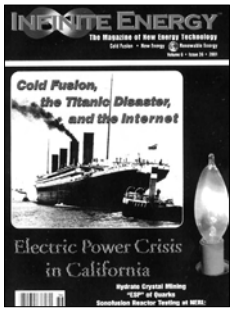
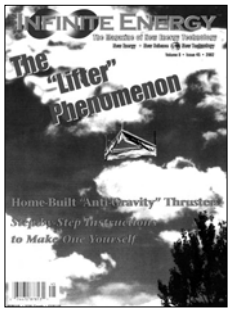
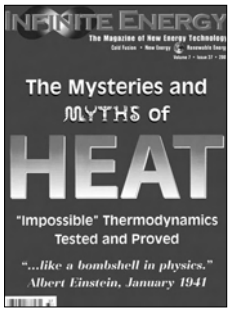
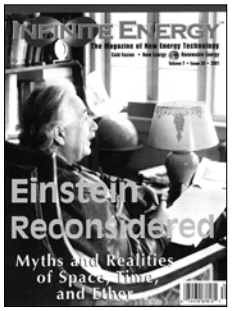


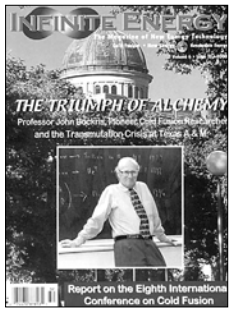




A Special Selection from

Infinite Energy Magazine





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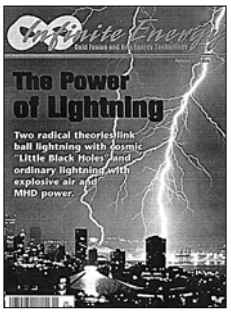

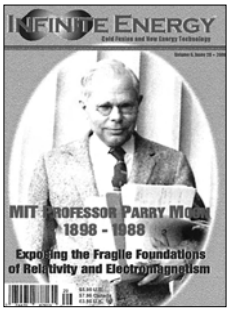

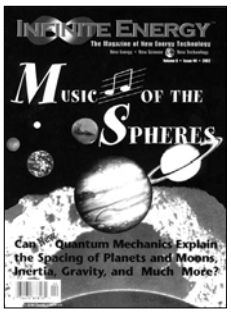

Index of Special Selection Articles

Introductory Note from the Editor

We are pleased that you have received this free selection of representative articles from past issues of *Infinite Energy*—the world's leading magazine of new energy science and technology. We hope that you will enjoy and learn from these articles, and that they will encourage you to get involved in this most exciting field. New energy is destined to put an end to the Fossil Fuel Age—and to the myth that we are nearing the "End of Science," as the scientific establishment's "near perfect" conception of the universe approaches completion. What better way to help yourself and a very worthwhile cause than to subscribe to *Infinite Energy*. We don't want you to miss any happenings in this exciting revolution.

Why have we been publishing *Infinite Energy* since 1995, and why does our magazine appeal to subscribers in over 40 countries?

Imagine, if you will, that a huge energy technology revolution is brewing, one built on spectacular, confirmed breakthroughs in science—and you don't know a thing about it, or have heard only rumors that you dismissed. Is it possible that a New Energy Revolution is really in the works and you don't know about it?

I am happy to inform you that, indeed, a Copernican-like revolution is in the making. You are thinking, perhaps, "That's too good to be true—that can't be right! How could such a major development have escaped my notice in newspapers, on television, or on the internet? Why would any responsible news organization not report such good news?" Sad to say, the answer is an emphatic, "Yes—the media have generally ignored this gigantic revolution-in-the-making, although there have been exceptions—stories have appeared in *Wired*, *Popular Science*, *Business Week*, *The Wall Street Journal*, *The New York Times,* and on ABC Television ("Good Morning America" and "Nightline"). These media have not always gotten the story right, and they sometimes have gotten it all wrong! And they certainly have not followed through even when they got it right. But even this bizarre media situation is nothing new. . .

