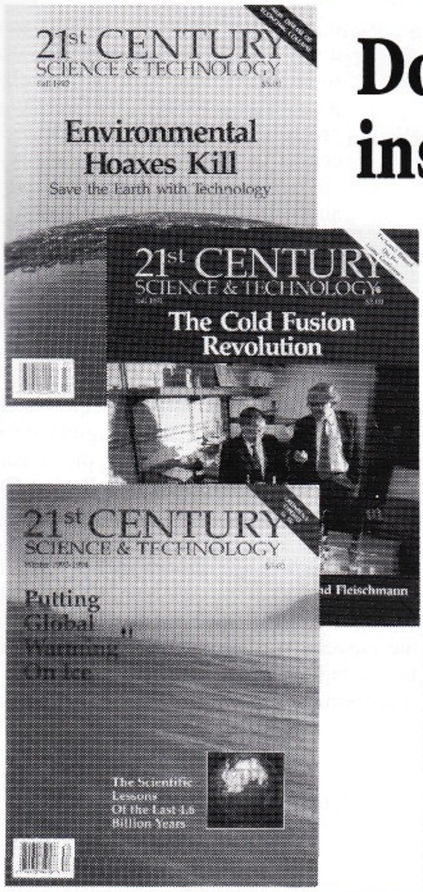
- Cold Nuclear Fusion (CNF) Devices.
- Theories and Models.
- Measurement of nuclear byproducts, including excess heat.
- Engineering topics pertinent to commercialization of CNF, such as heat transfer, thermo-electric devices, and mechanical energy production.

Papers summarizing national work on CNF in Japan, China, Italy, the U.S., and the Commonwealth of Independent States (CIS), and other nations.

Conference fees for hard-currency countries: Conference attendance, \$250; hotels and meals, \$50—\$100/day; airline fare from U.S. to Minsk, \$1,000—\$1,500.

Demonstrations and exhibits

Corporations and institutions will have exhibit space (a single booth is about 3 meters wide) to demonstrate devices, show videos of operating devices, or advertise their products. Exhibits of working devices should not include closed, pressurized, electrochemical cells.



"Cold Fusion is our only business."

ENECO acquires exclusive rights to original Pons-Fleischmann patent applications

The University of Utah has signed an agreement giving ENECO, Inc. exclusive worldwide licensing rights to the university's cold fusion technology developed by Drs. Stanley Pons and Martin Fleischmann. ENECO is a cold fusion research and development company.

"Cold fusion is our only business," says Fred Jaeger, president of ENECO. He returned to Utah recently from a special cold fusion meeting in Asti, Italy, sponsored by Fiat Corporation, where Drs. Pons and Fleischmann approved the agreement. Dr. Yan Kucherov, ENECO's director of research, was presented a Scientific Excellence Prize for best work in 1992 at the Italian Conference. Kucherov is an experienced researcher in cold fusion.

ENECO is a privately held Utah corporation founded in 1991. In addition to the Salt Lake City staff, the company maintains an external Science Advisory Board of experts in the field to provide a continuing resource for creating and evaluating new cold fusion technology.

ENECO has a broad portfolio of cold fusion patent applications in conventional electrolysis cells (both heavy and light water) molten salt electrolytes, gas discharge devices, solid state systems, and reaction-triggering methods. Last spring, the Utah company acquired full ownership of the first cold fusion patent granted in the United Kingdom.

"Each technology sub-division in our portfolio has the potential to fulfill unique commercial applications," says Jaeger. "The portfolio concept allows the company to minimize specific risks while enabling us to participate in all areas of cold fusion development and commercialization. The portfolio concept also facilitates open dialogue and cross-fertilization of scientific ideas among all sub-groups without concerns for conflict of interest."

ENECO officials plan to license the Pons-Fleischmann patent applications together with the rest of the company's technology portfolio to a broad spectrum of international clients. While licensing technology, ENECO envisions developing strategic alliances with large international entities that will assist with marketing and product development.

The company sponsored the attendance of 22 representatives, including eight Russian scientists, to the Fourth International Conference on Cold Fusion held in Maui in December, 1993.

ENECO has sponsored, and is currently sponsoring cold fusion research at the Massachusetts Institute of Technology, Texas A&M, Cal-Poly (Ponoma), University of Hawaii, and two Russian institutes, LUCH (Moscow region) and High Temperature Electrochemistry Institute (Ural branch of the Russian Academy of Sciences). ENECO also supports R&D efforts of individual scientists and inventors.

ENECO officials plan to license the Pons-Fleischmann patent applications along with the rest of the company's technology portfolio to a broad spectrum of international clients.

Jaeger says, "Now that this landmark agreement is signed, ENECO plans to work directly with Drs. Pons and Fleischmann who will provide technical support for our company's patent prosecution activities. Also, ENECO can now move forward to ensure that the patent applications just acquired from the University of Utah remain in a preemptive position with respect to other emerging international technologies."

Although the detailed contents of the license agreement remain confidential to protect ENECO's competitive position, it was revealed that it provides for an initial license fee and for a royalty structure to handle a wide variety of possible future outcomes. ENECO is also required to meet specific developmental goals.

"Presently, the value of the worldwide exclusive license to the Pons-Fleischmann patent applications is entirely unknown. As a private company, we understand and are well-suited to assume the consequences of the risk-reward dynamics involved with this type of acquisition. The agreement will save the University substantial on-going patent expenses, while commercial applications will provide them with generous royalty income for the life of the patents," says Jaeger.

ENECO also issued this umbrella statement: "We believe cooperation among all cold fusion participants is the fastest route to commercializing the technology. ENECO is structured to accelerate the development of cold fusion intellectual property discovered by individuals and organizations. We help develop patentable concepts, pursue patent protection, and offer technology licenses to industrial users. Inventors are compensated with generous royalties over the life of the patents."

Science magazine's take: U of U bailed out

In its news item on ENECO acquiring exclusive license rights to the original Pons-Fleischmann patent applications, the *Science* headline was: "Utah Puts Cold Fusion Out In The Cold," reporting: "Four years, eight months, and nine days after it all started... Utah taxpayers are out of the business of supporting [cold fusion research] it... Paying legal fees has been the primary focus of the university's involvement since the cold fusion bubble burst four years ago, and it's cost them about $700,000."

Yes, but just wait until the royalties start bubbling into U of U coffers.—Eds.

Texas A&M professor attacked for cold fusion and 'alchemy'

Distinguished Professor title was in limbo

What is worse, to call someone an "alchemist" or a cold fusion researcher? To a group of "Distinguished Professors" at Texas A&M University it seems that either term will do. There is a pre-history to this story. Professor John O'M. Bockris of the Department of Chemistry at Texas A&M University is widely regarded as one of the world's greatest electrochemists. He was also one of the earliest and boldest of the cold fusion pioneers, having accomplished successful cold fusion experiments yielding excess heat and tritium soon after the March 23, 1989 Utah announcement. He has consistently spoken out on behalf of the emerging new science we know as "cold fusion."

For this pioneering and success, Professor Bockris has been subjected to criticism from skeptical scientists mocking his experiments and methods—much as they ridiculed electrochemists Drs. Fleischmann and Pons.

In June, 1990, science journalist Gary Taubes, gave the skeptics more red meat against Professor Bockris. Writing in *Science* magazine, Taubes blasted him and his co-workers with allegations that deliberate spiking of cold fusion cells with tritium might have happened in the Bockris laboratory—most likely the perfidy of some graduate student, Taubes said.

The 1990 allegations in *Science* offered no "smoking gun," just five pages of circumstantial evidence, and Taubes' notion that fraud was easier to accept than the existence of nuclear reactions at room temperature. Later, these allegations of possible fraud were proved groundless. Nonetheless, Taubes repeated implications about tritium-spiking in his 1993 book, *Bad Science*.

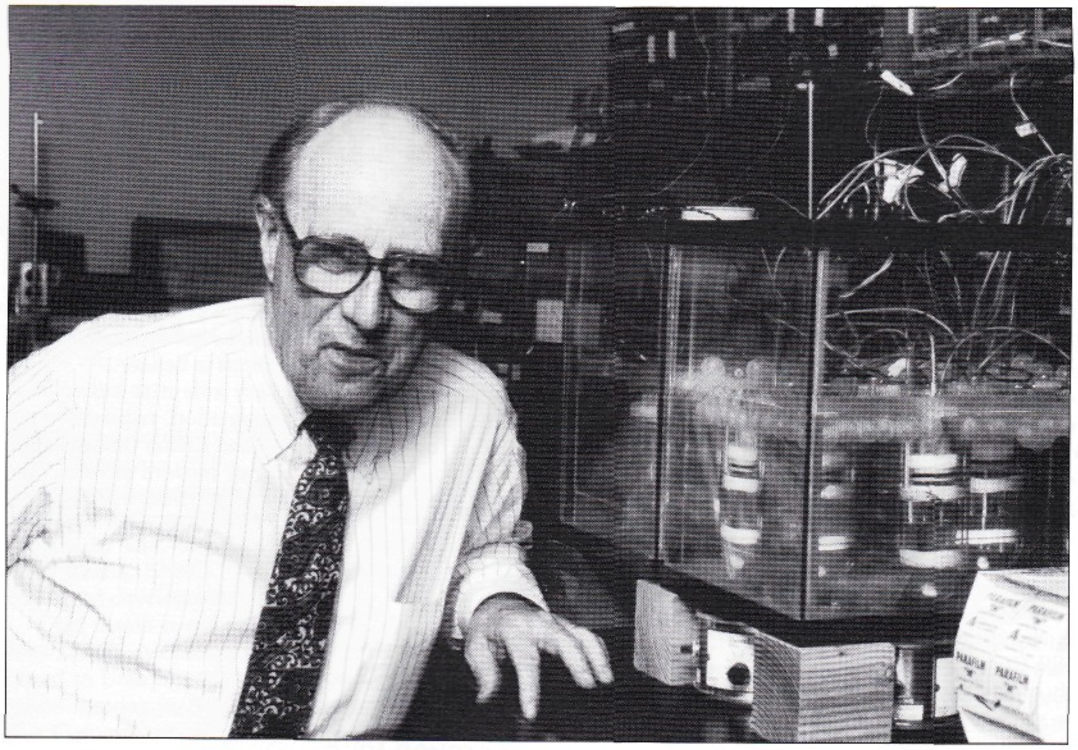
Other writers and reviewers have been fond of repeating the original allegations despite their flimsy grounds, and despite the growing number of laboratories that also find tritium production in cold fusion cells.

Recently, there was another challenge to Bockris's work on cold fusion, and his investigation of other low-energy nuclear phenomena. This attack took the form of a petition late last year to have Professor Bockris stripped of his "Distinguished Professor" status. In December, 1993 a group of 23 Distinguished Professors (not every Distinguished Professor signed) at Texas A&M submitted to the administration the petition that follows.—*E.M.*

The glass house syndrome

The petition appears to be an attack against "cold fusion," using heavy-element transmutation as a dummy target. [A technical point: For all we know "cold fusion" may involve both heavy and light element transmutations. However, this is irrelevant to the nature of the attack against intellectual freedom.] Note this "Request" was apparently so hastily put together and signed that it states: "For a trained scientist to claim or support anyone else's claim, to have transmuted elements is difficult for us to believe..."

Of course, every scientist knows that *high-energy* transmutation, radioactive decay transmutation, and fission transmutation are accepted facts of modern physics. The perpetrators of the petition were clearly



Professor John O'M. Bockris overlooks a fuel cell in his lab at Texas A&M University.

"Philadelphia Project," the Committee of Inquiry *unanimously* finds there to be no evidence to sustain these charges. Consequently, the Committee holds Professor Bockris to be exonerated of these charges and does not recommend an 'investigation.' It is recommended that the members of the Texas A&M University community allow the process of experimentation and peer review of published data to resolve any scientific issues."

The Committee members were Duane C. Kraemer, Associate Dean, College of Veterinary Medicine; John J. McDermott, Distinguished Professor, Philosophy; Professor John C. Slattery, Chemical Engineering; John C. Calhoun, Jr., Distinguished Professor, Emeritus, Petroleum Engineering.

It is refreshing to see the end of assaults against academic freedom at Texas A&M in the matter of cold fusion and cold nuclear transmutation.

thinking of *low-energy* or near room temperature transmutation, but they did not spell this out.

The story of this battle over academic freedom and the legitimacy of investigations into low-energy nuclear phenomenon became widely known. There were wire service accounts, and several dozen newspaper articles resulted. The January 10, 1994 issue of *Newsweek* (p.54) attacked Professor Bockris in its article, "All That Glitters Isn't Chemistry: Does academic freedom protect a professor who wants to be an alchemist?" The lead of the article stated: "In the revered name of academic freedom, universities tolerate faculty members who are avowed communists and lifelong fascists, outspoken racists and anti-semites, radical lesbians, and rabid homophobes. But alchemists?" *Science* magazine ran the story as "Alchemy Altercation at Texas A&M," (November 26, 1993, p. 1,367). The *Science* article lead: "Four years ago it was cold fusion, now it's alchemy, and members of the Texas A&M chemistry department say enough is enough." Both the *Newsweek* and *Science* articles drew attention to a related allegation that lurked in the background: the possible improper use of funding for Professor Bockris's research, which was provided by outside parties whose motives were being questioned.

The matter came to a head on January 18 at Texas A&M at an academic hearing. Then on January 31, the four-member Committee of Inquiry into Scientific Misconduct exonerated Professor Bockris of *all* charges against him. The conclu-sions and recommendations of the committee:

"With regard to the charges against Professor John O'M. Bockris, pertaining to misconduct in science in proposing, conducting, or reporting of research in the

A Request

Professor J. O'M. Bockris' activities since 1989 (the inception of the "cold fusion" embroglio), [sic] and particularly recent allegations that he lent his name and that of our university to a fraudulent scheme to promote a bogus engineering enterprise, has brought this university into disrepute.

Note that on page 6 of the "Policies and Procedures Regarding Distinguished Professor Appointments" (September, 1993), it is stated that "The Distinguished Professors....bring honor and recognition to the University..." Instead, we believe that Bockris' recent activities has [sic] made the terms Texas A&M and Aggie objects of derisive laughter throughout the world among scientists and engineers, not to mention a large segment of the lay public. The "Alchemy" caper is, everywhere, a sure trigger for sniggering at our university. And so it should be. For a trained scientist to claim, or support anyone else's claim, to have transmuted elements is difficult for us to believe, and is no more acceptable than to claim to have invented a gravity shield, revived the dead, or to be mining green cheese on the moon. We believe it is sheer nonsense, and, in our opinion, could not have been done innocently by one with a lifetime of experience in one of the physical sciences.

In view of the above considerations, we the undersigned Distinguished Professors of Texas A&M University hereby request the Provost to take steps to revoke the title of Distinguished Professor now carried by John O'M. Bockris. We do this because of our belief that Dr. Bockris's alleged disregard of the accepted standards of scholarly and professional behavior has brought great embarrassment upon this university and his colleagues. In our opinion he no longer merits the title of Distinguished Professor.

[Signed: by 23 Distinguished Professors]

From the White House

Nov. 22, 1993

Dear Eugene:

Thank you for your letter regarding cold fusion. I have referred your letter to my Office of Science and Technology Policy for appropriate review. I value your ideas and appreciate your taking the time to write.

As we work to meet the challenges that lie ahead, I hope I can count on your support.

Sincerely,
Bill Clinton

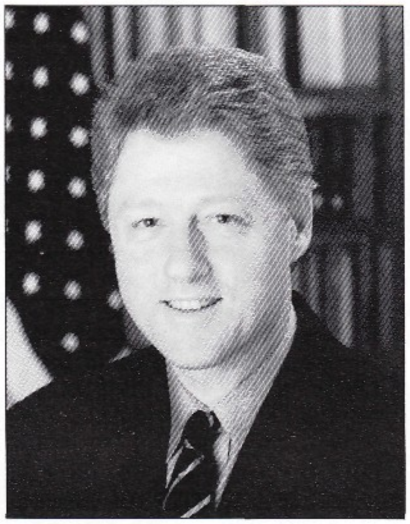
For fair treatment...

...acknowledging receipt of Dr. Eugene F. Mallove's letter of Nov. 25, 1992, along with material concerning the issue of cold fusion technology:

Vice President Al Gore

Dec. 9, 1992

I will convey a copy of your material to senior officials of the President and Vice President. Given the comments that then-candidate Clinton made about cold fusion, I feel that he will authorize a fair and unbiased review of the issues relating to the development of cold fusion in the United States. I was somewhat surprised that there was no reference to cold fusion in Senator Gore's book, "Earth in the Balance." His views on energy and environment matters will probably carry great weight in the new administration; therefore, I will do all that I can to make sure your letter is seen by Vice President Gore.



President Bill Clinton

Thank you very much for taking the time from your busy schedule to write to me concerning this matter. All the best to you in your work on cold fusion.

Kurt Schmoke,
Mayor of Baltimore, Maryland

Getting the cold shoulder, again?

Thank you [Dr. Mallove] for your letter to President Clinton suggesting that cold fusion research be made the subject of a line-item request in the FY 1995 Department of Energy budget.

Such a strategy certainly would lend urgency to the effort to understand the origin of the anomalous heat generated in the Pons-Fleischmann cells.

However, line-item funding is best reserved for major technology development efforts such as those addressing solar, wind, and geothermal energy sources, for which the underlying scientific base has already been well established. Cold fusion, in contrast, is still a topic requiring much more exploratory research before it will be ready for a technology development effort.

I am confident that DOE will support cold fusion research proposals to the extent that they represent good science, and I have asked my staff to discuss this position further with officials in that Department.

Karl A. Erb,
Acting as Associate Director for Physical Sciences and Engineering
Executive Office Of The President,
Office of Science And Technology Policy
Washington, DC 20506

Ed. Note: To our knowledge as of March 1994, no further communication from anyone at OSTP or DOE had been received by anyone in the cold fusion field. There were rumors, however, that positive developments were in the works.

We'll be waiting

As one who has carefully monitored the development of "cold fusion science," with a perspective predating the Pons and Fleischmann announcement, I would be genuinely interested in the opportunity to make a thoughtful study and careful evaluation of your publication prior to committing to a subscription. I'll do this through newsstand purchases, and if it merits the investment, then recommend that we subscribe.

We wish you well in this venture—it's a story that needs an impartial airing, and, to date, your position has taken some unfair but yet very difficult knocks.

J.L. Carroll, Associate Director,
Technology Transfer
Brigham Young University
Provo, UT

Jupiter mightier than its mass?

On the planet Jupiter, the protium/deuterium ratio is 100,000:1 as compared to 6,667:1 on earth. Conventional wisdom explains that deuterium was preferentially concentrated during the formation of the earth, when gravity was unable to hold down the lighter hydrogen. On the other hand Jupiter is so massive that primordial hydrogen and other light gasses are supposed to have remained trapped in its gravitational field.

However a closer look shows that in methathathane (a minor component of Jupiter's atmosphere) the H/D ratio (9,300:1) is much closer to earth's! Apart from invoking cold fusion (i.e. deuterium depletion in molecular hydrogen or synthesis in methane) can any readers suggest a non-nuclear

Continued on page 85

Cold fusion quietly takes off i

An over
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Japan

Cold fusion is not a big deal in Japan. In fact, it is something on the order of business as usual.

It is just an ordinary, quiet field of academic physics, like high temperature superconductivity, or X-ray lithography.

Scientists and industrialists are aware that cold fusion research is under way, and most approve of it, public opinion polls indicate. From time to time, articles about it appear in the leading scientific journals, technical magazines, and newspapers. Occasionally it makes the headlines, but press coverage is usually low-key.

Funding levels are moderate, probably around $90 million dollars per year. There is broad support for continued research from every major scientific society, and there are small groups working on the subject in most leading universities.

Most corporations working in this field probably have only five or 10 people working on cold fusion, and only a few having much larger programs.

Hideo Ikegami, one of Japan's leading cold fusion researchers, has written guest editorials in the *Asahi* newspaper, the Japanese edition of *Scientific American*, and has been quoted by the *New York Times*. When you ask him, "How are things? Is there any big news? Have there been any breakthroughs?" he always responds: "It's slow and steady. Everything is coming along normally."

Scientists are like farmers

The research is steady and uneventful. People work quietly in cluttered laboratories, spending weeks calibrating and preparing for experiments, and weeks afterwards analyzing data, and writing scientific papers. Basic scientific research usually works at this pace. The scientists are like farmers growing an apple orchard; the season is the shortest unit of time that matters to them. Most scientists think results may be years away, but Japanese scientists have made great strides in cold fusion because most of the mainstream science establishment ignores them. They get their grant money, do the research, publish their findings in the leading physics magazines, hold annual and semi-annual conferences at leading physics conferences—just like scientists in any other field.

American researchers envy their Japanese colleagues. They would love to be left alone, wishing cold fusion were treated as just another part of physics in the U.S., where the field remains a battleground of accusations and emotionalism—and research money is virtually unobtainable. Leading U.S. physics societies attack and denounce the work as criminal fraud and lunacy.

In Japan, cold fusion caused a lot of commotion in 1989, and it met very stiff skeptical opposition for a few years. After a while it slipped into the normal, quiet, calm, academic atmosphere. There are still, however, mainstream scientists who scoff at it, or ignore it, and a tiny minority which actively denounces and attacks it. The mood is open-minded, pragmatic, and patient. Most Japanese scientists are not sure yet, but they are encouraged by the results they have seen so far, and they are content to wait and see how it will turn out.

For science, technology, business, and even the media, Japan's soil is fertile for cold fusion.

Martin Fleischmann was quoted in the UK *Sunday Times*:

"Plenty of scientists in Japan responded just as skeptically as in Britain and America. But there was a willingness to say: 'Suppose it's true, what follows from that?'"

Open minds, open doors

The Japanese as a whole are more open-minded than Americans, but they are not *much* more open-minded. It is just a small difference; they are just a little bit more willing to suspend judgment. That little slice of extra open-mindedness has allowed a little slice of careful research, at a very modest $90 million dollar per year level. If every Japanese scientist were convinced the effect is real, Japan would be spending $9 billion per year.

The cold fusion scientists themselves are completely certain of their results. They have no doubt whatsoever that the effect is real, the tritium is real, and the heat is beyond chemistry. Other scientists in Japan are only dimly aware of the results. They have not read the formal scientific papers, so they are not as certain.

There is no gigantic Manhattan Project-style cold fusion research program in Japan—and nobody has ever thought of launching one. But as the 1989 controversy gradually ebbed away, more scientists came to accept that the effect is *real*, and projects were gradually ramped up. Research is now at an all-time high.

A dramatic announcement was made on the first morning of the Fourth International Conference on Cold Fusion in Maui, December 6, 1993. Japan's Ministry of International Trade and Industry (MITI) has established a new R&D Center and a laboratory for cold fusion research with a four-year budget of $30 million dollars. Kazuaki Matsui, the Director of the Institute for Applied Energy, made the announcement, which caused quite a commotion among many Americans and Europeans. But it did not surprise many Japanese participants. Details of the plan circulated long before the conference. This announcement was the culmination of a long, slow, deliberate planning process.

Like most important projects in Japan, it was bounced around and sent out for evaluation long before it was implemented. It's clear the details were mapped out in advance; just look at the address of the new research institute. It's one floor above the Toyota IMRA institute in Hokkaido, established two years ago. Obviously, the Japanese intend to foster close cooperation between the private industry lab on the ground floor, and the semi-private government-supported lab on the second floor.

Is the turtle winning?

Like everything else in Japanese cold fusion research, the announcement was the result of a slow, step by step, carefully planned effort. The Japanese researchers are committed to a long term effort. They are in no hurry to "crack the problem." They do not expect to find the cause of cold fusion, and win a Nobel prize next month, or next year. They say they will commit decades to the work, if that is what it takes. Noboru Oyama, of the Tokyo University of Agriculture and Technology, predicted that it might take a century for the research to pan out. Most researchers expect results before that—some say within two or three years, some say within 20 years.

Ikegami says scientists in Japan are gradually building up a mountain of essential but mundane materials science research into metal hydrides. They have come to realize that a palladium sample supersaturated with hydrogen or deuterium has many unique qualities. Palladium loaded with 50% or 60% deuterium is a relatively well-researched, well-understood metal. Palladium loaded with 90% is a brand new material—never explored before 1989—which you might say, is more hydrogen than metal at that stage.

off in Japan

BY JED ROTHWELL

An overview of scientific, political, and social attitudes toward cold fusion in Japan

American researchers envy their Japanese colleagues. They would love to be left alone, wishing cold fusion were treated as just another part of physics in the U.S., where the field remains a battleground of accusations and emotionalism—and research money is virtually unobtainable.

The Japanese are learning about surface properties, conductivity, internal structure, and about countless other properties of this strange new material. The data they have piled up is not controversial. Nobody questions the accuracy of the calorimetry, the loading measurements, or the surface analysis data itself. The data is accepted, but scientists vigorously debate what conclusions might be drawn from it.

There is no generally agreed upon theory yet to explain the data. Scientists expect that the mountain of evidence will eventually serve as the basis of a theory to explain the effect, hoping the answers will fall out of the data.

MITI's cold fusion R&D

MITI's Department of Natural Resource Development has been involved in cold fusion research for several years. They have supported low-level, small-scale research, "just enough to keep the pot boiling" as a program leader put it. Two guiding principles behind MITI's cold fusion thinking are often expressed in official statements, interviews, and discussions:

1. *The excess heat is definitely real.*
2. *If there is even a slight chance that this phenomenon might become a practical, useful form of energy, then the research must proceed.*

MITI, and private industry, are responsible for developing new technologies. That is their job. They do not see themselves as guardians of scientific truth, in the way our Department of Energy does. They don't care about scientific theories and debates. They try to avoid taking sides in the physics debates. It doesn't matter to them whether cold fusion is actual fusion, an exotic super-chemical reaction, or something else unknown to science. The Japanese look at the engineering aspects of power density, temperature, and fuel consumption. Pragmatic, results-oriented industrial planners, they resist esoteric academic debates and entanglements.

MITI appropriated $3 million dollars in 1993 for cold fusion research, money used for special equipment and other extraordinary expenses in national laboratories and universities to cover cases where normal discretionary funding was insufficient. The idea: Let the scientists go slowly and quietly, out of the spotlight. A large, lavishly-funded program might raise expectations too high, or push people to quickly produce positive results at the expense of careful science. MITI, and other Japanese R&D planners, realize that good science cannot be rushed. Research is like having a baby. Nine women cannot have one baby in one month, and nine scientists cannot do nine months of thinking and learning in one month.

In the spring of 1992, news of the MITI cold fusion R&D program began to appear in the Japanese press. On July 10, 1992 it made a big splash on the front pages of the *Yomiuri* evening edition with the bold headlines:

"Cold Fusion: Clean Energy Source to be Developed into Practical Use; Nation Begins Full Scale Research; MITI Energy Resources Div. Plans Several Hundred Million Yen Budget Next Fiscal Year [several million dollars]"

The lead paragraph said:

"The Energy Resources Department of the Ministry of International Trade and Industry, will appropriate several hundred million yen for basic research from the fiscal 1993 budget. The agency intends to spend several billion yen [several tens of millions of U.S. dollars] on the five-year project beginning in fiscal 1993. The agency has established a study group bringing together researchers, power companies, and large electric machinery producers. The group recently concluded that excess heat was, indeed, generated in cold fusion experiments, although it is not sure exactly how the process works. The research is aimed at finding this out."

> Like everything else in Japanese cold fusion research, the announcement was the result of a slow, step by step, carefully planned effort. The Japanese researchers are committed to a long-term effort. They are in no hurry to crack the problem.

The MITI program director, Taizo Nakatomi, was taken aback at the commotion this raised, going to great lengths to downplay the scale and importance of the program.

A Nikkei trade publication, the *Superconductor Newsletter* quoted him:

"This is not cold fusion research per se. We are investigating the prospects for improving fuel cell performance . . . The report in the *Yomiuri* newspaper that we will begin a program to study cold fusion in the next fiscal year may cause a misunderstanding. What we are going to do is to conduct exploratory 'paper' research to determine whether or not it is possible to improve the performance of things like fuel cells and to create a new, efficient battery by utilizing the heat generated by metals that have been loaded with hydrogen isotopes . . .

"The thing we are paying attention to is the excess heat, whether the mechanism explaining that heat is nuclear or chemical is no concern of ours. For one thing, our responsibility in the Technical Division is to develop fuel cells, not nuclear fusion. Since that article about our developing cold fusion came out, we have been inundated with calls, which have become a big nuisance."

One reason Nakatomi wanted to downplay the connection with fusion is because he was engaged in a turf war with the hot fusion program at MITI. On October 17, 1992, in a report on the Third International Cold Fusion Conference in Nagoya (ICCF3), the *Yomiuri* wrote:

> "[MITI's] decision to study cold fusion shocked the scientists who are researching orthodox 'hot' fusion, in MITI's Technology Research agency. It is no wonder they were surprised; under the auspices of the Nuclear Power Division, this group has been spending several trillions of yen per year to develop a gigantic hot fusion reactor, the 'JT-60.'"
>
> The Nuclear Power Division had already performed cold fusion replication experiments for two years, and judged that cold fusion was not suitable as a source of energy. When the cold fusion program was announced, the Nuclear Power Division retaliated by declaring: "The kind of fusion energy that the nation has decided to proceed with is hot fusion. Cold fusion development is not part of this plan.
>
> "Taking a dim view of this petty bureaucratic squabble, industry has again become excited by prospects for cold fusion. When cold fusion was first announced, the nation's large electric equipment manufacturers and others jumped into the research, and the stock prices of precious metal companies producing the palladium needed for cathodes shot up. However, that sort of response fizzled within a year. However, recently, in March, the Japanese Energy Department formed a 'New Hydrogen Energy Panel' to study cold fusion. Although the department asked that the panel be limited to only 10 companies, many more expressed interest, and the panel has now been expanded to 15 major corporations."

While this program was big news in the summer and fall of 1992, the program in fact started at least a year before that, in 1991. This is the list of consortium members in late 1992:

Chubu Electric Power
Hitachi
Toshiba
Fuji Electric
Mitsubishi
NKK
Kyushu Electric Power
Nippon Steel
Tokyo Electric Power
Tokyo Gas

Osaka Gas
NTT
Aishin (a subsidiary of Toyota)
Kepco Power
Mitsubishi Materials

In 1992 and 1993 top research directors from these corporations met from time to time to talk and share ideas about cold fusion. Some NHEP members were pursuing cold fusion research quite actively with many researchers, while others were holding back. A few research directors had their doubts about cold fusion as a source of energy, believing that a major commitment, with dozens of researchers, was premature. Their attitudes matched those in the *Trigger* public opinion survey *(see sidebar on page 86)*.

Encouraged by research results in 1992 and 1993, MITI instituted a scaled up $30 million dollar program, announced at the Fourth International Conference on Cold Fusion (ICCF4). Most NHEP members are taking part in the program, along with new participants, but a few of the original NHEP members have dropped out of the consortium.

MITI, NEDO, The Institute of Applied Energy

The cold fusion research program will be conducted by the Institute of Applied Energy, established in 1978. This complicated set of relationships were shown in the viewgraph from Matsui's ICCF4 presentation. MITI is at the top, followed by the New Energy Development Organization (NEDO), and then by the Institute of Applied Energy. NEDO will handle cooperation with foreign institutions. A steering committee within NEDO will oversee research at the Institute, which has set up two new facilities dedicated to cold fusion: the R&D center, and the NHE laboratory.

NEDO is a semi-private corporation established by MITI in 1980 to nationalize Japan's remaining coal mining industries, and carry out R&D in new forms of energy. NEDO's 1993 budget was $2.1 billion, including $725 million for R&D, and $1.4 billion for coal industry restructuring, and production of industrial alcohol. Run by the government, NEDO is an "implementing agency," with its budget appropriated by MITI. MITI's minister approves projects and financial plans, and "appoints or approves of" all NEDO officials, including its president. NEDO works closely with the private sector. Senior council members include prominent experts from the private sector, and its capital is partly comprised of private sector investments.

Quasi-governmental, quasi-private industrial organizations are common in Japan, and they are usually effective. The key to their success is that they have built-in industry support from the start. Private industry is often more careful with its investment dollars, and more realistic about the prospects for technology than the government.

Under the Japanese system, if a project cannot attract substantial financial and manpower contributions from private industry, it dies before it gets off the ground. Such projects are usually bad ideas, or boondoggles that only a bureaucrat could love. There is nothing quite like these government and private cooperative projects in U.S.; the Sematech Corporation was inspired by them.

In recent years, it has become fashionable to cite the "Japanese model" of joint government and private industry R&D. Many projects touted in Washington are supposedly based upon this model. Ironically, these projects are based on a model that went out of style a generation ago in Japan. The era of the big project dominated by the government ended when private corporations grew into world class organizations strong enough to carry out practically any project themselves. There are still joint projects, but they are smaller, more nimble, and more oriented to futuristic technology than nuts and bolts industrial planning.

"The group recently concluded that excess heat was, indeed, generated in cold fusion experiments, although it is not sure exactly how the process works. The research is aimed at finding this out."

NEDO conducts an amazing variety of R&D programs, some having little or nothing to do with energy. In joint ventures arrangements with private industry, it has established five dazzling laboratories engaged in some of the most high tech, advanced research I have ever heard of: the Ion Engineering Center; the Research Center for the Industrial Utilization of Marine Organisms; the Japan Microgravity Center; the Japan Ultra-high Temperature Materials Research Center; and the Applied Laser Engineering Center. These labs combine many unusual high tech requirements, and no other facility in Japan can do the kinds of work they do.

Cold fusion research is just another small part of NEDO's many programs. The initial research budget is only $30 million over four years, but there is great flexibility in the budget planning; if the experiments are a great success, the budget will be increased substantially.

The R&D Center and NHE Laboratory are both being run by the Institute for Applied Energy, but they will be funded and supported in two different ways. The $30 million for the NHE laboratory will come from MITI to NEDO to the Institute to the laboratory. Five corporations will contribute research manpower: Mitsubishi, Hitachi, Toshiba, the Aishin Group, and Tanaka Kikinzoku. Researchers from these corporations will work together in the lab in joint research. The R&D Center, in Tokyo, will be supported by funds from a group of 20 corporations, the original members of the NHEP plus other utility companies. The R&D Center will fund "multiclient basic research" programs at national laboratories and universities, as well as foster cooperation with foreign institutions.

The viewgraphs and organization tables shown by Matsui at ICCF4 are a small part of the NEDO planning that went into this new venture. A superb three-page planning document (which has not yet been translated into English) lays out the plan in considerable detail. Compared to Nakatomi's hesitant, noncommittal 1992 statement, the NEDO plans are bold and direct. This is because a great deal has happened in the last two years; experimental evidence is more solid, and many lingering doubts about heat, and nuclear effects have been laid to rest.

Titled "Project to Clarify the Feasibility of New Hydrogen Energy as a Practical Source of Energy," the plan gets right to the point, laying out a set of practical goals and strategies. It begins: "The New Energy and Industrial Technology Development Organization (NEDO) has embarked upon a research and development project to determine whether the form of energy we call 'new hydrogen energy' can be made into a practical source of industrial energy. This energy occurs in the form of excess heat, apparently when heavy hydrogen is absorbed in metal lattices.

"Purpose of the research: In 1989, Professors Martin Fleischmann of Southampton University (UK) and Stanley Pons, of the University of Utah (U.S.) announced the discovery of a new phenomenon. When heavy water is electrolysized with a palladium cathode, excess heat is created. Since the announcement, this experiment was replicated by a large number of researchers, to the point where reproducibility is now close to perfection. In some cases, the excess energies have been too large to be any form of chemical energy, so it is now thought to be some heretofore undiscovered new reaction.

"This reaction produces energy at levels commensurate with a nuclear reaction, and it consumes as fuel heavy water, which is available in virtually limitless supplies. It is hoped that this will become a major new form of energy…"

NEDO's planned experiments are almost all in the mainstream of "conventional" cold fusion. The emphasis will be on the original Pons-Fleischmann style heavy water palladium electrochemical cold fusion cells. A carefully coordinated set of experiments is planned with sophisticated on-line, in-situ de-

The following speech was delivered by the late Mr. Minoru Toyoda during the Third Annual Conference on Cold Fusion banquet in Nagoya, Japan, on October 23, 1992. Born August 3, 1913, died on December 15, 1992. He was a senior member of the Toyoda family, the founders and principal owners of the Toyota Motor Company. Before his passing, he was Honorary Advisor to all of the Aisin family industries (Toyota subsidiaries), including Technova, Inc.—Eds

My name is Minoru Toyoda, and I am the Honorary Chairman of Technova, Incorporated.

I was invited by Professor Ikegami, chairman of the committee of this international conference, to the dinner tonight, but because of a slight problem with my health, the doctor has advised me to excuse myself from official functions. I sincerely regret that I will not be able to enjoy meeting and conversing with all of you. I have asked Mr. Kyotani, Chairman of Technova, to kindly read this message expressing my convictions, on my behalf.

I am delighted that the Third International Conference on Cold Fusion is being held on such a grand scale here in the city of Nagoya, Japan. I am pleased to welcome eminent cold fusion researchers from all over the world. It is my fondest hope that you will be able to exchange ideas and information in spirited, open, and productive debates.

For a long time, I have held the strong belief that equitable growth in the world economy during the 21st century will only be achieved by the harmonious development of science and technology, through international cooperation.

To make this belief a reality, I established Technova in Tokyo in May 1978, as an organization that would have complete freedom to participate in the international forum of research. During the 14 years since its inception, in the ever-changing world of international research, we have made steady progress, thanks to the help of some of the best minds in the world.

Technova has been very active in the development and application of advanced technology, and in adapting advanced technology to practical uses. We have also actively promoted the international interchange of technology and ideas. Technova's staff and advisors have made continuous progress, leaving their mark both nationally and internationally.

I recall that in June 1982, at the Eighth Annual Summit of Developed Nations in Paris, French President Mitterand stressed the necessity for cooperation between science and technology. I examined future trends, and envisioned an ever-growing need for progress through the promotion of science and technology. With the cooperation of my many friends from beyond our borders, in July 1985 I established IMRA Europe, an international research and development laboratory located in Sophia Antipolis, a research park in the suburbs of Nice, France. The laboratory began operations in June 1988, and has been actively involved in advanced research, mainly in the field of energy.

When I established IMRA Europe, I had a vision, world-wide in focus, to set up a global structure for the development of future technology. I named this project the "IMRA Plan." It had its research base in Japan, Europe, the U.S., and Asia under the same name IMRA.

Its purpose was to network these four regions together in order to make more efficient use of human resources by exchanging people and ideas, while winning the world's confidence to achieve our goals. This plan progressed steadily, and now IMRA Japan, IMRA Europe, and IMRA America have already begun work. Today, we are planning the establishment of IMRA Asia.

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tectors for helium, tritium, and other particles. Also in the works are extensive materials sciences studies of cathode material.

The technical goals NEDO hopes to accomplish during the three-year schedule are in two categories: work with the excess heat phenomenon; and materials science, but *excess heat* is their main focus. Scientists hope to improve reproducability, measure reaction products, like helium and tritium, and identify and quantify control factors such as loading and temperature.

These issues have been the main focus of cold fusion research from the beginning—particularly mainstream research at places like SRI and IMRA. A great deal of progress has already been made in these areas, and NEDO will continue to build on this progress and finish the job.

Some people find the pace of research planned by NEDO a little too slow, and the choice of research topics a little too conservative, and safe. Perhaps this is so, but on the other hand, it is hard to argue with success. The Japanese approach has yielded slow but steady progress, and a gigantic data base of information about metal hydrides.

Electrochemist and cold fusion pioneer Tadahiko Mizuno of Hokkaido University visited the newly established NEDO labs in January, reporting that 12 palladium heavy water cells are up and running, six open, and six in closed-cell configurations. The experiments use conventional, "safe", old approaches, but Mizuno expects the program will include leading edge approaches, like his own work in ceramic material proton conductors.

NEDO's other activities include: research support in the national laboratories, both laboratory work and theoretical studies; establishment of a database of scientific papers and information on cold fusion; coordination with overseas institutions, particularly EPRI; promotion of the exchange of personnel and information.

What's in a name?

One term was conspicuously missing from Matsui's ICCF4 presentation, and it's also a term nowhere to be found in NEDO's planning documents either—in English or in Japanese. It is those two little words, "cold fusion" (in Japanese, "jyouon kukuyuugou"). MITI banned that term from its official vocabulary, rather calling the effect NHE: "New Hydrogen Energy" (in Japanese, "shin suisou enerugii").

MITI adopted "new hydrogen energy" for several reasons. It represents a fresh start, and avoids any controversy surrounding the term "cold fusion." NHE is a neutral term fitting whatever form of energy this actually is.

Many American and European scientists, including Hagelstein of MIT, and Storms, of Los Alamos National Laboratory, have also advocated a name change for these same reasons. Also, I suppose, the NHE planners are hoping the neutral term "NHE" will help in the turf war within MITI. They are declaring, in effect, that they are not competing with hot fusion because this is *not* "fusion," it's New Hydrogen Energy.

While the term they use is different, there can be no doubt that the effect MITI and NEDO refer to is what the rest of us call "cold fusion." The NEDO planning document makes this clear. Right up front they refer to the effect Pons and Fleischmann discovered, and announced in 1989, which is most easily recognized and characterized by massive excess heat far beyond the limits of chemistry.

Mainstream support; private industry research

There is no question cold fusion research enjoys quiet support from mainstream physics societies in Japan, that many corporations are working in the field, and that many others are interested in it. ICCF3's sponsor and attendance list proves that. [See attached list.] Support did not start in 1992, either. In September 1991, I called the Atomic Energy Society a few weeks before their annual conference, asking for the agenda for the fusion meetings. The secretary asked, "hot or cold?" I received agendas for both, but naturally,there were more hot fusion papers scheduled than cold fusion. There was a half-day cold fusion session, with 10 presentations. No comparable U.S. physics society would allow even one paper.

Editors at major Japanese science journals

'Cold fusion is not just something to be studied by a single enterprise or a single nation. I am confident it will become a precious asset to all mankind, as the ultimate, ideal form of energy, so it should be shared among all the nations of the earth.'

Thus, I have enthusiastically put my heart into promoting the development of future technology. At the same time, I have always felt anxious about the issue of alternative energy. The dire need to replace drained petroleum resources is a stark warning for the 21st century.

I felt strongly concerned in March 1989, when Dr. Fleischmann and Dr. Pons announced the cold fusion phenomena. Fortunately, I found an opportunity to invite both professors to Japan, where we became good friends. After close conversations with them, I became even more firmly convinced of the importance of cold fusion.

Later, when Technova received a joint research proposal from Professors Fleischmann and Pons, I was determined to do eveything I could to offer them an opportunity to work to their hearts' content, and allow them to become totally engrossed in the research. It was my judgment that IMRA Europe in Nice would be the ideal environment for them. So I offered this facility, and now they are giving their undivided attention to advancing their research there.

Furthermore, in July [1992], to advance cold fusion research more effectively, we opened IMRA Japan in the New Sapporo Technology Park, Hokkaido. With the cooperation of various experts, we are working on cold fusion right here in Japan.

To assure the success of a technology, obviously, there must be support from a wide variety of scientific fields. In other words, the harmonious development of science and technology is precisely the right way to achieve valuable results which can contribute to mankind. The reason we support cold fusion research actively is because, as a business enterprise, we feel we must contribute more to science.

Cold fusion is not just something to be studied by a single enterprise or a single nation. I am confident it will become a precious asset to all mankind, as the ultimate, ideal form of energy, so it should be shared among all the nations of the earth.

Therefore, this is my hope, and my message to you, the cold fusion researchers: Please continue to work with all your might to make this new form of energy a reality, because you offer such hope to the coming generations of the 21st century. You will help them fulfill their greatest dreams and ambitions for the future. Thank you for your attention.

take cold fusion seriously, regularly publishing papers on the topic. A good source of scientific papers is the Ministry of Education's *Jouon Kakuyuugou No Sougou Kenkyuu*, Government Publication No. 02305015, 359 pages, edited by H. Ikegami. A collection of reprinted papers from 1990 and 1991, it is mostly in Japanese, with some papers in English.

One of Japan's leading scientific journals is *Oyou Butsuri* (The Japanese Journal of Applied Physics). It has published articles about cold fusion, starting in November, 1989. The July 1993 issue, Vol 62, No. 7, ran six articles about cold fusion. A scientist outside the field told me, "this looks like semi-official recognition and acceptance to me." It is semi-official recognition because support from the mainstream physics societies is not always unanimous or whole-hearted.

There are still factions strongly opposing cold fusion. They complain, and campaign against funding research, as reflected in the Nakatomi statement and in the *Yomiuri* article quoted above. The *Yomiuri* ridiculed the opponents, describing hot fusion scientists' attempts to stop cold fusion as "petty bureaucratic squabbling."

A cold awakening?