Did you know that a century ago, the major media (only newspapers and magazines then) were almost completely silent about the discovery of heavier-than-air-flight by the Wright Brothers—until 1908, five years after these geniuses flew at Kitty Hawk on December 17, 1903? Yes, it's true! So amazing and "impossible" was human travel with flying machines—the best scientists for decades had been saying it was a preposterous idea—that major newspapers failed for five years to report the Wright brothers' activities, even though they were flying for all to see over fields in Ohio!

If you don't know anything about the *new* sources of energy that are the focus of *Infinite Energy*—***not* solar cells, wind turbines, ocean waves, or hydrogen fuel cells,** what we call conventional renewables—then read on. You are in for a most pleasant surprise.

Perhaps you have heard of the terms "cold fusion" or "zero point energy," with claims by some scientists that from new findings in the laboratory there could come effectively infinite, clean, and abundant sources of energy—inexpensive, safe, and widely distributed forms that could beneficially transform virtually every aspect of human civilization. Perhaps you have also heard that such claims are all bogus, "pathological science," or worse. (Of course there are some unsupported claims that *are* in that category, from deluded people or con-men.) But if all New Energy claims are in error, why then are hundreds of scientists and engineers reporting their paradigm-breaking findings in prestigious journals, and at international conferences? Why have some corporations, small and large, taken notice? Why do official scientific reports from U.S. government labs, which support the new phenomena, continue to be ignored?

What are you going to do about this? Will you join the pack of naysayers, and deny the existence of modern "miracles" in science and emerging technology, which you could easily check out for yourself, beginning at www.infinite-energy.com? I hope not! Do you dare decide to explore these New Energy developments in our magazine? We hope we have tempted you.

Here are just three facts that you can confirm on your own by subscribing to *Infinite Energy*, or simply checking our web site (www.infinite-energy.com):

- Substantial technical information from experiments around the world on cold fusion energy (more generically, "low-energy nuclear reactions" or LENR) confirms that at least 300 gallons of gasoline energy equivalent can be obtained from just one gallon of ordinary river, lake, or ocean water—with no nuclear radiation hazard, and zero pollution. The product of the reaction is a tiny amount of non-toxic helium gas.
- In just one cubic kilometer of ocean—the world has billions of cubic kilometers of water—there is enough hydrogen fuel for these LENR processes to equal all the known oil reserves on Earth.
- There exist patented electric discharge tube plasma devices (PAGD™ reactors) whose electric power output greatly exceeds electric input. Several scientists have developed a spectacular experimental and theoretical framework that explains how this anomalous energy can come from the "vacuum state."

And that's not all—far from it. *Infinite Energy* has been reporting on developments on the frontiers of the New Energy revolution since March of 1995. Regrettably, there are no commercially available cold fusion/LENR devices—yet—due to the difficult nature of the LENR phenomena and to the mostly unrecognized work of, and the tragic under funding of LENR scientists and technologists.

In the last few years irrefutable new evidence has emerged for New Energy sources other than LENR. Just as for cold fusion, in table-top experiments and prototype devices, it has been possible to tap into other new physics paradigm-busting sources—what some have called "zero point energy" (from standard quantum mechanics), but which other scientists who are most in the vanguard of such efforts call "aether energy." You can read about this shocking development too in *Infinite Energy* magazine. No, these are emphatically not "perpetual motion machines"! They obey the conservation of energy, but they just happen to tap energy in a form that is poorly or incompletely understood by modern physics.

So, with such great possibilities at stake, can you afford not to learn more? Why not join us in a courageous journey of

intellectual exploration? Why not subscribe immediately! The subscription price for our magazine is still just $29.95 a year for six fact-filled, exciting issues. (If you live elsewhere than North America, the cost is US$49.95).

But you can do more than subscribe—much more, we hope. *Infinite Energy* in late 2002 came under the umbrella of the newly formed, non-profit corporation, New Energy Foundation, Inc. in New Hampshire. Contributions to the Foundation are tax-deductible to the fullest extent of the law. The magazine very much needs your generous charitable support—if you decide (and we hope that you have decided already!) that we are doing extremely important work that will help lead to a better world for us all. The New Energy Foundation is now at the very eye of the storm in the swirling New Energy Revolution.