The fact that cold fusion threatens to put hot fusion out of business is not lost on anyone. Ikegami, one of the world's leading hot fusion scientists, is a professor at the National Institute for Fusion Science (NIFS), which is slated to become the home of Japan's next generation tokamak hot fusion reactor. In a June, 1992 article in *Aera* magazine, science journalist Atsuko Tsuji quoted Ikegami: "Officially, we aren't supposed to study cold fusion..." She pointed out:

"The National Institute is pushing a plan to spend nearly 100 billion yen [$900 million] to build a plasma fusion machine. If it were possible to create a fusion reaction on a budget only one thousandth of that, the very existence of the National Institute would be called into question."

Opposition is still active, but it is increasingly ineffective. Many of the scientists expressing exasperation or contempt for cold fusion are high energy particle physicists, who feel they are the creme de la creme of physics, and who deeply resent electrochemists and nuclear engineers declaring that a scientific revolution is under way in the low energy domain. They do not oppose cold fusion so much as they ignore it—give it the cold shoulder.

Occasionally, bitter, unethical opponents surface, trying the same dirty tricks that worked so well in the U.S., such as innuendo, ad hominem personal attacks, distortion, and so on—tactics more common in the early days in 1989 and 1990. Usually, the fight is conducted quietly, but twice in 1993 it broke out into the open: once in a strange NHK television science documentary, and once in a venomous, anonymous attack on cold fusion in the *Applied Physics Society Letters*. Fortunately, this kind of opposition is becoming rare in Japan.

The cold fusion arena

The big questions Americans always ask are: How many people are working on cold fusion in Japan? How much are they spending? How many companies are working on it?

My answers are honest, but disappointing: Nobody knows.

There is no way to tell. We can only estimate by counting the number of conference attendees, and those publishing papers, or filing patents, giving a rough count of 600 people working full-time on cold fusion. Multiply 600 by the average salary, overhead, and equipment costs and you get a total annual R&D budget of roughly $90 million, not counting major one-time capital expenditures like the construction of the Toyota IMRA laboratories in Hokkaido, and Nice, France.

At the Second International Conference on Cold Fusion (ICCF2) in June 1991, in Como, Italy, Ikegami said:

"There are more than 100 scientists at present working on cold fusion in Japan, spanning more than 40 universities and institutions. They are organized into about 20

Nagoya conference attendees

Statistics from the Third International Conference on Cold Fusion: The "Nagoya Conference" The data was compiled from "Frontiers of Cold Fusion"; Proc. 3rd Int. Conf. Cold Fusion, Nagoya, 1992, ed. H. Ikegami (Universal Academy Press, Tokyo, 1993), p. iii and p. 681.

Total number of Nagoya conference participants: 324

Participation by Country: Canada 2, China 11, France 5, Germany 2, India 1, Italy 20, Japan 203, Korea 1, ROC 4, Russia 12, Spain 2, Switzerland 2, UK 2, Ukraine 2, U.S. 55.

The Nagoya conference was sponsored by seven prestigious Japanese scientific societies:

The Physical Society of Japan
The Japan Society of Applied Physics
Atomic Energy Society of Japan
The Institute of Electrical Engineers of Japan
The Chemical Society of Japan
The Electrochemical Society of Japan
The Japan Society of Plasma Science and Nuclear Fusion Research

Additional sponsors included:

Aichi Prefectural Government, Aisin AW Co., Ltd., Aisin Seiki Co., Ltd., Central Research Institute of Electric Power Industry, Daido Steel Co., Ltd., Digital Equipment Corporation, Canon, Japan Godo Steel Ltd., Kubota Corporation, Kyoei Steel Ltd,. Mitsubishi, Materials Corporation, Mitsubishi Research Institute, Inc., Mitsubishi Steel Mfg. Co., Ltd., Nagoya City Hall, Nakatomi Satoshi, Niki Glass Co., Ltd., Nippon Steel Corporation, NKK Corporation, Nuclear Fuel Industries, Ltd., Ohyo Koken Kogyo Co., Ltd., Osaka Gas Co., Ltd., R-DEC Co., Ltd., Seiko EG&G Co., Ltd., Sumitomo Metal Industries, Ltd., Tanaka Kikinzoku Kogyo K.K., The Federation of Electric Power Companies, The Japan Steel Works, Ltd., The Tokyo Club Toho, Sanso Co. Ltd, Tokyo Gas Co. Ltd., Toshiba Corporation, and ULVAC Japan Ltd.

The conference was dominated by Japanese participation. More than 203 Japanese scientists, engineers, and industrialists attended. Well-known and little-known Japanese companies attended:

Air Liquide Lab, Aisin Newhard Co., Ltd., Aisin AW Co., Aisin Seiki Co., Ltd., Asahi Glass Company, Chlorine Engineers Corporation, Chubu Electric Power Company Cogema, Japan, Daikin Industries, Ltd., Electrotechnical Laboratory, Equos Research Co., Ltd., Fuji Electric Hitachi, Ltd., (Nuclear Power Systems Division), Honda R&D Company, Horiba, Ltd., IMRA Japan, IMRA Material R&D Co., Japan Development Bank, JGC Corporation (Nuclear Advanced Technology Division), Kansai Electric Power Co., Inc., Mitsubishi Atomic Power Industries, Inc., Mitsubishi Heavy Industries, Ltd., Mitsubishi Materials Corporation Mitsubishi Research Institute, N.E. Chemcat Corporation, Nippon Steel Corporation, NKK Co.

Also, NTT Basic Research Laboratories, NTT LSI Laboratories Nuclear Fuel Industries, Ltd., Nuclear Engineering, Ltd., Osaka Gas Company, Owners Engineers Co., Permelec Electrode Ltd., Power Reactor and Nuclear Fuel Development Corporation, Sanwa Research Corporation, Sumitomo Chemical Co., Ltd., Sumitomo Electric Industries, Ltd., Sumitomo Metal Mining Co., Ltd., Tanaka Precious Metals, Technova, Inc., Tokyo Electric Power Company, Tokyo Gas, Toshiba Corporation,Toyota Central Research and Development Labs, Toyota Motor Company, and Yokogawa Electric Corporation.

A large number of Japanese universities were represented, including: Aoyama Gakuin University, Chubu University, Chuo University, Ehime University, Hiroshima Institute of Technology, Hokkaido University, Iwate University, Kinki University, Kyoto University, Kyushu University.

Also, Meiji University, Musashi Institute of Technology, Nagoya University, Nagoya University (Plasma Science Center), Nippon Bunri University, Osaka Institute of Technology, Osaka University, Osaka City University, Ritsumeikan University, Shizuoka University, Tohoku University (Cyclotron and Radioisotope Center), Tohoku University (Institute for Materials Research), Tohoku University (Laboratory for Nuclear Science), Tokai University, Tokyo Institute of Technology, Tokyo Metropolitan University, Tokyo National College of Technology, Tokyo University of Agriculture and Technology, Tsukuba University, University of Osaka, Prefecture University of Tokyo (Meson Science Laboratory), University of Tokyo (Dept. of Nuclear Engineering), Utsunomiya University, Waseda University, Yokohama National University.

Other Japanese Institutions and Agencies represented were: Asian Office of Aerospace Research and Development, Biological and Agricultural Research Institute, Central Research Institute of the Electric Power Industry, Institute of Applied Energy, Japan Atomic Energy Research Institute MITI (Electric Power Technology Division), National Institute for Fusion Science, National Laboratory for High Energy Physics, New Energy & Industrial Technology Development Organization, Nomura Research Institute, Ltd., (Investment Research Dept.), Osaka Science and Technology Center,

The Institute of Physical and Chemical Research, Tokushima Research Center.

Fifteen of the corporations are members of the Ministry of International Trade and Industry's cold fusion R&D consortium. They are: Chubu Electric Power, Hitachi, Toshiba, Fuji Electric, Mitsubishi, NKK, Kyushu Electric Power Nippon Steel, Tokyo Electric Power, Tokyo Gas, Osaka Gas, NTT, Aisin Seiki (a subsidiary of Toyota), Kepco Power, and Mitsubishi Materials.

research groups which collaborate to carry out the experiments. Only three groups, Yokohama National University, Tokyo University of Agriculture and Technology, and IMRA Japan, are working exclusively on excess heat, while the others mostly study fusion products (neutrons and charged particles such as tritium, protons, and helium-3)."

There are about six times more scientists in the field now than in 1991. Most of the increase is in private industry, rather than government national laboratories and universities. There is no way to gauge the precise extent of academic funding for cold fusion in Japan because most of the work is done by professors in national universities. There is no central accounting for them, and no central control over their research activities. They are independent. They have tenure, and they have the use of fully-equipped laboratories. If they can interest a graduate student or two, they can get plenty of enthusiastic help.

Full professors get $45,000 yearly in discretionary funds to buy whatever equipment they want. Akito Takahashi, of Osaka National University, jokingly referred to this amount as "sparrow tears," but $45,000 can go a long way in a cold fusion experiment when one already has a fully-equipped lab. MITI provides grants for cold fusion research at national laboratories to cover the cost of special equipment and other expenses that exceed discretionary fund levels.

There weren't quite as many Japanese companies represented at Maui in 1993 as there were in Nagoya, geographical distance being the main reason. Also, some of the Nagoya attendees only came to satisfy their curiosity. Some were not performing research in this field—they were just curious. In 1983, Ikegami guessed that about 20 companies are seriously working in this field. But a "serious" cold fusion project takes many millions of dollars.

Perhaps there are only 20 big companies

with multimillion dollar projects underway, perhaps more. As far as is known, IMRA is the only Japanese company with two dedicated cold fusion labs and dozens of scientists working in the field, but there could be 10, 20, or 30 other companies with dedicated labs. No one has any real way of knowing. Corporations aren't in the habit of divulging information about important, competitive R&D efforts. Most such research remains secret until patents are granted, or finished products are revealed.

Judging from corporations' published papers, patents filed, and representatives at conferences, it's safe to estimate there are some 450 people working on cold fusion in private industry. Suffice it to say there is significant research going on behind closed doors.

The clarion call to science

Hideo Ikegami's introduction to the Third Annual Conference Proceedings on cold fusion reflects the deep-seated hope that drives scientists to work in this field—year after year, in spite of the immense difficulties.

"At the conference, the video produced by Drs. Fleischmann and Pons allowed us to see that a controllable excess heat generator was already at hand. These remarkable efforts were confirmed thanks to the efforts of Dr. McKubre, Dr. Takahashi, Dr. Kunimatsu, and Dr. Storms, who along with Drs. Fleischmann and Pons, reported on their work at the conference and offered extensive documentation of their experiments.

"It is my belief that cold fusion will become one of the most important subjects in science," Ikegami continued, "one for which we have been working so patiently, with dedication and courage, for future generations."

What really drives scientists, though, is not an urge to help future generations. They conduct frustrating, difficult experiments year after year because it's fun. It is what Oppenheimer called "a sweet problem."

Makoto Okamoto, of the Tokyo Institute of Technology, worked for a solid year with no results. Just as he was thinking of quitting, he finally got several positive neutron detections in succession. Atsuko Tsuji quotes him:

"You might call it a miracle. Once you have seen it happen, everyone in the lab becomes fascinated by the phenomenon, and you can't give it up. You can't let go."

Popular press coverage; the occasional big splash

News of cold fusion does not appear in the popular Japanese newspapers and magazines every day. Far from it. There are probably two or three dozen articles a year about it, ranging from a paragraph or two, to in-depth interviews with leading scientists in the field.

Japan witnessed a burst of excitement over cold fusion in 1989, similar to that seen in the U.S. and Europe, followed by angry debate. Japanese cold fusion scientists who have been in it from the beginning, like Mizuno, say the situation was not at all pleasant, but didn't begin to compare to the emotional flood in the U.S.

Articles tend to come in spates, after a conference, or after some breakthrough is announced. Sometimes, the breakthrough is more in the minds of the journalists than the scientists. Scientists usually take a year or two to decide whether an experimental result really is an important breakthrough. Newspapers look for quick resolutions of the issues. They want to know who is winning, who is losing, what's hot, and what's not. Periodically, they "rediscover" the field with renewed enthusiasm, apparently hoping for a return of the 1989 fever—the last thing any scientist in the field wants to see. In March 1992, headlines in a *Yomiuri* article speculated:

"Low temperature fusion: Will it boom again? *Lack of neutrons, the mystery deepens."*

It's safe to say that scientists generally hope the cold fusion field doesn't "boom" again the way it did in 1989.

The best in-depth technical articles for the general reader appear in *Trigger*. The March 1993 edition, Vol. 12, No. 3 banners on the cover: "A special edition on the Cold Fusion Revolution," and includes six excellent articles. The May 1993 edition, Vol. 12, No. 6 ran a three-page article describing the special meeting of the Electrochemical Society, in March 1993, devoted to cold fusion. Some good technical articles and notes have appeared in the Japanese edition of *Scientific American*.

Well-written technical articles geared for the general reader have appeared from time to time in every major Japanese newspaper, and in many different magazines, including Japan's leading monthly *Bungei Shunju*, the *Asahi* weekly magazine *Aera*, the Japanese edition of *Playboy*, and many others.

Cold fusion scientists sometimes write articles for magazines or newspaper columns, and they are often interviewed. A good case in point was *Bart* magazine, which published a three-page interview with Dr. Eugene Mallove in its April 1993 issue.

Most popular books and articles about cold fusion in Japan are neutral or positive. Authors usually accept as fact that the heat is real—and beyond the limits of chemistry. A few articles, however, still question the reality of the effect. The only extremist, negative book yet published in Japan is a translation of Gary Taubes' book *Bad Science*. It was reviewed in the January 1994 *Yomiuri* by Mitsuhiko Tanaka, who seems puzzled by it.

Tanaka naively asks why Pons and Fleischmann were not interviewed in the book, and says, "If the descriptions of the research in this book really are accurate, the scientists are not the only ones who should be roundly criticized..." At least Tanaka leaves the question open; he has some doubt about the veracity of the reporting. It's pretty well accepted that almost every U.S. reviewer fell for Taubes' story, hook, line and sinker.

Jed Rothwell

The weekly science page in the *Yomiuri* newspaper is fun to read. Editors, opinion makers, and ordinary people gung-ho about the future in Japan tend to enjoy technology. *Yomiuri* talks about the latest gadgets and discoveries, matters of a practical nature, such as promising new medical treatments, earthquake prediction, or advances in fiber optics and how they will contribute to the information highway.

This is quite a contrast to the *New York Times* weekly *Science Times* page, which features obscure articles about evolution, bizarre, esoteric articles about "string theories," and catastrophic, end-of-the world predictions about gigantic meteorite strikes in the earth's atmosphere.

The Japanese press *never* declares that cold fusion is a sure thing. It never trumpets cold fusion as a panacea. I have not seen one article predict that cold fusion will replace all other forms of energy. But, year after year, factual, low-key articles in newspapers, news magazines, and science magazines present the facts about cold fusion breakthroughs.

Newspapers cautiously endorse the research, saying it is definitely worth pursuing, at careful, low levels of funding. The press acknowledges that it might become a practical source of energy. That prospect is never far from anyone's mind, so the press strongly advocates more research to get to the bottom of it.

Some leading science magazines are more enthusiastic about cold fusion than the newspapers, particularly *Trigger*, whose April 1993 cover boldly declared: "The Cold Fusion Revolution is Here!" and ran six in-depth articles. Three articles appeared in the June issue, all excellent, factual, businesslike, and pragmatic.

The revolution is in physics and science. *Trigger's* editorial stance is that cold fusion is a proven, nuclear reaction, but makes no predictions that cold fusion will be a practical source of energy. Like the rest of the Japanese press, *Trigger* has adopted a wait and see attitude.

For science, technology, business, and even the media, Japan's soil is fertile for cold fusion.

The Fourth International Co

Report from Maui

By the sea at Maui—cold fusion conferences as far as the eye could see. The International Conferences on Cold Fusion have become milestones in a rapidly expanding field. The Fourth International Conference on Cold Fusion (ICCF4), held on Maui, December 6-9, 1993, did not disappoint. At the very beautiful Hyatt Regency in Lahaina, dozens of researchers announced landmark and breakthrough results. How fitting this time to have a cold fusion conference on a beautiful island surrounded by the fuel of the future.

In the background of Maui were sweet and bitter memories of earlier cold fusion conferences. One year after the March 23, 1989, cold fusion announcement, the First Annual Conference on Cold Fusion convened in Salt Lake City amid loud controversy. *Nature* magazine sent no reporter, but still felt free to attack the conference in its editorials. Robert Park of the American Physical Society on national television called that meeting a "seance of true believers."

The more serene Second International Conference on Cold Fusion (ICCF2) was held in Como, Italy, June—July, 1991. Nagoya, Japan, was the venue of ICCF3 in late October 1992, which had the full support of the Japanese scientific establishment. The Maui conference was sponsored by the U.S. Electric Power Research Institute (EPRI) of Palo Alto, California, the research arm of the American electric utility industry, which continues to fund cold fusion research at SRI International and elsewhere.

Future cold fusion conferences will continue the custom of following a rotation: U.S., Europe, and Asia. At the conclusion of the Maui conference, the Organizing Committee announced that ICCF5 will be held in the spring of 1995 in Nice, France, and ICCF6 in Beijing, China, thus fulfilling the strong wishes of the Chinese researchers and government to host a cold fusion conference.

The four-day conference consisted of morning plenary sessions— each with five or six speakers. The afternoons featured for the first time *parallel* sessions, e.g. one session devoted to calorimetry, one to theory, one to nuclear effects, and one to materials, in various combinations for the different days. A measure of how mature the cold fusion field has become is that it required parallel specialist sessions. Thus, it became impossible, for the first time, for one person to take in everything. The scope of the conference encompassed almost 300 participants, and more than 150 technical paper presentations. A technical poster room was open throughout the conference. On the afternoon of the closing day, a panel session of participants summed up ICCF4.

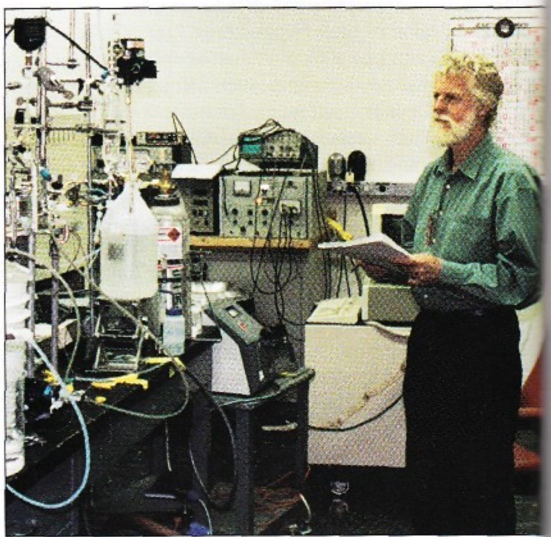
Since it is obviously impossible in a very short space to relate all that took place at the conference, this is a modest attempt to recall some of the most significant findings and events. To illustrate the depth and breadth of activities, we have appended the pre-conference listing of papers, which was necessarily incomplete. Also, Professor Robert A. Huggins of the Department of Materials Science at Stanford University, an early pioneer in the cold fusion field, has graciously provided *"Cold Fusion"* Magazine with his impressions of ICCF4, which we reprint after this overview of ICCF4 highlights. Professor Huggins writes of the conference from the special viewpoint of a materials scientist.

Japan's New Hydrogen Energy (NHE) Research Program

During the first morning session, Dr. K. Matsui, director of the R&D Center for New Hydrogen Energy in the Institute of Applied Energy in Tokyo, announced that the NHE program would be launched to "clarify the feasibility of NHE as one of the future energy sources." The period of the project: November 1993—March 1997. The budget: $30 million dollars for four years. [This is a budget request, for which confidence exists that it will be granted.] Leading industries are involved: Ten (10) electric utilities, plus Tokyo Gas, Mitsubishi Heavy industries, Hitachi, Toshiba, Nippon Steel, Aishin Group (Toyota), Tanaka Kikinzoku, NTT, IHI, and NFI. The organizational chart shows that MITI will provide the research funding to the New Energy Development Organization (NEDO), which will have under it the Institute of Applied Energy—the R&D center for the NHE project based in Tokyo and the NHE Laboratory in Sapporo. Cooperation with EPRI and SRI International in the U.S. is explicitly provided for.

Cold fusion with solid state devices

One of the most startling developments at ICCF4, one which has already captured the attention of the Japanese press, was that the cold fusion effect can be produced at a high level in a solid ceramic material, such as a strontium-cerium-oxide "proton conductor." These materials are similar to high-temperature superconductors. Professor T. Mizuno and his group at Hokkaido University have tested these "solid electrolyte" plates maintained at 300 to 400°C. Excess heat on the order of 100 watts per square centimeter emerged during absorption-desorption cycles of deuterium-containing hydrogen gas under the application of an alternating elec-



Roger Stringham, visiting at Los Alamos National Laboratory in 1993, conducts a "microfusion" experiment with the E-Quest device.

Conference on Cold Fusion

BY EUGENE F. MALLOVE

tric field. Excess heat amounting to 50 watts for some 20 hours was achieved. The input electric power was tiny—about 7.2 x 10^{-4} watts. The power ratio was thus a huge 70,000. This device was only 0.8 cm in diameter and 0.1 cm thick.

True, the Mizuno solid-state device requires elevated temperatures of a few hundred degrees C for the excess power to emerge, provided in this experiment by a separate electrical resistance heater. In a practical implementation, the reaction chamber would be well-insulated so that the energy of the reaction would self-heat the ceramic, unlike in Mizuno's experimental apparatus where heat transfer to the environment was large.

Independently of Mizuno et al, Jean-Paul Bibérian of R&D International, Orinda, California, had a poster display on his "Solid State Cold Fusion" device made of $AlLaO_3$, aluminum lanthanum oxide. He, too, has observed what appears to be copious excess heat evolution in a deuterium atmosphere. It appears that he is able to achieve 500 watts/cm^3 from his small wafer crystals, with a total output power of tens to a few hundred watts. The excess heat is highly reproducible. The originator of this work, according to Bibérian, was F. Forrat in France, who took out French patents in 1989 and 1990.

Confirmation of excess heat in ordinary water

The Indian group at the Bhabha Atomic Research Center (BARC) brilliantly confirmed its earlier series of tests with ordinary water. They continue to get excess heat and, in many cases, tritium above background level. They have even made a serendipitous discovery: that stainless steel poisons the process and prevents excess heat evolution. The group studied different types of nickel. Out of a series of 28 cells, 14 showed excess heat. Excess power in these cells was in the range 0.2 to 0.7 watt.

The BARC group has used off-the-shelf consumer "thermos bottles" to conduct its experiments. The thermal characteristics of the standard internally aluminized thermos bottles is typically 20-35°C temperature differential (between inside and ambient) per watt of input power (i.e., 20-35°C/watt). The group now reports a simple technique of de-aluminizing the thermos bottles with nitric acid. This allows the cells to reach steady state temperature within 10 hours instead of the 24 hours required earlier; the calibration constant now is 7-10°C/watt.

Dr. Srinivasan told this author that almost everyone at BARC now accepts the reality of cold fusion. He is planning eventually to put together a "kit" that will allow any experimenter to observe the cold fusion excess heat effect in light water—with >90% confidence. Dr. Srinivasan currently is on leave from BARC and is working at SRI International to help that group perform ordinary water-nickel experiments.

Electrochemist Dr. Reiko Notoya of Hokkaido University also has achieved excess heat in ordinary water, and like the BARC researchers, finds tritium above background level.

The team of Professors Robert Bush and Robert Eagleton of California Polytechnic University continues to report excess heat results in electrolysis with light water and potassium carbonate, as well as sodium carbonate electrolyte. The excess heat found in sodium-carbonate cells—a finding also of the BARC group—runs contrary to the claim of Dr. Randell Mills of HydroCatalysis Power Corporation in Lancaster, Pennsylvania, that no such excess is found in sodium carbonate solution, one of the requirements of Dr. Mills' electrocatalytic ("shrunken" hydrogen atom) theory for explaining excess heat.

Bush and Eagleton performed experiments in special "de-deuterated" light water having only 1% of the normal trace heavy water content. These tests, they say, prove there is a "genuine light water excess heat effect," because the de-deuterated light water cells gave the same order of excess heat as the ordinary light water cells.

The most remarkable claim of Bush and Eagleton is a seemingly definitive experiment demonstrating that rubidium (from rubidium carbonate electrolyte) transmutes into strontium during excess heat experiments. Examination of the strontium isotopes in the solution showed "statistically-significant enhancements in the ratio of $^{86}S_r$ to $^{88}S_r$ relative to the natural abundance ratio," i.e., by 325 standard deviations. The claim is that this rules out contamination from possible natural sources of strontium.

Heat After death

This follows work reported in Pons' and Fleischmann's May 3, 1993 *Physics Letters A* paper on heavy water cells that go to boiling, boil away virtually all of the heavy water, and then remain at 100°C for three hours without electric current input. This high temperature in the open dewar cell—caused by an electrode obviously hotter than the 100°C of the vapor in the cell—has been found to occur for much longer periods, for 40 hours or more. Other work reported at the conference (or hinted at as "work in progress") suggests that various materials, including special types of ceramics in deuterium atmospheres, can remain hot for long periods with minimal or absolutely no current input. This remarkable phenomenon of "heat after death," i.e., infinite power ratio, may well become a primary direction in a variety of cold fusion systems.

Triggering cold fusion by radio frequency stimulation and magnetic fields

Drs. John Bockris, Dennis Letts, and Dennis Cravens separately discussed the astonishing new finding, discovered by Dennis Letts, that radiofrequency stimulation (RF) in the MHz range (around 82, 365, and 533 MHz) produces excess heat in electrochemical cold fusion experiments. In particular, RF stimulation must be applied at precise frequencies to bring about the effect, and the effect typically occurs within 30 minutes of imposition of the RF. The power levels of RF imposed on various cells were low, in the range 10— 300 milliwatts. Magnetic fields, either from permanent magnets or electromagnets, also seem to enhance heat production.

Heat production with multi-layer thin-film electrodes

Professor George H. Miley, et al, Fusion Studies Laboratory, University of Illinois, have developed a unique process to coat a stainless steel plate electrode (25 mm x 25 mm x 3 mm) with alternating layers of titanium and palladium, which are deposited by a special electron-beam evaporation method. The layers have a total thickness of only 100 Angstrom units, topped off by a 60 Angstrom thick layer of chromium to "act as a barrier to retain a high loading of deuterium or hydrogen." In the reported experiments they work with light water and LiOH electrolyte. They have achieved about 2 kilowatts/cc power production in these thin films. They have actually gone as high as 10 kW/cc, but the layers have a nasty habit of peeling off at these power levels.

Preliminary confirmations of the Kucherov et al glow discharge experiment

Professor Peter Hagelstein of MIT's Department of Electrical Engineering and Computer Sciences showed one gamma ray emission line that this group attempting to replicate the Kucherov et al experiment thinks it has found—a 129 KeV feature that may represent the ^{105}Pd to ^{105}Rh transmutation found by Kucherov et al in Russia. This is only a very preliminary finding.

A group known as "Space Exploration Associates" of Cedarville, Ohio, has obtained preliminary confirmation of gamma radiation in a Kucherov replication attempt. The group was successful in seeing the gammas six times, with no failures. The MIT group is collaborating with them.

A lovely quote from the team's preprint: "It is perhaps worthy to note that Fermi won the Nobel prize for mistakenly believing that he had transmuted uranium by bombarding it with neutrons. So strong was the prevailing belief in the integrity of all atoms that the splitting of uranium was inconceivable. Hence Fermi's experiment was not interpreted correctly at first. Thus, the finding that palladium may be transmuted under the influence of electromagnetic fields in the presence of deuterium may likewise be inconceivable, but may nevertheless be the truth."

(L to R):Dr. Talbot Chubb (Research Systems, Inc.), Dr. Yan Kucherov (ENECO), and Prof. Robert Huggins (Stanford University)

Dr. Martin Fleischmann, in his concluding remarks at the end of the conference, praised Kucherov and said that in France they had reproduced "parts" of the Kucherov experiment.

Dr. Yan Kucherov spoke and reviewed both the thermal excess energy and nuclear effects data that his Moscow group has obtained over the past four years in more than 500 glow discharge experiments in deuterium atmospheres.

Helium-4 results

Evidence continued to build that at least in some varieties of cold fusion experiments, helium-4 (^{4}He) is produced as "nuclear ash." Dr. Melvin H. Miles of the Naval Air Warfare Center at China Lake presented convincing evidence of a correlation between the measured excess power in Pd-heavy water cells and the production of helium-4. His group claims helium production in the range 10^{11} to 10^{12} helium atoms per second per watt of excess power. Dr. Miles said that this is "the correct magnitude for typical deuteron fusion reactions that yield ^{4}He as a product." By this he meant that helium yield would be about right if the powerful 23.8 MeV gamma ray normally associated with the D + D to ^{4}He reaction emerged instead as thermal energy.

The group used metal flasks this time to collect gas samples, instead of the glass vessels used in earlier runs. Critics had complained that the glassware could be a source of contamination by atmospheric ^{4}He diffusion through the glass. Dr. Miles suggested that at the low excess power levels of his recent experiments (~0.1 watt), the possible measurement errors were large, but still he had confidence that the ^{4}He produced exceeded the levels in control experiments (runs with no excess heat).

The group of Professor Daniele Gozzi et al at the University of Rome examined ^{4}He from Pd-heavy water cells by continuous mass-spectrographic analysis—in six cells for over 1,000 hours. They found a correlation in the emergence of helium-4 peaks and rises in excess heat. However, the group is still trying to rule out contamination from atmospheric helium-4 by using the neon-20 (^{20}Ne) measurement simultaneously.

Cold fusion by hydrogen sparking

Dr. Jaques Dufour of Shell Research (France) reported on excess energy produced by sparking onto various metal electrodes in hydrogen gas atmospheres, both light and heavy hydrogen. He reported stable excess energy production of a few watts for periods of several weeks.

Ultrasonic activation—"microfusion"

Roger Stringham and Russ George presented their work. It is clear that they and others who have checked these experiments find substantial levels of ^{4}He after ultrasonic (20 kHz sound frequency) beaming against palladium immersed in heavy water. The ^{4}He is roughly proportional to the excess power evolved, and is said to account for about 20% of that excess. ^{4}He levels are higher than atmospheric concentration, up to 65 ppm, which is on the order of 10 times higher, so atmospheric contamination is unlikely. The excess heat level claimed by this group is at a level of up to 90 watts (input power, 350 watts). The local heating of palladium foils is so great that these occasionally melt through under even continuously circulated heavy water. No observable excess heat or melting was found in ordinary water control experiments. The reaction chamber is normally pressurized to several atmospheres by D_2 and argon gas.

Stringham et al suggest that the collapse of cavitation bubbles created by the ultrasonic transducer at the surface of the palladium injects deuterons into the Pd lattice. The metal lattice, thus locally and rapidly loaded to a high level, gives rise to D + D "microfusion" reactions. On microscopic examination, they have observed evidence for localized ejection of molten metal.

They also claim to have found after testing that isotopes of cadmium are present "skewed in relationship to natural abundance ratios." In particular, they suggest that ^{114}Cd has been formed from the reaction of an alpha particle (^{4}He nucleus) and ^{112}Pd. Measurements made at various laboratories found no tritium, gamma ray, or neutron evolution from the operating apparatus.

Stringham et al, who have a company based in Palo Alto, California, E-Quest Sciences, are offering to sell their "microfusion" apparatus to serious groups as a research tool. E-Quest will guarantee the ability of the device to produce excess power and ^{4}He.

IMRA Japan's work on material properties and triggering parameters

The IMRA Japan laboratory group of Dr. Keiji Kunimatsu continues to pioneer the investigation of alloys, current densities, and loading in Pd-heavy water cells. The group's work in closed cell calorimetry is viewed as among the finest in the world. IMRA Japan uses deuterium gas to pressurize its cells so that the anode becomes a gas-diffusion electrode.

The team has found alloys of Pd using 5% rhodium to be of particular value. IMRA Japan found that Pd materials from different vendors produced excess heat, with one exception. It found excess heat to be "almost proportional" to current density. The critical current required to turn on the excess heat is about 200 mA/Cm^2 of cathode surface area, and the critical loading ratio for turn on was found to be D/Pd=0.84. The addition of thiourea was effective in pushing the loading ratio over 0.9. No excess heat emerged in light water experiments with Pd cathodes.

SRI International's McKubre gears up for nuclear measurements

The EPRI-funded SRI International group, led by Dr. Michael C.H. McKubre, has continued to verify its earlier excess heat work, but is now rapidly mounting a search for nuclear products, including ^{4}He. It wishes "to attempt to quantify the appearance, and set limits on the non-appearance, of po-

...ential products of nuclear reactions: neutrons, gamma and x-rays, ^{3}He and ^{4}He, and isotopic shifts of Pd lattice and electrolyte-derived species." The group made a serendipitous discovery of the beneficial effect on excess power of going to higher temperatures. This temperature effect is now being found almost universally in the cold fusion field.

Dr. Reiko Notoya (Catalysis Research Center, Hokkaido University)

McKubre said that his group has "reproduced wholly, our previous observations of excess power, and are beginning to study the controlling parameters for the purpose of attempting scale-up."

Tritium production confirmation

Dr. Fritz Will, formerly director of the National Cold Fusion Institute in Utah, and now temporarily with EPRI, reported the reproducible and incontrovertible production of tritium in Pd-heavy water cells that were closed. This work has recently been published in the *Journal of Electroanalytical Chemistry*. It appears to show that tritium can be produced at low energy. Absolutely no tell-tale 14 MeV neutrons were detected that would have been evidence of collisions of T with D in the lattice. Dr. Will meticulously outlined all the reasons to believe that the tritium was generated and could not possibly have been pre-existing contamination. By chance alone, he said in conclusion, it would have required a probability of 1/130,000 to obtain the result of picking the four active electrodes out of the total of 17.

Drs. Tuggle and Claytor of Los Alamos National Laboratory reported continued success in producing tritium continuously and reliably in various "novel morphologies of palladium." They use small solid wires combined with pressed metal powders, and observed tritium production in the range over 5 nano-Curies per hour (>5 nCi/h), which they said "far exceed" their previous results.

The Case of the Missing Scientist

Dr. Kevin Wolf of Texas A&M University was supposed to have been the lead speaker for the December 7 session, but he didn't show up and no explanation was offered. Three high-level sources have seen the data report that Dr. Wolf has discovered astonishing transmutations in three of the palladium rods that he tested in Pons-Fleischmann-type protocols. He has observed many different gamma ray spectral lines from short-lived isotopes. He has no doubt that these transmutations are real. This makes his work similar to the results found by Kucherov in Russia, which was reported in *Physics Letters A*, November 9, 1992.

Unfortunately for Dr. Wolf, he has had an ambivalent position in the cold fusion field after accusations in 1990 by Gary Taubes of fraud in the Bockris lab at Texas A&M. This led Wolf to recant inappropriately his tritium results. Now he can barely imagine associating these extremely intriguing results—in which he allegedly fully believes—with the "discredited" field of cold fusion and transmutation. We know that Wolf wanted to attend the meeting, even though he was going to suggest that these transmutations were really due to cosmic rays from space, which "just happened" to be detected during a cold fusion experiment. It has been learned that he was under great pressure by an opponent of cold fusion,who funds him, *not* to attend the ICCF4 conference.

Hydrosonic Pump continues to evidence excess energy

One of the most unusual presentations at Maui, a last-minute addition to one of the special afternoon sessions, was prompted by a journalist's article. An unusual apparatus came to light as a result of Jerry Bishop's cover story on cold fusion in the August 1993 *Popular Science*. That story prompted Georgia inventor James L. Griggs to contact people in the cold fusion field. He wanted help to explain the baffling excess energy that he and his colleagues had regularly observed with their Hydrosonic Pump.

This device, which has been developed and patented by Mr. Griggs of Hydro Dynamics, Inc. of Cartersville, Georgia as an efficient heating unit for buildings, has regularly demonstrated (it is claimed) significant levels of excess energy. It consists of a specially-designed cylindrical aluminum rotor that spins at close tolerances inside a steel case. Ordinary water is forced through the gap between rotor and case, thus producing hot water and/or steam via turbulent action. The measured energy content of the steam and hot water apparently exceeds the electrical input power of the device by a large margin—10-100% and beyond.

If this effect is real, perhaps this device is related in some way to the cavitation-induced "microfusion" apparatus of Roger Stringham.

Griggs and his colleagues gave a brief presentation at the Maui conference. *"Cold Fusion"* Magazine will continue to investigate the Hydrosonic Pump's performance, and will bring you more news as further testing develops.

Theories abound

At ICCF4, as at previous cold fusion conferences, dozens of papers were devoted to theories that might explain the seemingly bewildering host of experimental findings. *"Cold Fusion"* Magazine will delve into these arcane theoretical matters in a subsequent issue, making a valiant effort to disentangle some of the most difficult issues theorists face. Apologies for this postponement to our theorist colleagues, who perhaps view their efforts as equally important with experimental findings.

We will make one bow toward discussing theory, because ^{4}He nuclear ash has been such a hot topic in the cold fusion field, and because Nobel laureate Julian Schwinger's paper (read in his absence by this author) suggested how a metal lattice reaction generating ^{4}He would not have to be commensurate with excess energy. This is the part of Schwinger's talk ("Cold Fusion: A Brief History of Mine") pertinent to that reaction:

"I note here the interesting possibility that ^{3}He produced in the pd [proton-deuteron] fusion reaction may undergo a secondary reaction with another deuteron of the lattice, yielding ^{5}Li (an excited state of ^{5}Li lies close by). The latter is unstable against disintegration into a proton and ^{4}He. Thus, protons are not consumed in the overall reaction, which generates ^{4}He."

"To this I add, as of some time in 1992, that observations of ^{4}He, with insufficient numbers to account for total heat generated, are consistent with the preceding suggestion. The initial pd reaction produces heat, but no ^{4}He. The secondary reaction generates heat and ^{4}He. There is more total heat than can be accounted for by ^{4}He production. The smaller the ratio of secondary to primary rates, the more the ^{4}He production will be incapable of accounting for the heat generation."

Concluding remarks

Rounding out ICCF4 were summations of the meeting by various senior participants. Dr. Edmund K. Storms, Los Alamos National Laboratory (retired) made one of the most comprehensive and eloquent statements, which *"Cold Fusion"* Magazine has reprinted *(see page 48)*. To summarize, ICCF4 showed that the cold fusion field is becoming ever more vital and expansive—clear evidence of a scientific and technological revolution in the making. Not all papers reported success in finding excess heat and nuclear products, but inexorably scientists are learning the conditions for repeatability of positive experiments, and discovering new methods for generating the phenomena. With small steps and large leaps, the pieces of the puzzle are falling together.

Cold fusion conference

Another leg of the journey

Above all else, the Fourth International Conference on Cold Fusion presented several elemental challenges: that the concept of "cold fusion," after five arduous years, has proved itself experimentally, and that cold fusion is a rapidly maturing science.

It was a conference abundant with scores of papers, seminars, and exchange of ideas for scientists thirsty for developments in the expanding spectrum of cold fusion research.

Organized under the auspices of the Electric Power Research Institute in Palo Alto, California, the conference attracted 274 participants from around the world—more than 150 from the U.S., 72 from Japan, and 34 from Europe, who presented 155 papers. What follows must necessarily be a condensation of thought, theory, and practice derived from the many presentations. Conference Proceedings are being assembled, and soon will be available.

The fifth international conference on cold fusion is set for early in 1995 in Nice, France, and is being organized by IMRA Europe, S.A. in Valbonne, France. Already in the planning stages, the succeeding conference will be in Beijing roughly a year later. One can only wonder at the limitless prospects for end-of-the-century conferences.

A recent industrially-sponsored cold fusion colloquium was held in Asti, Italy, and a conference will be held in Minsk, Belorussia, this May.

Notes and comments on Maui

Two major announcements fell out of the Maui conference, the first a presentation on the New Hydrogen Energy (NHE) Research Project in Japan by Dr. K. Matsui, director of the R&D Center for New Hydrogen Energy in the Institute of Applied Energy in Tokyo.

Initiated in November 1993, the program will run until March 1997 with funding of $30 million from MITI (Ministry of International Trade and Industry). NEDO (New Energy and Industrial Technology Development Organization) is the project coordinator. The program is operated through two major laboratories, the R&D Center for NHE in Tokyo, and the NHE Laboratory in Sapporo. A number of Japanese universities are expected to participate. Cooperation with overseas institutions evidently will be an important aspect of the activity. NHE also will encompass a group of leading industries, including 10 utilities, Tokyo Gas, MHI, Hitachi, Toshiba, Nippon Steel, the Aishin Group, Tanaka Kikinzoku, NTT, IHI, and NFI.

International aspects of the research project apparently will be coordinated through IMRA Europe. The major U.S. partners mentioned were the Electric Power Research Institute, and SRI International.

The program's mission is to clarify the feasibility of New Hydrogen Energy as one of the world's future energy sources. Four major activities were named:

Experimental demonstration of excess heat generation

- Reproducibility
- Verification of reaction products
- Identification of the controlling factors for excess heat generation

Except for a few ill-considered, and rather personal critical remarks by one of the attendees, the conference's tenor was quite normal for an area of newly-emerging science that has possible technological overtones.

Materials science

- Characteristics of existing materials
- Development of new materials

Database and feasibility studies

International Cooperation

- Exchange of personnel and information
- Expert workshops and symposia

The second announcement was that ENECO, Inc. has acquired exclusive worldwide licensing rights to the original Fleischmann and Pons patent applications from the University of Utah. ENECO, founded in 1991, and headquartered in Salt Lake City, is described as a cold fusion research and development company. ENECO evidently has also acquired rights to a number of other patents and patent applications related to the cold fusion area.

The firm brought a number of participants from the former Soviet Union to the meeting. Dr. Yan Kucherov, formerly at the Luch Laboratory in Moscow, is ENECO's director of research. In addition to a modest amount of experimental work in its own laboratory, ENECO is sponsoring small programs at a number of sites in the U.S. and Russia.

General remarks

Except for a few ill-considered, and rather personal critical remarks by one of the attendees, the conference's tenor was quite normal for an area of newly-emerging science that has possible technological overtones.

Earlier results in a number of areas were reinforced by more thorough and quantitative experimental work, and the critical parameters that are responsible for several of the earlier problems have now been established. The spectrum of experimental methods and observations has also been extended considerably. Nevertheless, there are still a number of major unresolved questions relating to the reasons for some of the important experimental observations.

The theoretical basis underlying the now well-established experimental documentation that nuclear processes can take place in solids under conditions that are far removed from those typical of hot plasmas is still far from mature. One of the complicating features is that the reactions expected from hot plasma experience are not found, and that different processes evidently dominate under different experimental conditions.

Identifying "nuclear ashes" has been addressed by a number experiments. The early experimental work in several laboratories that claimed to have *not* observed the Fleischmann-Pons effects has been carefully reconstructed and assessed by independent auditors. The results will be discussed below in these notes.

Excess heat observations in the palladium-deuterium system

The previous experimental observations of excess heat generation during electrolytic experiments were extended and reinforced.

gathers fuel for thought

By Robert A. Huggins

Robert A. Huggins

A number of laboratories reported the results of high quality calorimetry performed using several different methods. This involved experiments with chemically open systems as well as closed, the results being essentially identical when they were performed and analyzed properly.

Fleischmann (Valbonne, France) claimed that his laboratory is now getting excess power output levels of 4 kW/cm^3. He declined to discuss the experimental protocols that are being used, but did explain they first load the palladium with deuterium at low temperatures into the endothermic regime, and then quickly drive it to higher temperatures, where it becomes hyperloaded.

Gozzi (Rome) reported on his group's recent multicell experiments, in which they observed both excess heat and the formation of ^4He. In one case, the amounts of heat and ^4He correlated quite well. They concluded, as had others (e.g. Miles) in the past, that ^4He is the dominant "ash."

Kunimatsu (IMRA Japan, Sapporo) reported on his lab's results with closed cells with a partially submerged fuel cell type of positive electrode that recycles the deuterium instead of forming oxygen on the positive electrode. They operated at a D_2 gas pressure of 5-10 atmospheres. This type of electrode reduces the cell voltage below that necessary for water electrolysis, and is the type of setup considered as a possible advantageous configuration in the original Fleischmann-Pons paper.

They showed, as had McKubre (SRI) earlier, that it is critical for the level of loading (the ratio D/Pd) to be over 0.84 in order to observe excess heat. They used a gas pressure measurement method to evaluate the loading. They also found, as both they and others showed earlier, that the amount of excess heat is electrolysis current-dependent, with a threshold value of about 200 mA/cm^2. They investigated six types of palladium obtained from three sources, and showed that all but one produced excess heat. In that case, they found they were not able to achieve a sufficient loading ratio for some unknown *material* reason.

Their experiments also again demonstrated the well-established fact that no excess heat is observed in light water experiments of this type.

Bertalot (ENEA Frascati, Rome) also presented the Pd/D excess heat results obtained in their laboratory, which they measure with flow, rather than isoperibolic calorimetry. They also use palladium anodes to continually refresh the cathode surface so that it does not become influenced by the deposition of impurities from the electrolyte. They use the pressure change method to evaluate loading.They observed, as have others, that different batches of palladium behave differently, some giving the easily measurable excess heat, and others apparently being "dead." They are now in collaboration with Johnson Matthey, trying to determine the source of this materials effect.