What do we accomplish? New Energy Foundation disseminates information about these world-changing technologies—about the science, technology, patents, investment, and politics thereof; we measure and investigate new claims about new energy devices to determine whether they are sound; and—most important—we are now processing grant applications by scientists and inventors around the world, so that the most promising work—now highly under funded due to the very heretical nature of this work—gets the financial support that it so richly deserves. We are very demanding about these grants; we insist that the research must be headed in the direction of developing publishable scientific results and/or actual commercially useful technologies that operate on new scientific energy principles.

So please do subscribe to *Infinite Energy*, and also please do give generously to support not only this critical global information network, but also a deserving community of pioneering researchers around the world.

Thank you very much for taking the time to consider what we have to say about a bright New Energy future.

Best wishes,

Dr. Eugene F. Mallove
Editor-in-Chief, *Infinite Energy* Magazine
President, New Energy Foundation, Inc.

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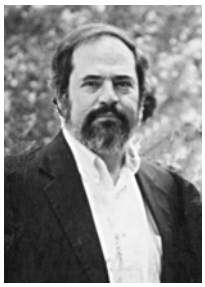
BREAKING THROUGH EDITORIAL

Ten Years That Shook Physics

by Eugene F. Mallove, Sc.D.



When I read George Gamow's book, *Thirty Years that Shook Physics: The Story of Quantum Theory* (Doubleday & Company, 1966), it was impossible to imagine that in less than twenty-five years another revolution would shake physics in ways every bit as dramatic as what happened from 1900 to 1930.

For the past decade, the Cold Fusion and Low-Energy Nuclear Reactions revolution has been underway, whether or not the mainstream physics/chemistry establishment and the general science media wish to acknowledge it. This month we celebrate what has indeed been "Ten Years That Shook Physics." The barrier that separated conventionally understood chemistry and nuclear physics has come crashing down like the infamous Berlin Wall. The barrier does not exist—at least not within special microphysical domains of palladium, nickel, and other metals in contact with hydrogen. Exotic new physics is at work, the "End of Science" again disproved.

The Revolution does not even have a *name* on which all the revolutionaries can agree! "Cold Fusion" is likely to stick, if for no other reason than that is where it all began. The terms LENR (Low-Energy Nuclear Reactions) and CANR (Chemically-Assisted Nuclear Reactions) have been tried. This displeases Dr. Randell Mills of BlackLight Power Corporation, who has a radically different theoretical approach and an apparently robust commercial activity. See Mike Carrell's assessment of the new Mills scientific initiative: "hydrino hydride compounds," page 36 - 39. To be neutral on this issue, we'll float a trial balloon. How about "Nu-chemistry?" It's certainly "new" (nu) and it certainly has nuclear (nuc) aspects—as even Dr. Mills agrees.

The Nu-chemistry Revolution began inauspiciously on March 23, 1989 at the University of Utah. Electrochemists Drs. Martin Fleischmann and Stanley Pons had worked for five years and spent some $100,000 of their own funds before they announced their findings. We are very privileged to have in this issue an essay by Dr. Fleischmann, which reveals some of the scientific thinking that led to the discovery (pp.25-28). Circumstances forced disclosure at a press conference some eighteen months before the scientists had wanted to publish.

Fleischmann and Pons claimed in 1989 that in a heavy water electrochemical cell near room temperature they had produced excess energy orders of magnitude beyond explanation by chemistry, and that they had detected neutrons and tritium as well. These were all signatures of nuclear reactions.

Unfortunately, they did not emphasize the difficulty of producing the effects. At the time, because their hands were tied by lawyers focused on patent issues and conflicts with nearby BYU, they were not even able to provide at their news conference a preprint of their forthcoming *Journal of Electroanalytical Chemistry* paper. It was published April 10, 1989 but was circulating via fax almost immediately. A great fuss has been made about this. In retrospect the delay seems short. Tokamak results have often been announced on the 7:00 o'clock news months before papers are made available. Their neutron measurements were flawed, as they later admitted. This was a failing, yet others would later confirm in cold fusion experiments *both* low-level neutron radiation as well as tritium evolution.