McKubre (SRI International, Menlo Park) reported on their recent excess heat results in closed cells using both isoperibolic and flow calorimetry, and demonstrated that one can obtain comparable results using either method. They have also used open cells in isoperibolic calorimeters of the Pons type, and got similar results. Their open isoperibolic cells have greater sensitivity and a faster response time, and thus provide greater flexibility than the closed flow calorimetric system for the investigation of the influence of various parameters.

McKubre confirmed their earlier conclusion that the D/Pd loading value is critical. When the loading is over 0.95 they always get excess heat, whereas this does not occur with a loading ratio below 0.9.

Using the resistivity method to evaluate loading, they have been investigating the influence of various materials, chemical, and electrochemical variables on the loading kinetics, and the value of the maximum loading obtained. As an example, full loading is never obtained when either nickel or a Pt-Nb alloy is used as the anode. They are also now operating a "movable wire" apparatus to investigate the influence of thermal pretreatment on the behavior of a single batch of palladium.

Another finding is that deliberate introduction of species such as Al or Si into the electrolyte increases the loading and decreases the long-time gradual deloading sometimes found. The use of periodic electrochemical cycling seems to improve the loading.

Most of their experiments are conducted at a constant temperature. However, in one

(L to R): Jed Rothwell (Cold Fusion Research Advocates), Charles Becker (ENECO), and Prof. Robert T. Bush (Calif. Polytechnic, Pomona)

case the temperature rose by mistake, and this resulted in the observation of increased excess heat generation. [See comments below about the influence of temperature reported by others.]

Also observed was the difference in the behavior of material from different batches-even from the same supplier. They are working with a palladium supplier to try to understand this effect's origin.

McKubre's group reported it has observed excess heat effects for several days after input power was turned off or way down. This behavior was observed earlier by others (e.g. Mizuno).

Pons showed similar data on significant heat generation for a number of hours after input power was shut off. He called it "heat after death." This only occurred in cells containing D_2O, not in those with H_2O.

McKubre reported another interesting, and perhaps important observation: they have observed a decrease (e.g. 30%) in the base resistivity of the palladium after long time operation. Since the resistivity in metals is due primarily to scattering from point defects, rather than from extended defects such as dislocations or grain boundaries in the lattice, this indicates a change in the point defect concentration with time.

Bockris (Texas A&M, College Station) reported on recent experiments on the influence of various square wave and ramp mode pulsing sequences upon loading, and their influence upon the observation of excess heat when the loading is sufficiently high. He expressed the opinion that dislocations play an important role in the excess heat phenomenon, and showed the effect of current density (and overvoltage) on microstructural changes in the palladium.

He proposed that the time-dependent exchange of deuterium for hydrogen in the dislocation atmospheres may account for the long incubation (switch-on) time generally observed in this type of Pd/D experiment.

He briefly mentioned the unexpected observation that the imposition of RF (radio frequency) power in the milliwatt range apparently triggers excess heat. Apparently this occurs at a specific frequency. *[See comments below about this.]* There is also some evidence for the influence of magnetic fields, but no substantial information was presented.

Arata (Osaka University) reported excess heat generation using a double (concentric) palladium cathode. Apparently this gives a greater magnitude of excess heat, and for times of the order of months. They observed an incubation time of over 250 hours.

Yoshinaga (Tokyo Inst. of Technology) reported excess heat observations using a square wave loading method in one case out of three. They also observed a much higher overvoltage in the case of the material that gave the excess heat. They had a typical incubation time of 120 hours.

Storms (Los Alamos NationalLaboratory) discussed his excess heat observations. Storms' group used four batches of material, two showing excess heat, and two not. In the latter cases, he observed a significant volume change due to the generation of cracks and pores. This is in accordance with earlier observations that internal void generation is accompanied by deloading. In one of the two successful cases, he used the SRI technique of adding Al to the electrolyte.

Storms suggested that one could use a measure of the volume change that occurs during loading as a relatively simple and fast way to determine whether a sample of palladium will give excess heat, or will act dead. He said he has only observed excess heat from materials exhibiting less than 2% volume change even if fully loaded. Ones that exceed that amount never give measurable excess heat.

He discussed several critical factors, including the effect of current density (he estimated the critical current threshold to be about 100 mA/cm^2), local surface composition that influences the loading, and the influence of the temperature. It is important that the geometry of the system be arranged so that the current density is relatively uniform around the sample. Otherwise, there is deloading through part of the interface.

Ota (Yokohama National University) showed the results of Pd/D (and Pd/H blank) electrolysis experiments in closed cells. They used both pure Pd and a 10% Ag-Pd alloy, and also found important inter-sample differences, with heat generation in only three of 16 Ag-Pd/D runs. Samples that had prior mechanical deformation produced large heat bursts. They observed gradual deloading of higher current density samples at long times, probably due to the generation of internal voids, as observed by others.

They reported the observation of the current (potential)-dependent diffusion of lithium into the Pd surface with time. [The possible relation between the slow diffusion of lithium into palladium and the observation of much longer incubation times than can be explained by the kinetics of the diffusion of hydrogen species has been a matter of considerable speculation]. Others also reported in-diffusion of lithium.

Miles (Naval Air Warfare Center, China Lake), who was the first to report the generation of ^{4}He in experiments showing excess heat, presented similar results using an all-metal system. His previous experiments had employed glass containers, which led to the criticism that the observed helium may have come from helium diffusion through the glass. He showed that the results obtained with the metal system were comparable with earlier observations, and that the amounts of ^{4}He found correlates quite well with the magnitude of the excess heat he observed.

Oyama (Tokyo Univ. of Agriculture and Technology) performed closed-cell experiments in a double-cell difference calorimeter. Low frequency AC was imposed on top of a DC bias current, and two regimes of behavior were observed, one with relatively stable behavior, the other with unstable behavior. The latter lasted for periods of 50-150 hours. Once excess heat appeared (after increasing the DC current) it remained, even under reduced current.

Cravens (Vernon, Texas) reported on a number of experiments he performed to identify parameters that influence the success of Pd/D electrolytic experiments in producing excess heat. He used both relatively simple open isoperibolic cells, and a flow calorimeter with closed cells.

He observed the microscopic topography and distribution of outgassing sites on the surface of different samples of palladium, and the influence of careful mechanical polishing of the surface on the uniformity of deuterium entry/exit. He found that samples exhibiting substantial surface inhomogeniety do not produce excess heat.

Cravens showed data that indicated the influence of current density and temperature during initial loading upon the final loading level, and thus the excess heat. The best results are obtained by loading at a very low rate until the composition is well into the beta phase, and then rapidly raising the input power and temperature.

Preloading with D_2 gas at elevated temperatures is useful [probably to reduce the hydrogen occupancy of the dislocation atmospheres]. He also had evidence indicating the advantage of the use of alloys, such as Ag or Rh, that depress the maximum temperature of the miscibility gap in the Pd. The presence of lithium seems to help as well.

His data indicated it is helpful to delay the deliberate addition of poisons (promotors) such as Al, Si, or B until after the loading is well into the beta phase. The geometry of the cathode/anode relationship is also important, as it influences the uniformity of the current density upon the palladium surface. Sharp edges and corners should be avoided. He showed that current density uniformity is necessary to achieve high loading. Cravens also briefly alluded to recent recognition of the influence of small amounts of RF power and a magnetic field in triggering excess heat events (first reported by Dennis Letts).

It became evident as he spoke that he had shown data relating to several matters others had been aware of, but had been keeping secret. Fleischmann said as much, and this may also be true for the SRI activity.

It is now clear that a number of groups know at least some important factors that determine whether excess heat appears during this type of experiment. The main point is that one must be sure that full loading is achieved, followed by a sharp jump into the hyperloading regime.

Several quandaries remain, however. One is the reason for the common observation of a long incubation time. Another is the source of the large difference in the behavior of different batches of palladium, even from the same supplier. The source of this materials effect is being studied in a number of laboratories. One negative indicator is the formation of internal cracks and voids, which prevents full loading, and often occurs.

Heat generation in electrochemical cells containing nickel and light water

Bush (Cal Poly, Pomona) described measurements in a closed system calorimeter that indicated excess heat generation at relatively low input power values. The ratio of excess thermal power to input power was higher the lower the input power, reaching values of 700 or so. They use a fine fibrous nickel (or a sintered Ni-Cu) cathode, and various alkali metal carbonates in the electrolyte.

When using potassium carbonate, Bush claims to have observed the gradual appearance of calcium in the electrolyte (so did Notoya at ICCF-3). When using rubidium carbonate, he observes the formation of strontium, which seems to have an isotope ratio different from the natural one, reducing the probability that this is an impurity effect.

Srinivasan, who reported a number of Ni/H experiments at ICCF-3, is now on a six-month visit at SRI International, where he has repeated experiments previously performed in the BARC laboratory in India. These involved the use of chemically open cells, and he is preparing to do them again in closed cells at SRI.

His results are quite convincing. Experiments in this area are evidently easier to reproduce than the high power Pd/D experiments, and he has done a significant number. His results indicate the magnitude of the excess thermal power generated is essentially independent of the input power. This is consistent with Bush's result that the ratio of output to input powers is highest at the lowest input levels. Taken to the extreme, this would imply an infinite ratio at zero input power.

An interesting aspect of the Srinivasan experiments was his observation that the presence of a stainless steel component in his cells seems to kill this effect—whatever it is. He suspects the presence of chromium is responsible.

Srinivasan reported they have used six types of nickel, and observed these excess heat effects in 14 out of 28 experiments not having stainless steel present. They also found indications of the generation of modest amounts of tritium in some cases. It seems there is no appreciable delay time in these experiments. The maximum value of excess thermal power measured is about 1 Watt.

Criddle (University of Ottawa) also discussed some Ni/H experiments in which he claimed to have observed excess heat evolution. While he gave most of his attention to the influence of silica dissolved from glass containers upon the behavior of the electrodes, it was interesting he found a large difference in the behavior of "soft" nickel and "hard nickel." The latter gave indications of significant excess heat evolution, whereas the former did not. This indicates there is an influence of composition and/or microstructure in the Ni/H experiments as well.

Liaw (University of Hawaii) reported they have repeated their earlier LiCl-KCl-LiD molten salt experiments on the Pd/D system using nickel and lithium hydride, rather than palladium and lithium deuteride. According to this report, they also seem to have found significant amounts of excess heat generation during thermal excursions in this case at about 400°C.

Several people are now convinced that the experimental work of Mills, who first claimed the generation of excess heat in light water experiments, is sound. Thermacore, Inc. (Lancaster, Pennsylvania) has reportedly reproduced his thermal experiments. Except for **Vigier** (Univ. of Paris), no one seems to pay much attention to his theoretical model, which attributes the excess heat observations to the formation of a new form of hydrogen, not a nuclear process.

Heat and ^{4}He from ultrasonic cavitation-induced hyperloading

George and Stringham (E-Quest Sciences, Palo Alto) presented another method to provide local hyperloading in the Pd/D system. It involves the use of ultrasonic-induced cavitation in heavy water on a palladium surface. Bubbles are made to adiabatically collapse in a 20 kHz acoustic field such that very high energy microjets of deuterium are injected into the metal surface.

By properly controlling the experimental conditions, acoustic energy arriving at the surface at a power level of 3 W/cm^2 gave rise to a large amount of excess heat-enough to cause melting of several grams of palladium. Calorimetric measurements were reported to indicate the generation of about 90 W of excess heat for periods of several days. This excess heat effect did not occur when the D_2O was replaced by H_2O, or the palladium was replaced by stainless steel.

Large amounts of ^{4}He were found in the heavy water, roughly corresponding to about 20% of the excess heat. Since the ^{4}He levels in the reactor gases were on the order of 65 ppm, which is many times greater than the normal amount in air (5.7 ppm), there was no possibility this observation was due to an air leak. The ^{4}He levels were evidently confirmed by two laboratories.

A third observation claimed that ^{114}Cd was found near the surface of the palladium after the experiment, whereas there was none before.

Efforts are underway to scale up these ultrasonic experiments into the range of several hundred watts. The authors said they would sell copies of their apparatus to a limited number of interested parties, and would warrant that they would produce both excess power and ^{4}He.

Heat From High Pressure Spark Experiments

Dufour et al. (Shell Research and CNAM, Paris) reported on the work at Shell, and what has been repeated and extended at CNAM, Lab. des Sciences Nucleaires in Paris. In these experiments, sparks are passed into metal electrodes through gases containing hydrogen isotopes at atmospheric pressure. The earlier work using this method was described in a paper in the September 1993 issue of *Fusion Technology*. Shell apparently has applied for two patents on the results.

The basic experiment concept is to impose a high local transient electric current (field) in the near-surface region of a metal containing hydrogen isotopes. This transient condition is generated by causing sparks to pass through an atmospheric pressure gas containing hydrogen isotopes. Stable excess thermal power production of about two Watts was reported to have been obtained over long time periods (48-1,000 hours), whereas this was not true for calibration experiments or when using other gases.

In a group of experiments with palladium and deuterium, the total power input in these experiments was about 29 watts, whereas the power input into the spark reactor itself was about eight watts. Thus, the excess power was approximately one quarter of the power put into the reactor, but less than 10 percent of the total power input in this non-optimized experimental setup.

Experiments were also undertaken with stainless steel instead of palladium, and hydrogen instead of deuterium that also apparently indicated the generation of excess heat. This material-independence raises questions, of course.

Measurements of the change of the gas pressure were used to demonstrate that the hydrogen isotopes were actually caused to enter the metal surface by the sparking. These species could be recovered by a degassing treatment at elevated temperature.

Essentially, no tritium or neutrons were observed in these experiments. On the other hand, it was reported that blackening of photographic film was observed with the experiments that showed generation of excess heat, but not with the ones that didn't blacken. This radiation persisted long after the sparking stopped, and was attributed to the generation of electrons with energies about 50 keV.

Sparking was caused to occur at a frequency of 310 Hz, with some five sparks per cycle, and a given spark lasted for about one microsecond. When breakdown occurs, an intense electric current flows on a very local scale, and a current of about 2 A passes through an area of approximately 10^{-7} cm^2. This produces an usually high local field in the metal of about 120 V/cm.

The spark canal is filled with the atomic hydrogen isotope, and this penetrates the surface of the metal, temporarily resulting in a very high localized hydrogen isotope/metal loading ratio.

A reaction model has been proposed that involves three-body collisions in the near-surface region of the metal and is triggered by the high local field. Participants include an electron, a hydrogen isotope, and a third nucleus, and an indirect transition in which a virtual neutron participates. This model provides a possible explanation for the fact that the observed effects were found with both hydrogen and deuterium, as well as predicting the formation of both ^4He and electrons.

A modification of the experimental reactor was described which makes it act like an ozonizer. In this case, the metal electrodes are separated by either one or two dielectric barriers. The discharge is different in this case, for it involves a large number of very small sparks, rather than one large high current spark. When only one dielectric barrier is used, the spark impinges upon the metal, while this does not happen in the presence of the two dielectric layers.

Researchers observed about 2 watts excess power with the single dielectric layer, so that the spark contacted a palladium electrode. With the second layer, where the spark did not reach the palladium electrode, no excess heat was found. This further reinforced the conclusion that the excess heat effect originates in the metal, not the gas.

This second type of experiment is easier to scale up, and the authors are evidently expecting to move into the range of 100 W excess heat soon.

Plasma (glow discharge) experiments

Kucherov (ENECO, Salt Lake City), who was formerly at the Luch laboratory in Podolsk near Moscow, reported on the many glow discharge experiments that had been performed using palladium targets in dilute deuterium gas.

These well-instrumented experiments have evidently produced a wide range of unexpected phenomena, including excess heat (in 50% of the experiments), neutron fluxes up to 107 n/s, gamma radiation that persisted for days after the power was turned off, charged particles, and both hard and soft x-rays. There are also indications that transmutation reactions occur, as new elements were found to be present after the experiments.

Many details were published in *Physics Letters A* in 1992. ENECO is now funding an effort to try to reproduce these experiments in the Department of Electrical Engineering of the Massachusetts Institute of Technology.

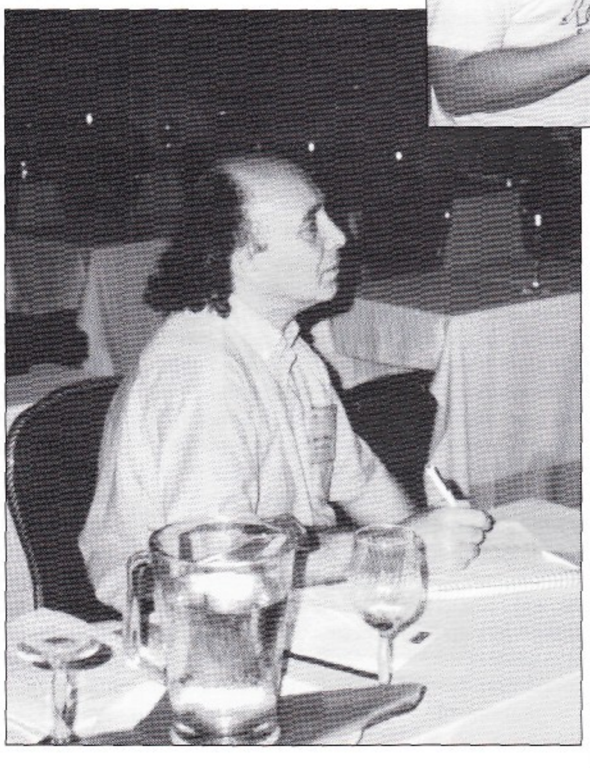
Less comprehensive, but similar experiments were reported by **Kennel** (Space Exploration Assoc., Cedarville, OH). His major conclusion was that gamma radiation suddenly began to appear when deuterium was added to the gas in the system, but that this was not the case upon the addition of hydrogen.

Tritium formation in the Pd/D system

Tuggle, who is in the Claytor group at Los Alamos National Laboratory, reported on the recent extension of their experiments, in which hyperloading is produced in gas-loaded palladium wires and pressed powders by pulsed resistive heating of material previously gas loaded. Their earlier experiments involved passing current through stacks comprised of alternating palladium and silicon sheets.

These new wire experiments have produced a factor of 100 more tritium for the same amount of palladium, reaching rates over five nCi/h (nano Curies per hour). The tritium generation rate is current-dependent, and the results are strongly dependent upon the palladium's impurity content. Samples with over 400 ppm total substitutional impurities produce essentially no tritium. Sulfur has been found to be an especially strong killer. Tuggle has evidence that the tritium is formed inside the solid palladium, rather than on the surface.

Mark Hugo (Excelsior, Minn.) (L.) describes at a poster session his basement experiments.



Dr. Jean-Paul Bibérian, a pioneer in solid-state cold fusion.

Will (EPRI, Palo Alto) presented his very careful study of electrolytic tritium generation in the Pd/D system, recently published (*J. Electroanal. Chem.* 360, 161, 30 November 1993). An important feature of this work was the clear demonstration of the electrochemical parameters' influence on both the rate of loading and the final loading value. As with excess heat observations, full loading (>0.8) was necessary to observe tritium formation. Researchers used a gas volume loading measurement method and an acid electrolyte.

When using a soft cycle loading technique, they obtained full reproducibility (16 times) with material from one supplier, but absolutely no tritium from material from two others. Thus, there is also an unknown "materials effect" in this case as well. Light water experiments conducted in parallel never showed any tritium generation.

Analysis of the palladium samples as well as the electrolyte and gas phases showed that the tritium was formed inside the solid, and that it was not uniformly distributed, appearing primarily where the local electrolytic current density, and thus the loading, was the greatest. Observed tritium levels were reproducibly more than 50 times background concentrations. Analysis of a large number (150) of samples clearly showed that the observed tritium did not come from contamination of the initial materials.

As has been repeatedly found by others, the amount of tritium generated in such experiments is much less than that necessary to correspond with the amount of excess heat found in other (higher input power) experiments.

Generation of excess heat in solid state experiments

There were three reports of the generation of excess heat during elevated temperature electrochemical experiments on proton (deuterium)-conducting solid electrolytes. Two of these, from Russia (**Samgin**, et al. from

Prof. Steven Jones (Brigham Young University) poses challenging questions.

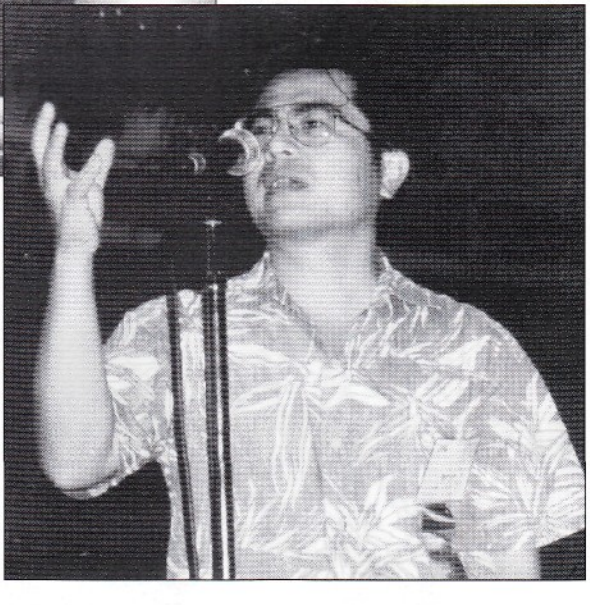
the Institute of High-Temperature Electrochemistry in Ekaterinburg) and France (**Bibérian** from Marseilles), were presented as posters and were difficult to understand.

The Russian work followed the approach presented in their talk at the Third International Conference on Cold Fusion on the behavior of modified tungsten bronzes, and involved the use of a doped cerate proton-conducting oxide. Researchers claimed the observation of both neutrons and heat generation when conditions were changed. They believe the presence of layers in the solid with different conductivity types is important.

The Bibérian poster dealt with the use of $LaLaO_3$, also known to be a proton-conducting solid electrolyte. Upon application of DC voltages, regions of both n-type and p-type conduction form on the two sides of the solid electrolyte. Although the poster discussed excess heat generation, it was not clear what had actually been measured.

The third was by **Mizuno** (Hokkaido University) who imposed low frequency high voltage (tens of volts) AC across solid state cells with doped $SrCeO_3$-type perovskite electrolytes (0.1 cm thick) and porous platinum paste electrodes. Experiments were conducted in the temperature range 300-500°C in various gases. These materials are known to be proton (and thus deuterium ion) conductors, so that such experiments produce large transient local loading in the solid electrolyte near the electrolyte/electrode interfaces.

They found excess heat generation in the order of 100 W/cm^2, but only in experiments with these materials in deuterium gas, and not when other (aluminum silicate) ceramics were used that are not proton conductors. They also reported observation of tritium generation, but didn't give further details.



Prof. Bor Yann Liaw (University of Hawaii at Manoa)

In one example experiment at 410°C, the magnitude of the observed excess thermal power (50 W) was very much greater than the input AC power (18 V and 40 microamperes, or 7 x 10^{-4} W) for a number of hours. This is a very large ratio. The overall heat balance must, of course, include the power input to attain the experimental temperature.

Re-evaluation of the early experiments

Three papers, by Swartz (Jet Technology, Weston,MA), Melich (Naval Postgraduate School, Monterey), and Hansen (Utah State University), were presented in which some of the early experiments reporting negative results of excess heat experiments were re-evaluated.

In the hectic period following Fleischmann's and Pons' announcements, efforts were undertaken in a number of laboratories to try to duplicate their results. In several highly visible cases, these experiments' results were negative. In light of current knowledge in this area, several people are interested to know why those laboratories apparently obtained negative results. Harwell experiment data have been carefully evaluated, and it is now claimed that they did, indeed, observe excess heat in two cases, which were hidden by the method of data analysis.

In the second case, precision of the measurements was such that no reasonable conclusions could have been drawn. None of the experiments was conducted long enough to have exceeded the now well-established incubation time. It was also clearly demonstrated that data in another of the highly visible efforts were altered before publication.

Influence of the imposition of RF energy and the presence of a magnetic field

Letts (Energy Research Group, Austin, Texas) offered a poster presentation indicating an apparent effect of both RF energy (at a specific frequency) and a magnetic field in triggering thermal effects in the Pd/D system. These experiments, although described in some detail, for the moment should only be considered as preliminary. They may be indications of something interesting, and other laboratories (including ENECO, Cravens, and Bockris) reportedly are following them up.

Points in connection with the "Materials Effect"

Papers by **Oriani** (University of Minnesota, Minneapolis) and this author's pointed out the great influence of both point defects and extended defects, as well as other microstructural and nanostructural features upon the behavior of interstitial species, such as hydrogen or deuterium, in metals. These matters are well-known in other areas of materials science and engineering, and take them into consideration and interpretation in experiments in this area. It is well established that hydrogen isotopes preferentially reside at dislocations and grain boundaries in metals. They also preferentially enter and exit the solid locally, at the intersections of these structural features with the surface. Interstitial species move rapidly along these defects, and are also swept along by their translational motion.

Experiments in which metals have been pre-loaded with tritium have been used to demonstrate both the preferential location of hydrogen isotopes at dislocations and grain boundaries, and that they are carried to the solid's surface by dislocations during plastic deformation. Local concentrations can be very different from the average composition. Thus, it is unrealistic to think of interstitial hydrogen isotopes in palladium or nickel as though they are in a structurally homogenerous medium of uniform composition, residing only in normal interstitial lattice positions, and that their transport processes obey Fick's laws for diffusion in a structurally-uniform concentration field.

It would be foolish to continue to consider the whole matter to be related to experimental mistakes, as has been done in some quarters.

Interstitial loading and deloading, especially if this involves formation and propagation of new phases, as is the case for palladium at room temperature, results in the generation of large and inhomogeneous stresses. Phase transitions and dislocation motion are influenced by such local stresses, and are often characterized by delay times and sporadic behavior. It should not be surprising that similar delay times and "burst" characteristics are also found in solid state "cold fusion" experiments.

Two possible dislocation models that could produce transient local hyperloading were described in my presentation. One involves the sudden breakaway of a dislocation from its equilibrium solute atmosphere, leaving the excess interstitial behind. This produces a region of perfect lattice with an extremely high local supersaturation. Another possibility involves the intersection of dislocations and their solute atmospheres moving on different slip planes. When this happens, the superposition of their high local solute concentrations also produces a region in the lattice with a very large local supersaturation.

The role of lithium

Lithium's role in the electrolytic experiments is still not clear. The in-diffusion of lithium has been observed by a number of investigators, and its kinetics seem to correspond roughly to the magnitude of the sample-dependent incubation times that are generally observed in the Pd/D system when the palladium does not already contain lithium. The only experiments of this general type that did not show an appreciable incubation time were those (Stanford and Naval Ocean Systems Center) in which lithium was already present in the palladium.

On the other hand, lithium does not seem to play a role in a number of other types of Pd/D experiments that evidently show excess heat production, such as the cavitation-induced microfusion, atmospheric pressure spark loading, solid state electrolytic hyperloading, and the plasma experiments.

This implies that lithium may well play a secondary role, influencing some other parameter, rather than being involved in the primary phenomenon itself. Because of the

Hal Fox (Fusion Information Center, Salt Lake City) chairs the Special Topics session.

large "materials effect," which seems to be related to either the presence or absence of minor compositional constituents—most probably interstitials—or to the microstructure, or perhaps to the interaction of the two, it seems reasonable to suggest that the role of lithium involves its influence upon these.

Closing thoughts

Although there wasn't a great sense of excitement and controversy, ICCF-4 proved to be an interesting conference. A number of things now seem to be well-established, based upon repeated and believable experiments. These include the following:

Excess thermal power *and* energy can be obtained from electrolytic experiments in the Pd/D system. The magnitude, as well as the appearance, of this effect depends upon a number of experimental parameters, some of which are not known. If the proper conditions are met, this can be a large and easily measurable effect.

There is a "materials effect" that prevents the appearance of excess heat in some batches of palladium. The origin of this is not known, but it seems to be related to the presence of minor, probably interstitial, impurities.

The observations of small amounts of excess thermal power in electrolytic cells with nickel cathodes and light water carbonate electrolytes also seem to be real, although the experimental conditions are very different from those necessary for the Pd/D electrolysis system.

Excess thermal power can also be obtained in a number of other types of experiments in which hyperloading of hydrogen isotopes is caused to occur inside a solid. These include the high voltage-induced hyperloading in solid electrolytes, the ultrasonic cavitation loading experiments, the spark loading experiments, and the glow discharge experiments. It may well be that one or another of these will turn out to be more useful in practical applications than the Fleischmann-Pons electrolytic type of experiment.

Products have now been seen in many experiments that can only be explained by some kind of nuclear reaction. These include the multiple and well-substantiated observations of the generation of tritium inside palladium. A number of laboratories have now observed the formation of ^4He, and there are increasing indications of the appearance of transmutation products. It is possible that transmutation reactions will explain the effects that have been seen in a number of laboratories involving hydrogen, rather than deuterium, and that tritium formation should be considered in this category. Radiation and various other products are seen in several types of experiments, sometimes in large and indisputable amounts.

Whatever is happening in the solid state in these many experiments is definitely different from the nuclear reactions that occur in hot plasma systems. It also seems that a variety of different, but probably related, phenomena occur under different conditions.

Cold fusion is now a very active area of science worldwide (although with notable exceptions), and a number of organizations are considering ways in which some of the experimental observations might be put to practical use. It would be foolish to continue to consider the whole matter to be related to experimental mistakes, as has been done in some quarters.

Robert A. Huggings is a Professor of Materials Science at Stanford University. He may be contacted at the Center for Solar Energy and Hydrogen Research, Helmholtzstr. 8, 89081 Ulm, Germany.

Patience, patience…

A very "unscientific" and personal take on other cold fusion effects

With special thanks to the skeptics by Edmund K. Storms

I will try to describe the field of cold fusion in five stages. We are now in the transition between Stages 3 and 4.

Stage 1 started when Professors Stanley Pons and Martin Fleischmann partly jumped, and were partly pushed, into the colosseum of public awareness with their claim for low energy nuclear fusion. Sadly, the lions were hungry. Fortunately for Prof. Jones, his claim for a similar but smaller effect was not as tasty to the skeptics.

Pons' and Fleischmann's claim caused every scientist, who was lucky enough to have an imagination and access to suitable equipment, to attempt a duplication of the results. From the smoke and confusion there arose several positive results. These were very carefully examined—and rejected. A few negative results were reported by several well-known laboratories. These were given no examination and were accepted.

> **I would like to suggest an award for those people who most successfully keep the world in the dark. This award would be called the 'Flying Pig Award' in memory of past comments about how cold fusion would be proven real when pigs fly.**

Thus began the double standard that has plagued the field ever since. It is germane to point out that recent analysis shows that one of these negative studies could not possibly have produced positive results. One other accepted negative study apparently actually produced small positive results. This stage of unrestrained enthusiasm ended with publication of the ERAB report, a very incomplete and harmful document showing only a minor amount of objectivity.

The battle was joined

With the advent of Stage 2, the field went underground and became defensive. Work continued at isolated laboratories by people whose special circumstances made them immune to the growing negative attitude. Much of this work wasted time trying to prove the reality of the effect rather than understanding its nature. Frank Close made the case for the skeptics and the message was carried into the lion's den by Douglas Morrison.

Eugene Mallove wrote a clear account of the positive results, and several scientific reviews also supported the reality of the effect. Thus the battle was joined. During this time, the dedicated skeptics actually performed a service to the field. They encouraged better work, and forced an appreciation of the issues. Gradually, the work continued with the support of a few courageous institutions, EPRI being a major contributor to the sparse studies in the U.S.

Stage 3 began when Japan and several other countries initiated major research programs, government and privately sponsored. Significant efforts are under way in India, Italy, Russia, and recently in China. More than 1,000 papers are available in the field—many peer reviewed and many showing positive results. This growing work resulted in improved methods and new ways to initiate the effect.

Presently, more than eight different environments have been found to produce the phenomenon, some completely reproducible. This fact alone should cause some pause on the part of skeptics. In addition, evidence for several different types of nuclear reaction is accumulating. Indeed, some of the results are still too amazing for even people in the field to believe.

Skepticism's new meaning?

As new evidence accumulated, the contribution being made by skeptics changed. In general, they failed to keep up with the field and continued to complain about irrelevant issues. Two recent books by Professor John R. Huizenga and Gary Taubes failed completely to present a balanced view. And to make matters worse, the press has not been much help in presenting the facts. As a result, important issues are not receiving the necessary attention, and unnecessary confusion and even misinformation is being spread. The cold fusion field deserves better treatment. Active skeptics and journalists who distort the facts should consider how they will be viewed should this field eventually be accepted.

On the other hand, by spreading doubt and confusion these skeptics have allowed a few of us to achieve intellectual and financial advantages that would not have been possible had major institutions been in the field.

As a result, many people will be in a very good position to profit when Stage 4 starts. For this reason, some skeptics should be thanked. Therefore, I would like to suggest an award for those people who most successfully keep the world in the dark. This award would be called the "Flying Pig Award" in memory of past comments about how cold fusion would be proven real when pigs fly. Nominees are being accepted.

Stage 3 is now gradually changing to Stage 4, the stage in which the U.S. government and major companies will realize that the phenomenon is real—and vitally important. These new converts will look around for someone who knows how to do competent work in the cold fusion arena—finding few people available. Those of us in the field can, in fact, expect to be awash in money and attention. Only patience is needed at the present time to realize this reward.

Stage 5? This world-changing moment will come when a working device is found on the shelves of a Japanese equivalent of Wal-Mart. This stage is still in the future.

Dr. Storms obtained a Ph.D. in radiochemistry from Washington University (St. Louis) and has recently retired from the Los Alamos National Laboratory after 34 years of service. His work was in basic research in the field of high temperature chemistry applied to materials in nuclear power reactors. His recent studies of the "cold fusion" phenomenon resulted in four publications plus a complete and objective scientific review of the field.

AN ITALIAN COLD F

Late breaking news: An apparent startling new method to generate excess power from hydrogen and metals

Physics professor Francesco Piantelli of the University of Siena, Italy had relegated Pons' and Fleischmann's and others' claims about "cold fusion" to the proverbial "dust bin of history." He just didn't believe them. Then in late 1989, by sheer accident, he glimpsed an aspect of the cold fusion "Genie" others hadn't seen, and since then he hasn't let go of the phenomenon.

The method of generating excess energy that he and his associates have discovered is simplicity itself. It is powerful, reproducible, and without the many problems of electrochemistry—*if* the results hold up. The excess energy released in their small reactors, they say, are "at least three orders of magnitude"—a thousand times— beyond any possible chemical explanation.

Piantelli and his colleagues, physics professors Sergio Focardi and Roberto Habel have just published a paper in the Italian physics journal *Il Nuovo Cimento* (February 1994) about work, which if substantiated by others, may soon revolutionize all of cold fusion. It could make excess energy much easier to generate than heretofore possible.

[Since the brief scientific paper had just gone to press shortly before this magazine went to press, only preliminary information was available for this magazine. *"Cold Fusion"* will follow up on the story with further details in its next issue as more facts emerge. Editor Eugene Mallove attended a seminar at the University of Siena on February 14, 1994, at which this pioneering research was presented to a group of about 40 scientists.]

Serendipity personified

In December 1989, serendipity struck at the University of Siena, which is an ancient and beautiful city. Above the stone streets of Siena in a laboratory that specializes in biomedical applications of physics, Professor Piantelli was trying to measure the charge on an organic molecule called a "ganglioside." He was working at an ultra-cold, cryogenic temperature more than 70 degrees C below zero (near 200 degrees K), and magnetic fields were involved in the apparatus employed.

The sample of biological material had been tagged with deuterium, the doubly heavy non-radioactive isotope of hydrogen. Unexpectedly, the cooling apparatus was having difficulty maintaining the low temperature necessary to carry out the measurement. It seemed that there was a mysterious source of heat production coming from the sample—heat that Piantelli could not account for in any way.

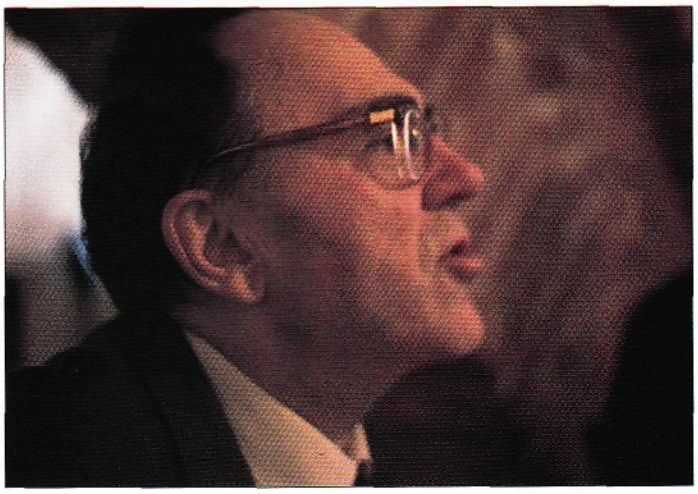
The organic sample was resting on a piece of nickel, an element whose crystal structure bears some resemblance to that of palladium, and which has figured prominently in cold fusion experiments in ordinary water. [Nickel is element 28 and palladium is element 46, but they both are in the same column in the Periodic Table of Elements.] A non-observant scientist might have dismissed the apparent heat generation and a possible link to claims of excess heat associated with palladium-heavy water cells. But Piantelli and his colleagues Focardi and Habel, who soon joined him in the scientific detective work, were up to the task. In the back of Piantelli's mind was another mysterious electrical anomaly he had seen back in 1966 in hydrogen-loaded palladium, but which he had not then explained.

The Italian group, which until its 1994 disclosure had been unknown to mainline cold fusion researchers, worked part-time on this excess heat problem after 1989. Their goal was to design an experiment that would demonstrate on a larger scale and in another way the heat anomaly they had seen in late 1989. By the end of 1992, Piantelli, Focardi, and Habel had their equipment ready. Their first major success in producing excess heat was achieved apparently in the spring of 1993—a few tens of watts excess power.

Simplicity in action

Their present apparatus is simple, indeed. It is described in the *Il Nuovo Cimento* article, "Anomalous Heat Production in Ni-H Systems," by Sergio Focardi, Dept. of Physics, Bologna University and INFN Bologna; Roberto Habel, Physics Institute, Faculty of Medicine, Cagliari University and INFN Cagliari; and Francesco Piantelli, Department of Physics, Siena University, IMO Siena, and INFN Siena. [IMO stands for International Center for Biophysics and Biochemistry of Molecules and Organisms; INFN stands for National Institute for Nuclear Physics].

The one-sentence abstract: "Evidence for a 50 watt anomalous heat production in a hydrogen loaded nickel rod is reported." The three-page article references only the famous 1989 paper by Pons and Fleischmann in the *Journal of Electroanalytical Chemistry*.



E. MALLOVE

Professor Sergio Focardi at 14 February 1994 lecture at the University of Siena.

A cylindrical stainless steel chamber 50 mm diameter and 100 mm long houses a rod made of nickel, 5 mm diameter, 90 mm long. The nickel rod is enclosed by a cylindrical ceramic spindle (20 mm diameter), around which are wound 42 turns of platinum (Pt) wire (1.0 mm diameter wire). The Pt wire is a resistance heater that is

Fusion Hot Potato

By Eugene F. Mallove

fed by a voltage-stabilized power supply. The heater's purpose is to bring the nickel rod to a temperature over a few hundred degrees C.

This reactor chamber is connected through a valving system to a high-performance vacuum pump and (alternately) to a bottle of ordinary hydrogen gas, H_2, and to a bottle of deuterium gas, D_2. The team has developed specific protocols for loading the hydrogen into nickel by alternately evacuating and then pressurizing the reactor (always to below one atmosphere) with either hydrogen or deuterium. They found clear evidence for this loading of the nickel with hydrogen, because the chamber pressure drops with time slightly below its initial value. Keeping the temperature elevated is the key requirement for loading.

This work has the initial appearance of a great breakthrough in 'cold fusion.' If it turns out to be truly a reproducible experiment that can be duplicated by others, it has major implications.

The group carried out very accurate calibration of the apparatus by putting in various heater powers up to a few hundred watts and observing the stable temperatures reached at the surface of the Ni and the Pt. Upon triggering the effect, presumably with a gas pressure pulse and/or magnetic field, the nickel temperature rises rapidly tens of degrees C. [The triggering method is temporarily incompletely disclosed (for patent considerations), but the known precondition is temperature above 173°C and sub-atmospheric pressure in the gas.]

Then the input power falls, the temperature of the nickel drops back, and it is found that the original high temperature (over 400°C is possible) can be maintained with much less input power—clear evidence of a heat source operating within or at the surface of the nickel. The highest excess power produced, they claim, was 57 watts excess for 20 days, but the group has observed 37 watts excess for as long as 100 days. The example cited in the short journal article is 44 watts for 24 days. These excess power levels for those extended periods of time imply an energy source within the reactor that releases many thousands of times the energy of any conceivable chemical reaction between the hydrogen and nickel. Bear in mind, there was no sign that these reactions were weakening; it is possible they would have persisted much longer.

The group has hypothesized that the "cold fusion" nuclear reaction involved might be between H and D—even when ordinary hydrogen gas is being used. Hydrogen gas, of course, has naturally-occurring deuterium in it. However, the group has not presented evidence to prove that, say, helium-3 nuclear "ash" has been found. Test results for helium were awaited. The group has looked for neutrons and gamma rays—at least for safety purposes—and finds no evidence of radiation.

The paper offers seeming proof that at all input powers to the platinum (Pt) heater, it remains several degrees hotter than the nickel (Ni) sample *before* the triggering of the reaction. The evidence looks solid that the Pt is on the order of 10°C cooler than the Ni *after* triggering the reaction. This seems to substantiate that the heat source is within the nickel rod or at its surface. The excess power in the tens of watts range is roughly constant no matter what the input heater power, provided the temperature of the Ni is kept hot enough after triggering. The phenomenon appears to be surface area-dependent, leading to the expectation that it can be readily scaled up.

The upper limits to this reaction may be even higher. One day in late 1993 at about 4 a.m., while the reactor was being monitored by the computer data collection system, a nickel rod undergoing an excess heat production test suddenly elevated in temperature hundreds of degrees and destroyed the attached temperature probes. The nickel sublimed partially—evaporated!—and blackened the white ceramic holder. [Note: The melting point of Ni is 1453°C.] This apparent "run-on" reaction lasted six-hours, the last few hours of which were spent by Piantelli and others trying to quench the reaction—even after the heater was (presumably) shut off.

Cold fusion breakthrough?

This work has the initial appearance of a great breakthrough in "cold fusion." If it turns out to be truly a reproducible experiment that can be duplicated by others, it has major implications. These are some of them:

* Easy-to-produce large magnitude excess heat from nickel surfaces in contact with gas, hydrogen or deuterium. Inexpensive materials, works with hydrogen or deuterium gas and non-precious metals. Easy scale-up.
* Uses a completely dry, non-electrolysis process.
* Is completely reproducible and apparently never fails to start up.
* Triggering of excess power production is nearly instantaneous.
* Works at temperatures of hundreds of degrees C, leading to possibly excellent thermodynamic efficiency in technological applications. No known upper limit to temperature, other than the melting point of the metal.
* The reaction continues for months, with no apparent decrease in the reaction intensity during months of operation. The reaction has reportedly *never* stopped by itself.
* It seems nearly certain that the reaction will self-sustain (i.e require no input heater electrical power), if the system were engineered to maintain adequate temperature in the nickel sample through self-heating.

The present experiments are scientific calorimetry trials that do not attempt to self-sustain. The six-hour temperature excursion re-

The ideal cold fusion demonstration

By Bill Collis

More than five years have gone by since the dramatic announcement by Fleischmann and Pons that unexpected heat can be generated in palladium charged with heavy hydrogen (deuterium) by electrolysis. Since then, many groups have confirmed excess heat, that is to say, more heat is produced than can be explained by the energy input after taking account of any possible chemical reactions. But there have been few public demonstrations of cold fusion devices, and these few have not clearly validated the technology. As a result, many of the world's scientists remain politely skeptical of "cold fusion."

So what demonstration would be ideal? That, of course, depends on with whom you talk. The public wants cheap, safe, versatile, and reliable energy. Let us see what these factors imply. We cannot afford to use expensive noble metals like palladium in energy devices. We should prefer a fuel which is nearly free (like tap water) rather than rare hydrogen isotopes. (Actually heavy water costs about one-thousandth of the cost of other fuels on an energy basis.) It goes without saying that any radioactivity must be negligible and the unit must use non-poisonous materials. Hot caustic electrolytes should be avoided in any household device, as should any system containing liquids operating near boiling point or under pressure.

Our cold fusion reactor must be reliable, capable of being turned on or off at will, and must be reproducible and predictable. Unexpected overheating should automatically reduce power output, and there should be no sudden increase of pressure. The system must be intrinsically safe.

A versatile energy producing system should be capable of simple scale up to industrial dimensions. While it should produce low temperature energy for heating purposes, it should be capable of high temperature operation for producing useful work, such as turning an electric generator. For example, operation at say 700°C should be adequate to drive a turbine using air as the working fluid.

The skeptical scientist will be aware of all the potential pitfalls of calorimetry before proposing his ideal demonstration. We will be told that in many experiments the excess heat has been small and sporadic, that proper account was not taken of heat production when hydrogen is absorbed by metals, or recombined with oxygen in the air. To avoid any discussion as to whether or not any heat produced is truly in excess or not, let us impose as a condition for our demonstration that there should be no energy input at all. Thus, all and any heat produced must be "in excess."

We are fortunate that this is a *demonstration*, not *a scientific experiment*. We do not need to measure the heat output accurately because we shall require the power output to be so obvious that even non-experts can appreciate it. The complications of calorimetry are unnecessary. In fact, excessive instrumentation will actually be detrimental to the demonstration. Simple demonstrations are always the most impressive.

We shall present our skeptic friend with a simple isolated cold fusion device. There should be no external wires, not even for instrumentation, so there can be no question that any electrical power is being inadvertently supplied. Once turned on, it should become and remain hot.

Now it is quite possible to conceal inside our device some chemicals or a battery which slowly release heat. If our device is small enough this energy will be limited. Let us suppose that our device is the size of a household lamp bulb and too hot to touch. We can therefore estimate that it is producing some tens of watts of heat, perhaps much more. We might be able to hide a concentrated source of energy, say 70 g of lithium metal inside. On burning to the oxide this could liberate 3 MJ (mega Joules) of energy.

Don't try this at home

I don't recommend trying this experiment as lithium burns vigorously, producing clouds of caustic smoke. Three MJ is a powerful punch when liberated in a short time. But even just 100 watts over 24 hours is equivalent to 8.6 MJ. I dare say there are other ways of concentrating even more energy than using lithium, but even the most powerful chemical reactions are going to liberate energy of this order of magnitude. So if we can sustain this kind of heat production for days, we will have eliminated all possible chemical, electrical, and mechanical causes—even if there were a deliberate attempt at fraud.

Simple demonstrations are always the most impressive

So far the demonstration will have shown an extraordinary heat production of unexplained origin. By process of elimination then it looks like a nuclear reaction. To prove this, we should look for nuclear ash, the products of a nuclear reaction. Alpha, beta, gamma rays, neutrons, tritium, and X-rays are not very suitable ash for our purposes. Penetrating radiations corresponding to measurable heat output would probably kill the experimenters nearby! Although these nuclear signatures have been detected in small quantities, they are clearly not part of any significant heat producing reaction.

We want a safe demonstration, and we know that cold fusion does not produce radiation as its main product. For the sake of argument, let us assume that non-poisonous helium is the product. There is growing evidence this is, in fact, the case. Ordinary helium, ^4He, is present in the air at a concentration of about 5 ppm. Regardless of the precise reaction, we expect to produce about 0.5 ml of helium for every 100 MJ of nuclear energy. One hundred MJ or so has been claimed as excess heat in electrolysis experiments. Unfortunately, so much deuterium is evolved that the expected helium concentration in the evolved gases is considerably less than 1 ppm.

Obviously, this makes contamination by helium from the air a real possibility and makes detection difficult. To avoid this problem we need a demonstration that uses a limited quantity of fuel so that any helium produced is not excessively diluted. If about 500 ml total hydrogen is used then 100 MJ worth of helium will represent 1000 ppm, easily detectable.

Is this ideal demonstration just a dream, an exercise in wishful thinking? That is the $64 trillion dollar question. Wait and see.

Bill Collis graduated in biochemistry from Balliol College, Oxford in 1976. Since then, he has pursued the career as a freelance consultant in information technology, and has lived in Italy for the last 15 years. He has followed cold fusion since 1989.

...ed to have occurred late in 1993 is more than an indication that the reaction can self-sustain.

The group is reported to be working actively with (unspecified) industrial support to produce a demonstration device(s) as soon as possible. It has long since applied for patent protection and promises to release further details on its process—perhaps within a few months—when it has received proper clearance. The group appears to be aware that powerful demonstration units are required to eliminate any possible doubt about the reality of this phenomenon.

> 'I found the whole thing very convincing and very beautiful.'
>
> —*Professor Giuliano Preparata of the University of Milano, February 14, 1994*

Scientists at the February 14th presentation seemed pleased with what they heard. Dr. Francesco Celani, a cold fusion experimentalist from the Frascati National Laboratory, mentioned a few concerns about how input power was being measured. Others thought these would not affect the basic result. Cold fusion theorist Professor Giuliano Preparata of the University of Milano, ended the seminar with praise for the work: "I found the whole thing very convincing and very beautiful."

Monument in a tranquil atrium at the University of Siena.

! Impossible & Preposterous

"I was convinced for a while it was absolute fraud. Now I've softened. They [Pons and Fleischmann] probably believed in what they were doing. But how they represented it was a clear violation of how science should be done."

Dr. Richard D. Petrasso, MIT hot fusion scientist, New York Times, *Sunday, March 17, 1991, p. 1.*

* * *

Petrasso said he thought there was a "one in a trillion" chance that the claims of cold fusion are correct.

Dr. Petrasso quoted in Boston Globe, *April 17, 1992, article by David Chandler.*

"If you buy the excess heat measurements then you have to invent some kind of nuclear process to explain them. I just haven't bought into the heat claims yet. I think there is a subtle mistake . . . if it's a mistake, it's a very interesting mistake."

Petrasso quoted in Popular Science, *August 1993, article by Jerry Bishop.*

* * *

"Inept scientists whose reputations would be tarnished, greedy administrators who had involved their institutions, gullible politicians who had squandered the taxpayers' dollars, lazy journalists who had accepted every press release at face value— all now had an interest in making it appear that the issue had not been settled. Their easy corruption was one of the most chilling aspects of this sad comedy.

"To be sure, there are true believers among cold-fusion acolytes, just as there are sincere scientists who believe in psychokinesis, flying saucers, creationism, and the Chicago Cubs. The lesson from "Too Hot to Handle" by Frank Close is that a Ph.D. in science is not an inoculation against foolishness—or mendacity."

Dr. Robert Park, Professor of physics, University of Maryland, and Director of the Washington office of The American Physical Society, quoted in The Washington Post, *May 15, 1991.*

* * *

"Cold fusion, the all-but-utterly discredited notion that once promised to create cheap nuclear power in a jar of water, made a tentative bid for credibility yesterday as two groups of researchers separately announced new explanations for the controversial phenomenon."

Reporter Curt Suplee, The Washington Post, *April 26, 1991.*

* * *

"What they [Pons and Fleischmann] had was nothing. Yet they started an avalanche that swept up scientists all over the world, mesmerized gullible backers from the Utah state legislature to the Electric Power Research Institute, and wasted tens of millions of dollars of seemingly not-so-scarce research funds. Even now the rubble has not completely ceased to jitter."

Nicholas Wade, science editor of The New York Times, *in* Nature, *August 5, 1993, in a review of Gary Taubes' "Bad Science".*

* * *

"Do we regard this [cold fusion] fiasco with detached resignation, or express more strongly our dissatisfaction with the deceptions, exaggerations, and ethically disoriented presentations that stimulate vast diversion of international resources? If science does not ensure that its house is in order, who will?"

Dr. Frank Close, Theoretical Physics, Rutherford Appleton Laboratory, Chilton, UK, in American Scientist, *January-February 1993.*

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A Cold Fusion Primer

Cold fusion, 1994: What's it all about?

BY EUGENE F. MALLOVE AND JED ROTHWELL

What happened to cold fusion, the "miracle or mistake," announced at the University of Utah by Drs. Martin Fleischmann and Stanley Pons in March 1989? It would not be surprising if you thought that cold fusion were "dead," because, unfortunately, the scientific establishment, the hot fusion community, and many in the news media have ignored or maligned cold fusion research.

But cold fusion is far from dead. It is alive not only in dozens of laboratories in the United States, but in numerous foreign research centers, particularly in Japan.

Here are the basic facts about cold fusion as they stand in early 1994. For continuing monthly coverage of this rapidly expanding field, consider subscribing to this magazine, which every month will provide information unobtainable elsewhere, plus summaries of what is being reported worldwide in technical journals.

Hot fusion versus cold fusion

Hot fusion is the kind of nuclear reaction that powers the Sun and the stars. At temperatures of millions of degrees, the nuclei of hydrogen atoms can overcome their natural tendency to repel one another and join or fuse to form helium nuclei. This releases enormous energy, according to Einstein's famous $E=mc^2$ formula—the mass being lost in the reaction being converted to energy. Fusion is the opposite of fission, which is the release of energy by splitting heavy uranium or plutonium nuclei.

Scientists the world over have spent more than four decades and billions of dollars (an estimated $15 billion in the U.S. alone) to investigate the possibility of mimicking with devices here on Earth the fusion reactions of the stars. These are complex and large machines that rely on high magnetic fields or powerful lasers to compress and heat fusion fuel—typically the isotopes of hydrogen, deuterium and tritium.

The controlled hot fusion program has made enormous strides, but all agree that the earliest possible time when practical hot fusion devices may be available is about three decades away. Hot fusion is a very tough engineering problem. Many engineers—even those favorable to hot fusion—suggest that the "tokamak" reactor approach being followed by the U.S. Department of Energy will never result in commercially viable technology.

The U.S. hot fusioneers and their international collaborators now want to build a big, complex test reactor called ITER (International Thermonuclear Experimental Reactor), which might begin to operate in 2005. A commercial hot fusion power plant would not be on-line until at least 2040. The annual budget for hot fusion research in the U.S. regularly exceeds $500 million, and they now seek increased funding for ITER.

Mind you, the hot fusion program has never produced a *single watt* of power beyond the electric power that was put into each experiment. Occasionally, such as in December 1993 at the Princeton Plasma Physics Laboratory, "breakthroughs" in hot fusion are announced in which the power of hot fusion reactions reaches a record level, but the level has always been *below* the electric power put in.

You can't pinch it, but it's real

"Cold fusion" is a real but still incompletely explained energy-producing phenomenon, that occurs when ordinary hydrogen and the special form of hydrogen called deuterium are brought together with metals, such as palladium, titanium, and nickel. Usually, some triggering mechanism, such as electricity or acoustic energy, is required to provoke the "cold fusion" effects. Both ordinary hydrogen and deuterium are abundant in ordinary water—whether fresh water, ocean water, ice, or snow—so the process will likely end many of the world's energy concerns, if it can be developed commercially. Now, this seems all but certain. (The deuterium form of hydrogen is present naturally as one out of every 7,000 hydrogen atoms and is easy to separate.)

Cold fusion releases enormous quantities of energy in the form of heat, not radiation, as in hot fusion. This heat energy is hundreds to thousands of times what ordinary chemical reactions could possibly yield. If "cold fusion" is a heretofore unknown form of benign nuclear reaction—as most researchers in the cold fusion field believe—there is more potential cold fusion energy in a cubic mile of sea water than in all of the oil reserves on earth. Whatever the explanation—nuclear reactions, exotic "superchemistry" perhaps requiring some modifications to quantum mechanics—or something even more bizarre (such as tapping of the zero-point energy of space at the atomic level), cold fusion seems destined to become a dominant source of energy.

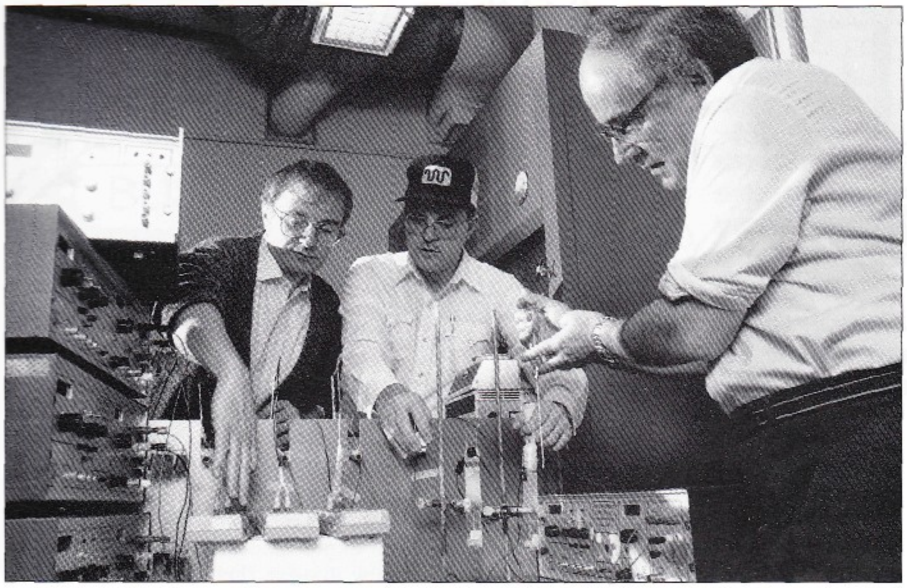
Cold fusion, in contrast to hot fusion, occurs in relatively simple apparatus, albeit not yet without some difficulties. Cold fusion reactions are not at all like conventional hot fusion reactions. If they were, cold fusion experimenters would have been killed by massive flows of radiation—neutrons and gamma rays. The continuing wonder of cold fusion is that it is apparently a very clean reaction that gives very little of the radiation common to fission and fusion reactions. In cold fusion experiments, low-level neutrons, tritium, helium-4, and isotope shifts of metal elements have been seen.

Cold fusion researchers have attempted to find theoretical models to explain the observed cold fusion effects—the large thermal energy releases, the low-level nuclear phenomena, and the absence of massive, harmful radiation, and other conventional nuclear effects. There is yet no single, generally accepted theory that explains all these phenomena. There is no doubt, however, that the phenomena exist and will eventually be explained—most likely in the next few years.

The cold fusion evidence

The most important evidence for cold fusion is the excess heat energy that comes from special electrochemical cells—much more heat coming out than electrical energy being fed in. Competent and careful researchers have now confirmed that under the proper conditions it is possible to obtain excess power output beyond input power anywhere from 10% beyond input to *many thousands of times the input power!* In fact, in experiments reported at the Fourth International Conference on Cold Fusion (December 1993), one researcher, Dr. T. Mizuno of Hokkaido University, reported an output/input power ratio of 70,000! Sometimes this power comes out in bursts, but it has also appeared continuously in some experiments for hundreds of hours, and in some cases even for many months. When this power is added up to give kilowatt-hours, the inescapable conclusion is that much more energy is being released than any possible chemical reaction (as we ordinarily understand such reactions) could yield.

And there is more. Neutrons, tritium, energetic charged particles, and other ionizing



(L. to R.): Prof. Stanley Pons, graduate student Marvin Hawkins, and Prof. Martin Fleischmann in University of Utah laboratory in 1989.

radiations have been detected in a variety of cold fusion experiments. In the past few years, there has also emerged a startling body of experimental evidence that elements have been *transmuted* in cold fusion experiments. Several laboratories have found helium-4, for example, and low levels of radioactive metal atoms. Isotopes of silver and rhodium have appeared in palladium electrodes from cold fusion cells where no such atoms existed before the experiments began. Moreover, many of these experiments differ significantly from one another in their approach and conditions.

So, there is no chance that the various laboratories are all making the same systematic errors in all these experiments. These nuclear effects are clearly the hallmark of nuclear processes of heretofore unknown character. By itself, this nuclear evidence points to an entirely new realm of phenomena of staggering scientific importance. The excess energy in some of these experiments is virtual proof that something very extraordinary and of enormous potential technological significance has been discovered.

In the early days of cold fusion research, when scientists were struggling and learning how to replicate the effect, there were many poorly done experiments, and many mistakes. In the weeks following the 1989 announcement by Drs. Martin Fleischmann and Stanley Pons at the University of Utah, large numbers of scientists tried to replicate the phenomenon, and failed—or *thought* they had failed. They actually might have obtained positive results, but for various reasons falsely interpreted and improperly reported their data.

The experiment is considerably more complicated and difficult to perform than originally reported in some scientific and popular news journals. Many scientists became disillusioned with the field after the initial "boom and bust," but a smaller number of determined scientists dug in and continued to work on the problem. Some of them continued, day in and day out, and finally achieved success. Soon after the discovery was announced, in the National University system of Japan, a low-key, long-term program was established, involving over 100 scientists in 40 institutions. The program was coordinated by Dr. Hideo Ikegami of the National Institute of Fusion Science in Nagoya.

Another long-term, well-financed program was sponsored by the U.S. Electric Power Research Institute (described below). These programs have gradually yielded a solid body of carefully replicated experimental evidence. Many of the experiments performed during the last five years produced so much heat, and used such accurate and sensitive instruments, that the results from them are certain. It is revealing that the only people saying that these experiments must all be in error either have never done cold fusion experiments themselves or have left the field of cold fusion experimentation, following their early and hastily-drawn conclusion that "cold fusion" was impossible.

Major research organizations

Several hundred laboratories around the world have obtained positive cold fusion results. A partial list, which appeared in "Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor," in 1991 is already outdated. In the spring of 1991, a conference in the former Soviet Union revealed many more positive results; at the Second Annual Conference on Cold Fusion held in Como, Italy, in July 1991, much more positive evidence for cold fusion emerged. At the Third International Conference on Cold Fusion in October 1992, the evidence became overwhelming. At the Fourth International Con-

Excess heat: Count the ways

Many other methods of obtaining excess energy have been added to the roster since Pons' and Fleischmann's breakthrough five years ago. This is the current (and growing) list of apparent "cold fusion" processes giving excess energy:

1. *The original Pons-Fleischmann process*
   Heavy water solution with a current-carrying electrolyte such as lithium deuteroxide (LiOD). Current is passed between a palladium or palladium-alloy cathode and a platinum anode.
2. *Molten salt process*
   High-temperature molten electrolysis process typically involving a lithium chloride (LiCl) and potassium chloride (KCl) molten solution saturated with lithium deuteride (LiD). Electrodes are of palladium and aluminum.
3. *The Randell Mills process*
   Ordinary water solution with (typically) potassium carbonate (K_2CO_3) electrolyte. Electrodes: nickel cathode and platinum or even nickel anode.
4. *Deuterium gas discharge process*
   Low voltage electrical discharge onto various metals through a deuterium gas atmosphere—ordinary hydrogen gas too!
5. *Ultrasonic activation*
   Using ultrasonic frequencies, acoustic energy bombards palladium metal submerged in heavy water, producing excess energy and helium-4.
6. *Ceramic proton conductors*
   Certain ceramic materials related to high-temperature superconductors (such as strontium-cerium-oxide and aluminum-lanthanum-oxide), when very low current is passed through them in a deuterium gas atmosphere, give significant excess energy.
7. *Magnetic field and radio frequency stimulation*
   Magnetic fields and radio-frequency stimulation have now been proved to enhance the excess energy from other cold fusion processes, e.g. electrochemical cold fusion cells.
8. *Turbulent activation*
   An aluminum cylinder with a geometric hole pattern on its periphery rotates with close tolerances within a steel casing. Ordinary water is pumped through the interface and flashes to steam. The Hydrosonic Pump (of Hydro Dynamics, Inc., Cartersville, Georgia) has now

ference on Cold Fusion (Maui, December 1993), the field blossomed in many new directions: new methods of generating excess power, and new observations—especially *the apparent transmutation of heavy elements at low-energy*. Research facilities in the U.S. and elsewhere in the world reporting important cold fusion results include:

- Electric Power Research Institute (EPRI)/SRI International
- Los Alamos National Laboratory
- Oak Ridge National Laboratory
- Naval Weapons Center at China Lake
- Naval Research Laboratory
- Naval Ocean Systems Center
- Texas A&M University
- ENECO, Salt Lake City
- Hokkaido National University
- Osaka National University
- National Institute for Fusion Science, Nagoya
- Tokyo Institute of Technology
- Bhabha Atomic Research Centre, Bombay, India
- Technova Corporation
- IMRA Corporation
- NTT (Nippon Telephone and Telegraph company)
- And many other private research laboratories in the U.S. and abroad.

Major financial support for cold fusion research comes from these sources:

The Ministry of Education, Government of Japan. Research is coordinated through Japan's National Institute for Fusion Science, in Nagoya, and conducted in National University Laboratories. The Ministry of Education annually spends $15 to $20 million on cold fusion. In the Autumn of 1991, the Ministry of International Trade and Industry organized a research consortium of 10 major Japanese corporations to advance research in cold fusion. Prior to this, only the Ministry of Education was involved in this research. This consortium is called "The New Hydrogen Energy Panel" (NHEP). In the spring of 1992, as the activities of the Panel became widely known, Japanese newspapers reported that five other major Japanese corporations asked to be included.

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Balanced scientific evaluations and reference material

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Besides *"Cold Fusion" Magazine*, published monthly, which is the world's first magazine devoted exclusively to cold fusion R&D and investment, there are several newsletters, newspapers, and popular magazines now covering cold fusion regularly, or from time-to-time, including *The Wall Street Journal*, *Business Week*, *Cold Fusion Times* newsletter, *Fusion Facts* newsletter, *21st Century Science and Technology*.

Information is also available from *"Cold Fusion"* Magazine Contributing Editor, Jed Rothwell, who co-founded Cold Fusion Research Advocates:

Jed Rothwell
Cold Fusion Research Advocates
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Suite 313
Chamblee, Georgia 30341
Phone: 404-451-9890; Fax: 404-458-2404

The question of reproducibility

Cold fusion effects have not always been easy to reproduce, but that does not make them any less real. The difficulties with reproducibility, however, are rapidly disappearing as researchers discover the conditions required to provoke the phenomena, such as sufficient deuterium loading of met-ference on Cold Fusion (Maui, December 1993), the field blossomed in many new directions: new methods of generating excess power, and new observations—especially *the apparent transmutation of heavy elements at low-energy*. Research facilities in the U.S. and elsewhere in the world reporting important cold fusion results include:

- Electric Power Research Institute (EPRI)/SRI International
- Los Alamos National Laboratory
- Oak Ridge National Laboratory
- Naval Weapons Center at China Lake
- Naval Research Laboratory
- Naval Ocean Systems Center
- Texas A&M University
- ENECO, Salt Lake City
- Hokkaido National University
- Osaka National University
- National Institute for Fusion Science, Nagoya
- Tokyo Institute of Technology
- Bhabha Atomic Research Centre, Bombay, India
- Technova Corporation
- IMRA Corporation
- NTT (Nippon Telephone and Telegraph company)
- And many other private research laboratories in the U.S. and abroad.

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The question of reproducibility

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Critics of cold fusion research have regularly dismissed positive results simply because the effects have not always been repeatable. Of course, there are many natural phenomena that are highly erratic, not repeatable, and definitely not predictable, such as meteorite falls, lightning strikes, earthquakes, and the elusive "ball lightning." There are also a host of modern technical devices that will not function if subtle, sometimes poorly understood composition parameters are askew; semiconductor electronic devices are good examples of this. It is not so surprising that the exotic cold fusion phenomena are subject to similar difficulties.

Negative results not necessarily negative

It is shocking but true. In the case of three major research groups that had supposedly negative results in the spring and summer of 1989—Caltech, the Harwell Laboratory in England, and MIT—there now appear to be significant questions about their work which the scientific community at-large has not addressed. Three scientists have found simple algebraic errors in the Caltech work, which invalidate the paper's negative conclusions. These scientists wrote many times to *Nature* magazine, but *Nature* refused to publish the corrections. A critique, however, was published in *Fusion Technology*.

In the MIT Plasma Fusion Center case, serious questions have arisen about the methods used to evaluate excess heat results. The unpublished data appear to show indications of excess heat, but the published version does not show these indications. Furthermore, analysis of the methodology employed by this group revealed fatal flaws—even *if* the data had been properly handled. (A technical discussion of the 1989 MIT Plasma Fusion Center cold fusion calorimetry appeared in *Fusion Facts*, August, 1992.)

In the case of the widely-touted and supposedly completely "negative" Harwell Laboratory (U.K.) calorimetry results, independent analysis of that laboratory's raw data show evidence of excess heat production. Details of the Harwell Laboratory problems have been published in both the Third and Fourth International Conference on Cold Fusion *Proceedings*.

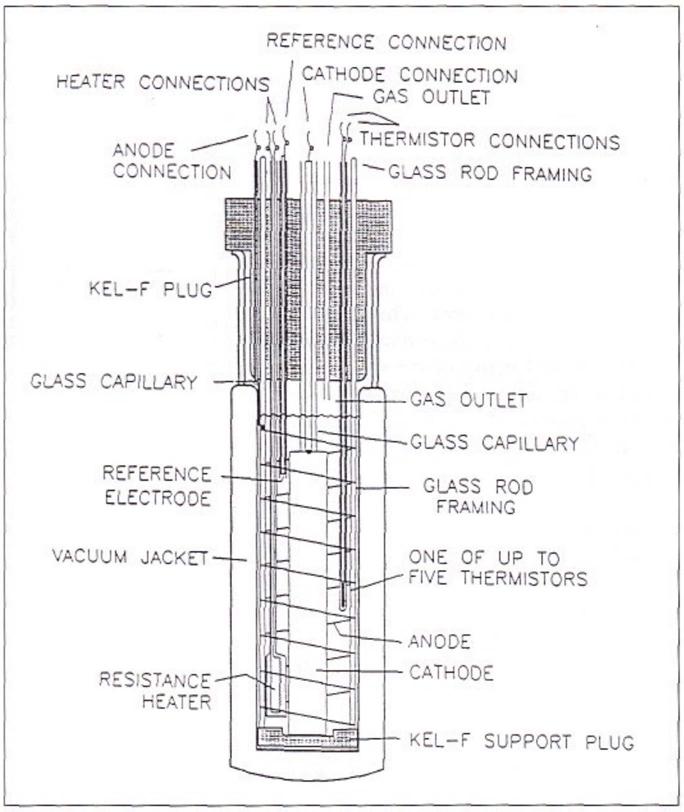
Theories of cold fusion

When conventional (low temperature) superconductivity was discovered accidentally in 1911, there was no physical theory that could explain it, nor was there any such theory for about the next half century. The much discussed high-temperature superconductivity, which appeared in 1986-1987, still has no satisfactory theory to account for it, yet industries and governments are bent on developing and commercializing it.

The same should be true for cold fusion. However, because cold fusion seems to be an even more radical departure from conventional physics wisdom than high temperature superconductivity, and because of the past reproducibility problems of cold fusion, the latter has not been accepted as readily as high-temperature superconductivity.

Cold fusion does not operate like hot fusion. That has been clear from the start. It must have some other explanation.

Happily, several scientists have proposed theories to explain cold fusion. Each of these theories might explain all or aspects of this astounding new physical phenomenon. Cold fusion theorists include physics Nobel laureate Julian Schwinger, Peter Hagelstein of MIT, Robert Bush of California Polytechnic Institute (Pomona), Scott and Talbott Chubb of the U.S. Naval Research Laboratory, Akito Takahashi of Osaka National University, Giuliano Preparata of the University of Milano hot fusion expert Frederick Mayer, Randell Mills of Hydrocatalysis Power Corporation (Lancaster, Pennsylvania), and many others.



Cross-sectional view of an early Fleischmann-Pons-type cold fusion cell. It is set up for heat-measurement as a dewar calorimeter with a glass vacuum jacket integral to the cell structure.

Notable cold fusion conferences

- First Annual Conference on Cold Fusion, Salt Lake City, March 1990.
- Anomalous Nuclear Effects in Deuterium/Solid Systems, Provo, Utah, October 1990.
- Conference on Cold Fusion under the auspices of the Soviet Academy of Sciences, March 1991.
- Second Annual Conference on Cold Fusion, Como, Italy, June-July, 1991.
- Japan Nuclear Energy Conference, cold fusion seminar, October 15-18, 1991, at Kyushu National University, Engineering Department, Fukuoka City, Japan. Part of an annual conference sponsored by the Atomic Energy Society of Japan.
- The ISEM conference on January 27, 1992. Principal sponsors were Nagoya University, the JSME, and the IEEE.
- The Third International Conference on Cold Fusion, October 21 25, 1992, in Nagoya, Japan. Principal sponsors were the Physical Society of Japan, the Japan Society of Applied Physics, Atomic Energy Society of Japan, The Institute of Electrical Engineers of Japan, The Chemical Society of Japan, The Electrochemical Society of Japan, and the Japan Society of Plasma Science and Nuclear Fusion Research.
- The Fourth International Conference on Cold Fusion, December 6-9, 1993, Maui, Hawaii, sponsored by the Electric Power Research Institute (Palo Alto, CA).
- The Fifth International Conference on Cold Fusion will be held in Nice, France in April 1995.
- The Sixth International Conference on Cold Fusion will be in Beijing, China in mid-1996.

The Future: Too good to be true?

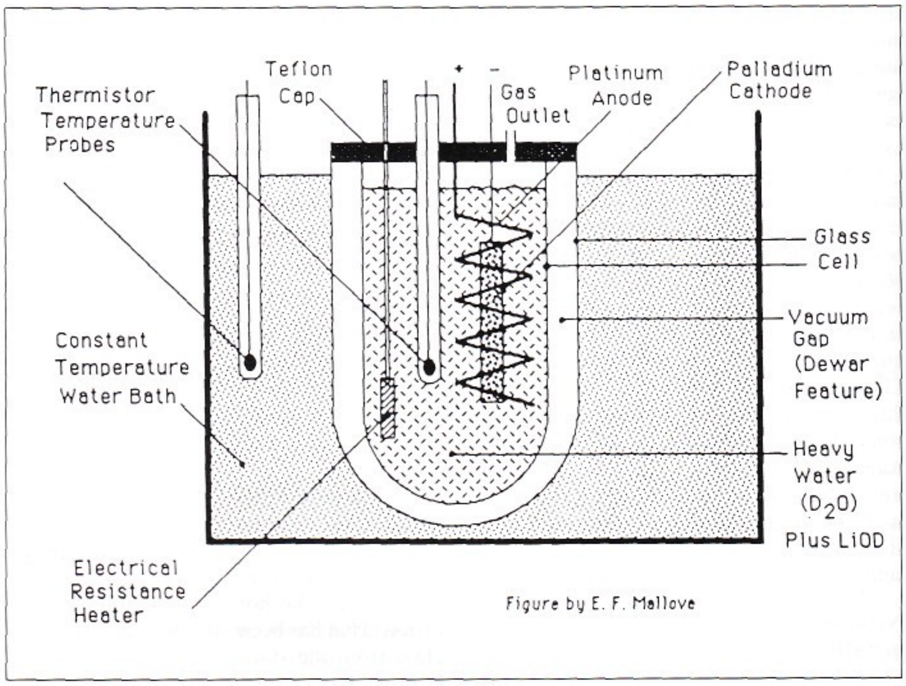
Cold fusion research is not "Big Science." It does not need massive installations, just relatively small-scale dedicated work at national laboratories, universities, and in private industries, which are already beginning to enter the field in the U.S.

Cold fusion does, however, require the talents of top scientists and engineers, combined with sophisticated analytical instrumentation. Federal laboratories, floundering in search of a new mission, are well-equipped to support cold fusion research. Cold fusion research could well become a major mission for scientists at these laboratories. Cold fusion energy development, however, will dominantly be the territory for private industry. There is no need for massive government investment. But government must smooth the path for private efforts.

Is it really possible that a revolutionary energy technology has been inappropriately cast aside in the U.S.? That is exactly what has happened, as scientific and engineering developments will show. This need not be true any longer. For the economic and environmental well-being of the nation and the world, every citizen must become aware of the facts about cold fusion, and help encourage funding for American research.

Probably the most difficult hurdle in trying to come to terms with cold fusion is that it seems too fantastic scientifically, and "too good to be true" economically and socially. But the same could have been and was said about many other technological revolutions as they began to happen.

Cold fusion will likely revolutionize the world in ways we can barely begin to imagine. We believe that before the year 2000 there will be cold fusion powered automobiles, home heating systems, small compact electrical generating units, and aerospace applications. These technologies will revolutionize the world as they speed the end of the Fossil Fuel Age.



Schematic view of a Fleischmann-Pons-type cell, showing the essential features. (From Fire from Ice, *courtesy John Wiley & Sons.)*

The stakes have never been higher. We should remember the sentiment of the famous scientist, Michael Faraday, in the last century, to whom we owe our revolutionary electrically powered civilization. He wrote, "Nothing is too wonderful to be true."

CALENDAR OF EVENTS

- National Workshop on the Status of Cold Fusion in Italy, University of Rome III, Rome, Italy, February 14-16, 1993
- Russian Conference on Cold Fusion, Abrau Durso, Russia, September 29-October 1, 1993
- Fourth International Conference on Cold Fusion, Maui, Hawaii, Sponsored by the Electric Power Research Institute December 6-9, 1993
- IAP Cold Fusion Seminar Series at MIT January 22, 1994
- "Renew '94," Stamford, CT, A Conference and Trade Show Promoting A Renewable Energy Future, April 11-13, 1994
- International Conference on Cold Fusion, Minsk, Republic of Belarus, May 24-26, 1994
- Fifth International Conference on Cold Fusion, Nice, France, March-April, 1995
- Sixth International Conference on Cold Fusion, Beijing, China, Mid-1996

Load cold and slow, run hot

Dennis Cravens' five years of home-brewed cold fusion

Dennis Cravens looks relaxed in his leather bomber-jacket as he describes his experiments on the frontier of science. He speaks with a Texas drawl as he meticulously describes his five-year climb up the cold fusion "learning curve."

Professor Cravens spends most of his time teaching students chemistry and physics at Vernon Regional Junior College in Vernon, Texas—a small community in north-central Texas near the Oklahoma border. Whatever time is left over from his teaching and family life, he uses to explore cold fusion. Since shortly after Drs. Fleischmann and Pons announced their remarkable discovery to the world on March 23, 1989, Dr. Cravens has been conducting his own cold fusion experiments in his home-laboratory. He has had remarkable success. In fact, he has invented several ways to promote the still-mysterious cold fusion excess heat reaction.

As in the case of many other cold fusion pioneers, the patents for which Dr. Cravens has applied, are still mired in the log-jam at the U.S. Patent Office, which has held up a few hundred patent applications because of the prevailing skepticsm about cold fusion. However, Dr. Cravens is now also affiliated with the new Salt Lake City-based cold fusion corporation, ENECO, which has gathered his patent applications (and those of many other investigators) under its wings. When the log-jam inevitably breaks, the persistence of home-experimenters like Dennis Cravens is likely to pay off.

At the recent Fourth International Conference on Cold Fusion on Maui, Dr. Martin Fleischmann, Fellow of the Royal Society, and one of the foremost electrochemists in the world, paid Dennis the highest compliment. It was after Dennis had finished discussing his methods in a paper entitled simply enough, "Factors Affecting the Success Rate of Heat Generation in CF Cells." Fleischmann exclaimed: "You have learned all our tricks!"

Later, as the conference was ending on December 9, Dr. Fleischmann further complimented Dennis during a panel discussion summing up the proceedings. He suggested that Dennis's paper should be awarded the honor of "best paper" at the conference. (Dennis wasn't there to receive the praise in person, because he had to return home the day before.) Fleischmann said, "We have to compliment Dennis Cravens. This is real science as it should be done—in his garage, on a very limited budget. He has produced an unbelieveble amount of useful information. If you really want this to work, take it to him. He'll put you straight and get you going on the correct route."

What is it about a modest young scientist at a small college in the hinterlands that drove him to perform the kind of creative and painstaking research for which Michael Faraday—one of the great British experimentalists in electricity and magentism—would have been proud? After all, scientists at the prestigious places—MIT , Caltech, Yale, and Princeton—had all finished their Spring '89 cold fusion experiments within a matter of months and dimissed the field as nonsense. The MIT group had actually held a "Wake for Cold Fusion" on June 26, 1989, even before their group had analyzed its data. The smirks of these "big name" places, with research teams that thought they could rush science, spooked the world and gave cold fusion a bad name. Fortunately, this derision didn't stop scientists like Dennis Cravens.

Another professor of chemistry and physics, Dr. John R. Huizenga, of the University of Rochester in New York never lifted a finger to perform a cold fusion experiment. He *knew* it was all preposterous nonsense to begin with. He went on to head the federal panel that rendered a negative report against cold fusion in November, 1989, while numerous researchers, such as Dennis Cravens, were still reporting positive results on excess heat.

Clearly, Dennis's prime asset was his open-mindedness and adventuresome spirit. He engaged a lengthy series of experiments with both the "conventional" Flesichmann and Pons approach us-

Dr. Dennis Cravens in his garage cold fusion laboratory.

and fast!

by Eugene F. Mallove

ing palladium and heavy water. Later, in mid-1991 when Dr. Randell Mills announced success in obtaining excess power with ordinary water cells with nickel electrodes and potassium-carbonate electrolyte, Dr. Cravens pursued those just as vigorously. He determined which factors contributed to demonstrating excess power and which ones prevented the effect from emerging. The accompanying sidebar is a summary of what he found.

The pragmatic approach

Dennis provided *"Cold Fusion"* Magazine with helpful advice for those contemplating cold fusion experiments using palladium and palladium-alloy cathodes in heavy water. These are open cells, in which the electrochemically dissociated deuterium and oxygen from the heavy water is permitted to escape the cell as a gas—as in the original Fleischmann-Pons experiment.

We have to compliment Dennis Cravens. This is real science as it should be done—in his garage, on a very limited budget. He has produced an unbelieveble amount of useful information.

—Dr. Martin Fleischmann

Dennis's cells have been relatively small, with volumes from 2 ml to 30 ml. Most of his work has been with 3 ml cells. He followed a "pragmatic" approach to determine what factors helped to increase the temperature differential between a cell and its coolant bath. Therefore, he does not claim high-accuracy in these experiments, only that the results show general trends in what works and what doesn't work.

The excess power achieved in Cravens' cells ranges up to around 40% excess beyond the input power—referenced to a non-excess heat producing cell. Dennis says that his cells have heat transfer characteristics that produce a temperature difference between cell and its surrounding water bath of about 3°C/watt of input power. This is measured by thermocouples and a resistance heater is used to calibrate. His overall temperature measurement uncertainty is about 0.2°C, which translates to an error level of about 0.6 watt.

Dennis provided *"Cold Fusion"* with the following specific advice to enhance prospects of success in generating anomalous excess heat.

Selecting host lattice materials for electrodes

- Avoid voids.
- Do not use fold-formed Pd (palladium).
- Polish cathode surfaces uniformly.
- Avoid sharp edges and convoluted forms.
- Identify good regions of electrodes by observing bubble formation and surface coloration.
- Use cast as well as cold-rolled materials.

Preventing cracking of the host lattice:

- Use special alloys (10% Ag in Pd, 5% Re or Rh in Pd, 10% Mg in Ni, 15% La in Ni, 5% V and Sn in Ti).
- Load at low current densities (<60 mA/cm^2) until the electrode material is well into the beta phase [of palladium-deuterium crystal structure].
- Delay adding "poisons" (Al, Si, B, Thiourea) until after the beta phase.
- Pre-load by gas at elevated temperatures.
- Add materials such as Li to increase diffusion rates of D.
- Use thin films of Pd on Ag to decrease loading stresses.

Loading the cathode in a uniform manner:

- Select the geometry of the electrode configuration to avoid large variations in electrical fields over the surface of the cathode.
- Assure the uniform surface texture of the cathode by avoiding sharp corners on the cathode and properly constructing the anode.
- Use low current densities with high cell resistance (>5 ohms) during initial loading stages.

Avoiding some kinds of contamination:

- Electrolytically clean the anode material.
- Place anode connections well out of the cell.
- Minimize hygroscopic uptake (of water vapor in the air into heavy water).
- Prevent inclusion of diamagnetic materials at the surface of the cathode.

Initiating reactions:

- Employ dynamic conditions by rapidly changing the temperature (changes of 30°C or heating to 80°C), suddenly increasing the current density (by about 10 times), or exposing the cathode to magnetic field variations (RF at 82 MHz or non-homogeneous magnetic fields).

Factors affecting success rate of heat generation in cold fusion cells

by Dennis Cravens

I have found that the success rate of anomalous heat generation from deuterated metal systems depends on two categories of factors. One set of factors are those that help to achieve high loading of deuterium into the metal lattice. The second group of factors are those that cause *dynamic* changes of the lattice and the flow of deuterium within. These factors seem to help initiate and control the reactions.

It appears that high loading ratios of deuterium to palladium are helpful, if not actually *required*, to achieve anomalous heating events. Although the dynamic conditions discussed below may cause temporary regions of high deuterium ratios in the metal, by and large the most important factor in achieving the heat is to achieve a large over-all deuterium/palladium (D/Pd) ratio. To achieve the large loading ratios, steps should be taken: (a) to select host lattice materials; (b) to prevent cracking of the host lattice; (c) to load the cathode in a uniform manner; and (d) to avoid contamination.

Selecting host lattice materials

The quality of the metal host selected to contain the deuterium is important to achieving anomalous heat. Remember, the goal is to load deuterium (D) into the host cathode at a rate faster than the D can escape. This is equivalent to saying that before trying to blow up a rubber balloon, one should make sure there are no holes in it. This means that conditions that increase the egress of the D should be limited and conditions that increase loading should be enhanced. The proper loading techniques and the limitation of cracking are perhaps the most important factors in reproducing the anomalous heat.

High purity (>99.5%) Pd is recommended. I use Pd from Aldrich or investment-grade Pd from Englehart. It is especially important that it have low levels of platinum (Pt) impurities. The Pd should be visually inspected (or examined by sonic imagery) to assure that it is free from obvious cracks, deformations or Lutter lines. A quick dip in formic acid or an electrolyte (to wet the surface) often helps in visualizing the surface structure of the material.

I determined empirically that bubble patterns on the electrodes are often predictors of excess heat later in the experiment. A "good" piece of metal will initially take up deuterium very readily at low current density; few if any bubbles are seen on the surface. A "bad" sample often exhibits surface bubbles very early in the initial loading stages. It is sometimes disconcerting to see marked differences from samples cut from adjacent regions of the same wire or rod, but that is the nature of palladium.

Readily available materials for heavy water cold fusion experiments. (L. to R.): heavy water, lithium, palladium, and platinum wire.

It is often possible to identify good regions of the Pd by its ability to form gas bubbles under water. I use clear quartz containers (B&L brand spectroscopic tubes) so that I can observe the cell's operation. If the current is momentarily stopped, the deuterium can be seen out-gassing. Uniform and very fine bubbles usually indicate a good piece of Pd. On the other hand, localized regions of large bubbles indicate regions where there are pathways for the D to rapidly deload the lattice.

If you observe such regions, you should try again with another piece of Pd. Subsequent microscopic examination usually shows voids, cracks, or other deformations in such regions. You can eventually come to recognize potentially good pieces of Pd by observing bubble patterns. I developed a quantitative method of pretesting to catch the bubbles in inverted tubes. This allows me to determine the bubble volume as a function of unit cathode length. Also, I can determine which Pd samples are the best to use. The "best" samples will be the last to show bubbles at their surface—perhaps the single best way to predict a sample's future success.

The time of onset of the bubbles is a better early predictor of a sample's performance. A sample which never bubbles until it is loaded to 0.6 to 0.8 D/Pd is a "very good sample." This indicates that the surface is not permitting recombination. If the cathode does not bubble early in the loading process, you are doing it right and you can use Faraday's Law to estimate loading. In an array of similar cathodes loading simultaneously, the last one to bubble is the best.

It is also possible to screen samples by their volume expansion. Using a micrometer, a cathode sample is first measured. Then it is electrochemically loaded into its beta phase ($0.6 < D/Pd < 0.7$). "Good" samples do not increase their volume more than 10%. "Bad" samples exhibit volume changes of 15% or more.

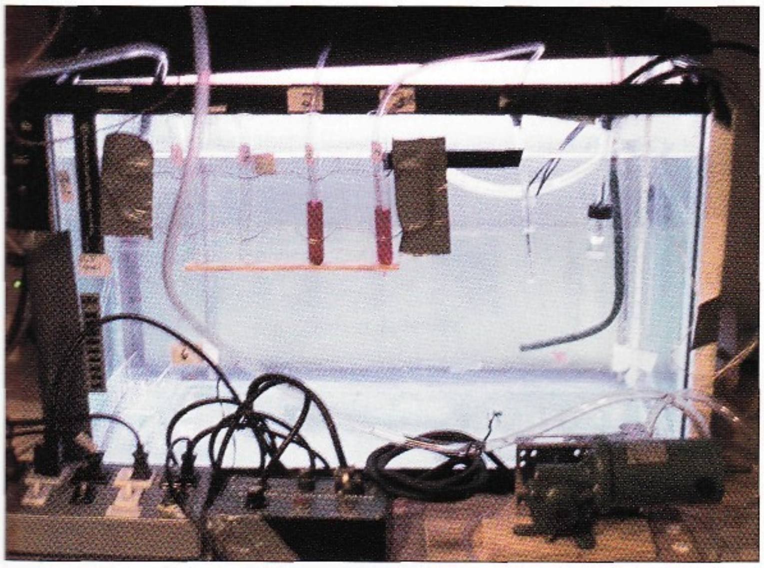
Preventing cracking of the host lattice

Care must also be taken to prevent cracking of the metal host lattice after you have selected a good piece of Pd. A larger piece requires greater care in loading and handling. T he reason is that the Pd lattice expands as it loads. The loading is analogous to heating a piece of glass. Large pieces cannot withstand rapid expansions without cracking. However, smaller pieces can more easily withstand abrupt expansions. Thus, it is best to *slowly load* the Pd to avoid internal stresses which would result in increasing the D pathways out of the Pd. If cracks do develop, it is best to remelt and recast the Pd and start again. It is very rare for Pd once it is cracked ever to be successful in demonstrating the anomalous heat.