Most important to an understanding of the heated debate of the past decade: The Fleischmann-Pons announcement threatened an entrenched Federal research program. Billions of dollars had been invested by the U.S. government in its decades-long "hot" fusion program, which sought to emulate the thermonuclear conditions in the cores of stars. Hot fusion had promised a distant era of safe, clean, infinite energy from the hydrogen isotope deuterium, which is abundant in water. These programs have resulted in useful plasma physics research, but no net energy release—ever. Thermonuclear bombs were far above "breakeven," but controlled thermonuclear fusion reactors at Princeton and at MIT were not.

Fleischmann and Pons were saying that they had achieved breakeven *already*, and, unlike hot fusion, there were no deadly emissions. The claim of a chemically-assisted nuclear fusion reaction with net energy release threatened to divert Congressional funding from the hot fusion program. With private zeal, and later public scorn, scientists supported by the hot fusion program—particularly at MIT—sought errors in the Fleischmann-Pons work.

When the exact radiation signatures and end-products of hot fusion reactions in a vacuum were not found in the Fleischmann-Pons results or in quickly-done tests at other laboratories, scientists at the MIT Plasma Fusion Center (PFC) yelled "possible fraud," "scam," and "scientific schlock." On May 1, 1989, the MIT PFC planted story in the press unleashed a torrent of anti-scientific bigotry. It did not occur to most scientists that a new class of nuclear reactions might have been discovered. As Nobel Laureate Julian Schwinger would say in a lecture at MIT in November 1991, "The circumstances of cold fusion are not those of hot fusion" (see pg. 81). He was ignored.

The furor over cold fusion in the spring of 1989 prompted President George Bush through Energy Secretary Admiral James Watkins to convene a "Cold Fusion Panel" of the U.S. Department of Energy's Energy Research Advisory Board (ERAB). Nobel Laureate Glenn Seaborg had told President Bush in the Oval Office on April 14, 1989 that the Utah discovery was "not fusion," thus poisoning the well and precluding an honest investigation. One of the twenty-three ERAB panelists had thought at the time: "Just by looking at Fleischmann and Pons on television you could tell they were incompetent boobs." (Prof. William Happer of Princeton, quoted by G. Taubes in *Bad Science*.) So much for the theory of the "unbiased" ERAB panel, which included Professor Mark Wrighton from MIT and the much less involved and (in 1999) apparently "neutral" Prof. Mildred Dresselhaus of MIT.

This panel, convened by the Department of Energy, was

assigned to assess reports from various laboratories and to make recommendations to the U.S. government. Three major laboratories submitted negative reports. These were MIT, Caltech, and Harwell (England). The ERAB report was negative, and quickly so. A preliminary negative conclusion came in July 1989 and the final report November 1, 1989, with the following consequences: 1) No special funding by the U.S. government for further research; 2) Flat denial by the U.S. Patent Office of any application mentioning cold fusion; 3) Suppression of research on the phenomenon in government laboratories; 4) Citation of cold fusion as "pathological science" or "fraud" in numerous books and articles critical of cold fusion in general, and of Fleischmann and Pons in particular.

The 1989 reports of MIT, Caltech, and Harwell have each been analyzed by other scientists and these analyses have been published (see references, page 34). Each of the widely cited 1989 "null" experiments has been found to be deeply flawed in experimental protocols, data evaluation, and presentation. Each, in fact, contained some evidence of excess heat as claimed by Fleischmann and Pons. There is evidence that the MIT data was deliberately altered to erase an indication of excess heat. The altered data was published officially by MIT, and it was included in reports to a government agency under the official seal of MIT. The experiment was paid for out of federal government funds. This report had a dramatic impact on the perception of many scientists and journalists.

It is ironic that each of these negative results were themselves the product of the kind of low quality work of which Fleischmann and Pons were accused. The difference was that the reports said what the hot fusion community wanted to hear. This was the legacy of the 1989 ERAB report, but that legacy must now be reversed—and it will be, however long that takes.