The cracking difficulty can also be overcome by use of alloys (such as 20% Ag in Pd, 5% Re or Rh in Pd, 5% V and Sn in Ti, etc). The 10—25% silver alloy of Pd resists cracking on deuterium absorption. However, it does lengthen the loading time due to decreasing the D diffusion rates. Adding Li can shorten loading times. Though difficult to form and costly, the 5% Rh in Pd alloy seems to be best suited for achieving anomalous excess heat. Another tactic is to plate Pd on Ag or another substrate such as Ni or Cu. The advantages are two-fold. First, the plating can be preformed to partially form Pd D. This avoids cracking that results from expansions during rapid loading. Second, it provides better thermal conductivity. (I recommend that the plating be done in an ammoniated solution and the film formed should be at least 5—20 microns thick.)

Pre-annealing (heating) and gas loading at elevated temperature seem to decrease the tendency of the lattice to crack. I often heat the Pd in a quartz tube. The tube and Pd is heated (in air) until just below the softening point (estimated 800—900°C). The idea is to reduce the internal stress of the Pd metal lattice by the annealing. After about two hours, the tube is brought to one atmosphere of pressure of D_2 gas to help partially load the Pd lattice.

As a sample is annealed, impurities such as Pt, Cd, etc. can migrate to the surface. So if you anneal, be sure to grind and polish the surface to remove impurities after the annealing. Otherwise, the sample will not properly take up deuterium. This is because Pt, for example, catalyzes hydrogen recombination with oxygen. The deuterium will simply recombine at the surface and bubbles will be observed early in the loading process. Annealing is not required, but if you do it you must always follow it with polishing and surface preparation.



Aquarium constant-temperature bath with cold fusion cells.

Loading the cathode uniformly

The shape and surface texture of the cathode is also important. The cathode should be shaped to avoid sharp or jagged corners. Such points do not allow proper loading to be achieved. I find slightly better success with cylindrical shapes with rounded bottoms or narrow plates with rounded corners. The idea is to remember to minimize electrical-field (E-field) gradients across the surface of the host lattice. Such variations could lead to unequal loading. The D would tend to diffuse through the Pd and then escape from the lattice from areas with the least surface E-field directed to accelerate the positive deuterium ions into the lattice.

Rough surfaces also tend to limit the loading. In my view, this is one of the greatest sources of confusion in understanding variations in success with different samples. Deep grooves or valleys normally are the regions with the least surface electrical potential (the smallest absolute value of surface potential). I polish the Pd to avoid such difficulties. This is done by first polishing on a buffer wheel. The purpose is to round any burrs, rough areas or sharp corners. The Pd is then finely polished with a series of finer aluminum oxide powders and finally with cerium oxide (Aldrich, optical grade) on a cotton cloth.

Rough surfaces on the anode should also be avoided. Burrs and points seem to be sites of oxidation and corrosion (this has been seen with nickel anodes). I sometimes use an entire "spiral loop" as an anode with both ends outside of the cell. If you have a free end, you may wish to place it in glue (silicon rubber, epoxy, etc.) to prevent corrosion from the point when the cell is run for extended times. The best results seem to come with Pt anodes, but if you use Ni it should be oxidized first (by flame heating or with hydrogen peroxide solution).

It is important that the initial loading of the Pd be done slowly and carefully. The object is to use a low current density (with respect to the cathode surface areas, 30-60 mA/ cm²) so that there will not be unequal expansion and the development of large internal stresses. Do not be tempted to raise the current above 100 mA/cm² until the D/Pd ratio within the lattice is at least above 0.6—0.7. If you use electrical pulsing techniques, do not raise the peak current levels above the 100—200 ma/cm² region until the Pd is loaded to at least D/Pd = 0.65. You can calculate the amp-seconds needed to load the lattice to 0.7 or better. I prefer to load until much longer, or about 150 amp-hours have passed for each cubic centimeter of Pd (with at least one dimension of the Pd less than 1 mm). Be patient! It is better to spend too much time in the initial loading stage than too little. Any sudden application of large currents before the cathode reaches the beta phase is likely to crack the host lattice. This could lead to rendering the Pd useless until it is recast. In some early work, I was unable to observe anomalous heat because of rapid initial loading and then using the cracked and stressed Pd over and over again.

A series of experiments were conducted to see the effect of various initial loading rates on the percent excess power produced (all compared when running at 50°C and 500 mA/cm²).The cathodes that were slowly loaded out-performed those that were initially loaded at a high current density. It should be noted that the rapidly loaded wires had a greater volume expansion.

Additives: It would seem that one role of the additives that have often been used in cold fusion experiments is to increase the so-called over-potential and the internal fugacity within the lattice and to select certain electrochemical pathways. Do not add "poisons" or other additives (Al, thiourea, Si, B, Mo) until you are loaded above 0.6—0.7 D/Pd. This could cause a creeping or spreading of any pre-existing cracks or voids. The expansion of the lattice on absorbing the D may help to seal such voids. Thus, the goal is to first load slowly to allow the lattice to "heal" any deformations. Materials which can increase the internal fugacity should be added after most deformations are self-corrected. A really good host lattice will not swell more than about 1% excess volume on loading.

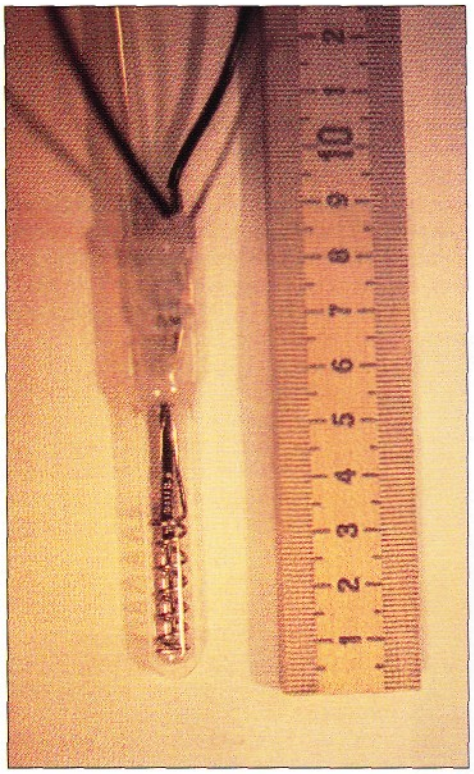
The shape of the anode can also affect the results. The anode should have the proper geometry to load the metal host uniformly. In a spiral-wound Pt anode, use enough turns to supply a uniform E-field at the cathode. This is similar to trying to increase the internal pressure of a balloon; a few points of pressure do not suffice. It takes uniform pressure from all sides.

Avoiding some kinds of contamination

Impurities can prevent excess heat generation. I clean the anode and the electrolyte by running it in a separate system with a dummy electrode. This removes any reactive species and most impurities from the platinum and plates them onto a dummy cathode. It also removes some impurities that may be in the electrolyte. The dummy cathode is then removed and with it any such impurities. If Ni is used as an anode, it should first be oxidized to render its surface passive.

Some experimenters have placed their electrical connections to the anode inside their cells. This can cause materials from the lead to enter the cell and be plated onto the cathode. Over an extended time, the foam and spray within the cell will wet the connections and leads and contaminate the cathode. It is advisable to keep the anode connections well out of the electrolyte and preferably well out of the cell.

On the other hand, the connections to the cathode should be well below the surface of the electrolyte. Remember that the highest loading ratio achieved by your cathode is limited, in part, on the portion of the surface of the cathode which has the least inward deuterium flux. Thus, the metal host lattice should be well under the surface of the electrolyte, and the anode should be positioned to extend above and below a cylindrical cathode. The idea is to have all surfaces on the cathode at approximately the same potential with respect to the incoming deuterium ions. I use platinum for the connection to the Pd. If you use silver or another more active material, be careful not to turn off the current or reverse current for extended periods. This can in some cases lead to oxidation of the lead and the transmission of ions from the connections into the electrolyte.



One of Dr. Cravens' ultra-small cold fusion cells.

Heavy water is hygroscopic and will uptake normal water from atmospheric water vapor. When working with large open water baths you should take steps to avoid the cell taking in normal water via the atmosphere. One approach is to connect a tube to the cell and run it to a U tube with heavy water or oil. The best approach seems to be to keep a positive pressure of deuterium gas in the region above the cell. This limits water uptake and also keeps the electrolytic pathways favoring those which lead to deuterium. I prefer to bubble deuterium in at the bottom of my cell. This complicates the calorimetry but is not too bad if you reach the point of seeing high levels of anomalous heat. This also lengthens the times of usefulness of some additives such as thiourea. It should also be mentioned that a small amount of ordinary water (about 1%) seems to level out heat bursts but that high levels (in excess of 10%) seem to preclude excess heat. Mixtures of 10% H_2O can, in fact, be used as controls.

Note well: Contamination of cells by bismuth and diamagnetic materials seems to be especially detrimental to observing excess heat.

The fun begins

Now that we have examined factors to enhance loading of deuterium, let us turn to factors that help to create the dynamic conditions that seem to be necessary for triggering excess heat.

Only after the initial loading stages should the system be subjected to large dynamic conditions. Subjecting the system to dynamic or non-equilibrium conditions can cause regions of larger deuterium ratios than the global average over the cathode. Such dynamic conditions often initiate or alter the heat generation from within the metal lattice.

Changing the temperature (large rate of heat gain as a function of time), going to a higher current density, imposing an over-pressure (especially by deuterium gas), imposing RF (radio frequency) signals on the cell, imposing a magnetic field in the region of the cathode, or altering the geometry of the cathode (via magnetorestriction) often causes the initiation of greater heat flux from the cathode. [Note well: Thermocouples or mercury thermometers were used for most measurements but they cannot be used for RF experiments. Thermocouples act as diodes at MHz frequencies and give false signals.]

Changes in temperature

The anomalous heat yields seem to be more noticeable at higher temperatures. My greatest sustained heat yields to date have been from cells running near 100°C. These were outfitted with a reflux column so that prolonged boiling could be sustained. (The device is shown in the accompanying diagram). It should be noticed that the condensation is returned off-axis to prevent returning cold and non-conductive water to the active part of the cell—the cathode. After calibration, the cell is run with a constant inlet temperature (40°C). The flow rate is adjusted manually so that the bottom of the reflux condenser stays at a nearly constant temperature (60-80°C). The idea is not to quench the reaction by returning overly cool water, or water devoid of electrolyte, to the region of the metal host lattice. The off-axis return and the elevated temeprature at the return portion of the reflux column are subtle but important features.

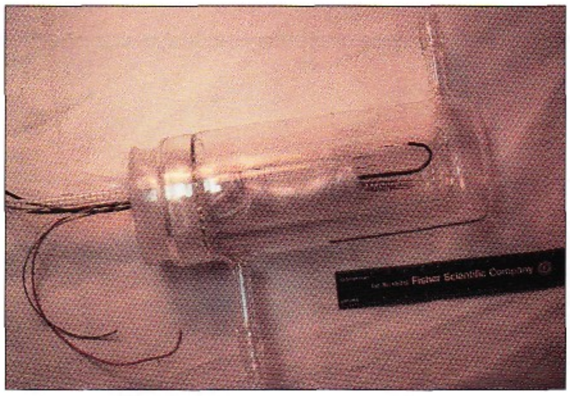
Cold fusion cells prepared for radio frequency stimulation tests.

The calorimeter consists of: (a) the electrolytic cell contained within a dewar flask, (b) a central reflux column containing crushed aluminum oxide for a vapor condensation surface and recombiner material (not shown is a piece of Tygon tubing connected to the top and turning downward to prevent rapid escape of deuterium and to minimize convention currents), (c) an outer water jacket to provide cooling to the reflux column, (d) a series of pieces of insulation, (e) inlet from a constant temperature bath fitted with copper strip packing to prevent laminar flow in the region of temperature sensors, and (f) a condensate return directed away from the cathode. (See Figure 1 next page.) The temperature differential between the inlet and outlet was held relatively constant. The upper inlet temperature was held constant by the bath temperature. The lower outlet temperature was held approximately constant by adjusting the flow rate.

Increases in the current density

Although it is best to load at a low current density with respect to the cathode surface area (30—60 mA/cm^2), the anomalous heat effects are not seen until the current density is raised (usually to at least 200 mA/cm^2 and normally near 500 mA/cm^2). Recall that a low initial loading rate is required to prevent cracking of the lattice and ultimately the escape of the deuterium from the lattice. Deuterium/Pd ratios above 0.8 can usually be reached only when there is a large influx of deuterium during high current densities. There does not seem to be a universal threshold for the current density for all cathodes.

Instead, it appears that there is a range somewhere near 200—700 mA/cm^2 for most "good cathodes." The best cathodes have a tendency to have a lower threshold than do the marginal cathodes. It would seem that the good cathodes leak less (perhaps due to fewer cracks for deuterium to leak out) and can reach higher ratios at lower current densities. It also seems that the initial slow loading is best at lower temperatures and low current densities. However, the heat production should be done at an elevated temperature and at higher current densities.



Water-jacketed cell described in accompanying diagram.

One can make the analogy to filling a bucket having a hole. If the bucket has only a small hole, it can be filled with a small inflow. If the bucket has a large hole or several small holes, it can only be filled with a very large flow. Using higher current densities works, in part, by increasing the inflow above the outflow of the deuterium. Holding the current low during the initial stages works by decreasing the number of "holes" from which the deuterium will leak when one goes to the high current densities after the initial loading.

A few rapid pulses of current may also set up localized regions of high deuterium ratios within the cathode. The use of dynamic currents that are more rapid than the deuterium diffusion times can lead to high D/Pd ratios in some regions of the cathode.

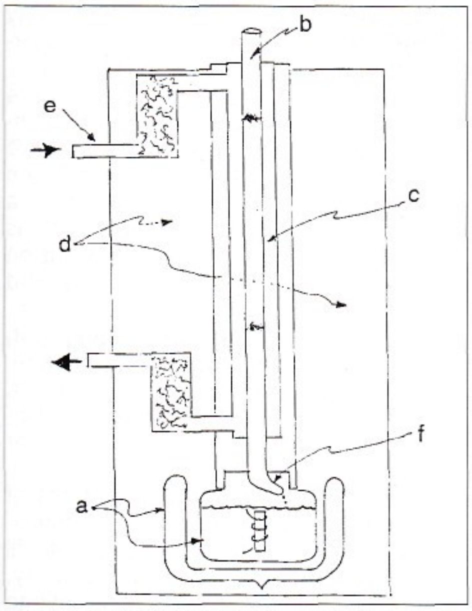
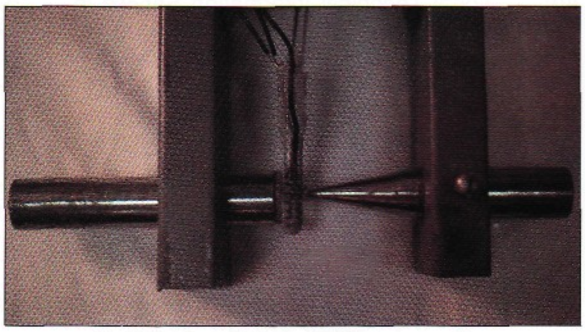


Figure 1. Dr. Cravens' reflux cold fusion cell. (See text description on page 60.)

Pulsing the electrolytic current

Changing the electrolysis current can set up dynamic variations and non-equilibrium ratios of the deuterium within the cathode. One way to do this is to pulse the electrolytic current. The goal is to set up a condition that would cause a crowding of the deuterium within regions of the cathode.

To do this, I simply short out some of my current to ground to cause a pulse. It seems best that the pulsing is done while keeping a DC bias on the current. In other words, the pulses should be added on top of a DC bias. The bias is selected (I use 60 mA/cm²) so that the cathode is not in a reverse-bias condition, but is always the cathode during the pulse. This prevents any de-loading that could occur if it was rendered anodic. For those new to cold fusion work, it is recommended that they first slowly load (30 mA/cm² surface area for 150 amp-hours per cubic centimeter of Pd), then slowly ramp the current to 60 mA/cm² (over two hours), and finally start pulsing between the 60 mA/cm² and 500 mA/cm² levels. The pulses should have a fast rise time and a period between a few seconds to a few hours long. I use a period of 30 minutes and a 50% duty cycle.



Cold fusion cell inserted between poles of a magnet.

It is also possible to pulse the current through 0 volts and then reverse the current direction. If this protocol is used, it is important that the Pd never be de-loaded past the beta phase and into the alpha phase. (The reverse voltage level should be kept low (<0.15V) so that oxides do not form on the surface.) This method may be somewhat better than the above-described pulsing method. However, the DC bias method is recommended until experience is gained in estimating loading ratios from amount of current passed through the cathode.

Imposing a magnetic field on the cathode

The use of magnetic fields to enhance excess heat is a new direction that I have taken, so consider these recommendations preliminary. A magnetic field does seem to alter the heat flux from cells—an inexplicable, but tantalyzing observation. Due to the complexities of calculating the magnetic field in the region of a paramagnetic material (and surface dendrites), it is hard to characterize the magnetic field within a cell. Furthermore, the effects due to magnetic fields may be spurious. Certainly this work needs more replication before instrument error can be ruled out. When a cold fusion cell is placed in a magnetic field which has approximately 2000 gauss (as measured in air between the poles before placement therein of a small cell) it often changes its heat flux. On several separate occasions the author has witnessed a change in heat flux as a cell was placed within a magnetic field (above 200 gauss) or when an electromagnet was switched on. The effect is more pronounced when the magnetic field is non-homogeneous. This was done in a small cell (3 ml) by bringing a magnet near the cell with a pointed iron bar on the opposing side. Since it is a simple thing to try and non-destructive, others are invited to try it on their cells. So far, the effect has only been seen to change the flux on cells that are already exhibiting some degree of anomalous heat.

The imposition of RF fields in the range of 80—84 MHz likewise seem to trigger some anomalous heat reactions. This is done by pulsing (both sine and square waves seem to work) the magnetic field at the cell by wrapping the cell with a coil of wire and connecting it to a RF unit. The excess heat is enhanced by the application of the RF magnetic field. The excess is roughly proportional to the power level of the applied field. The effect was most pronounced when the RF coils were impedance matched at about 81.9 MHz (impedence matching units and Standing Wave meters are recommended) for optimum RF power transfer. This work was done at 200 mW of power and 5—10 turns on the field coils. It is recommended that 1—2 watts of RF power be used to be sure of the effect. Assuming a working cell is used (already at about 30% excess), the effect usually is quickly seen (within seconds or minutes). If the cell is run at above 70 or 80°C, the additional power levels are often large enough to cause rapid boiling. The increases are typically from the initial 30% to the 100—200% range and remain as long as the RF field is on. Often, cells receiving this treatment later seem to "run better" (i.e., greater than the initial 30% excess). Again, this magnetic effect is a preliminary observation, but it is so dramatic that others are encouraged to investigate.

Murphy and Fish

COMPILED BY GENE MALLOVE

We are indebted to Hal Fox of *Fusion Facts* for this bit of wisdom:

Reality is not self-evident.

—One of Murphy's Laws

Corollary:

A fish would be the last to discover water.

Ahoy! U.S. Coast Guard sees cold fusion on the horizon

A federal agency known to specialize in watery activities is on the move. A United States Coast Guard publication, "The Road to 2012: Looking Toward the Next Two Decades," has rediscovered cold fusion. Maybe when a "critical mass" of government agencies has re-discovered cold fusion, the feds will be able to get back on the right track

The document claims to be a "scan of the global environment in which the Coast Guard is likely to operate in the next 20 years." It "emphasizes the critical roles that political, economic, societal, technological, and environmental driving forces have in shaping the future." Produced by a commercial outfit, the report was published under the auspices of:

Strategic Planning Staff (G-CX),
Commandant, United States Coast Guard,
2100 Second Street S.W.
Washington, DC 20593-0001
(202-267-2690/6813-FAX)

The report reads, in part:

"Room-temperature fusion research, effectively dismissed by the American scientific community, is alive and well in Japan and India.

"Cold fusion is the process that researchers believe takes place when an electric current is sent into palladium and platinum electrodes that are immersed in a jar of heavy water, rich in deuterium. The experimental results sometimes suggest that lots of energy is released in the process.

"If it is true, it could lead to the development of a virtually unlimited supply of inexpensive energy.

"The idea was largely depreciated, especially in the U.S., when the experiments of Drs. Pons and Fleischmann, who claimed to have discovered cold fusion, could not be replicated by other researchers.

"But Japanese scientists, intrigued with the concept, invited Pons and Fleischmann to Japan to discuss their work in 1989. Since then, Japanese experimenters have apparently replicated Pons and Fleischmann's findings, producing up to 70 percent more energy in heat than was put into the system in electricity.

"The *Wall Street Journal* reported that five to six labs in the U.S., India, and Japan had conducted experiments that produced as much as three to four times more excess heat than the input power, although researchers are questioning whether it is "cold fusion" they are seeing or another phenomenon.

"Twenty or so university groups are pursuing cold fusion in Japan, most on financial shoestrings. But now, Japan's Ministry of International Trade and Industry (MITI) has decided to fund some of the research.

"If this new effort produces more concrete results it could open a whole new avenue of potential future energy sources and would shift the focus of a good deal of energy research."

Copies for non-government purchasers will be available from the National Technical Information Service in Springfield, VA, (document control number AD-A272448), telephone 703-487-4650. Government agencies (or organizations with contractual relationship with the government) can obtain copies now from the Defense Technical Information Center, 703-274-7633.

A congressman stands up for cold fusion

On February 16, 1993, Congressman Dick Swett of New Hampshire made this statement before the U.S. House of Representatives Science, Space & Technology Committee, Energy Subcommittee hearing, "Investing in Technologies for America's Energy Future":

"I am pleased to see our subcommittee holding a hearing on an issue of such critical importance. Last year's energy bill did much to help move our country in the right direction, but I believe that there is much more that we must do to accelerate our nation's transition from being a fossil-fuel based economy to being an energy self-sufficient economy based on alternate and renewable energy sources.

"I'm sure our panelists will have much to say about new energy technologies, and I expect that I will have numerous questions for them. I am particularly interested, however, in a field which is commonly referred to as cold fusion. Back in 1989, cold fusion first hit the media when Pons and Fleischmann made their startling announcement. Their announcement, however, turned out to be premature. The DOE issued a negative report, and cold fusion research has been languishing, at least in this country, ever since.

"Despite the unfortunate events of 1989, cold fusion research has continued. Japan has launched a substantial research effort, and, in this country, the Electric Power Research Institute (represented on today's panel), has undertaken a $12 million cold fusion research effort. Scientists at SRI and Los Alamos National Laboratory, along with scientists at various other places around the country and around the world, report getting excess heat from the electrolysis of 'heavy' water. I have recently received letters about cold fusion which I would like to have entered into the record. One is from Dr. Edmund Storms, a scientist at Los Alamos. Another is from a constituent of mine, Dr. Eugene Mallove, author of "Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor." Both letters address the need for increased government attention to cold fusion.

"Not being a scientist, I'm not in a position to evaluate the technical merit of recent cold fusion research. It does seem to me, however, that there exists sufficient factual evidence to warrant another comprehensive scientific review of cold fusion. In any case, I hope that our subcommittee will play a role in helping to find the truth behind the cold fusion furor. The mere possibility that there may exist a new source of energy that uses water as a fuel makes it too important to pass up."

Cold Fusion and SBIRs

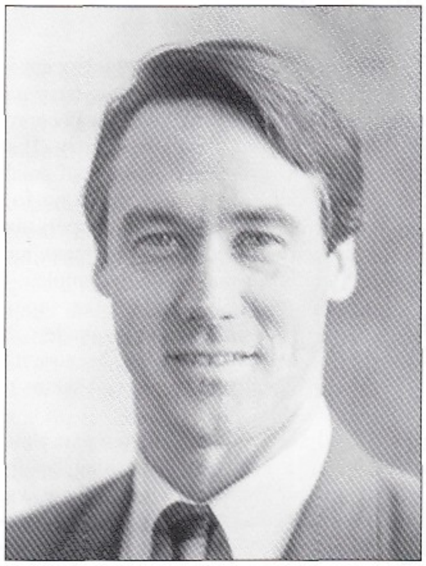
Cold fusion entrepreneurs need not apply for a U.S. Department of Energy SBIR (Small Business Innovation Research) grant. Not only is there no conceivable category for cold fusion in DOE's publication announcing SBIR grant areas, but cold fusion is specifically *excluded* under two of the DOE's SBIR categories (30. Plasma Confinement Systems Technology, and 31. Fusion Energy Systems). The DOE language leaves no doubt in anyone's mind: "Excluded from this topic is research pertaining to cold fusion, inertial confinement fusion, and other fusion energy concepts that are not based specifically on magnetic confinement of plasmas."

Profile of the future?

A headline in the *New York Times*, January 1,1994: "Saudi Arabia to Slash Its Budget As Oil Prices Continue to Plunge?" The Saudis were reported to have slashed their budget by 20% (down from the $52.3 billion in 1993) because of the precipitous drop in oil prices. A glut of oil on the world market and the international recession brought this about. Question: If it's this bad now, what will happen when it becomes crystal clear to oil-producing nations that cold fusion technology is for real?

Happer sweats

Dr. William Happer, the outgoing director last year of DOE's Office of Basic Energy Science, has never thought much of cold fusion. A man with a hot fusion background from his days at Princeton, Happer was one of the key members of the DOE's ERAB (Energy Research Advisory Board) Cold Fusion Panel who helped railroad a negative report on cold fusion by November, 1989. Vice President Al Gore, who is known to have been instrumental in forcing Happer to vacate his DOE post—not over cold fusion, of course,



Congressman Dick Swett (D-NH)

but because of another budgetary and scientific disagreement.

Before Dr. Happer left his federal post, he encountered cold fusion once again. At a Department of Energy "Fiscal Year 1994 Budget Overview" hearing (April 20, 1993) before the House of Representatives' Subcommittee on Energy of the Committee on Science, Space, and Technology, Congressman Dick Swett of New Hampshire provoked this exchange with Dr. Happer:

Congressman Swett: "I've been following the recent developments in cold fusion. That's one of the areas that I've been particularly interested in. As you know, in this field there have been substantial developments since the initial recognition and uproar back in 1989. The U.S. scientists at SRI, Los Alamos National Laboratory, and at various places around the country and around the world are reporting excess heat from cold fusion experiments once again. The Third International Conference on Cold Fusion was just recently held last October in Japan, and I have in my possession in my office positive technical evaluations of the conference submitted by researchers for MIT, the U.S. Office of Naval Research in Japan, and the U.S. Army Far East Research Office. I also recently received a letter regarding this issue from Dr. Edmund Storms at Los Alamos National Laboratory.

"Dr. Happer, given all that's happened in this field over the last four years and given the wide range of potential ramifications of the work, do you think it would be appropriate for the U.S. Government to consider taking another look at this field and to update the 1989 report? And I'm talking about literally just updating, not pushing forward into further research expenditures."

Dr. Happer: "Yes. First of all, we are keeping up with this field, and we would be happy to consider any proposal that is submitted to our research offices. To my knowledge, we have not gotten any in the last year or so."

Editor's note: Dr. Happer knew full-well how reluctant CF researchers were to waste their time in proposals to DOE knowing its extreme negative position. Also, it is known that several earnest proposals have been rejected.

Congressman Swett: "To my knowledge, there has not been a dime spent on keeping up with cold fusion research as far as what appears on the budget, and I am curious if there might be a delineation in that regard."

Dr. Happer: "Well, we have scientific attaches, for example in the Tokyo Embassy, and I got reports from them of the conference there, and also reports from some of the participants. These were informal. Perhaps it would be a good thing to put together a little more formal report that could be circulated. We would be happy to look into that."

Congressman Swett: "I would appreciate it if you would do so, and I'll expect to hear back from you."

Dr. Happer: "Okay."

The words of outgoing bureaucrats are cheap. If Dr. Happer or his successor have bothered to prepare such a report, would they kindly forward it to the offices of "Cold Fusion" Magazine?

Editor's note: No DOE official attended the Nagoya conference, as far as was known; it's only a few hours on the Shinkansen to Nagoya from Tokyo. Perhaps the "participant" that Happer spoke to was former ERAB Cold Fusion Panel Co-chairman, negativist Professor John Huizenga?

A deeper history of cold fusion

What do we do if it's the philosopher's stone?

If we consider the birth of cold fusion to have been the famous (infamous to some) press conference given by professors Fleischmann and Pons, then it is now a healthy child, just reaching its fifth birthday. Even so, it has had a sufficiently eventful life to make some sort of biography possible. The birth was difficult enough, and few thought the infant would survive. Since then, it has been given such a stack of death certificates that, if it were human and the same age, it would have difficulty seeing over them.

It's very difficult to talk about this strange creature, because we don't know what exact species it belongs to. What we do know is that it is not chemistry, because the energy released is hundreds or thousands of times as great as it could be if it were. We also know that it is not "hot fusion," though it may be a distant cousin. In some cases there are most definitely nuclear reactions going on, and some think that is the only source of the energy. Others think that the energy comes mostly from a completely new kind of super-chemistry, with nuclear reactions as just a small side effect. There are also some strange but well-attested results from "weird machines" which seem to be doing something which is at least closely related.

The super-chemistry dimension

For the moment, I'll use "cold fusion" as a catchall phrase to cover all unexplained energy-releasing phenomena which occur in ordinary matter at reasonably low temperatures. And I'll throw in all nuclear phenomena which don't fit modern theory. It is worth remembering that a number of solid- and liquid-state phenomena have no full theoretical explanation: high temperature superconductors, conducting polymers, and sonoluminescence make good examples. It is the addition of the nuclear (or super-chemistry) dimension which has made cold fusion so immensely unpopular, because it's not just outside current theories, it seems to run against them.

Curiously enough, cold fusion can claim a much longer gestation than either hot fusion or nuclear fission. By the 1920s it was realized that the stars could not go on generating heat for very long if they got their energy from chemistry or even gravitational collapse. The equivalence of mass and energy was understood, even though hydrogen fusion as a realistic explanation for the sun's heat would not come until 1929. Certainly, when Fritz Paneth and Kurt Peters first tried to convert hydrogen into helium at the University of Berlin in 1926, the difficulty of doing such a thing was not understood.

Paneth and Peters thought that, by using the famous ability of palladium to absorb great volumes of hydrogen, they might put the hydrogen under such huge pressures in the lattice of the metal crystals that some of it might be forced to turn into helium. And sure enough, they found helium. Perhaps if they had realized that the sun only manages to make a few watts of energy from each cubic foot of its interior, they might have been more suspicious of their results. Sure enough, they realized eventually that the helium came from contamination. Glass absorbs helium. Not much, but enough to ruin the experiment. Meanwhile, John Tandberg in Sweden proposed that using electrochemistry would improve the absorption of hydrogen by the metal.

In 1932, Harold Urey isolated deuterium (the heavy isotope of hydrogen) and Tandberg had another try, this time using deuterium with his palladium. Two years later, the first "hot" fusion experiments began. Finally, in 1938, it was realized that bombarding uranium with neutrons caused them to split. This was amazing. The researchers had been expecting something much less dramatic, and had been looking for the wrong products. It would not be in the least surprising if this lesson from history is relevant to the cold fusion story.

And it may be worth remembering that fission of this kind needs slow (cold) neutrons. The first thermonuclear bombs demonstrated rather effectively the power of fusion, and the race began to tame the reaction. But the "hydrogen bombs" were not hydrogen or even deuterium bombs. They also needed tritium, because even the conditions inside an exploding fission bomb are not enough for pure deuterium fusion.

500 BC-1600 (and beyond)
Alchemists attempt to discover ways of transmuting base metals (specifically mercury) into gold.

1803
The British chemist John Dalton proposes his atomic theory, which suggests that any element is composed of identical atoms whose properties determine their chemical behavior.

1868
Pierre Janssen and Joseph Norman Lockyer discover helium in the sun, by studying its light spectrum.

1895
Sir William Ramsay isolates the element helium in an uranium-bearing mineral.

1896
The French physicist, Henri Bequerel, discovers radioactivity.

1905
Einstein's Special Relativity theory proposes the equivalence of mass and energy.

1915
American chemist William Draper Harkins suggests that fusion of hydrogen to give helium would release energy by converting a tiny proportion of its mass.

1919
Rutherford transmutes nitrogen into oxygen using high energy alpha particles.

1926
Fritz Paneth and Kurt Peters claim to have transmuted hydrogen into helium at the University of Berlin, using palladium as a catalyst.

1927
John Tandberg tries to patent an electrochemical cell with a palladium cathode, intended to produce helium. Paneth and Peters claim shown to be erroneous, the helium was contamination. Tandberg's patent is rejected.

1932
James Chadwick discovers the neutron. Harold Urey isolates deuterium, the heavy isotope of hydrogen. John Tandberg tries again, this time using high energy electrical discharges in palladium wire.

1939
Hans Bethe, George Gamow, and Ralph Adler publish details of the mechanism by which fusion reactions power the stars.

1947
Charles Frank suggests the possibility of muon—"heavy electron"—catalyzed cold fusion.

1952
First fusion bomb detonated by U.S. in the Pacific.

1956
Louis Alvarez and colleagues at Berkeley observe muon-catalyzed cold fusion.

1984
Pons and Fleischmann begin their collaborative search for evidence fusion in electrochemical experiments with palladium and heavy water.

1986
Stephen E. Jones of Brigham Young University and his colleagues plan cold fusion experiments with metal. Jones and Clint van Siclen publish paper on piezonuclear fusion, the use of extreme pressures to force nuclei to fuse.

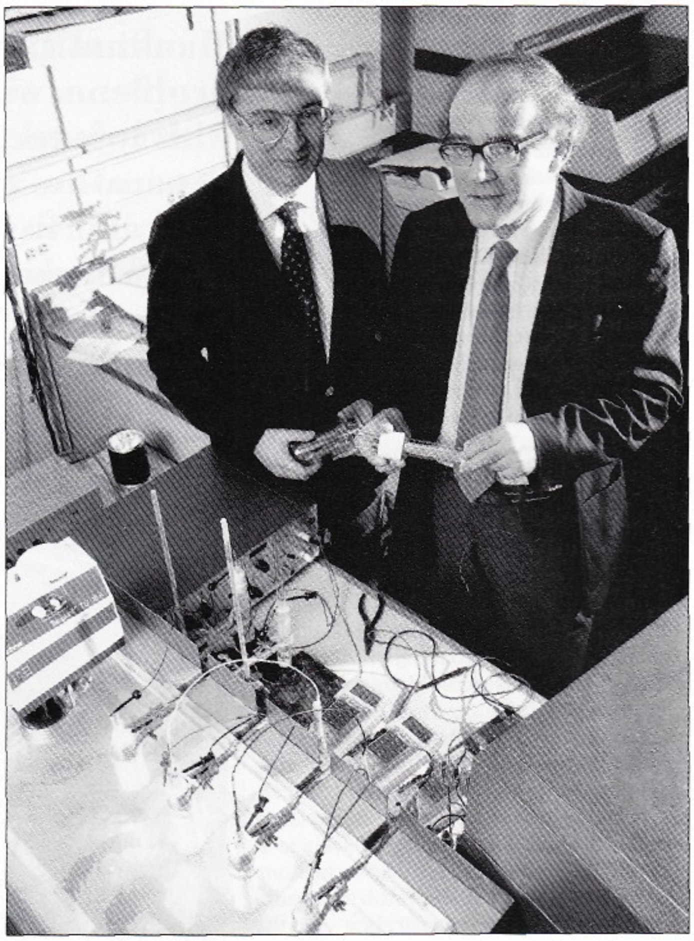
1987
Johan Rafelski and Stephen Jones publish in Scientific American an article on muon-catalyzed cold fusion, indicating its future potential as an energy source.

BY CHRISTOPHER TINSLEY

It wasn't going to be easy. In the end it would turn out to be impractical, but initially optimism was high. In England, in 1958, it was thought for a while that the ZETA machine's apparent success meant that fusion energy would soon be a commercial reality. However, it was all another mistake, and since then the projected date for usable hot fusion power slips further into the future with every passing year. The understanding of just how difficult fusion is, and the seeming success of the hot fusion program, kept things pretty quiet in cold fusion. But in 1985, Fleischmann and Pons started their self-funded crazy experiments in electrolytic deuterium/palladium cold fusion. And in March of 1989 they went public at the University of Utah.

What happened next will doubtless keep science historians, sociologists, and maybe even anthropologists busy for years. In nearby Brigham Young University, similar work by Steven Jones had been in progress for some time, and he was also due to publish. There seems to be no doubt that the two at Utah came under pressure to hold a press conference. They themselves have said that they would have preferred to do a further eighteen months of work, but apparently they did think at the time that their results would be fairly easy for others to reproduce—which of course they were not. Perhaps they had just been lucky in their choice of supplier of palladium, or maybe they were better at electrochemistry than were their imitators, better than they knew.

A claim of such breathtaking audacity could hardly expect a welcome from the ranks of hot fusion. With quite a few billions spent already, with half a century of effort behind them; with half a century and many more billions ahead of them, the news that a couple of mere chemists might have solved the problem on a laboratory bench was absurd and yet alarming. Had these not been scientists of considerable reputation—Martin Fleischmann is a Fellow of the Royal Society—their claim would have been ignored. Instead there began an international scramble to try to reproduce the findings.



Professors Stanley Pons and Martin Fleischmann in their University of Utah laboratory in 1989.

Anybody who follows events in science can tell you what happened next. Some universities rushed onto our television screens to report success, and then retreated in embarrassment when they found their mistakes. A few obscure research outfits in strange foreign places continued to claim success. But solid, definitive work by such prestigious institutions as the Plasma Fusion Cen-

1989

March 23
Professors Martin Fleischmann and Stanley Pons announce that a simple electrochemical experiment produces sustained nuclear fusion at room temperature—or at least that some unknown form of nuclear reaction is producing the excess energy.

March 31
Stephen E. Jones of Brigham Young University in Utah speaks at Columbia University, contrasting his research with that of Pons and Fleischmann.

March-April
Major cold fusion experimental efforts begin in India and in Japan.

April 10
Researchers at Texas A&M University claim to have duplicated the Pons and Fleischmann experiment (later proved to be erroneous).
Researchers at Georgia Tech announce the detection of neutrons from an electrochemical cell. (A claim subsequently retracted, three days later.)
Pons and Fleischmann paper on cold fusion appears in the Journal of Electroanalytical Chemistry and Interfacial Electrochemistry.

April 12
Meeting of the American Chemical Society in Dallas, Texas, is attended by some 7,000 chemists; Pons defends the alleged fusion breakthrough.

April 14
Peter L. Hagelstein at MIT announces he has submitted for publication four speculative papers to explain cold fusion by a mechanism he calls "coherent fusion."
MIT announces that it has applied for patents related to Hagelstein's theories.
Keith Johnson of MIT is the first to suggest that ordinary water could also produce excess heat, with his unusual "super-chemistry" theory (Dynamic Jahn-Teller Effect).

April 18
Italian physicist Francesco Scaramuzzi, with a group at the Italian Atomic Energy Authority at Frascati claims success in detecting neutrons from cold fusion using a titanium metal infused under pressure with deuterium.
Robert Huggins, at Stanford University, says he has obtained excess heat in an electrochemical cell with heavy water, but not in one with light water.

May 1-2
Researchers at MIT Plasma Fusion Center say they have found serious flaws in the neutron emissions data reported by Pons and Fleischmann; claim that Pons and Fleischmann were unlikely to have detected neutrons.
Boston Herald story planted by MIT Plasma Fusion Center backfires.
At a meeting of the American Physical Society in Baltimore, MD, physicists and chemists roundly criticize and mock the work of Pons and Fleischmann.

May 8
At meeting of the Electrochemical Society in Los Angeles, Pons and Fleischmann defend their experiments against criticism, but back off on their claims to have detected neutrons.

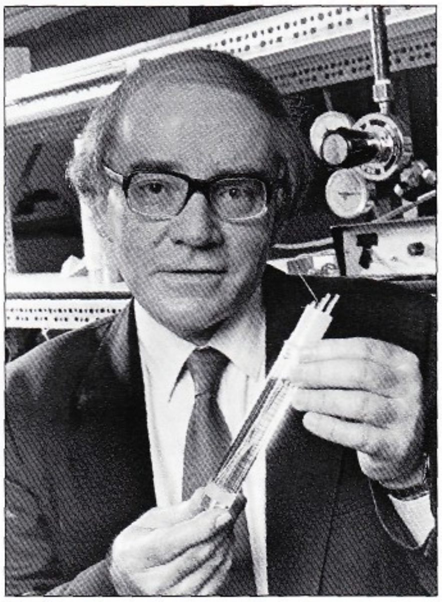
May 18
An extended scientific correspondence by researchers at MIT's Plasma Fusion Center appears in Nature, critiquing the original Pons and Fleischmann neutron data and finding it to be seriously flawed.

ter at MIT, Caltech, and the British atomic research laboratory at Harwell proved that it was all a terrible mistake, or worse. Since then, most people have heard little about cold fusion research.

So, what really did happen? Is it just that there are a few rather strange scientists who cannot see that it is a dead issue? What really happened was the greatest failure—and the greatest triumph—of science in modern times. Not of science the abstraction, but science as practiced by people. Anybody who has followed the story carefully should have lost any illusions about science, but gained a lot of admiration for some scientists. Electrochemistry is an exacting discipline, and requires a lot of "job knowledge" not generally available in our great nuclear laboratories.

In any case, asking a group of physicists to check out a weird idea in electrochemistry is a bit like asking Ford to report on the latest offering from General Motors. Few people questioned the conclusions made by these great institutions, but study of their experimental results indicates that something is wrong, very wrong. In the case of Caltech, the basic argument is about the way they calculated heat. How can it be that there is no clear agreement about how we measure heat? What use are the laws of thermodynamics if we cannot measure the quantity they describe? Harwell had access to Fleischmann's advice, and they did a very long series of tests. One commentator finds that their raw data shows at least ten unexplainable bursts of heat from the cells. As for MIT, I believe that the Gentle Editor and those who have studied the "unusual data reduction methods" of the MIT PFC (Plasma Fusion Center) are better qualified than I to comment on that unattractive episode.

Mankind's energy problems will be over, with low-technology generators replacing oil, coal and fission. Beyond that will come a new understanding of how to provoke nuclear reactions at will . . .



Prof. Martin Fleischmann with cold fusion cell in 1989.

And the score is . . .

So far, the score for science was nil. The score went up when many good labs made an honest effort to reproduce the effect but could not find it. Now the heat was on the two scientists. The science community arose, mighty in their wrath, and fell upon them in a quite unprecedented manner. It is for others to ask why, but it made one thing clear. If you are a scientist, then don't make a mistake, or what *other* scientists decide is a mistake. Boldness and risk-taking are out, go for the safe options.

Science journalists showed an unhealthy enthusiasm to join the baying mob; presumably there is a great reservoir of bad feeling in science, and this episode let it all out. There was much hunting for anything which might be considered to be impropriety, but the real crime seems to have been that most unholy wickedness of all—Fleischmann and Pons had worked outside their own field of study. But it is quite true that many honest efforts were made to do the experiment, and many failures. Like many solid-state effects, success depends on a number of factors, and not all of them are understood even now. This became a hopeless situation, it became impossible for research to go on in a normal manner. The "normal manner" would (supposedly) go something like this: An experimenter would find an unexpected effect and submit to a scientific journal a paper describing the results. The paper would be reviewed by a number of experts in the field. If they felt that the results were worthy of publication, the paper would eventually appear in print and, if the results were suffi-

May 23-25
The first conference dedicated exclusively to cold fusion is held in Santa Fe, NM, and draws some 500 participants.

June 15
Harwell Laboratory in England stops work on cold fusion experiments.

June 26
Los Alamos National Laboratory scientists announce finding tritium in cold fusion experiments.
MIT Plasma Center holds "Wake for Cold Fusion"—weeks before analyzing its own calorimetry results.

July 12
Interim report by ERAB panel of DOE, finds no good evidence for cold fusion and rejects a formal research center for cold fusion.

October 16-18
EPRI/NSF meeting on cold fusion in Washington, DC. Positive results continue to be reported.

November 1
Final DOE ERAB report approved concluding that evidence for cold fusion is not persuasive.

1990

March 28-31
First Annual Conference on Cold Fusion held in Salt Lake City is attended by over 200 researchers. Nature magazine (March 29) publishes scathing editorial attacks on cold fusion research and prints an article by University of Utah physicist Michael Salamon et al claiming that in 1989 tests they detected no nuclear products coming from Pons's cells.

June 15
Publication in Science of a lengthy exposé by Gary Taubes in which he strongly suggests that high levels of tritium found in Texas A&M University cold fusion experiments were the result of fraudulent spiking of the cells.

July 23
Bruce Liebert and Bor Yann Liaw and colleagues at the University of Hawaii report multi-watt excess power output from electrochemical cells incorporating molten salts and deuterium. Results reported at the cold fusion sessions of the World Hydrogen Energy Conference.

October
Conference on Anomalous Nuclear Effects in Deuterium/Solid Systems, Provo, Utah.

1991

March 17
Sunday New York Times, page 1: Frank Close's accusations of ethical violations by Pons and Fleischmann. Implication of fraud is strong.

March 22-26
First Russian National Conference on Cold Fusion, Dubna-Moscow. Research at 45 institutes reported, 80 papers submitted.

June 7
Frank Close speaks on "An Exposé on Cold Fusion" at MIT Plasma Fusion Center.
Eugene Mallove publicly disavows MIT press release of May 1, 1989 which contradicted Boston Herald story, and challenges 1989 PFC experiment. PFC Director Ronald Parker publicly promises full disclosure of all data. Mallove resigns from MIT News Office.

June 29-July 4
Second Annual Conference on Cold Fusion, at Como in Italy.

June 30
National Cold Fusion Institute in Salt Lake City closes because of insufficient funding.

August
Publication of the Mills & Kneizys paper, in Fusion Technology, claiming excess heat with light water, potassium carbonate and nickel.

ciently surprising, other experimenters would try the experiment. If they were successful, they in turn would publish, and the theoreticians would get to work to explain the whole thing.

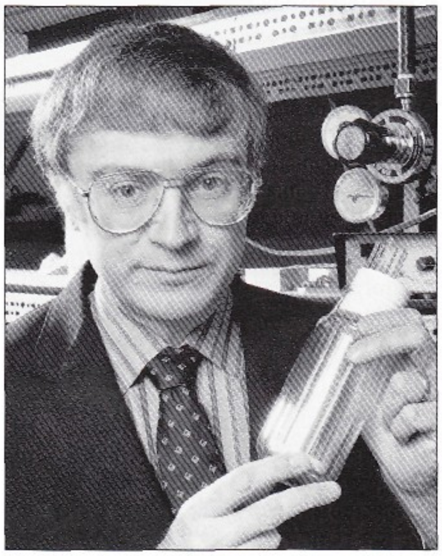
So much is made of those rare occasions when this system has allowed a nonexistent effect (like the infamous "polywater" affair, which misled scientists for years) to gain apparent respectability, that this process is very strict—perhaps too strict. Fear of being involved in an affair which may later harm their reputations makes a lot of people prefer to play safe and reject the work.

In cold fusion the whole system collapsed. Once MIT, Harwell, and Caltech had issued their (unquestioned) results, the field was no longer respectable. Never mind the results trickling in, they never even made it to peer review in those journals which had editorialized in the most scathing tones upon the whole affair. When results were so good, so careful (and a lot of the early positive papers were not) then reviewers—sometimes apparently selected for their known views—would reject them on the most absurd grounds.

When good work (like the results reported by Yamaguchi of NTT in 1992) was obviously going to be reported in the newspapers, some of the science journals would mention it and get comment from a known opponent who had not reviewed the findings. When a sympathetic editor of the European journal *Physics Letters A* started to publish some of the most spectacular results (from Kucherov in late 1992, from Fleischmann and Pons in early 1993) the silence from the journal *Nature* (which had published an editorial, "Farewell (not fond) to cold fusion") was deafening.

Others think that the energy comes mostly from a completely new kind of super-chemistry, with nuclear reactions as just a small side effect.

Undaunted, the semi-popular British magazine, *New Scientist*, published a wholly ludicrous, inaccurate and misleading account of the latter paper—accompanied by an editorial pontificating on the need for accuracy in science journalism. It caused me some innocent merriment when the same writer, in the same magazine, published a rather more balanced report in January 1994. Commenting on a paper in the same *Physics Letters A*, he reported that Otto Reifenschweiler found in the early 1960s that a mixture of titanium and tritium lost 28% of its radioactivity when heated to a temperature not much above the boiling point of water. Hendrik Casimir, the then director of research at Philips says the paper "supports the idea that the solid state environment could have an effect on radioactivity" and that the recent rows over cold fusion had finally persuaded them to publish the findings. This may all be a big mistake, but it adds to the pile of related reports.



Prof. Stanley Pons with cold fusion cell prototype in 1989.

The philosopher's stone

Is this just a story of intellectual skullduggery, of an arrogant science community actively working to stamp out heresy, of dedicated scientists struggling to unlock nature's secrets despite derision from their former friends? It's all of those things, but there is another side to the tale. There was overenthusiasm at the beginning, leading to sloppy work and rushed papers. Above all there was the big problem: if you think you may have the key to unlimited energy, unlimited wealth—and maybe the philosopher's stone as well—how objective are you going to be? What are you going to do for funding? Will you even want to publish your work? Probably you will end up torn between all manner of conflicting interests.

It would take far too long to describe the hundreds of good, solid results achieved around the world during the past five years. Better to review the field as it stands, but

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July 10
Japan's MITI announces major funding of cold fusion research.

July 27
Edmund Storms of Los Alamos National Laboratory announces replication of Takahashi's (Japan, Osaka University) excess heat. DOE Washington immediately restricts his funding.

August
Dr. Mitchell Swartz publishes "Reexamination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center" in Fusion Facts. Analysis finds numerous faults in the analysis, as well as strong evidence that data were inappropriately altered and processed.

October 21-25
Nagoya Third International Conference on Cold Fusion. Numerous results confirm reproducibility of excess heat and nuclear phenomena.

November 17
NTT Laboratories of Japan announces $565,000 "Cold Fusion Kit" to demonstrate helium-4 production.

December 3-4, 9-10
Dr. Reiko Notoya of Hokkaido University comes to MIT to demonstrate her light water cold fusion experiment.

1993

March 25
Congressman Dick Swett (D-New Hampshire) and Eugene Mallove testify before the House Appropriations Committee, Subcommittee on Energy and Water to ask for funding for a National Academy of Sciences review of cold fusion.

May 3
Publication by Fleischmann and Pons of their high-power cold fusion work, including continuing energy output when energy input ceases.

June 24
Canadian Broadcasting Corporation airs documentary program on cold fusion, which is highly positive.

August
Popular Science publishes objective account of latest cold fusion developments, the first cover story on cold fusion since 1989 by a U.S. publication.

December
ENECO, a Salt Lake City-based cold fusion company acquires exclusive licensing rights to Utah cold fusion patents.

December 6-9
Fourth International Cold Fusion Conference held on Maui, Hawaii.

1994

January 3
Publication of 33-year-old work by a former chief physicist at Phillips in The Netherlands, reporting anomalous radioactivity of tritium in titanium.

February 21
Publication in an Italian physics journal of definitive cold fusion experiment showing unambiguous evidence for long duration, high excess power at high temperature in a gas phase experiment with nickel material.

firstly there is the odd matter of geography. The great physicist Ernest Rutherford once said, "There are no 'sciences,' there is only physics, the rest are just stamp-collecting." Rutherford was a New Zealander, but he worked in England. Michael Faraday, arguably the greatest experimental physicist who ever lived, was in contrast a man of great humility.

But it looks almost as if for nearly a century Britain felt that she did the physics, the Germans did the chemistry (just barely respectable), and lesser breeds were allowed to pick over the leavings. Britain has the Royal Institution, the Royal Society, the journal *Nature*, all hoary with age and honors—and there's nothing so very bad about all that. But it can be no coincidence that Britain led the battle to crush cold fusion, that only in Britain is there no research that I know of. Next to none in Germany, either.

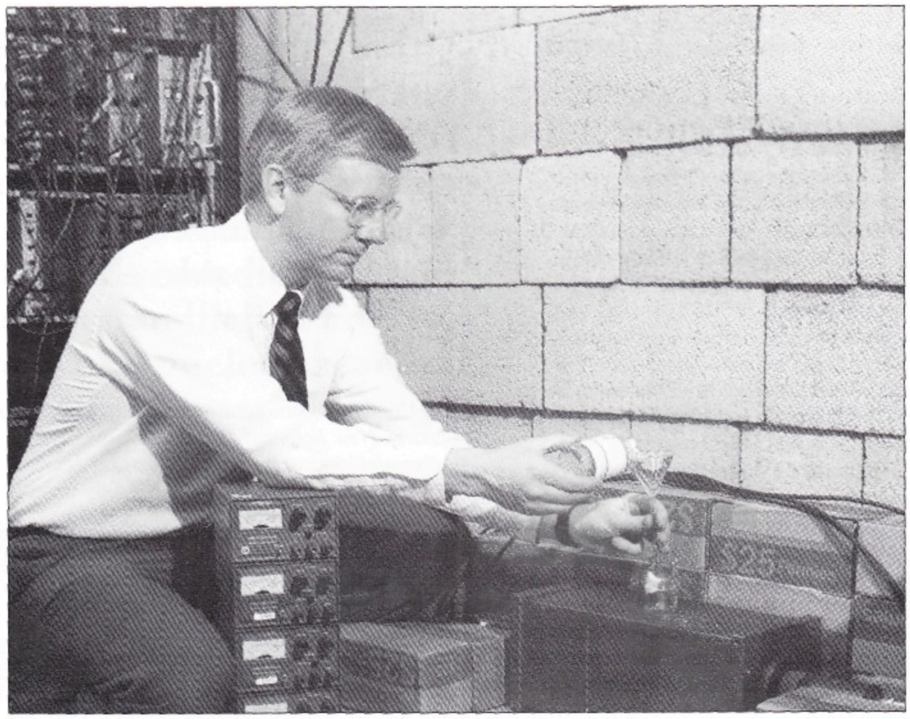
In the United States, cold fusion is an essentially "underground" activity, with the remarkable exceptions of the funding of SRI International by the Electric Power Research Institute and the continuing publication of cold fusion papers in the journal of the *American Nuclear Society Fusion Technology*. The intellectual climate ("emotional climate" might be a better term) is such that any ideas of Federal funds being either available—or acceptable by universities—can be forgotten for the immediate future.

Work is proceeding, and has produced impressive results considering the limited funds. Elsewhere, it might be fair to say that levels of work are patchy. A little in France, more in Spain and Italy, a lot in the former Soviet Union and China, and of course the extraordinary level of activity in Japan, where government and industry are collaborating in the uniquely Japanese way that has brought them such success in other fields.

However, things are changing. Pragmatic business interests in Britain and the U.S., and probably elsewhere, are more interested in results than theoretical objections to them.

As for experimental results, the original electrochemical experiments have continued, but have been joined by a whole host of other techniques which can be presumed to be related. These include electrochemistry of molten salts, dry experiments involving palladium and also a variety of complex crystalline substances like tungsten bronzes and others resembling those used in high temperature superconductors. The effect of ultrasound on palladium in heavy water seems to be to trigger the reaction. Fleischmann and Pons (and others) have reported finding that a used electrode from their experiments will continue to give off substantial amounts of heat (hundreds of times that which might be expected if the trapped deuterium were fully oxidized) for hours after the experiment is ended. At least three groups have found extraordinary isotope changes and blatant transmutations in the metal, and solid evidence is available for the production of tritium and helium-4.

There is also the strange energy-releasing phenomenon of "light water cold fusion," electrolysis of potassium carbonate solution in light water with nickel electrodes. This was first reported by Randell Mills in 1991, and has subsequently been replicated by several researchers.



Prof. Steven Jones of Brigham Young University (1989) preparing for neutron measurements in a "mother earth soup" cold fusion cell.

However, the whole field is dogged by insistence that contamination or fraud is the cause of the nuclear products—and even the heat. It might be thought that the overwhelming evidence would convince anybody, and it is true that many who have not been emotionally involved are now convinced of the reality of at least some of the effects.

The biggest difficulty of all is the one of reproducibility. With no understanding, with no fully satisfactory theory, without even tools (or the access to them) to investigate fully the detailed structure of different batches of metals to find out why one works and the other does not, the whole area of study has big problems.

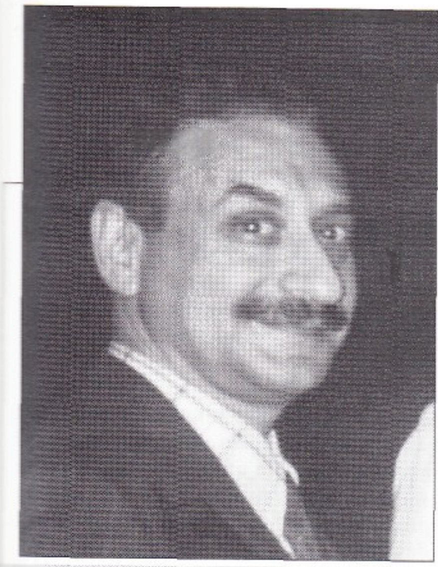
But even with a fully reproducible cold fusion generator, producing many times more energy than is put into it, for long periods of time (and you might well see them in 1994), do not imagine that the views of the opposition will change. Industry and commerce may well be interested, but the only thing which will even shake the certainties of the so-called "skeptics" (they are in fact no such thing) will be a fully self-sustaining machine, one which will produce enough energy in the form of electricity that it can drive itself. And it will have to come with a design from which anybody can build.

What can we now expect for the future? It is probably foolish to assume that no new, reliable devices will appear in the coming months, but let's assume they won't. If not, then firstly there will be "energy amplifiers" which produce several times as much heat as they consume in electrical energy. Following close behind them will be electricity or heat generators of sufficient power that they will make possible the cold fusion car or domestic power and heat generator.

Mankind's energy problems will be over, with low-technology generators replacing oil, coal and fission. Beyond that will come a new understanding of how to provoke nuclear reactions at will, leaving the way open to large-scale transmutation, and the conversion of nuclear waste into harmless substances. Just as happened a century ago, the face of science and of the world will change forever.

The press conference of 1989 will probably be seen in the future as the event which broke the mold. Already the climate of opinion has shifted. If some backyard inventor insists that he can do something weird like bending a light ray into loops (to take an utterly absurd idea) then he may find a more sympathetic audience than before—one which will ask for proof rather than laugh at him. Those scientists who have "impossible" results in a back drawer will, like the Philips researchers, dust them down for possible publication. The work of such as Kervran, who seems to have demonstrated "biological transmutation of elements" in the 1950s will be—is being—looked at again.

It is not for every generation to see the world turned upside down. They tell me that an ancient Chinese curse is, "May you be reborn into interesting times." I take the other view, this is an exciting time to be alive. Perhaps we can hope that we will handle these changes in a responsible and humane way.



MITCHELL R. SWARTZ

A new information pathway to the future

Cold Fusion is here to stay. Despite a dwindling but vocal band of skeptics, there is now something new under the sun.

To be sure, the cold fusion naysayers are still attempting to counter the growing literature of positive results by doing what we engineers call "hand-waving." They spout non-sequiturs such as, "the reactions are not those of the sun nor do they generate large numbers of neutrons." That is true enough. Cold fusion reactions are not those of the stars. But they do occur in materials which are ordered within a crystalline lattice, not in plasmas at millions of degrees. A new physics of materials has been discovered in the solid state. It is precisely in that area where the secret to "cold fusion" resides.

At the recent Fourth International Conference on Cold Fusion (ICCF-4), which was held on Maui last December, there were scores of experimental confirmations of cold fusion. Both nuclear and thermal products created by several cold fusion phenomena were reported. These confirmations now range over numerous physical systems, including both the initial Fleischmann and Pons heavy water/palladium system (with and without lithium) and progressing through other systems involving diatomic hydrogen gas (also D_2, heavy hydrogen gas) and high voltage glow discharge.

ICCF-4 was four days full of idea-swapping and resource-sharing, amid intensive lectures on theory, materials science, and nuclear physics. Skeptics were vastly outnumbered by the representatives from the many developing businesses in this new field. The cold fusioneers reported that they weren't too bothered by the three or four hard-core cold fusion detractors.

Supporters, on the other hand, were far more concerned about finding time to deal with the cornucopia of more than 150 new papers, and scores of laboratory efforts confirming cold fusion phenomena. There were many new developments and more than 100 papers reporting experimental results.

The field is warming up

How this field has evolved from the spring of 1989! Now there are hundreds of scientists, thousands of students, and millions of potential beneficiaries of this new technology. The rise in power levels of cold fusion systems from 1989 to present (from 10 Watts/cm^3 to now over 1000 Watts/cm^3 of palladium) matches the increase in publishing that informs us about this dramatic new research area.

It has become increasingly difficult to keep abreast of the latest cold fusion happenings with the torrent of information pouring out from California, Massachusetts, Illinois, Texas, Utah, Russia, Italy, China, the UK, Japan, and France. Add to this the developing variety of materials used in cold fusion, theories to explain the latest findings, nuclear products generated, and the emerging technologies—all with vast implications for business, technology, and industry.

"Cold Fusion" Magazine will be a major contributor in processing and taming this information overload. With this issue, the number of publications focused on cold fusion has increased from three to four, with *"Cold Fusion"* being the first *magazine* in the world on the subject. But the significance of this magazine is just another reflection of the continuing demand by scientists, engineers and laypeople to obtain even more valuable information on this field.

Before, there were *Fusion Facts* and *Cold Fusion Times* newsletters. There was also the cold fusion section of the academic journal *Fusion Technology*, a publication of the American Nuclear Society, which is primarily devoted to *hot* fusion. These publications were pioneers in their attempts to present the growing lode of new information to the world.

With the inaugural issue of *"Cold Fusion,"* another information pathway has opened. The world's first magazine devoted exclusively to disseminating cold fusion information will also play a major role in correcting flawed perceptions regarding "cold fusion".

I am delighted to come aboard as a Contributing Editor. In the months and years ahead, I will devote this column to materials, physics, and other innovative scientific issues in cold fusion. Next issue will bring a discussion of one of the nuclear products of this exciting new technology, for which there is an increasing body of scientific evidence—the inert gas ^{4}He.

Dr. Mitchell Swartz (BS, 1971; MS, EE, and ScD, 1984, electrical engineering, Massachusetts Institute of Technology; MD, Harvard University, 1978) conducts research into cold fusion, as well as dye-catalyzed photochemical reactions and proton reactions on Group VIII metal electrodes. His interests include molecular sensors, viral inactivation, and radiation oncology, and the interactions of radiation and matter. Dr. Swartz is Editor-in-Chief of Cold Fusion Times newsletter.

HAROLD FOX

A challenge to the cold fusion skeptics: Attack all of these devices!

If Drs. Stanley Pons and Martin Fleischmann should be criticized, it would be for not stressing the difficulty of designing and operating a successful cold fusion electrochemical cell. Almost none of the criticism poured out on Pons and Fleischmann came from their peers, the world's foremost electrochemists. For example, Dr. John O'M Bockris, a world-renowned electrochemist, was one of the first to replicate the effect. Dr. Edmund Storms also achieved early successes *(see cover)*.

Operating on the assumption that if there were a nuclear reaction it had to be the fusion of deuterium-deuterium, the hot plasma physicists leaped to the conclusion that fusion in a metal lattice must produce the same results as d-d fusion in high temperature deuterium plasma.

Because most of the hot fusion physicists had immediate access to neutron measuring equipment, many of them hurriedly put together experiments, operated for a few hours, found no neutron emission, and then began labeling Pons and Fleischmann poor experimenters (at best) and frauds (at worst). Most of such early negative papers indicate that the authors' study of electrochemistry had been neglected. Electrochemists, who served as consultants, had a depth of knowledge of this new procedure apparently gathered from little more than TV news footage.

Obviously, what took Pons and Fleischmann several years of work was not replicated in a few days by those unskilled in the art.

Frightened by the concept that two chemists could produce "excess power" in a relatively simple table-top experiment, some of the hot fusioneers (who have yet to achieve excess power with hot fusion devices) began an unprecedented, and vocal, attack on cold fusion in general, and Pons and Fleischmann in particular, an attack still waged by a few confirmed skeptics—two of whom attended the Fourth International Conference on Cold Fusion (Maui, Hawaii, December 1993).

The laughable part about this on-going skirmish is that the "pathological skeptics" are still denouncing the heavy-water, palladium, lithium system that Pons and Fleischmann announced March 23, 1989. Many other scientists, recognizing a good thing when they learn about it, have forged ahead with their own ideas about other ways in which nuclear reactions can be produced and controlled in other types of devices.

Here is the latest scoreboard of devices that produce nuclear reactions (and nuclear byproducts including heat):

1. Molten salts using potassium, sodium, and lithium salts and a palladium electrode. (Liaw and Liebert, University of Hawaii, 1990, *Fusion Technology* January 1993.)

2. Demineralized water (light water), nickel cathodes, and, for example, potassium carbonate electrolyte. (Randell Mills, 1989-90, *Fusion Technology* August 1991, Bush and Eagleton, Cal Poly - Pomona, 1991, *Fusion Technology* September 1992.)

3. Glow Discharge (low pressure), palladium cathode, deuterium gas system. (Kucherov, Karabut, & Savvatimova, LUCH, Russia, 1990-94, *Fusion Technology* December 1992.)

4. Sparking discharges using deuterium gas and palladium cathodes. (Dufour, France, 1989-90-93, *Fusion Technology* September 1993.)

How would you feel if you had just invented the airplane, and someone else announced a spaceship? The hot fusioneers are suffering from technological obsolescence.

Introducing Hal Fox

Immediately after Hal Fox heard about the announcement of cold fusion, discovered at his Alma Mater (the University of Utah), he decided he had to get involved. Although he had just retired, he and a few friends established the Fusion Information Center, Inc. and laid plans for publishing Fusion Facts, a monthly technical newsletter that put out its first issue in July 1989. By attending and reporting on all of the international conferences on cold fusion, plus many regional conferences, Hal has gained a reputation for the rapid dissemination of factual information about cold fusion, and has made many friends among cold fusion scientists. We are pleased that Hal has agreed to write a column for "Cold Fusion" Magazine.—Eugene Mallove, Ed.

5. Capillary cold fusion. (Work funded and reported by J.P. Vigier, 1990-93, in former Czechoslovakia; *Physics Letters A*.)

6. Bronze crystals, sodium tungstate crystals which form tiny tubules. (Kaliev, Baraboshkin, Samgin, et al, 1992-93, Ekaterinburg, Russia, ICCF-3.)

7. Proton Conductors. (Mizuno, Enyo, Akimoto, & Azumi, Hokkaido Univ., Japan, 1993, ICCF-4.)

All of these new methods have been experimentally verified, have peer-reviewed results published in scientific journals, and were reported to conferences of peers in various scientific and technical meetings. Cold fusion skeptics haven't begun to keep up with the rapid developments in the field. They have scarcely mentioned these additional ways in which nuclear reactions have been produced.

Perhaps hot fusion lobbyists haven't had time to read the literature because they've been so self-congratulatory for having achieved in recent few-seconds tests an output power 20-30% of the 24 million watt input power using their $500-plus million toy at Princeton.

By the way, this "toy" is now so radioactive that a one-year cooling down period is required before being dismantled and shipped off to some nuclear waste repository. *And this is heralded as their greatest success.* With $500 million in annual funds at stake, is it no wonder these lobbyists are not applauding cold fusion advances?

Does the flat-out rejection of the new science called cold fusion indicate it is bad science, or that the peer-review system is wrong, or that scientists are incapable of recognizing new discoveries? Not necessarily. It depends on whose ox is being gored. In 1989, one unnamed hot fusioneer was quoted as complaining, "How would you feel if you had just invented the airplane, and someone else announced a spaceship?" The hot fusioneers are suffering from technological obsolescence. They will struggle to obtain funds to create bigger and more expensive toys while the new science of cold fusion develops commercial products. Then they will declare that they knew cold fusion was a good idea from the very first.

It is unusual that we do not, as yet, fully understand the complex processes of cold fusion? No! We don't even understand fully, as yet, the process of chemical catalysis. There are some, including this author, who believe that *various nuclear reactions are being catalyzed on the surface or within metal lattices*. Edison, the Wright brothers, Goddard, the developers of atomic energy, and many other pioneers have been strongly criticized for their revolutionary ideas. Pons and Fleischmann are just the most recent of the scientific pioneers to receive arrows in the back. Just don't bet against their ultimate success and future acclaim.

So what is next in this engaging saga of new science? I don't know, and that is what makes it all fun. Watch for comments about the article "Inertia as a Zero-Point Field Lorentz Force." (Haisch, Rueda, & Puthoff, *Physical Review A*, February 1994.) In my opinion, scientists will soon have to deal with the concept of a strongly energetic Lorentz-type energy that pervades all space and most matter, including cold fusion cells. [A Lorentz electromagnetic field cannot be sensed, perceived, felt, or measured from a fixed frame of reference, but only from an accelerated frame of reference.]

In cold fusion devices, deuterons (ionized deuterium atoms) should not be able to fuse because the same charge should mutually repel the deuterons (the Coulomb barrier). However, could a strongly energetic, pervasive, Lorentz-type field plus a metal lattice, plus a constant flow of electrons, provide a means to easily overcome this charge barrier? Don't miss an issue of *"Cold Fusion"* Magazine.

Hal Fox is the editor of Fusion Facts and New Energy News, Salt Lake City, Utah.

Skeptics beware

Protocols for conducting light water excess energy experiments

ASSEMBLED BY EUGENE F. MALLOVE AND JED ROTHWELL FROM PUBLISHED AND UNPUBLISHED MATERIAL.

This column is intended to augment the Fusion Technology paper by Mills & Kneizys. Fusion Technology is carried in many major libraries, for example, the Boston Public Library, and the MIT science library.

Purpose

Many people have heard of the light water excess energy experiment reported by Mills and Kneizys in *Fusion Technology*. By January 1992, this excess energy effect had been reproduced by at least a half-dozen other groups. Even though the experiment is simple and apparently highly reproducible, many would-be experimenters might be deterred from trying it because of the well-known history of difficulties with Pons' and Fleischmann's heavy water palladium-platinum approach.

Although Mills et al do not think their excess energy is due to "cold fusion"—they have an elaborate theory of shrinking hydrogen atoms to explain the excess power—their experiment *was* inspired by the Fleishmann and Pons announcement. The purpose of this brief collection of experimental protocols is to encourage others to try the Mills experiment, and perhaps go beyond it in their investigations.

How to begin

The first order of business is to read the *experimental* part of the Mills-Kneizys paper in *Fusion Technology* to familiarize yourself with the basic approach. Don't try any fancy pulsed input power in the beginning. Stick with continuous (DC) input power. Don't be concerned about Mills' and Kneizys' exotic theory, which may be wrong or right. It's the validity of the experiment that's important at the moment. Other theories, including "conventional" cold fusion mechanisms working with the trace amount of deuterium, might be invoked to explain the excess energy in this light water experiment.

Conditions to be employed

1. The volume of solution could be from 100 ml to 1,000 ml in a vacuum-jacketed glass dewar cell. You might try using an ordinary thermos bottle, favored by the Indian researchers at the Bhabha Atomic Research Center! Some people have tried a non-dewar cell, using a heavily insulated glass beaker with plastic materials to give the same insulating dewar effect. The cell should be closed at the top with a tapered rubber stopper. Teflon might be better to reduce possible contamination.

2. The electrolyte should be: 0.6 M aqueous K_2CO_3 of high purity.

3. The electrolyte should be stirred continuously with a magnetic stirring bar to ensure temperature uniformity.

4. The nickel cathode does not apparently need the exact configuation of the "spiral wound" sheet described by Mills-Kneizys in their paper. It could be just a flat sheet of nickel, but the ratio of the *total surface area* (i.e. both sides) of the nickel cathode to the surface area of the platinum anode should be no less than 20/1.

5. The anode is of platinum wire, 1 mm diameter. Mills and Kneizys used a spiral-shaped piece 10 cm long.

6. Above all, avoid impurities and contamination of the cell materials, whether in handling or in environmental conditions. Particularly ensure that no organic contaminants are in the cell or on the electrodes, and don't forget that remnant soap film could be a problem.

7. Dr. V.C. Noninski, (now at Fitchburg State College, Fitchburg, Massachusetts) who has replicated this light water work, recommends:

"Before starting the experiment, mechanically scour the platinum anode with steel wool, soak overnight in concentrated HNO_3, and then rinse with distilled water. Remove the nickel cathode from its container with rubber gloves, and cut and bend it in such a way that no organic substances are transferred to the nickel surface. Preferably, dip the nickel cathode into the working solution under an electrolysis current, and *avoid leaving the nickel cathode in the working solution in the absence of an electrolysis current.*"

8. Before attempting to run the cell to demonstrate excess energy, reverse the cell polarity for about one hour to anodize the nickel cathode. However, Professor John Farrell of the Mills group has said that 0.5 hour of this treatment is adequate. He says this "electropolishes the Ni."

9. Use distilled H_2O.

10. There have been claims and counter-claims whether the experiment will work in "closed-cell" mode with a catalytic recombiner. Begin your work without one to be on the safe side. Professor Farrell and, independently, Dr. Noninski have measured the oxygen and hydrogen evolution in the absence of a recombiner and find these gases in the expected quantities, that is, unsuspected recombination is *not* causing the excess power effect.

11. The current density on the cathode should be on the order of *one milliamp per square centimeter.* This is very low, compared to Pons' and Fleischmann's heavy water experiments.

12. To calibrate the cell, introduce a pure resistance heating of known power by using a 100 ohm precision resistor encased in teflon tubing.

The basic goal of the experiment is to demonstrate that significantly more heat emerges from the cell under electrolysis than the joule heating of the cell. This is how the basic analysis works:

The cell has a particular heating coefficient (HC), which can be determined by employing (in the absence of electrolysis) *pure resistance heating* by an ordinary precision resistor with an applied voltage. One might find, for example, that the HC of a particular cell is, say, 25 C/watt. This means that for a watt of input power, the temperature of the liquid contents of the cell should rise 25 C above ambient. Keeping the ambient temperature stable is important; this is a source of possible error in the experiment.

The heat input to the cell that would ordinarily be expected from electrolysis (the so-called "joule heating") is given by the expression:

(V-1.48) I

where V is the voltage applied to the cell, and I is the current passing through. The "I x 1.48" quantity here is the power lost by electrolytic production of oxygen and hydrogen. Because the cell is open to the atmosphere, this "power" in the form of potentially recoverable chemical energy simply escapes the cell.

If, for example, the current is 80 mA and the applied voltage is 2.25 volts, the joule heat input to the cell would be 61.6 mW. [An example used by Professor Farrell]. If the HC were 25 C/watt, the expected *temperature rise* of the cell due to the 61.6 mW input power would be 25 x 0.0616 = 1.54 C. If the temperature is observed to rise any more than 1.54 C, an unknown excess power source may exist in the cell. If, for example, the temperature were observed to rise 3.08 C, rather than only 1.54 C, this would represent 100% more heat than 61.6 mW coming from the cell, that is, 133.2 mW.

Excess powers on the order of 100-300%, calculated in this manner, are said to be readily achievable. As Professor Farrell has said, "We have never *not* gotten the effect." [With these general conditions.]

Caveat

This has been a tutorial for beginners by someone who has not done the experiment himself, but who has talked to the people who have. You should be able to go off on your own now and find bigger and better ways to achieve this. You might begin by trying pulsed power input, which supposedly increases the output. If you are a cold fusion skeptic, you should really relish this experiment. It offers an easily reproducible effect. If you can find a *trivial* explanation for the excess power, think how famous you'll be. At the very at least you'll be a very frustrated skeptic. Watch out!

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2. *Noninski, V.C., "Excess Heat During the Electrolysis of a Light Water solution of K_2CO_3 With a Nickel Cathode," Fusion Technology, accepted for publication in the March 1992 issue.*
3. *Mills, Randell L., William R. Good, and Robert M. Shaubach, "Dihydrino Molecule Identification," Fusion Technology, Vol. 25, January 1994, pp. 103-119.*

Two-balance method of Faraday efficiency measurement with external recombiner and open cell calorimetry for identifying origin of excess heat in Ni-H2O electrolytic cells

By M. Srinivasan and M.C.H. McKubre, SRI International, Menlo Park, CA 94025

It is now three years since the first reports of observation of "excess heat" by Randell Mills and his collaborators[1] during the electrolysis of a light water solution of K_2CO_3 in an open cell using nickel as cathode and platinum as anode. Since then, at least seven other groups [2-8] claim they have confirmed the generation of "excess power" in such Ni-H_2O cells. Most of these groups also have employed open cell calorimetry similar to that of Mills et al.[1] Bush and Eagleton[3] are perhaps the only group to have carried out extensive closed cell experiments which appear to confirm "excess heat" generation in such systems.

Noteworthy features of the Ni-H_2O cells, as described by those who have experimented with them, are: (a) they have very short initiation times, i. e., the "excess power," if present, appears within the first day of electrolysis and (b) the success rate of observing "excess power" is high compared to Pd-D_2O systems. On the whole, the system appears to be much more robust and easily amenable to experimental investigation.

Despite these favorable features, however, it is rather surprising that more groups have not undertaken study of such cells. This is probably because the majority of active researchers continue to look upon excess power claims in light water cells with skepticism, dismissing them as a "chemical effect" of the nickel/carbonate system, most probably due to recombination of H_2 and O_2 within the cell. Indeed, some unpublished studies of Faraday efficiency measurements in open N-H_2O cells carried out simultaneously with calorimetry suggest that the apparent "excess power" at modest levels (<=30%) in their cells could be attributed to recombination effects, or to an incorrect estimate of the system thermoneutral voltage due to electrochemical processes other than the electrolysis of water.

On the other hand, the originators of this concept, namely Mills et al, have presented[9] details of their Faraday efficiency measurements in a heat-producing cell, which clearly rules out recombination effects as the source of excess power, at least in their cells.

The wide disparity of claims and counter-claims has naturally given rise to confusion in the minds of those scientists who are earnestly attempting to interpret these experimental findings. A factor in resolving the question of whether the "excess heat" in Ni-H_2O cells is genuine, or due to an experimental artifact, is the existence of two diverse theories put forward to explain "excess heat" in these systems.

As is well known, Mills et. al.[1] claim that "excess heat" is due to the formation of compact hydrogen atoms (or "dihydrino molecules" as they describe it), while Robert Bush[1] has proposed that it is due to nuclear transmutation reactions involving a proton (from the hydrogen of H_2O) and alkalai metals. But the important point to be noted here is that according to Mills,[9] dihydrino molecules do not combine with oxygen to form water.

To shed more light on these questions, we propose a simple experiment which could possibly resolve most of the issues. The basic objective of the experiment is to measure simultaneously the mass of water lost from a cell due to electrolysis (Faraday efficiency), as well as mass of water reformed in a neighboring flask containing a large area Pt catalyst, into which the electrolytic gases are directed through flexible tubing. These two masses are to be measured while open-cell calorimetry is performed. The ouput of the recombiner flask is connected to ambient atmosphere via a water bubbler. The electrolysis cell and recombiner flask (along with attached bubbler) are placed separately on two independent electronic balances reading to an accuracy of 0.01g.

The interconnecting gas tubing between the electrolytic cell and recombiner flask is strung over a sturdy stand in such a way that it does not "load" the balances and result in erroneous balance readings. It is advisable to ensure that the tubing forms a smooth arc so that no condensed water can accumulate. After inserting the usual temperature sensors and electrode connection leads via the top plug of the electrolytic cell, all gas leakage paths are sealed. The water bubbler serves additionally as an on-line manometer monitoring system pressure, thereby confirming gas tightness.

There are four possible outcomes of such an experiment: (a) Mass of water lost from cell equals mass of water reformed in recombiner, and both correspond to the Faraday value. Simultaneously, if calorimetry confirms absence of "excess heat," then all is well and there is no anomaly to be explained. (b) However, in the above case when Faraday efficiency is 100%, if there is some "excess heat" observed, then one may postulate the presence of some new phenomenon, as suggested by Robert Bush. (c) Alternately, if mass of water lost equals mass of water reformed, but both fall short of the Faraday value, then clearly it must be due to recombination effects. If further calorimetry indicates some amount of "excess heat" generation, then it may be compared quantitatively with the heat due to recombination. (For the present, we rule out the possibility that in this case, wherein Faraday efficiency is less than 100%, there could be a situation wherein no "excess power" is observed with respect to [V-1.482]*I). (d) In the event, however, that the mass of water lost from the cell corresponds to the Faraday value but that of water reformed is lesser, then Mills' theory of formation of dihydrino molecules gains support.

This is what, in fact, Mills[9] claims for his cells. (Before concluding that there is indeed an imbalance between masses of water lost and gained, it has to be ensured that the Pt catalyst is functioning properly and that there is negligible gas leakage). [Note that both the electrolysis vessel and the recombiner flask, as well as all communicating tubing, contain potentially hazardous quantities of H_2 and O_2, and hence any experiment undertaken along the lines suggested above must be performed behind adequately assured shielding to ensure the experimentalist's safety.]

In conclusion, we suggest that this type of two-balance method of Faraday efficiency measurement with simultaneous open-cell calorimetry can help resolve the present impasse regarding the nature of the apparent excess power in Ni-H_2O cells. It would be particularly instructive to carry out the above experiment at both low and high current densities, and with the nickel electrode driven both cathodically and anodically with alkali hydroxide solutions in place of alkali carbonates. Such an experiment was set up at SRI International in March 1994 and the results, if conclusive, will be published in due course. We would like to encourage other interested experimenters to attempt such experiments carefully.

It may be noted that, strictly speaking, in order to establish that recombination is not the source of apparent "excess heat" a single balance for weighing the electrolytic cell would do. The second balance is required primarily to distinguish between the two postulated mechanisms of excess heat production, in the event that it is not due to recombination effects.

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M. Srinivasan is currently on leave of absence from the Bhabha Atomic Research Centre, Trombay, Bombay 400085, India.

Cold Fusion Patent News as of February 1994

Attorneys working in the cold fusion patent field in the U.S. report there are over 200 patent applications being held up in the U.S. Patent Office logjam. The reasons most often offered by patent examiners for rejection or delay are that "cold fusion" does not exist, therefore the application is based on a spurious phenomenon.

Cold fusion patent-filers regularly receive bad-press news clips from 1989 or negative passages from books written about cold fusion in the last five years. One wonders whether there is any historical precedent for this: the use of media news clips citing the negative views of antagonist scientists, five years in the past, to direct and over-rule the course of technology patenting?

The latest word, however, is that the hangup at the U.S. Patent Office is about to break, with at least one patent reportedly either granted or about to be granted. "Across the pond" in England, one cold fusion patent has already been granted, UK Patent GB 2,231,195 B for "Thermal power generation by electrically controlled fusion," granted to Professor Harold Aspden of Southampton University.

Patent application by Canon on proton conductor device:

A brief note in *Chemistry & Industry* (December 6, 1993, p.936) says that Mitsutoshi Hasegawa and Nagao Hosono of Canon in Tokyo used an alloy of calcium, mish metal (a mixture of rare-earth metals), nickel, and aluminum in a cold fusion experiment. When this material was inserted into a deuterium or hydrogen atmosphere and a low-voltage (about 100 V) arc was set up between it and another electrode, the investigators said they observed an increase in gamma radiation. The material also increased in temperature by an amount greater than could be explained by chemical reactions. They obtained a 20-fold enhanced effect by cycling the voltage, and have applied for a European patent under application #0568118.

Other cold fusion related patent applications recently cited in chemical abstracts

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- Matsumoto T, Harada H (Kurorin Engineers Kk; Mitsui Bussan); Jpn. Kokai Tokkyo Koho JP 05,134,098 15-Nov-91. Cited in Chem. Abstr. 119:258256 (1993). "Creation of elements from water by nuclear fusion."

A friendly patent attorney, in a position to know, wrote to us: Internationally, cold fusion research and related intellectual property activity continues an unrelenting movement toward overcoming the early negative bias associated with the discovery of Drs. Stanley Pons and Martin Fleischmann.

"Since March 1989, the United States Patent Office has received more than 200 cold fusion patent applictions. Worldwide, patent applications have been submitted by scientists from many countries including the Bahamas, Belgium, Brazil, Canada, France, Germany, Great Britain, Italy, Japan, Netherlands, Poland, Russia, Ukraine, as well as the United States. In addition, scientists are known to be actively pursuing cold fusion research in Belarus, Bulgaria, and India."

Scientific papers from the field

Each month, "Cold Fusion" will publish for its more technically inclined readers the abstracts of some of the most significant technical papers in the field, as well as in related areas. In this selection we do not intend to diminish the significance of many other critical papers which are too numerous to mention. Because this is the premier issue, we also list several significant papers that appeared in 1993 and in 1992.

"Nuclear product ratio for glow discharge in deuterium"

A.B. Karabut, Ya.R. Kucherov, and I.B. Savvatimova (Scientific Industrial Association, LUTCH, Moscow, Russia), Physics Letters A., Vol.170, November 9, 1992, pp.265-272.

New results for glow discharge in deuterium calorimetry are presented. In separate experiments a heat output five times exceeding the input electric power was observed. The result for the charged particle spectrum measurement is presented. Charged particles with energies up to 18 MeV and an average energy of 2-4 MeV were seen. Beams of gamma rays with energies of about 200 keV and a characteristic X-ray radiation were registered. The summed energy of the registered products is three orders of the magnituede short of the values needed to explain the calorimetric results.

"Calorimetry of the Pd-D_2O system: from simplicity via complications to simplicity"

Martin Fleischmann (Dept. of Chemistry, University of Southampton, Southampton, UK) and Stanley Pons (IMRA Europe, Sophia Antipolis, Valbonne, France), Physics Letters A., Vol.176, May 3, 1993, pp.118-129.

We present here one aspect of our recent research on the calorimetry of the Pd-D_2O system which has been concerned with high rates of specific excess enthalpy generation (>1 kW cm^{-3}) at temperatures close (or at) the boiling point of the electrolyte solution. This has led to a particularly simple method of deriving the rate of excess enthalpy production based on measuring the times required to boil the cells to dryness, this process being followed by using time-lapse video recordings. Our use of this simple method as well as our investigations of the results of other research groups prompts us to present also other simple methods of data analysis which we have used in the preliminary evaluations of these systems.

"Cold Fusion by Sparking in Hydrogen Isotopes"

Jaques Dufour (Shell Research S.A., France), Fusion Technology, Vol.24, September 1993, pp. 205-228.

Excess energy production, well above the background and in amounts of the same order of magnitude as the input energy, has been measured that has been caused by sparking in hydrogen isotopes between electrodes made of metallic hydride-forming metals (palladium and stainless steel). This excess energy production is stable over long periods (several weeks) and is observed with both hydrogen and deuterium. Only extremely low levels of neutrons and tritium have been detected, many orders of magnitude below what would be expected from the excess energy production measured. On the contrary, copious emission of low-energy radiation (likely to be beta rays) has been observed. A class of hypothetical nuclear reactions, based on the action of the weak electronuclear force, is proposed that accounts for all the experimental facts observed.

"Reduced radioactivity of tritium in small titanium particles"

Otto Reifenschweiler (Philips Research Laboratories, Eindhoven, The Netherlands), Physics Letters A, 184, January 3, 1994, 149-153.

By heating a $TiT_{0.0035}$ preparation consisting of extremely small monocrystalline particles (f ≈ 15 nm) a decrease of the radioactivity by 40% was observed. In further experiments the concentration of tritium in such preparations was varied (TiT_x experiments) showing that the radioactivity of the tritium increased less than proportionally to its concentration. Careful analysis of the experiments seems to rule out the possibility of trivial errors. A provisional hypothetical explanation is formulated. Our experiments may point to a connection with cold DD-fusion.

"Notes on Two Papers Claiming No Evidence for the Existence of Excess Energy During the Electrolysis of 0.1 M LiOD/D_2O with Palladium Cathodes"

V.C. Noninski (Fitchburg State College, Fitchburg, MA) and C.I. Noninski (Sophia, Bulgaria), Fusion Technology, Vol.23, July 1993, pp.474-476.

Technical correspondence (no author abstract) finds serious flaws in the "null results" on excess heat published in 1989 in *Science* and *Nature* by the Nathan Lewis et al group at Caltech.

"Dihydrino Molecule Identification"

Randell L. Mills and William R. Good (HydroCatalysis Power Corporation, Lancaster, PA), Robert M. Shaubach (Thermacore, Inc., Lancaster, PA), "Fusion Technology", Vol.25, January 1994, pp.103-119.

Three sets of heat production and "ash" identification data are presented. An exothermic reaction is reported wherein the electrons of hydrogen and deuterium atoms are stimulated to relax to quantized potential energy levels below that of the "ground state" via electrochemical reactants K^+ and K^+; Pd^{2+} and Li^+; or Pd and O_2 of redox energy resonant with the energy hole that stimulates this transition. Calorimetry of pulsed current and continuous electrolysis of aqueous potassium carbonate (K^+/K^+ electrocatalytic couple) at a nickel cathode were performed. The excess output power of 41W exceeded by a factor >8 the total input power given by the product of the electrolysis voltage and current. The product of the exothermic reaction is atoms having electrons in the ground state, which are predicted to form molecules. The predicted molecules were identified by their lack of reactivity with oxygen, by separation from molecular deuterium by cryofiltration, and by mass spectroscopic analysis.

"Inertia as a Zero-Point Field Lorentz Force"

Bernhard Haisch (Lockheed Palo Alto Research Laboratory, Palo Alto, CA), Alfonso Rueda (Dept. of Electrical Engineering, California State Univ., Long Beach), and H.E. Puthoff (Institute for Advanced Studies at Austin, Austin, TX), Physical Review A," February 1, 1994.

Under the hypothesis that ordinary matter is ultimately made of sub-elementary constitutive primary charged entities or "partons" bound in the manner of traditional elementary Planck oscillators (a time-honored classical technique), it is shown that a heretofore uninvestigated Lorentz force (specifically the magnetic component of the Lorentz force) arises in any accelerated reference frame from the interaction of the partons with the vacuum electromagnetic zero-point field (ZPF). Partons, though asymptotically free at the highest frequencies, are endowed with a sufficiently large "bare mass" to allow interactions with the ZPF at very high frequencies up to the Planck frequencies. The Lorentz force, though originating at the sub-elementary parton level, appears to produce an opposition to the acceleration of material objects at the macroscopic level having the correct characteristics to account for the property of inertia. We thus propose the interpretation that inertia is an electromagnetic resistance arising from the known spectral distortion of the ZPF in accelerated frames. The proposed concept also suggests a physically rigorous version of Mach's Principle. Moreover, some preliminary independent corroboration is suggested for ideas proposed by Sakharov [1] and further explored by one of us (Puthoff [2]) concerning a ZPF-based model of Newtonian gravity, and for the equivalence of inertial and gravitational mass as dictated by the Principle of Equivalence.

"Calorimteric Principles and Problems in Measurements of Excess Power During Pd-D_2O Electrolysis"

Melvin H. Miles (Chemistry Division, Research Department, Naval Air Warfare Center Weapons Division, China Lake, CA), Benjamin F. Bush (SRI International, Menlo Park, CA), David E. Stilwell (Connecticut Agricultural Experiment Station, New Haven, CT), J. Physical Chemistry, February 17, 1994.

A major experimental problem in many isoperibolic calorimetric studies is the fact that the decrease in the electrolyte level due to electrolysis produces a significant decrease in the apparent calorimetric cell constant if the temperature is measured in the electrolyte of the electrochemical cell. Furthermore, heat transport pathways out of the top of the cell can produce large errors, especially at low power levels. There is no steady state in electrochemical calorimetry, so accurate results require the evaluation of all terms in the differential equation governing the calorimeter. These factors have contributed to the controversy involving measurements of excess power during Pd-D_2O + LiOD electrolysis experiments. A critical analysis is presented for several key publications that have impacted this scientific topic.

"Reproducible tritium generation in electrochemical cells employing palladium cathodes with high deuterium loading"

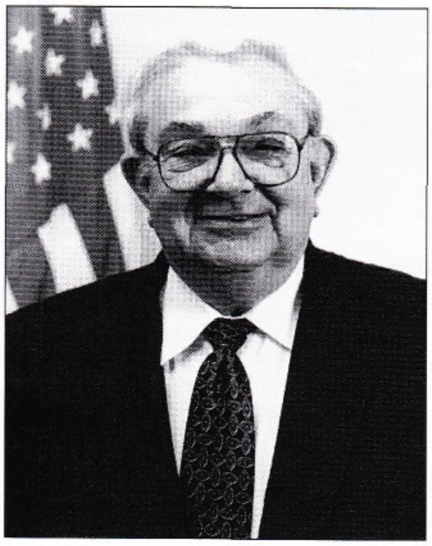
Fritz G. Will (Department of Chemical Engineering, Univ. of Utah), Krystyna Cedzynska (Technical University of Lodz, Poland), Denton C. Linton, Journal of Electroanalytical Chemistry, Vol. 360, 1993, pp.161-176.

"Reproducible tritium generation well above background has been observed in tightly closed D_2SO_4-containing cells in four out of four Pd wire cathodes of one type. Tritium analysis was performed before and after each experiment on the Pd, the electrolyte, and the gas in the head space. No tritium generation was observed in four identical Pd cathodes in H_2SO_4 cells operated at the same time under the same conditions. A cyclic loading-unloading regime with low current densities, rather than the usual continuous constant current regime, was employed to attain D/Pd and H/Pd loadings of 1 ±0.05 reproducibly. D/Pd loadings greater than 0.8 ±0.05 appear to be necessary to generate tritium. The largest amount of tritium, generated in 7 days of continuous electrolysis, was 2.1 x 10^{11} tritium atoms, compared with a background of 4 x 10^9 tritium atoms. The concentration of tritium and its axial distribution in the Pd were determined and concentrations of up to 9 x 10^{10} atoms/g Pd were found compared with a maximum background of 5 x 10^8 atoms g^{-1}. The T/D ratio in the Pd is about 100 times larger than in the electrolyte or gas, and indicates that tritium generation occurs in the Pd interior rather than at its surface. No tritium generation was observed in two other types of Pd electrodes in D_2SO_4, despite the attainment of D/Pd ratios near 1:1. Thus high D/Pd ratios appear to be a necessary but not sufficient condition for tritium generation in D_2SO_4 electrolysis."

The scientific literature is expanding at such an accelerated pace that "Cold Fusion" Magazine may not always receive notice of every technical publication's material that is germane to our publication's scope. Readers are encouraged to bring scientific articles to our attention that may have a bearing on any of the explorations in this field.

A profile of things to come?

The Washington, D.C. labyrinth may soon have a warmer response to cold fusion. An important, but not widely circulated, statement was released on June 22, 1993, by press secretary Rick E. Borchelt of the House Committee on Science, Space, and Technology, who was acting on behalf of Congressman George Brown (D-Calif.), Committee Chairman. Borchelt gave permission to Professor Mary Marcus of the University of Oklahoma School of Journalism to attribute this to Congressman Brown. Brown's statement followed inquiries about what the House Science, Space, and Technology Committee intended to do about the increasing number of experimental confirmations of "cold fusion" phenomena.



Congressman George Brown (D-CA), Chairman, House Space, Science & Technology Committee.

Congressman Brown's statement:

"The Committee hearing on fusion energy provoked considerable interest, both from supporters and detractors of electro-chemical fusion (a more precise term than "cold fusion"). The witnesses who presented testimony on electro-chemical fusion may have created some additional interest from members and the audience, but also sparked violent disagreement from other scientists on the panel who are engaged in current fusion programs.

"The hearing did prompt a decision to conduct an electro-chemical fusion demonstration project at one of DOE's national laboratories. Plans are now under way to carry out such a project pending receipt of a credible scientific plan describing procedures, equipment, methods of analysis, and methods of measurement. To date, such a plan has not been presented.

"I believe we will proceed with a demonstration project, and in the meantime will reserve judgment until the project is under way and produces scientific results. In the interim, the Committee will remain cautiously optimistic that some advances in electrochemical fusion might obtain from the project. We are strongly dedicated to the advancement of energy sciences."

The fusion hearings to which Brown referred were held on May 5, 1993, before the Subcommittee on Energy of the HSST Committee. For the first time since 1989, several "cold fusion" scientists were allowed to make statements before a subcommittee of the HSST Committee. Dr. Edmund Storms (Los Alamos National Laboratory, ret.) and Dr. Randell Mills of HydroCatalysis Power Corporation were the two non-hot fusion scientists who made statements on May 5.

The hearing documents and transcript has been compiled in a 617-page book, "Fusion Energy—Hearing before the Subcommittee on Energy of the Committee on Science, Space, and Technology, U.S. House of Representatives," published May 5, 1993, No.38, for sale by the U.S. Government Printing Office, ISBN 0-16-041505-5. Ask your Congressman for one. It's a good read, we promise you!

Congratulations were in order

The following is a letter of December 23, 1992, sent to Hazel O'Leary, then-U.S. Secretary of Energy Designate from "Cold Fusion" *Magazine editor Dr. Eugene F. Mallove.*

Hazel O'Leary

Dear Mrs. O'Leary:

Congratulations on your selection to be the next U.S. Secretary of Energy. Knowing something about your background from my colleague Mr. Mark Hugo, an engineer with Northern States Power Company, I am confident that the Department of Energy will be in much better hands than it has been.

Mark Hugo and I, as well as hundreds of other sincere and dedicated scientists and engineers around the world, share the conviction that cold fusion power has been confirmed as a spectacular new energy source beyond any reasonable doubt. This new energy source is being developed in Japan with a vengeance. Unfortunately, cold fusion has not fared very well in th U.S., largely because of vested interests—the hot fusion program, etc.

I'm sure that Mark Hugo, who is an excellent engineer and cold fusion investigator, will be able to brief you extensively on the subject of "what went wrong" here in the U.S. The bottom line is that the DOE cold fusion panel report of 1989 was a travesty. It has been downhill for the U.S. ever since in this exciting new field.

You will, I hope, take firm measures to change this unfortunate situation. May I humbly suggest that among them should be these actions:

1. Postpone or cancel large components of the hot fusion program and

arrange to have some or all of the funds saved go toward cold fusion research and development. This should be done carefully, because there is much good in some of the plasma physics work that occurs in hot fusion. But, in my view, the tokamak and inertial confinement fusion programs are white elephants that have been made completely obsolete by cold fusion.

2. There are literally hundreds of cold fusion researchers in the U.S. who need immediate support at universities, federal laboratories, and in small businesses. The required funding will certainly be in the range of $20-$100 million. It is not necessary perhaps that DOE should provide all of this, because with DOE encouragement—and non-interference—private corporations will soon be involved in full force. There are, however, scores of instances in which federal laboratory and university researchers have been denied support because of the current blanket DOE position of refusing to fund cold fusion. In some instances, successful work already under way was actively interfered with by DOE administrators!

3. Since there will be anti-cold fusion hold-outs at federal laboratories and in the DOE administration who have actively opposed cold fusion, you should promote an internal policy that strictly prohibits interference with investigators of this frontier science. Also, bear in mind that it would not make sense "to give the fox the keys to the chicken coop" by turning over significant involvement with cold fusion to those, such as the hot fusion people, who have so selfishly opposed cold fusion.

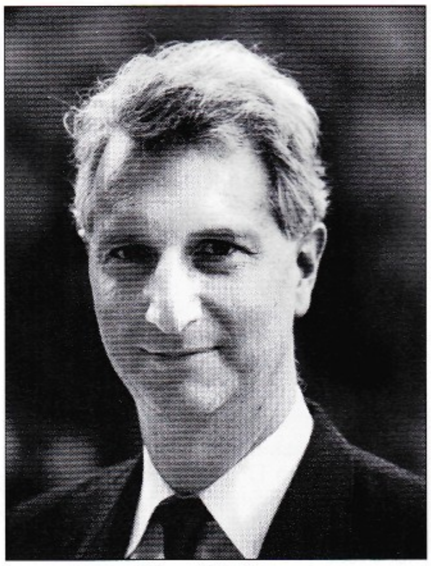
4. Establish as early as possible a regulatory policy that will insure public health and safety, but which will not obstruct development of cold fusion technology.

5. Investigate why the U.S. Patent Office is obstructing scores of patent applications for cold fusion and cold fusion related technology. I am convinced that there has been DOE meddling in the patent process in this area.

I know that you will be burdened by many other issues as you begin your work after confirmation by the Senate, but I hope you will come to appreciate how central cold fusion energy may become to our energy well-being. Cold fusion is every bit as important—probably more so—as energy conservation and increased use of natural gas. If there is any way that I can help your efforts, please don't hesitate to contact me.

Ira Magaziner speaks out

In recent years, letters from cold fusion researchers to Ira Magaziner, President Clinton's senior advisor for policy development on health care, have gone unanswered. In April, 1989, Rhode Island-based industrial consultant Magaziner voluntarily (and without pay) helped promote the University of Utah position before the House Science, Space, and Technology Committee. Shortly afterward, DOE coopted the process and issued an unfavorable report on cold fusion in November, 1989. Since then, Magaziner has taken lots of flak from cold fusion opponents, which may explain his gun-shyness.



Ira Magaziner

Nonetheless, in the *National Journal* of December 12, 1992, (p.2,837) we

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ADVERTISERS' INDEX

News Coverage of The Fourth International Conference on Cold Fusion (and Lack Thereof)

COMPILED BY EUGENE MALLOVE, SC.D.

THE WALL STREET JOURNAL.

The Wall Street Journal

The Wall Street Journal, Thursday, December 9, 1994, page B10 published a strangely written piece by Jerry Bishop (uncharacteristic of him), though to his credit he was the only national news reporter present. Titled, "Japanese Funds Warm a Conference of 'Cold Fusion' Die-Hards in Maui." Why "die-hards" and not "scientists"? Also, why was it said—terribly in error—that the point of the Japanese effort was to "find out if cold fusion is real?" Bishop was given the complete set of slides that Dr. K. Matsui used in his December 6 presentation. These detailed slides completely contradict Bishop's characterization. Bishop's lead is: Cold fusion "believers" who "have been sipping gratefully on a Japanese tonic at this luxury resort..." Makes it seem like the scientists were having a luxury vacation and hanging on word of Japanese money, not the science that was presented.

Then there's negativist Steven Jones's retraction of his low level neutron claims. And the supreme obfuscator, Professor John R. Huizenga, is quoted: "I still demand reproducibility and I don't see it forthcoming." The conference featured numerous papers that spoke of full-reproducibility. Just to round out this treatment of ICCF4, the article ends with a mention of CERN's Douglas Morrison: "Nothing in the meeting changed his mind that cold fusion is a case of 'pathological science' where scientists are seeing only what they want to see."

Why should the 1% of the attendees at the conference, who were obvious negativists get 40% of the column inches? Why did Bishop give such cursory coverage to a conference packed with so many new developments?

Jerry Bishop wrote a second article for the *Journal*, December 10, 1993, B5, titled "Alchemists' Ancient Dreams Hover Over 'Cold Fusion' Parley in Maui." This piece mentioned the transmutations of elements that Professor Bush and Eagleton at California Polytechnic University claimed in light water excess heat experiments, as well as results from Hokkaido University. The only other supporting evidence for transmutation reported by Bishop was at Professor John Bockris' lab at Texas A&M University. The article segued into the controversy surrounding Bockris and Texas A&M faculty seeking to have him stricken from the "Distinguished Professor" ranks. The transmutation work of Yan Kucherov and others, reported at ICCF4 and documented in *Physics Letters A*, was not mentioned.

The New York Times

The New York Times

The New York Times ignored the ICCF4 conference, even though both their Japan reporter (Andrew Pollack) and NYC former "cold fusion beat" man, William Broad, were informed of the conference.

The New York Times offered only the book review section of December 5, 1993, in the list of the year's 400 "Best Books of 1993," third book down from the top of the science list, "Bad Science" by Gary Taubes. The *Times* summary: "It seemed too good to be true and it was, showing that true scientists, properly motivated, can deceive themselves like anyone else."

Quoting the *Times's* science section's lead story headline December 7, 1993, p. B7: "Reactor Passes Point of No Return in Uphill Path to Fusion Energy," by Malcolm W. Browne. It's a story about the Princeton TFTR tokamak about to be fired up with 50-50% D-T fuel, thus making the reactor intensely radioactive (for which we all shall eventually pay the cleaning bill). A candid admission by Dale Meade, deputy director of the Princeton laboratory: "We ourselves may not live to see a commercial fusion reactor in operation, but we have a chance of leaving fusion technology to our grandchildren [It used to be "children" not "grandchildren".] It would be nice if they could inherit something besides deficits in energy and the Federal budget."

The *Times'* op-ed piece, Monday, December 6, 1993, p.A11, by Hans A. Bethe of Cornell University: "New Lives for Nuclear Labs" contained an interesting line: "The labs could free us from our dependence on fossil fuel, especially for road transport. They could work on designing cells in which liquid fuel is directly converted into electricity to drive engines, greatly increasing efficiency and decreasing pollution."

And another quote from the piece: "Once a general plan has been approved, the lab should be left largely to itself, without the micromanagement from Washington that has become endemic and stifles creativity."

The December 11, 1993, p.10, *Times*carried the piece, "Into a New Frontier After Fusion Success," by Malcolm W. Browne (a funny remark in an article that generally lavished praise on hot fusion): "But contrary to popular notion that simple sea water could serve as a source of hydrogen fusion fuel, the fuel required by a real thermonuclear reactor would probably be neither free nor abundant." The director of Princeton's lab, Dr. Ronald Davidson, was quoted: "We probably won't see deuterium fusion reactors until some future century." He was speaking of pure D-D reactions, not D-T. Davidson wasn't at Maui to hear about Roger Stringham's ultrasonic D-D reactor

The December 11, 1993 p.23, *Times* op-ed by Lyman Spitzer, Jr., headlined "Harnessing the Sun." Spitzer wrote: "With a steady flow of money, the year 2025 seems a reasonable goal for putting a working fusion plant into operation. Fusion is a field where patience is necessary. But it is also a field where periodic breakthroughs, like this week's, move us ever closer to the dream of safely harnessing the power of the sun here on earth." Too bad Spitzer was referring to the wrong "breakthrough" (the Princeton work) and not the real breakthrough—the Maui conference.

Other Recent Cold Fusion Coverage

American Scientist

American Scientist, the journal of Sigma Xi, The Scientific Research Society, has largely ignored cold fusion. But its January-February, 1992, issue knocked cold fusion in a thoroughly ill-informed account, part of "Case Studies of Pathological Science," by Denis L. Rousseau, identified as a "Distinguished Member" of the technical staff at AT&T Bell Laboratories. Rousseau's assessment considers none of the results post-1989, and doesn't even handle 1989 very well. He writes: "Cold fusion was doomed from the start when a race to be first took precedence over the desire to be right."

In the magazine's January-February, 1993, issue, we have one anti-cold fusion author, Frank Close ("Too Hot to Handle"), reviewing the book of another critic, John R. Huizenga, "Cold Fusion: The Scientific Fiasco of the Century." This "patting on the back" among cold fusion critics has become the norm in publications such as *Science* and *Nature*. *American Scientist* bannered the Close review, "From Farce to Scientific Fiasco." Struggling to find legally defensible language to accuse Pons and Fleischmann, in effect, of fraud, Close describes one diagram in the original Pons and Fleischmann paper thus: "...whose origins were ethically disoriented."

Close, the high-energy theoretical physicist, rails against "huge expenditures of public and private monies on fruitless research" in cold fusion. Close calls for a "house-cleaning" against cold fusion: "Do we regard this fiasco with detached resignation, or express more strongly our dissatisfaction with the deceptions, exaggerations, and ethically disoriented presentations that stimulate vast diversion of international resources? . . . If science does not insure (sic) that its own house is in order, who will?"

Canadian Broadcasting Corporation

By far the best television documentary yet on cold fusion was broadcast across Canada on the evening of Thursday, June 24, 1993. "The Secret Life of Cold Fusion," a half-hour program, was produced by the Canadian Broadcasting Corporation, airing during *CBC Prime Time News*. Narrated by Canadian television science journalist Jerry Thompson, with Robin Christmas as producer, the Canadian program featured extensive reporting from laboratories in Japan, France, Russia, and at SRI International in the U.S.

The program proved to be a stunning eye-opener for those who have not closely followed cold fusion developments. Though at least four-dozen copies of the program were subsequently sent to U.S. television media outlets, television networks, and print media, not a single complete televised rebroadcast of the program occurred in the U.S. Those who wish to receive a video tape copy of this excellent program should contact publicity at CBC News in Toronto (416-975-7831)

L'Express International

The noted French-language magazine ran a three-page article in its May 6, 1993, edition (p.14-16), "Cold Fusion - yet giving heat!" The article reports on recent advances by Fleischmann and Pons in their Japanese-financed laboratory in Sophia-Antipolis, near Nice, France. The article also highlights the work of Dr. Jaques Dufour of Shell Oil Corporation, France. It quotes professor Jean-Paul Vigier, research director at the French nuclear science center, CRNS, and an editor at *Physics Letters A*.

Said Vigier of the May 3, 1993, scientific paper by Fleischmann and Pons, "If I gave my green light for publication, it is indeed because it is a paper of high quality. After this, you can't go on ignoring it."

Los Angeles Times

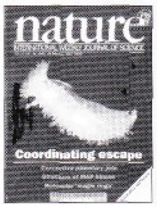
The front page of the Book Review section of August 22, 1993, offers a laudatory review of Gary Taubes's "Bad Science" with the headline, "Cold Shoulder to Science." The reviewer is James Gleick, former *New York Times* science writer and author of the Richard Feynman biography, "Genius."

Among other assaults on cold fusion, Gleick attacks the generally respectable (and award-winning) cold fusion coverage by Jerry Bishop of the *Wall Street Journal*. Gleick was especially miffed by Bishop's cover story article in the August, 1993 *Popular Science*." He called it an "all-to-familiar example of the 'If True' style of journalism typical of the affair, by the same *Wall Street Journal* reporter whose rose-colored coverage played such a central role in cold fusion's start." Gleick dubs cold fusion "one of our century's grandest hoaxes."

He follows so many other science journalists who were deceived by the negative views of quick-on-the trigger skeptical scientists, and their poor or deceptive 1989 experimental results. Gleick has violated his own rule: "... not to be scared away from using your own judgment—don't abdicate responsibility to the experts. Just because the jargon is incomprehensible doesn't mean that the basic facts are."

Gleick and Taubes got the cold fusion story all wrong. They did abdicate responsibility to "instant experts," attacking the real experts in whom they preferred not to believe.

Nature



Since March, 1989, *Nature* magazine has had nothing good to say about cold fusion. It has editorialized against cold fusion research at every turn, and following this editorial policy, published no scientific papers showing evidence for cold fusion since the weak initial neutron results of Professor Steven Jones at Brigham Young University in 1989. (Cold fusion researchers, eager to publish their results, no longer even waste their time knocking on Nature's door.)

In fact, as *"Cold Fusion"* Magazine will report in a future issue, *Nature* has refused to publish letters of technical correspondence from independent scientists, which find serious errors in a so-called "null result" cold fusion calorimetry experiment done in 1989 at Caltech and published in the magazine.

Nature loves to review anti-cold fusion books. The issue of August 5, 1993, offers a review of Gary Taubes's "Bad Science" by Nicholas Wade, the head of the *New*

York Times science reporting unit. Under the banner "The good, bad and ugly," Mr. Wade reveals his colors and weighs in harshly against cold fusion. This may explain the absence of appropriate *New York Times* coverage of cold fusion.

Wade says of Drs. Pons and Fleischmann: "What they had was nothing. Yet they started an avalanche that swept up scientists all over the world, mesmerizing gullible backers from the Utah state legislature to the Electric Power Research Institute, and wasted tens of millions of dollars of seemingly not-so-scarce research funds. Even now the rubble has not completely ceased to jitter."

Oblivious to the present facts about cold fusion research, Wade pontificates: "Just as physiologists learn about the body's normal functions from its pathology, young researchers might usefully study this book, since it bears compelling witness to the human mind's irrepressible propensity for self-delusion." Make up your own mind. Wade and *New York Times* science writer William Broad are co-authors of the 1982 book, "Betrayers of the Truth—Fraud and Deceit in the Halls of Science."

New Scientist

In recent months, *New Scientist* has been a useful harbinger of development relating to cold fusion. Witness the magazine's reporting January 8, 1994, p.8, of the startling technical paper by Otto Reifenschweiler in Physics Letters A (see a report on Reifenschweiler's work in this issue). New Scientist, in general, has adopted an anti-cold fusion stance. In an article last spring there was serious mis-reporting of the experimental results contained in the paper by Drs. Fleischmann and Pons in Physics Letters A of May 3, 1993. ("Cold Fusion" Magazine in a future issue will explore this serious journalistic departure, and the failure of New Scientist to correct it.)

The September 11, 1993, edition of *New Scientist* (pp.49-50) carries an essay by Francis Slakey, the science policy administrator for the American Physical Society, and physics professor at Georgetown University. The essay, "When the lights of reason go out—Francis Slakey ponders the faces of fantasy and New Age scientists," contains this gem about cold fusion. It would be amusing, if it were not meant to be taken seriously:

"Sometimes the faithful don't completely turn off their reason, but become captive to a fantasy they hear in one ear, but listen for science with the other ear. So begins a deterioration that dims the wits but leaves a zealous heart beating—the result is a cult of fervent halfwits. Some of them believe the universe is only 6,000 years old. Some sing praises to satellites. Some claim to fuse hydrogen in a jar.

"Cloistered in southern France are the cold fusion team of Martin Fleischmann and B. Stanley Pons. While every result and conclusion they publish meets with overwhelming scientific evidence to the contrary, they resolutely pursue their illusion of fusing hydrogen in a mason jar. They warn of fireballs that will be hurled from closed-cell experiments. They promise to produce an energy source by the end of the year that can power a home for 10,000 years. And a few scientists, captivated by the team's fantasy and exile, pursue cold fusion with Branch Davidian intensity."

Newsweek

The widely distributed edition of Newsweek of July 19, 1993, contained no news about cold fusion. Yet an excellent piece, "Cold, But Not Dead—Scorned as bad science, cold fusion makes a comeback," by science writer Robert F. Service, appeared in a special edition of the magazine. Tens of thousands of Newsweek subscribers who wish periodic in-depth reports on science and technology received this several page Newsweek Focus update. If only Newsweek's regular subscribers had received this information.

The New York Times

The New York Times, again

The New York Times has not published any cold fusion news since its anomalous and well-written November 17, 1992, article in its Tuesday "Science Times," section in a piece titled, "Cold Fusion, Derided in the U.S., Is Hot in Japan."

A letter to the editor by Eugene Mallove in March 9, 1993, A18, headlined "An Idealist's New Task: To Revamp Health Care," stated, "Your otherwise excellent Feb. 26 profile of Ira C. Magaziner, President Clinton's senior adviser for policy development, does a great disservice to him in belittling his prophetic testimony to Congress on cold fusion in 1989.

"Mr. Magaziner was right in 1989. As he warned, and a *Science Times* article last November 17 confirms, Japan has made the quest for cold fusion commercialization a priority, while support in the United States lags.

"The obsolete Department of Energy negative report on cold fusion of 1989 doomed the United States to inaction, except for the Electric Power Research Institute and a handful of other companies. The Energy Department spends no money on cold fusion research and the Patent Office—following Energy's line—refuses to grant patents to scores of inventors. Instead, for more than $500 million a year the Energy Department pursues a doomed quest for commercial *hot* fusion power in 2050. Cold fusion applications will emerge *this* decade."

THE OBSERVER

The London Observer

"Western skeptics hand Japan cheap power on a plate," headlined the December, 6, 1992, p.33, opinion piece by Michael White. Following his report on highlights of the Third International Conference on Cold Fusion in Nagoya (October 1992), White concludes: "One thing is certain. If cold fusion does become a reality, its creator and those who control the patent will become the wealthiest people who ever lived. Maybe as the most successful industrialized nation of the 1990s, the Japanese have money to throw away on such fantasies. But, if they succeeded in their search, it would not be the first time that oriental ingenuity had paid off through persistence and hard cash—at the expense of Western skepticism."

Physics Today

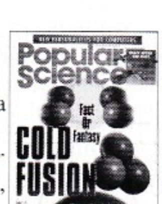
The magazine of the American Physical Society, which for a long time has had essentially a complete blackout on reporting cold fusion results and conferences, occasionally finds room to review cold fusion books. The September, 1993, issue had a review of Gary Taubes' "Bad Science" by Peter Bond of Brookhaven National Laboratory. It begins with: "Gary Taubes's 'Bad Science' is not the first book to conclude that shoddy scientific research and overblown claims were associated with cold fusion..." Other negative books were mentioned. No mention of the positive book, "Fire from Ice."

The physicist makes no references—not even negative ones—to continued activity in the cold fusion field. For the uninformed (or willfully neglectful) Dr. Bond, as for so many other physicists, cold fusion is a dead issue.

Bond whines about all that DOE money spent in 1989 to investigate cold fusion: "Taubes' statement that 'DOE would cover the expenses of cold fusion research' could be misinterpreted. The national labs were told to use their own resources; there was very little if any new DOE money directed to research on cold fusion. To be sure, other research certainly was delayed, and if one adds the cost of researchers' time, DOE support of cold fusion was considerable."

In the January 1993 *Physics Today*, we have cold fusion opponent, David Williams, formerly of Harwell Laboratory in England, reviewing John R. Huizenga's book, "Cold Fusion: The Scientific Fiasco of the Century." Perhaps because of being ill-informed Dr. Williams writes, "Now investigations of so-called cold fusion are confined to only a few laboratories." His weak and misleading conclusion: "Indeed, it is important to say that there do seem to be some good measurements which indicate the possibility of an interesting phenomenon. But what profit is there in such an inefficient, unreliable, dangerous, and expensive energy storage method?"

Popular Science



The honor of being the first U.S. magazine to run a cover story on cold fusion since 1989 goes to *Popular Science*. The August, 1993, issue's cover reads: "Cold Fusion: Fact or Fantasy—Cheap, abundant, non-polluting energy source? Or pseudoscience? The debate refuses to die." This is an exemplary and nicely balanced article by Wall Street Journal reporter Jerry Bishop. Editor-in-chief Fred Abatemarco writes in the issue: "Bishop's report on the fact-or-fantasy debate over cold fusion should convince you that in science, as in baseball, it ain't over till it's over."

Following its August, 1993, issue, *Popular Science's* September issue ran a feature article, "Hydrogen: The Forever Fuel," which delves into conventional aspects of hydrogen power as a pollutionless energy source—*after* it has been produced from electrolysis by energy sources that in many cases do pollute.

It was good that *Popular Science* inserted in this article a provisional, though not entirely accurate remark: "Dramatic breakthroughs that would diminish the cost of hydrogen produced by electrolysis of water—high-efficiency photovoltaics or cold fusion—have not materialized, leaving that route still too costly to compete with fossil energy sources." In the coming months, *"Cold Fusion"* Magazine readers will learn just how cold fusion will make hydrogen, indeed, "the forever fuel."

Science Magazine

Science, the weekly magazine of the American Association for the Advancement of Science, has been no friend of cold fusion. Since Science chose not to publish the following letter, in response to another assault on cold fusion, we reprint it here:

Richard M. Crooks praises as "definitive if not [a] fully objective" Taubes' "Bad Science: The Short Life and Weird Times of Cold Fusion" in January 7, 1994, pp.105-106). Science magazine now has a perfect record for having cold fusion opponents review cold fusion books. No proponents need apply.

The Crooks review raises an interesting question. Since the very title of Taubes' book implies the death of cold fusion, how can such a book be "definitive" and anywhere near objective, if world-wide research on the phenomenon of excess energy and nuclear reactions at room temperature continues—and is, in fact, accelerating?

Perhaps Dr. Crooks was unaware of the recent Fourth International Conference on Cold Fusion on Maui (December 6-9, 1993), which was sponsored by EPRI. So "dead" is the field that parallel scientific sessions had to be held to accommodate all the presentations. No reporter from *Science* was there (nor for the conference in Nagoya, Japan in October, 1992). This perhaps explains the widespread misinformation that cold fusion research is "pathological science."

If Taubes, a contributing correspondent for *Science*, is such an expert on cold fusion, why wasn't he at either conference? Cold fusion must be *very* pathological indeed. It has apparently "fooled" a host of major Japanese, Chinese, Russian, and American research organizations and corporations. At the Maui conference the major Japanese organization responsible for alternative energy, NEDO, outlined in meticulous detail Japan's cold fusion program and the tens of millions of dollars that it intends to devote to this new energy source.

Further evidence of the "death" of cold fusion: The Fifth International Conference on Cold Fusion will be held in April, 1995, in Nice, France near the IMRA Europe S.A. laboratory where Drs. Fleischmann and Pons continue their Japanese-funded work. The Sixth International Conference on Cold Fusion (mid-1996) will be held in Beijing. Dr. X.Z. Li, who heads the Chinese hot and cold fusion programs took the lead to have ICCF6 in China.

Dr. Crooks writes in his review: "We move between the main players at the big-name institutions (MIT and Caltech) and, as Taubes paints them, the somewhat suspect universities in the provinces (Utah and Texas A&M)."

It happens that MIT and Caltech were both wrong and the "provincials" were right. Dr. Crooks should consult the recent published technical literature that calls into serious question both the "null" results for excess heat of MIT and Caltech, and he should examine the several definitive papers that show reproducible "cold" generation of tritium. The latter belie Taubes' well-known accusation of possible fraud at Texas A&M (June 15, 1990, *Science*). Incidentally, Dr. Crooks was a team member on the published MIT Plasma Fusion Center/Chemistry Department 1989 study of excess heat, whose methodology has now been proved faulty at best.

By the time Dr. Crooks and Mr. Taubes wake up from their self-induced intellectual stupor, their worst nightmare will have become a reality: practical technological applications of "cold fusion" energy. It will be interesting to see how these people will explain themselves. One thing is certain: they will have an excuse, but it won't hold any water.

In fairness to Science, we note that it did publish a factual article (with minor errors) on the advent of *"Cold Fusion"* Magazine. The unsigned piece (we suspect its author) was in *Science's* "Random Samples" section, in the February 4, 1994, issue (pp.606-607). The article opens: "Now that cold fusion has faded from the front pages, it's tough to find reliable news on this seemingly unreliable subject. But the January mail may have brought a solution: an invitation to subscribe to the first monthly magazine dedicated to cold fusion."

Not to be too generous to *Science*, we note that the "Random Samples" section of December 10, 1993, p.1,643, manages to put a negative spin on a positive cold fusion development. "Utah Puts Fusion Out in the Cold" suggests that the main point of ENECO's acquisition of licensing rights to the University of Utah's cold fusion patent applications is that "Utah taxpayers are out of the business of paying for it."

Ivan Amato's report, "Pons and Fleischmann Redux?" (*Science*, May 14, 1993, p.895) also has unkind words for cold fusion, giving prominence to the skeptics' view of Pons and Fleischmann's recent work: "..they say the new work contains baffling assumptions and complicated arguments, fails to document key controls, and leaves the field where it has been since the beginning: in a state of confusion, albeit hovering around the remote possibility of an intriguing new phenomenon in solid state science."

Cold fusion researcher, Professor Richard Oriani of the University of Minnesota, found it necessary to write in response that his views were misrepresented by Amato (*Science*, July 16, 1993, pp.279-278).

Science Friday, National Public Radio

Radio science commentator Ira Flatow presents excellent science programs broadcast on Friday afternoons by many NPR affiliate stations carrying "Talk of the Nation." Flatow has kept an open mind about cold fusion. On June 25, 1993, he featured via phone-link cold fusion researchers Professor Peter Hagelstein of MIT, Dr. Michael McKubre of SRI International, and Dr. Melvin Miles of the Naval Air Warfare Center, China Lake. Also appearing were outspoken cold fusion critic Professor John R. Huizenga of the University of Rochester, and Professor Bruce Lewenstein of Cornell University, who is studying the scientific sociology of the cold fusion controversy.

The program allowed listeners to hear how cold fusion researchers are pushing the frontiers of the science. It also let them hear Professor Huizenga decry all cold fusion research. At one point Dr. Huizenga even suggested that EPRI's funding of SRI International cold fusion research was, in effect, inappropriately "taxing" the electric power consumer.

Flatow challenged Huizenga at the end of the program: "Four years later, you don't think the situation has changed?" Huizenga answered, "The situation is much worse now." [Those wishing to order an audio tape of this one-hour program should call, NPR Tapes, 202-822-2323, and refer to the June 25, 1993, broadcast date.]

"Cold fusion research is alive and well—but not in the mass media." —
Professor Mary Marcus

The St. Louis Journalism Review

Professor Mary Marcus of the University of Oklahoma School of Journalism published an excellent review of the media's treatment of cold fusion in the February, 1993, issue of *The St. Louis Journalism Review*, titled "Cold fusion research is alive and well—but not in the mass media." We expect that Professor Marcus will soon be publishing more fine work on this topic which will merit mention in *"Cold Fusion."*

THE SUNDAY TIMES

London Sunday Times

This distinguished British newspaper ran an exceptional, accurate, and balanced cover story on cold fusion in its magazine section of June 27, 1993, "Storm in a Bucket—What happened to the 'greatest scientific discovery of the century'?" The article, by science reporter Neville Hodgkinson, who visited with Fleischmann and Pons in their spacious Japanese-funded laboratory near Nice, France, concludes: "The cold fusion story raises some major questions about the scientific establishment's openness to new ideas, and industry's willingness to fund research into them."

The piece quotes Professor John Bockris of Texas A&M University: "Solid state nuclear physics has been born. A great law we all used to believe in, that nuclear reactions can only take place at huge temperatures, is not true. That is the shibboleth we have only just got over."

Readers are invited to send along to "Cold Fusion" Magazine any press items or broadcast media references that come along.

LETTERS

Continued from page 23

explanation? By the way $13CH_4$ shows no concentration anomaly with respect to ordinary methane.

It is known that Jupiter radiates more energy than it receives from the sun. The "excess heat" corresponds to 1018 watts or the hypothetical fusion of 1030 atoms of deuterium per second. Jupiter's mass is 2×10^{27} kg. If this were hydrogen, with the same deuterium composition as the atmosphere, then the current power output could last as long as 4×10^{10} years. This is rather longer than the age of the solar system, but is probably an overestimate as the core of Jupiter may contain less deuterium than supposed, due to imperfect mixing and the presence of other material.

Assuming a higher fusion rate in the past, due in part to a higher deuterium concentration, it is not unreasonable to suppose that the primordial concentration at the birth of the solar system was an order of magnitude higher than that measured in the Jovian atmosphere today.

In other words, the deuterium concentration on earth,th, and in methane on Jupiter may reflect the true primordial solar system deuterium, and the lower concentrations in the sun and major planets reflect the result of nuclear fusion.

Bill Collis,
Italy

The Trigger Poll: deep interest in Cold Fusion.

JAPAN

Continuation from page 33.

Many Americans want to know what the middle-aged "decision makers" in the Japanese scientific establishment think of cold fusion. Do scientists in industry, universities, and national laboratories believe cold fusion is real? Do they think it will become a practical source of energy?

Most countries would have us just speculate on such questions. Fortunately, Japanese publishers are fond of public opinion polls, polls which give us detailed answers. *Trigger*, a science magazine from the Nikkei publishing company, conducted a soundly-based public opinion survey, featured on the June 1993 cover.

Trigger sent 300 questionnaires to leading scientists at universities, industrial corporations, and national laboratories, and received 180 responses—a staggering 63 percent response rate, high even by Japan's standards. Respondents represented the decision-making class of Japan: the largest age group represented is 55—59, 24 percent of the total. Not only was the response rate high, but the editors of *Trigger* were flabbergasted to find that over 100 respondents included extensive additional comments averaging about 200 words, some including a whole page or two of comments.

Question 1 revealed how much interest there is among the experts.

Interested in the topic of cold fusion?

A. Yes, 180 respondents (95%)

B. No, 10

Not all 180 respondents believe that cold fusion is real, as shown in question 6. The editors targeted this question in an attempt to characterize the respondents' stance.

"We would like to ask this question of respondents who answered yes to the first question. It has now been four years since the discoverers of cold fusion, Drs. Pons and Fleischmann, and Dr. Jones, announced the work. Yet replication experiments are still in progress, and the work is still controversial. Why do you think it has taken this amount of time?"

A. Because the effect does not exist, and attempts to replicate it are a waste of time, 27 respondents.

B. Because replication attempts have been low quality scientific work, 23.

C. Because it is a new phenomenon, and it is natural that replication should take a long time, 127.

D. Don't know, 3.

Of the 190 respondents, 81 percent thinks there is something to cold fusion, and 19 percent are either not interested, or think it is a non-existent phenomenon.

Most of the respondents do not believe cold fusion will ever become a practical source of energy, as shown by question 9

"What do you think of the future prospects for cold fusion?"

A. I think it will become a practical, desirable form of energy, 29

As Ikegami often says: "If you know it will work, it isn't science."

B. I think the prospects for practical use of cold fusion are poor, 95

C. Don't know, 52.

Trigger editors printed four pages of excerpts from the many comments. They were divided into supporting, neutral, and anti-cold fusion categories. (The last category is called "denial" in Japanese; in the U.S., it is generally termed the "skeptic" school.) Most of the comments were thoughtful, and several were enthusiastic. Many of the supporting comments and neutral comments were along these lines: Cold fusion looks like an interesting scientific phenomenon. Even if it turns out that we cannot make it into a useful source of energy, there is more than enough justification to continue funding the research as pure science.

Looking at the "denial" group of comments, the consensus was: "Funding for this research should not be allowed to grow large enough to have a negative impact on other fields." Most negative comments were very similar to what moderate U.S. skeptics say, although the tone of most comments was far more polite than in the U.S. Most of the "denial" comments were legitimate scientific doubts about issues that were open to question in 1989, but which were settled long ago, like recombination. Other comments claimed that experimental techniques are poor, and the work is sloppy.

A few comments reflected the typical "skeptic" views common in the U.S.: that this is misguided, "pathological" science. One writer said that cold fusion is "like The Emperor's New Clothes, there's nothing to it." One of the most vehement statements was: "The [cold fusion] scientists are a group of fools, there is nothing scientific about their claims, but it is interesting to watch these developments from the point of view of sociology."

The extreme hard-line anti-cold fusion views that dominate the U.S. dialog were absent. Only one person said cold fusion is a hoax, and nobody said that cold fusion scientists are criminals or lunatics. Perhaps *Trigger* editors filtered out such extreme views. The Japanese "denial" scientists do not hesitate to express their views. They say cold fusion is physically impossible, and the experiments are all sloppy nonsense. They say "it's a simple mistake of some sort." They do not, however, recommend that cold fusion scientists be thrown into jail, the way some U.S. and European scientists have.

What cold fusion scientists think

The 95 *Trigger* respondents who selected Answer B: "I think the prospects for practical use of cold fusion are poor" seem a little presumptuous. If they are not working in the field, how can they judge the future prospects? I think the cold fusion researchers themselves would select Answer C: "Don't know." Scientists or MITI planners asked this type of question, typically answered:

"We don't know if it will be a practical form of energy. We hope so. We don't even know what it *is* yet. That's what we are trying to find out. That is why we are doing this research. Ask again in a couple of years."

'Too Hot To Handle: *The Race for Cold Fusion'*

Real scientists don't use vitriol

BY CHRISTOPHER TINSLEY

ISBN 0-691-08591-9 Penguin Books UK pounds 7.99 Princeton University Press, pp 376

In some ways "Too Hot To Handle" gives an excellent account, if perhaps a little cluttered with trivia, like which airlines people used, of what happened in the heady days following the Utah press conference of 1989. It is a well-written book, whose author is the head of theoretical physics at the Rutherford Appleton Laboratories.

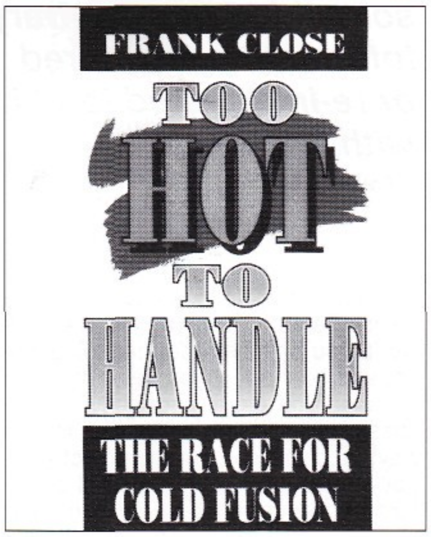
The paperback edition includes an Epilogue dated August 1991, later than the main body of the book, which should give the clearest indication of Professor Close's views. It would be surprising if August 1991 were the publisher's deadline for a book published in September 1992, so we may assume that Dr. Close felt no desire to make a later amendment. The flyleaf states that the book has been revised, so presumably the opportunity will have been taken to remove errors.

Every year, I think Faraday started them in the 1830s, there is a series of Christmas lectures for young people, given at the Royal Institution in London by a scientist of distinction. In recent years these have been televised, so I had the opportunity a few weeks ago of watching the full five hours of the lectures, as Close handled the difficult subject of particle physics and the Big Bang.

Some of the demonstrations were beautiful—the cloud chamber built especially for the series was magnificent. But it became clear what Close sees to be the goal of physics: to unravel the origins of the universe and to probe ever deeper into the most fundamental building blocks of matter, to hunt for the top quark and the Higgs boson, to solve the mysteries of the neutrinos and the rest.

And it is clear that theory is the guide. In case anyone might wonder whether this might be a search useful other than in the purely intellectual sense—intellectual curiosity is quite sufficient justification, so long as we can afford it—Close said that if we can ever fully understand matter, then we might be able one day to take full advantage of the Einsteinian equivalence of mass and energy to solve the energy problem forever.

The lectures showed a tendency to teach theory as fact, and gave me at least the impression that Close feels that it doesn't matter that we don't know all there is to know about the atomic nucleus. Particle research is where it's at, perhaps that's because



If nature gives us excess heat, then that is what we get.

nowadays the word *nuclear* is so detested that we even have to use the vague term "magnetic resonance imaging." This diversion may or may not throw some light on the mental attitude behind this book. Certainly the Epilogue, along with the extravagant praise from *Nature* and *Science* which adorns the cover, tells its own story.

It begins with a straight attack on what he sees as the ethical shortcomings of Drs. Fleischmann and Pons, and his evident satisfaction at the way the book's concentration on this aspect of the whole affair was taken up by the *New York Times* and the *Washington Post*. He continues at length with a discussion of the famous gamma emission peak controversy. In doing so, he makes a barely-concealed claim that Dr. Pons invented the whole thing. This shows a fundamental problem with the book.

The technique used throughout is to deny the results by attacking the man, or to attack the man by denying the results. He criticizes the finding of helium-4 by a group at the China Lake Naval Weapons Research Center for the preliminary nature of their paper, and for using so crude a radiation detector as "dental film" being "fogged" (as if that were more than a side issue in the paper), and for the group having found some evidence for excess heat with light water. The paragraph ends: "If helium were the end product, and if heat is [sic] being produced at laboratories around the world, as True Believers insist, then we should be hearing of many such experiments finding helium along with their heat. Instead—nothing."

This is an example of another Close device: disparaging the results and then pretending they don't exist. It may not be clear that the whole concept of heat being found in light water (or with hydrogen as in the Yamaguchi experiments) is regarded by Dr. Close as proof that excess heat with heavy water is an error. This results because hydrogen fusion is so much harder than with deuterium. Certainly we would not expect identical results with hydrogen (which always contains a little deuterium anyway) but it is nonsense to deny an experimental result on the grounds that there are reports of another, even less probable than the one you regard as impossible anyway. If nature gives us excess heat, then that is what we get.

Next we come to conspiracies. Close has a quick laugh at certain dubious results, like an alleged meltdown, which apparently was no such thing (he says), and moves on to laugh louder at the paranoia of the True Believers, such as accusing the MIT group of having suppressed certain data that "verified" heat production in cold fusion cells," and "denied absolutely" by MIT PFC's Ronald Parker.

If someone supports positive results he is suspect, since we know the results are wrong. If someone says that a result is negative, that's fine, because we know he's right. Close refers to "the opinion of some in the Establishment" that there are "less honorable motives at work in the orchestration of the episode." The section ends with a quote from Robert Parker in the *Washington Post*: "Inept scientists whose reputations would be tarnished, greedy administrators who had involved their institutions, gullible politicians who had squandered the taxpayers' dollars, lazy journalists who had accepted every press release at face value—all now had an interest in making it appear that the issue had not been settled. Their easy corruption was one of the most chilling aspects of this sad comedy."

Close asks, where are they now? He can tell us that the heroes (Lewis and Koonin at Caltech, Petrasso at MIT) are doing well, very well indeed. The villains, Fleischmann and Pons, got their comeuppance. The poor duo, True Believers, are "reduced" to working for some Japanese outfit in France. What about all those dozens upon dozens of published positive results? None are referred to (except the one mentioned above), and he does not mention later evidence which disproves some of the slurs in the book.

"Too Hot To Handle" does deserve something better than the sparse and even inaccurate index. Close's account of what happened after March 1989 is very good, marred only by the constant reference to "test tube fusion," an unnecessary (and pejorative) inaccuracy. One interesting feature of the account of various replication attempts is how so many groups were clearly concentrating on the problems of radiation measurement, the assumption seeming to be that the actual cell was a trivially easy gadget to build.

Did Fleischmann and Pons give them this idea? If so, was that because they thought others were as good as they were, or was it that they had been lucky? Certainly Close regards any such subtleties as deuterium/palladium loading ratios as irrelevant.

The book stresses the problems with neutron detectors, but the contention

"It is a well-known trait of human psychology that people can become so committed to a preconceived belief in something that contrary information is ignored or re-interpreted to fit in with the 'facts.'"

—Frank Close

(p. 160) that "by trying to do the experiment different to the way the chemists did it, you changed the magic ingredient and so the fact you see nothing is because you did it incorrectly" is dismissed as "making excuses." In this way, he dismisses the science of electrochemistry, perhaps more full of pitfalls for the unwary than even nuclear physics. As Close remarks (p. 72), one of Fleischmann's earlier papers was thought to be in error, because even specialists in his own field took two years to replicate it.

A great virtue of science is its international character. The sort of narrow parochialism which, for example, puts collaborators from two countries (like Crick and Watson, or Watson and Crick) in an order which suits the writer, should be avoided. But I will be honest and admit that, as a Brit, I can smile at some of the rantings of American cold fusion "skeptics," yet be deeply embarrassed by the lack of intellectual rigor which so often appears in "Too Hot To Handle."

Take the matter of the Texas A & M tritium scandal, where accusations of fraud were made in *Science*. On p. 226 he says: "...Bockris [at Texas A & M] continued to see tritium, not regularly but on occasions, which cynics noticed tended to coincide with visits to his laboratory by administrators associated with the funding..." Notice Close is stating this as fact.

As it happens, statisticians could find no such link. On page 226 Close takes up the matter of the tritium again: "Whether the tritium was introduced innocently or malevolently has serious implications for the ethics of the experiment, but whichever of these it is, for test-tube fusion the general message is destructive—the tritium emerging during the electrolysis has not been produced by a nuclear reaction within the experiment, and there is no sign of any nuclear products ever having been produced at levels significantly above the ground."

"As with helium, so now with tritium. Rutherford's edict of 1930 applied right to the end: 'The presence of an element has been mistaken for its creation.'" Clearly, nobody accidentally contaminates a running experiment with tritium, and the University investigation, and the demonstration by Storms of the impossibility of reproducing these results by any kind of contamination, deliberate or otherwise, effectively (later) cleared the experimenters.

Close's comment that the presence of light water in the cell "...fits with the rumors that the source of the tritium had been a bottle of tritiated water..." is a startling error when every nuclear scientist knows that heavy water rapidly absorbs (light water) atmospheric moisture. But the main point is that he assumes the source must be from outside the cell, then uses this assumption to prove his case. This is circular reasoning, and how the book has attracted such rave reviews in serious publications when its pages are littered with this nonsense is beyond my understanding.

In essence, the book boils down to this: Cold fusion is impossible. Therefore positive results can be dismissed, since they must arise from delusion or incompetence or fraud. Negative results, since we know them to be correct, can be accepted without question. Therefore, cold fusion is impossible.

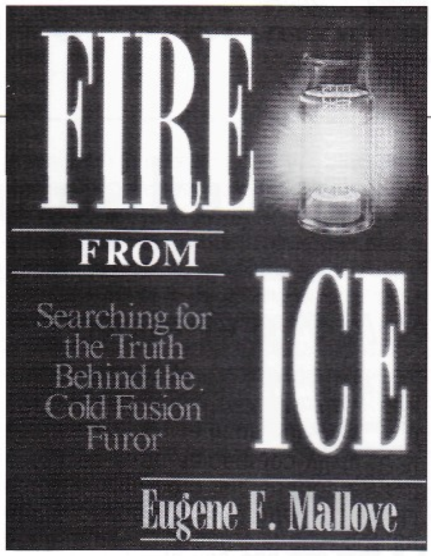
To support this idea, Close makes clear his contention that energy

Continued on page 95

'Fire from Ice: *Searching for the Truth Behind the Cold Fusion Furor'*

BY CHRISTOPHER TINSLEY

John Wiley & Sons, Inc., New York, 1991, pp. 334, $22.95, Eugene F. Mallove, Sc.D.



What can be the value of a cold fusion book written almost three years ago, when there are more recent ones? Answer: it depends on the books.

There are now four histories of cold fusion, but Dr. Mallove's "Fire From Ice" is the only one which does not come to a firm conclusion. It is also the only one which does not start with a firm conclusion. However, it does become quite clear that Mallove was already fairly sure of the reality of the effect, and it took some of us a little longer.

Does this make the book biased? I don't think it does.

For example, the book (in 1991) supports hot fusion, even though Mallove in 1994 no longer does. I had given up hope on hot fusion in the early 1970s, except in the sense that we seemed to be stuck with it until something better came along. To raise any real enthusiasm for such a program, you have to believe very strongly that all that will ever be known about energy is known already.

In fact, hot fusion is now dying without any help from the cold fusion variety—though I'd like to think that some of the less expensive, ultra-long-term ideas might be worth trying. The kiss of death for the tokamaks comes now in the form of semi-popular articles—like the recent one in *New Scientist*—fervently supporting it. That reminds me of all those articles in the computer press arguing that there was a real future for punched cards, for paper tape—probably a future for computers built with racks of glowing bottles, but I don't go (quite) that far back.

Mallove was still a hot fusion fan in 1991. In fact, I get the impression from the book that the writer was at the time in the process of painfully extracting himself from a scientific consensus where he had been reasonably content. And here we find his essential honesty, which gives the book much of its appeal.

There is none of the anger of John R. Huizenga ("Cold Fusion: The Scientific Fiasco of the Century"), nor the seductive and selective certainties of Frank Close ("Too Hot To Handle"). The world of technology—and even of science—is a bit like a steam locomotive, in that every revolution is driven by a crank. You could safely define the word as "someone who promotes a revolutionary idea which is not generally accepted."

If you want the world to turn, you have to become a crank and start pushing the wheel. If it turns, you are a hero—maybe. You have shown vision, courage, and all that stuff. If it won't shift, or somebody puts a spoke in it, that's tough. You remain a crank. While some are born cranks, and others achieve crankiness, some have it thrust upon them—and they don't always like it.

"Fire From Ice" opens with a review of fusion and the hot fusion program. Perhaps many readers would feel that such a primer is unnecessary, but in fact it covers the ground clearly and quickly, and is worth anybody's time.

Mallove proceeds with the story of the initial rush to replicate—and the confusion and disappointments which followed. The fundamental difference between the Mallove and Close accounts is that Mallove goes on to tell us what happened after all the excitement had died down, starting with an account of the 1990 International Cold Fusion Conference. He mentions perhaps the most intriguing result of those early days, one which seems to be missing from the Close book, that some electrodes from two labs (BARC in India, and SRI in California) would produce strange patterns (reproduced on p. 219) on photographic film.

Certainly all the rows, accusations, and lawsuit threats are covered. But we also see the theoretical ideas and, to my mind the most important item, there is the list (pp 246-248) of 92 positive reports from around the world. This list is the central feature of the book and, together with a good index and the resource guide and bibliography, continue to make the book an essential reference work.

The chapter on *Hard lessons in science* was something of a revelation to me, giving a list of the many scientific developments which have encountered similar opposition in the past. Since "Fire From Ice" is split into topics, it is more than tempting to go straight for the good bits, which read almost like essays on the various themes. The writing is excellent, (even a colleague of Dr. Close, who might be called the devil incarnate in this context, once commented favorably on it).

Most important, it manages to entertain and inform without any of the tiresome emotional overtones and sniping rhetoric of all the other books on the subject. Arthur C. Clarke said that this is the only good cold fusion book. I have no doubt that the author would be delighted to see some better competition. It does not end with a firm conclusion. Instead, the evidence is considered to be overwhelmingly compelling but not conclusive.

Much has happened since then, and one wonders whether the statement in the acknowledgments, "I now return to the home planet," would now be regarded as being entirely correct. And perhaps I am not the only one to wonder what *was* the upshot of the story (p. 232) of Dr. Morrison and the bottle of wine?

Perhaps the most concise and apt praise for "Fire From Ice" was offered by physics Nobel laureate Julian Schwinger: "Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly-held preconceptions, he bares the truth implicit in a provocative variety of experiments."

The publishing fiasco of the century

BY EUGENE F. MALLOVE

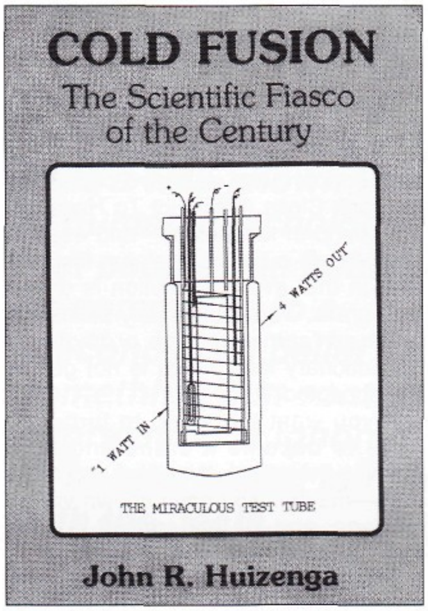
Cold Fusion: Scientific Fiasco of the Century

University of Rochester Press, 1992, 259 pages, $45
by Professor John R. Huizenga
Co-chairman, U.S. Department of Energy Cold Fusion Panel, 1989.

What can one say about an author who ignores essentially all the positive evidence for cold fusion, who leaves out virtually *everything* after 1989, who says *nothing* about the revolutionary yet ultra-cautious cold fusion work carried out by the conservative Electric Power Research Institute, who *denies* that a major cold fusion effort is under way in Japan, who doesn't even mention the cold fusion experiment controversy at MIT and the controversy about the Caltech "negative" cold fusion experiment? Not much. That is, unless you know that John Huizenga was the first Department of Energy-designated general in its unconscionable war against cold fusion.

In 1992, as the pace of the cold fusion revolution quickened in Japan and in numerous "underground" laboratories in the U.S., France, Italy, and elsewhere, John Huizenga finally got his cold fusion book. Since 1989, attacking cold fusion research and scientists curious about the strange phenomenon has been Mr. Huizenga's pastime—one might say, his obsession. *And well it might be, because his reputation is at stake.* Huizenga says that interest in cold fusion is "dying," but it is *he* who is going down for the last time.

As the co-chairman of the Department of Energy's Energy Research and Advisory Board (ERAB) Cold Fusion Panel for the mere six-months in 1989 that it officially investigated cold fusion, John Huizenga and his fellow panelists were given an awesome civic responsibility. Their duty: to assess the numerous shocking, sometimes apparently conflicting, and always intriguing reports of what had come to be called "cold fusion"—provocative evidence of significant excess power production in palladium-platinum electrochemical cells filled



with heavy water, as well as nuclear products from possible heretofore unknown nuclear processes.

Drs. Martin Fleischmann and Stanley Pons made the original bold claims in Utah at a press conference on March 23, 1989, which has since been severely criticized by skeptics and even so-called "believers."

However, the method used to disclose this shocking finding and the intriguing political-scientific-media interplay before and after is not the main issue. Of overriding importance was and is:

Is cold fusion real, and, if so, what to do about it?

Yes, in retrospect, the premature announcement was a disaster, which Pons and Fleischmann themselves had resisted; they wanted to work another year and a half before reporting their results. And when they did announce, their hands were tied by patent lawyers who prevented adequate disclosure. The politics, the intrigue, the ongoing fight over patent rights are not pretty, and they are real. Nobody denies that. But these have nothing whatsoever to do with the basic science of cold fusion. The news conference and the problematic press release are not the issue. The DOE did not ask Huizenga to judge the aesthetics or the propriety of people's behavior, it asked him to look at the scientific facts—and clearly he didn't.

Disagreement has its limits

In March, 1989, if the cold fusion report was not just a big mistake, it was possible that a new kind of nuclear process had been discovered—possibly a new form of metal atomic lattice-induced fusion, or possibly new kinds of chemical reactions, but in any case a chance of something new to science. Most people thought this was wonderful. *At least there would be an exciting new detective story in science*, but Huizenga and his ilk thought it was terrible. If the claim of excess power generation were true and this heat were accompanied mainly by relatively low-levels of neutrons and tritium, cold fusion would, indeed, be a revolutionary discovery with far-reaching implications for the world energy economy.

If the new power source could be reduced to practical technology, the planet would have an infinite source of energy from sea water in the hydrogen isotope, deuterium. Many working in the cold fusion field, including myself, after being quite skeptical for many months or years, are now convinced that "cold fusion" phenomena are real, and that the revolution will come. It will only be a matter of time.

Of course, there are those who disagree with this assessment, and this is how it should be in science. Still disagreement has its limits.

In 1989, instead of adopting a completely open-minded view of startling new data and getting to the bottom of what was going on, the scientific process was derailed.

Disaster struck: in walked Professor John Huizenga. Under the strong influence of Huizenga and his preconceived belief that cold fusion "contradicted the foundation of nuclear science" (p.viii), the Department of Energy cold fusion panel engineered a highly flawed, indefensible document. Its negative conclusions were largely solidified (by Huizenga's own admission) by July, 1989, barely three months after the panel began its work.

By the end of October, 1989, the group's work was reduced to a final report with conclusions that have, if not entirely stymied, seriously hampered the scientific investigation and commercial development of cold fusion power in the United States.

But from Huizenga's book, which recounts the "investigation" by the ERAB panel, there may yet come *some* good, because *Fiasco* proves beyond question that at no time in the panel's deliberations were serious thoughts given to even the *possibility* that a new form of nuclear process might be occurring in metal lattices.

It is proof positive that the study was biased from the outset and easily could be construed as a major deception perpetrated on the people of the United States. Each of the panelists bears some responsibility in this regard, but none as much as Huizenga, who was clearly the driving force in the rush to judgment. Some of the less-involved panelists, upon investigating for themselves what has really been going on in cold fusion research since they signed the ERAB report, might have second thoughts about their approval.

From this influential document has come a poisoned scientific atmosphere, media ridicule of continuing honest scientific investigations, a cut-off of DOE funding for cold fusion, the banning of even "bootlegged" cold fusion research at some federal laboratories—in other words, at the very least, an embargo against a new scientific field.

The continuing news of successful and provocative cold fusion research in Japan, the funding for cold fusion from the private Electric Power Research Institute (EPRI) in the U.S., and multi-million dollar funding by the Japanese Ministry of International Trade and Industry (MITI) must grate on Huizenga. His two favored villains, Pons and Fleischmann, hard at work at a laboratory in France funded by Japanese businesses affiliated with Toyota, have achieved reproducible boiling in cold fusion cells, which has vaporized all the heavy water electrolyte—over and over again.

But Huizenga says 'nay'

Such spectacular results have become the order of the day in the cold fusion field. But Dr. Huizenga doesn't believe *any* of the cold fusion evidence. He says that every bit of it can be characterized in two words—"pathological science." All these hundreds of scientists in over a dozen countries still working or interested in cold fusion are deluded.

Professor John Huizenga

John Huizenga attended the Third International Conference on Cold Fusion (ICCF3) in Nagoya, Japan (Octobe 1992) and ICCF4 on Maui, in December 1993. At no conference session did he offer any public technical comment, nor did he present any paper. However, after both cold fusion conferences the U.S. press dutifully printed Huizenga's remarks to reporters that he still considered cold fusion to be "pathological science" and that no further evidence had emerged to change his mind. Clearly, Huizenga is still at war with cold fusion. Why?

Excess heat in cold fusion experiments is illusory, says Huizenga, even though he cannot point to any errors being made. Tritium generation found by over 40 cold fusion groups is a vast mistake, and in one famous case, possibly fraud, says Huizenga. In his book he continues to give credence to Gary Taubes' sophomoric charge of tritium-spiking of cold fusion cells at Texas A&M University. Texas A&M looked into these charges long ago and showed they were without foundation. Furthermore, others have shown that it is not even physically possible to "spike" with tritiated water to get the results of that research group.

Neutron bursts and continuous neutron emissions encountered by numerous laboratories around the world are all erroneous. The ones Huizenga thinks he can't outright dismiss he says are likely to be wrong or unimportant if they are right. Charged particle emissions and several helium findings are all wrong, says Huizenga. Cold fusion research is a gigantic waste of time and money, says Huizenga. "The world's scientific institutions have probably now squandered between $50 and $100 million on an idea that was absurd to begin with," (p.184) says Huizenga.

Over and over again, Huizenga employs the pathological science label and inference. That is precisely why this book may come to be known as the "publishing fiasco of the century," perhaps in a class with Rene Blondlot's famous tome on non-existent "N-rays" earlier this century. Long after "N-rays" were convincingly disproved in a decisive experiment, their inventor, Blondlot, carried on with his obsession. Similarly, we ask: Has Huizenga fallen into the same kind of trap? Will Huizenga find it virtually impossible to accept cold fusion, to admit that he was wrong—even when commercial power devices pass juice into his wires?

Don't expect to learn about the other side, the latest positive cold fusion experiments, in Huizenga's book. In fact, there isn't even appropriate discussion of some very good cold fusion experiments that were done long ago—at the time Huizenga was masterminding his DOE report. And definitely don't expect to find what most "believers" really *believe.* He does not adequately discuss their theories. He cites preliminary 1989 versions of theories that have since gone through many iterations and have been expanded and improved. He cites theories, such as the valiant Walling-Simons theory, that were abandoned in 1989 in favor of better ideas.

In fact, virtually every time the word *theory* appears in *Fiasco* it is set off in sarcastic quotes—as in "theory." It is as though no one should even attempt to explain what to Huizenga from the outset was so obviously impossible nonsense.

Huizenga maligns the evidence and wantonly dismisses all theories *a priori.* He says of theorists: "Conventional nuclear physics was declared invalid in metallic lattices by fiat." No cold fusion theorist ever made any such assertion. Here is what most cold fusion believers *do* believe has been established: The excess energy—beyond the electrical power put into an electrochemical cold fusion cell—is in many cases so large (tens to hundreds of megajoules per mole of palladium atoms accumulated over long and sometimes short periods of time) that this heat cannot be due to ordinary chemistry.

Chemistry or other conventional energy storage mechanisms cannot explain these excess energies, period! So the energy could be due to nuclear reactions, mini-black holes, interactions with cosmological dark matter, or microscopic broccoli plants, but *it must be explained* and conventional chemistry doesn't hack it.

Most cold fusion "believers" are very conventional and are thus more attracted to nuclear explanations than to mini-black holes. They have noted that in the same or similar electrochemical systems, nuclear products—tritium, neutrons, helium—have been convincingly detected. It matters not a jot that these particular already detected particles are not fully commensurate with the demonstrated power levels (as Huizenga demands *ad nauseam*). Cold fusion investigators have been saying all along that the particular main-line reaction path could be isotopic shifts from the transfer of neutrons (from deuterium, for example) to other elements—the palladium, the lithium, the light hydrogen, or even other impurities known to be in the complex metal system. These are not easy to unravel, particularly because reproducibility of cold fusion reactions has been a serious problem, exacerbated by the zero DOE funding situation that Huizenga and Company have left us.

In most cases so far, these reaction products would not have been readily seen because of their low levels or because resources were not available to hunt for them. That these products have not been neatly packaged for him, and the mechanism explained from day one, proves to Huizenga that cold fusion is without substance. This appears to be a deliberately misleading contention and a faulty approach to science. *Fiasco* is, quite literally, a bizarre pastiche of misinformation.

"Surprises do occasionally occur in science." (p. viii)

—John R. Huizenga, Tracy H. Harris Professor of Chemistry and Physics

Huizenga's opinion of cold fusion at the outset of the controversy is documented compendiously in *Fiasco*. When the Congressional inquiry into this matter is eventually held, the evidence of his words will be more than adequate to prove that he was hopelessly biased from the start. Why was such a skewed chairman chosen?

Is that you, Darth Vader?

If you know Washington and the federal research establishment, you already know the answer: Huizenga was chosen to be the hatchet man. The DOE wanted to strangle the cold fusion "nonsense" in its cradle, before it became too noisy an embarrassment to DOE's expensive and troubled $500

"A foolish consistency is the hobgoblin of little minds..."

—Ralph Waldo Emerson, 1841

million a year *hot* fusion program. In describing his appointment to head the ERAB panel, Huizenga says, "My initial feeling was that the whole cold fusion episode would be short-lived and that it would be wise to delay appointing such a panel." (p.42) Yet Huizenga asserts, "In my close association with each of the panel members, I saw no evidence of bias on the part of any panel member." (p.104) He forgot to examine his own soul.

Huizenga claims that those who thought they had found no evidence of cold fusion were ". . . reluctant to publicize their findings. Firstly, these groups were cautious, wanting to be sure that they themselves had not made mistakes."

Not so! I was the chief science writer in the News Office at MIT at the time and I can testify that the MIT Plasma Fusion Center-Chemistry Department group investigating cold fusion salivated at the prospect of destroying Pons and Fleischmann, almost from the first day. Director of the Plasma Fusion Center, Professor Ronald R. Parker, and Professor Ronald Ballinger in late April, 1989, helped launch a vicious attack on Pons and Fleischmann with a deliberately planted story in *The Boston Herald.* The taped interview with the Herald Reporter is now public record.

So many other things are mysteriously missing from *Fiasco*. Although the group is mentioned by name, we read nothing of the pioneering work of Dr. Michael McKubre and his EPRI-funded group at SRI International in Menlo Park, California. This team is widely acknowledged by both believers and even fence-straddlers to have done some of the most careful cold fusion calorimetry in the world. Although a tragic and still puzzling explosion in a SRI cold fusion experiment killed electrochemist Andrew Riley on January 2, 1992 (also not mentioned by Huizenga), EPRI is still solidly behind the McKubre work. Why *doesn't* Huizenga discuss McKubre's work? Readers would very much like to know what errors he was able to find in it.

He devotes only one paragraph to the Second Annual Conference on Cold Fusion in Como, Italy, held in July, 1991. He says nothing about the new findings that were presented there, which were numerous and impressive.

In fact, world-class electrochemist Dr. Heinz Gerischer of the Max Planck Institute attended the conference as a skeptic, but left convinced that nuclear reactions at some level were, indeed, occurring in metal lattices. Later he wrote in a memo to the German government: "The fact that in the Republic of Germany this work has been inhibited is no longer justified. It could, later on, be regarded as a very unfortunate gap in German research when compared with the present activity in other countries and particularly in Japan."

However, Huizenga managed to find space in his brief paragraph on Como to explain why Steven Jones, a proponent of low-level cold fusion neutrons who does not believe that the excess heat is of nuclear origin, declined to attend the conference.

Huizenga distorts the truth in many ways. The final report of the Utah National Cold Fusion Institute (1991) was available to him and contains clear descriptions of *reproducible* tritium generation in experiments by the ultra-cautious electrochemist Dr. Fritz Will. Yet Huizenga asserts, "There has been no sign of this growth of understanding of cold fusion either in the production of fusion products or excess heat." (p.126) He makes the same assertion at greater length on page 102.

Describing the conclusions of his panel's report, he points proudly to its objective to cut off funding: ". . . there should be no special funding for cold fusion. This recommendation was meant to insure [sic] that no research centers or special programs to investigate so-called cold fusion phenomena would be developed with federal funds."(p.101) Huizenga obviously fancies himself a hero upholding the purity of science.

And it was not only funding that he sought to halt. For example, he unabashedly admits his role in trying to prevent *Wall Street Journal* senior science writer Jerry Bishop from receiving the American Institute of Physics award for science writing for his series of cold fusion stories. Even some skeptics, slightly less ardent than Huizenga, have appreciated and praised Bishop.

Huizenga bemoans the publication of theories of cold fusion. He wants to keep the technical literature safe from pernicious ideas, that is, ones he can't compass.

Fiasco does, however, have several high points for which we should thank the author. I was also delighted to see confirmed the facts which I disclosed in *Fire from Ice* about the attempted resignation from Huizenga's panel by physics Nobel laureate Norman F. Ramsey. It was the last session of the panel's meeting and the less involved co-chairman Ramsey, though not convinced that cold fusion effects were real, was uncomfortable with the report's thoroughly negative tone, given the uncertainties that remained. In exchange for not resigning, which would have been a major PR snafu for Huizenga's panel, Ramsey was apparently able to extract from an unwilling Huizenga the addition of a qualifying preamble. This "weakened the report somewhat," according to Huizenga. (p.92) Huizenga says he was perturbed that this (to others) eminently reasonable preamble was "noncommittal and evasive." Just as I wrote in my book, "He [Huizenga] wanted the final knife to go in with a totally negative report." Among other things, the preamble states clearly: *"Consequently, with the many contradictory existing claims it is not possible at this time to state categorically that all the claims for cold fusion have been convincingly proved or disproved."* In my view, Ramsey was a hero for having won that much from Huizenga, but it would have been far better had he resigned from the panel.

Huizenga's book is a masterpiece in reasoned irrationality—a seeming contradiction in terms, but very apt for this exotic work. Professor Huizenga's words reveal him to be an incompetent, who even with his presumably broad and detailed knowledge of nuclear physics, has not assimilated the most elementary understanding of science. He does not recognize it as a process of discovery whose lifeblood is anomaly, the new, the different. How else can science progress? Huizenga's view of science is stodgy, status quo, stultified, and steeped in mind-boggling conventionality.

Read it and weep

With all this said, "Fiasco" deserves to be read, but not bought. It is an object lesson in the hazards of labeling honest science "pathological." Perhaps we should rejoice in "Fiasco," because here we have, once and for all, the perfect demonstration of how science should not be done when confronted with puzzling results:

1. Start with the preconceived idea that reported data suggest a phenomenon that is impossible; 2. Use a standard theory to prove impossibility (much as Simon Newcomb "proved" that heavier than air flight was impossible a few years before it was accomplished by the Wright brothers); 3. Continue for years to reject *every* experiment with positive results as "obviously in error," while accepting supposedly "negative" experiments done in a few months or weeks time by "first-tier" universities and labs; 4. Ignore and mock all theories that attempt to explain the phenomenon—even those that employ accepted mathematical formulations of quantum mechanics. (And, for good measure, ignore later published versions of initial theories that are available in the open literature.); 5. Command a clique of like-minded scientific doubters on a federal panel and foist a deceptive rush-to-judgment onto an unsuspecting public, ill-equipped to second-guess "official" scientific wisdom.

This is a lesson we could well have done without. It would have been so much better had Huizenga been scientifically up to his civic responsibility on the cold fusion panel. Huizenga is at least partially right in one of his ideas (though not in the anti-cold fusion sense that he intended), as expressed in part of the last sentence in his book: "...the scientific process works by exposing and correcting its own errors." A bitter irony here: the data from cold fusion experiments weren't "preposterous," Huizenga was. He will be corrected.

Cold fusion's impact on financial markets

It won't be business as usual

BY WM. A. BOAS, JR

The prospect of cold fusion should be a warning shot across the bow of the global financial and investment community. Besides revising some dogmas of physics and bruising some scientific egos, the promise of virtually unlimited, cheap, and clean energy is going to have a profound effect on some multi-national pocketbooks.

In modern times, there has never been a case of a radically new energy technology about to collide with an international monetary system whose major currency assets are tethered to a fossil energy order. Geo-politics and international capital flows will never be the same. It's hard to predict how much some financial horizons will darken, and for how long. The implementation of the technology itself might illuminate the way.

It's been five years since the Utah public announcement seemed to confirm prospects of a new hydrogen energy process. At the speed new technologies develop, it's reasonable to expect a report soon that a cold fusion product is ready to heat a home, power a car, or otherwise prove its capacity to launch a new energy age.

There is a consensus that an announcement to that effect can be expected within the next 18 months. With cold fusion popularly thought "dead," that might come as a surprise to both the public and global stock markets. The problem is stock markets don't like surprises. History shows that a surprise can quickly reduce a market to a crowd, and crowds easily panic. A panic could ignite a sell-off in certain stocks, bonds, mutual funds, and commodities that would make the global experience of October 1987 seem like a mild market correction.

Unsuspecting individuals and investment managers holding substantial assets and securities in coal, gas, oil, and related industries, are vulnerable to be left holding a near-empty bag or barrel if cold fusion development unfolds as expected. While the 1987 crash was blamed on an inflated paper market and lack of fail-safe codes in computer trading programs, this time the cause would be fundamental and the losses irrecoverable.

Fundamentally, there are a couple of hundred years of BTUs in proven underground reserves of coal, gas, and oil now valued at $75 trillion by transnational private companies, state monopolies, and governments playing in today's fossil energy arena. Those assets, and the cash flows derived from them, would erode to a fraction of their current value, as cold fusion technology starts to capture and finally dominate all energy consumption markets. When real fossil-based BTUs stay underground and lose value arithmetically, highly-leveraged paper securities based on them drop geometrically.

Wm. A. Boas, Jr.

It is not only private energy companies that stand to lose. The economic prospects of countries are now at risk. Russia, once the world's leading oil producer, still earns about 60 percent of its hard currency from crude oil sales. With even that income now declining, there may soon be more chaos in the Caucasus and dacha parlors than anyone dares imagine as cold fusion becomes practical.

The OPEC cartel and North Sea oil and gas producing nations seem equally destined to become a collective footnote to world energy history.

Fossil energy oil and coal giants like Royal Dutch Shell, Exxon, British Petroleum, Mobil, Texaco, Burlington Resources, and Peabody Holding Co. are going to have to rethink their business plans to survive. Many think cold fusion won't need their massive integrated resources and distribution systems to come to market. Some applications of cold fusion can be financed at modest levels using low-tech production resources.

Ultimately, cold fusion will impact other industry groups adversely as well, such as sea-going oil tankers, railway coal car leasing, oil field equipment manufacturers, pipeline companies, internal combustion engine makers, and countless peripheral fossil energy industry suppliers throughout the world economy. Thus, the down side of cold fusion is the inevitable confusion in financial markets for the next few years. It can't be avoided, it's just new business with new players.

Up to now, excuses can be made for ignorance and neglect of cold fusion by the investment community, mass media, and public. Mass media publishers and broadcasters can hardly be blamed for failing to put out information about a tentative scientific discovery whose nature can't yet be explained or commercially demonstrated by its advocates. If the established scientific community is still at odds over the new process, what can editors or commentators really tell their audiences?

It's the same with the investment community. With securities markets skewed to quarterly results, it's no wonder analysts, brokers, fund managers, and bank trustees have not been paying attention to something perceived as speculative at best, and scientific hallucination at worst. Their jobs hinge on performance and generating an income stream for the proverbial widows and orphans. Exxon's quarterly dividend is something they can hold in hand. Mere rumors about a new science that might reduce the big oil giants' market share back to lubrication oils and enough gasoline to supply vintage car hobbyists, isn't sufficient to change their investment outlook and strategy from business as usual.

However, at this stage of cold fusion's evolution, ignorance by default is no excuse. A visit to a good public library can bring anyone up to speed. There are more than 100 citations in the periodical literature, proceedings of four international conferences on cold fusion, and numerous books both pro and con about cold fusion's dynamic and controversial past four years of development. Now, of course, there is *"Cold Fusion"* Magazine.

The mystery and financial pitfalls can be avoided if individual investors and investment advisors take the time to learn about cold fusion and its impending and viable commercial implications. Timely information can anticipate surprises, and always makes rethinking the investment equation easier.

The ubiquitous short sellers are no doubt poised on the market sidelines waiting to capitalize on the slightest hint of any authoritative cold fusion validation. To avoid having to participate in any market abyss, prudent and defensive investment rules should be invoked with skepticism about cold fusion put aside. A discreet incremental shift out of potentially vulnerable securities to cash might be the best investment strategy to hedge all bets for the foreseeable future. Sacrificing some yield points now might preserve principal for opportunity tomorrow.

Opportunity for cold fusion investment is certain to be rife after it is validated. However, after the popular media and public have embraced it, legitimate promoters as well as charlatans will be primed to offer a myriad of new issues and investment paper in the new technology. Human nature being what it is, greed will bring many a preserved family fortune to ruin in what will surely be a confusing speculative frenzy. For now, due diligence is the best course for both the down and up side of cold fusion.

"Cold Fusion" Magazine's money and investment section will track the trends and keep technical developments, financial options, predictions, and opinion in credible perspective.

William A. Boas Jr. has been a correspondent for The Wall Street Journal, Business Week Magazine, and UPI. He has additionally contributed to numerous periodicals on financial and business topics.

TOO HOT

Continued from page 88

released in all cold fusion experiments has a purely chemical origin, or is a function of experimental error—or worse. Cambronne, the commander of the French Old Guard, was called upon to surrender at Waterloo. He is supposed to have responded, "The Guard dies, but it never surrenders." In fact, observers at the scene recalled that what he actually said was "Merde!" so this reply is now known in France by the euphemistic term "Cambronne's word." Such stubborness may be admirable in war, but not in science.

Whatever happened in 1989, whatever errors were made then and since, the book leaves me with a profound distaste for the Big Sneer, for the personal attack. Certainly the distorted record has to be put straight, but the sooner we can forget all this mudslinging and all get on with making progress, the happier I personally will be.

Professor Close should perhaps be allowed the last word (p. 345): "It is a well-known trait of human psychology that people can become so committed to a preconceived belief in something that contrary information is ignored or re-interpreted to fit in with the 'facts.' "

FUSION TOONS BY ETHAN MALLOVE

It's not that simple

BY CHRIS TINSLEY

It is easy to prove that cold fusion is nonsense. The nuclei of atoms are positively charged, so they repel one another more and more as they get closer together. To get over the energy hill, to allow the nuclear forces to take effect and allow fusion, is a bit like trying to putt a ball into a hole surrounded by a wall half a mile high. Enormous velocities are needed, which means a temperature of hundreds of millions of degrees.

It sounds good

The only alternative is to apply pressure, to force the nuclei together. That is the appeal of the deuterium-palladium cold fusion idea. A piece of palladium will absorb literally hundreds of times its own volume of hydrogen or deuterium, and the effect of squeezing so much into the gaps between the palladium atoms is just like a huge pressure. It sounds good. But, in fact, the atoms are probably farther apart than they are in an ordinary molecule of gas.

An even more satisfying disproof is this: whatever may be going on—and it must be something very extraordinary—it is most certainly not deuterium fusion, the "d-d" fusion that partially occurs in the great toroidal tokamaks. Even enough d-d fusion energy to make a nice cup of tea would be quite sufficient to exterminate all life in the vicinity. That is not some sort of pedantic quibbling. The rules for this kind of fusion are well understood. Either the reaction makes the heaviest hydrogen isotope, tritium, and a proton—or it produces helium-3 and a fast and deadly neutron. Once in 10 million fusions it does produce helium-4, but even then the energy is carried off by a gamma ray of remarkable unpleasantness.

This is why a hot fusion reactor of the kind currently envisaged would never be a clean source of energy. In fact, it would be a nasty, dangerous, and extremely expensive contraption. No commercial machine will ever be built. The British minister responsible for such matters has admitted that the UK fusion effort is a scientific rather than a commercial program—and the U.S. utility companies have said they don't want them.

So where is all the cold fusion energy coming from? And where is all the helium-4 coming from? Often there is not enough to explain the heat, but sometimes there is. It can't be d-d fusion, except possibly a tiny amount near the edge of detectability. What is it? I don't know. All I can be reasonably sure of is that all these arguments are completely irrelevant.

For more than half a century there was controversy—a polite word for mudslinging, derision, and contempt—over the claim by the whacko meteorologist Wegener that the continents move. It may not yet have been agreed as to how they do it, but the lack of a good explanation does not stop the evidence speaking for itself. It looks now as if the Coulomb barrier, the wall around the 18th hole, is not as insurmountable as we thought. Then again, it may not be that simple. In science, things rarely are.

Take the history of science. History is supposed to be written by the winners. But in science it is rewritten to make it seem that there has been a broad, majestic sweep of progress. We hear of villains, like those who persecuted Galileo. We hear of heroes like Ohm, he of the Law, or of Semelweiss, who thought that washing his hands before an operation might be a good idea. We don't hear about all the trouble these gentlemen had from their peers. If you want a recent example, look at what happened to the man who proposed a bacterial cause for stomach ulcers.

Or look at cold fusion.

Breaking down the barrier

For some curious reason, theorists usually get a good press. I would have been much happier if anyone else—believer or atheist—had roared with laughter when Hawking talked about how we would soon know the mind of God. Pope wrote in the 18th century: "Nature and Nature's laws lay hid in night: God said, *Let Newton be!* and all was light."

But, of course, it wasn't quite that simple. Two centuries later, Sir John Squire added: "It did not last: the Devil howling *'Ho! Let Einstein be!'* restored the status quo."

If a theorist puts up an attractive idea, billions will be spent investigating it. But if a lowly experimentalist reports a result which does not fit, especially if it is very hard to duplicate, then he can expect an end to his funding or his tenure. He may even face an academic kangaroo court or be hounded from his job, even if those who tried to duplicate knew next to nothing of his discipline.

No, I don't know how cold fusion works. I can, however, suggest an analogy. If you burn sugar, you get carbon dioxide and water. In fact, you get a black sticky mess at the outset, but if you try very hard you can eventually burn it all. You will need very high temperatures, and you will get a "characteristic radiation signature" in the form of light. Imagine a hypothetical chemist who knows nothing of biochemistry. Tell him that animals burn sugar, and he'll laugh. But animals "burn" sugar completely, in water, at low temperature. They do this by cleverly evading the problem, by establishing the correct "electron environment," and doing the job in stages.

Perhaps in cold fusion the complex electron environment subverts the Coulomb barrier, and a multi-stage process involving the metal atoms takes place. Perhaps it is suspicious that cold fusion so often produces helium-4, an alpha-particle, a common product of nuclear decay.

The human brain is very good at finding simple patterns in a complex world. It needs to be. Without that trick we would be lost in confusion and chaos. The trouble is, we are too good at it.

The universe is not as simple as we think it is, and we are finding out just how true that is all the time. Take a look at a technical text, 50 or 100 years old, and see how it reflects an oversimplified view—or a completely wrong one—and look at its misplaced confidence in its correctness. There is hardly a 19th century "law of science" which has survived unmodified into the present, yet we are expected to believe that science has got it right THIS time.

If there is any truth in even a few of the wild and wonderful experimental claims which have recently bombarded the desk of your Gentle Editor, then not only is it not quite that simple—the conventional understanding is also plain wrong.

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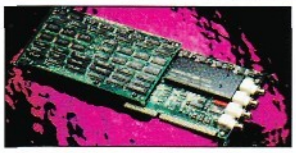

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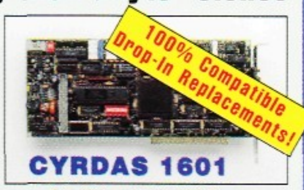

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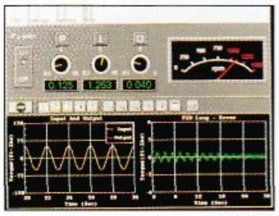

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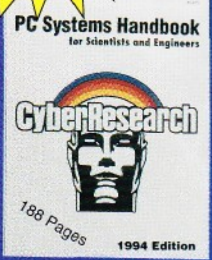

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```
Numerical Computation
3^70
2503155504993241601315571986085849
Hypergeometric2F1[7, 5, 4.1, 3-I]
-0.00403761 - 0.00295663 I
```

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```
Symbolic Computation
Integrate[x / (a + Exp[x]), x]

  2              X                     X
 x     x Log[1 + E ]    PolyLog[2, -(E )]
 --- - ------------- - -----------------
 2 a        a     a            a     a
```

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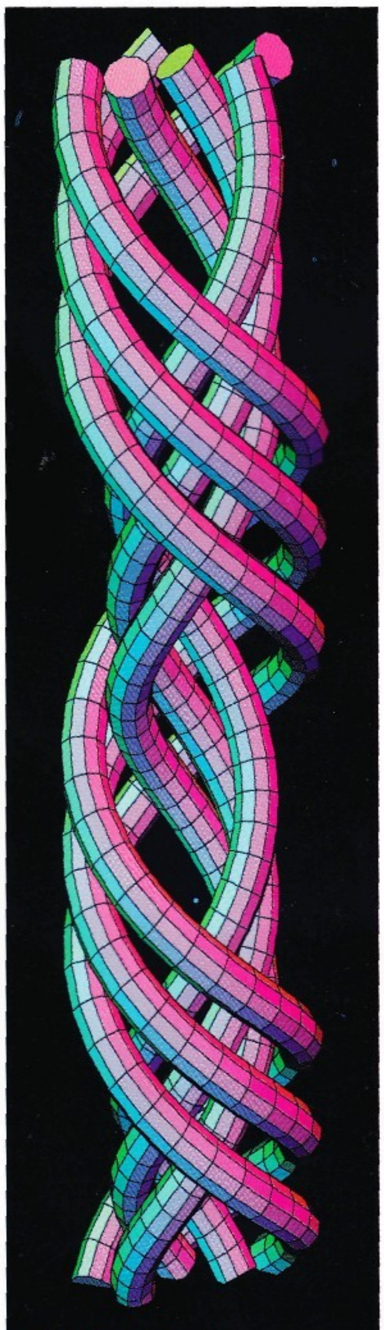

Graphic generated and rendered with Mathematica.

```
Programming
log[1] = 0
log[E] = 1
log[x_ y_] := log[x] + log[y]
log[x_ ^n_] := n log[x]
log'[x_] = 1/x (* derivative *)
log/: InverseFunction[log] = exp
log/: Series[log[x_],{x_, 1, n_}] :=
       Sum[-(-1)^k (x-1)^k/k, {k, 1, n}] +
        O[x, 1]^(n+1)
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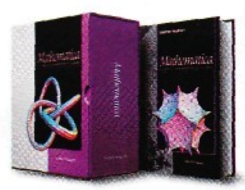

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