Almost two years after they were concocted, Prof. Ronald R. Parker of MIT's Plasma Fusion Laboratory publicly stated that the MIT PFC cold fusion calorimetry data were "worthless" (June 7, 1991). In the same period (August 30, 1991) after I had challenged this data, Parker stated that "MIT scientists stand by their conclusions." Which is it?

The full story is given in detail in a "MIT and Cold Fusion: A Special Report" in this issue. I was there and I saw what went on—behavior far beneath what one would have expected from MIT. In 1991 I resigned my job in protest, and later founded this magazine. My 1991 book, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (John Wiley & Sons) did not tell all that could have been told then. It took years to put it in proper perspective. Now the story has much more significance because Fleischmann and Pons have been vindicated—if not by the media and by the establishment, certainly by mountains of high quality published results.

We shall see what the MIT authorities of 1999 will do about the misrepresentations some of its staff made in 1989 and in the years thereafter. MIT continues to receive large Federal funding for its tokamak hot fusion project. In fact, as our Special Report reveals, MIT President Charles M. Vest is on a Federal panel that continues to recommend funding for tokamak fusion. The president of an institution as influential as MIT should weigh issues of intellectual integrity and conflict of interest very carefully. But past experience with Charles Vest and cold fusion, documented here, does not inspire confidence.

The literature on the Fleischmann and Pons effect is now voluminous—as most readers of *Infinite Energy* or *Fusion Technology* (an American Nuclear Society publication) know very well. It strongly suggests that what Fleischmann and Pons discovered was but the tip of an iceberg of a much more widespread phenomenon—"Nu-chemistry," if you wish. Selected papers are cited in this issue as a starting point for those who need to study some of the best hard evidence. These are not fantasies. This is solid work, the kind of pioneering, exhaustive experimentation that could have been done at places such as MIT, Caltech, and Harwell, but wasn't.

The production of excess heat in the range of hundreds of megajoules per mole of metal has been confirmed, as well as the production of helium, tritium, and other elements. Power densities of kilowatts per cubic centimeter of electrode have been achieved by some researchers. The field of Low-Energy Nuclear Reactions has been established, if not yet widely recognized. Low-energy neutron or weak gamma radiation are seen in some experiments, but most produce excess heat with no radiation or radioactive byproducts. Rapid remediation of radioactive materials has been demonstrated. What a fantastic opportunity for universities such as MIT to become involved!

The replication and commercial application of the Fleischmann and Pons effect has been inhibited by a lack of understanding of the exact nature of the reactions, which are not those known to plasma physicists. There is a severe and widespread materials and theory problem related to cathode materials that produce the effect. Criteria are available to test cathode materials for potential activity, but knowledge of how to produce such material at will is not available.

Sad to say, solving the materials problem may be beyond the financial resources of the scattered researchers who have worked to validate the Fleischmann and Pons effect. Unfortunately, the negative reports by key hot fusion laboratories to ERAB prevented diversion of government funding from the failed hot fusion program to the more promising field of cold fusion. The patent-crushing ERAB report also became a severe deterrent to private investment in the new energy field.

Ending where this began, we return to George Gamow's musings of 1966, when I was a sophomore at MIT in aero/astro engineering. Gamow thought that the next major physics revolution would be in understanding the very existence of elementary particles. He wrote, "There is hardly any doubt that when such a breakthrough is achieved, it will involve concepts that will be as different from those of today as today's concepts are different from those of classical physics." He was both wrong and right. He could not have suspected that the next physics revolution would begin not with high energy particle physics but with fundamental electrochemistry—and that it would end with the birth of modern alchemy. The revolution will be the end of the world that we have known, this time for the better.

Arthur C. Clarke:

The Man Who "Predicted" Cold Fusion and Modern Alchemy

by Eugene Mallove

"To predict the future we need logic, but we also need faith and imagination, which can sometimes defy logic itself."
—Arthur C. Clarke, *Profiles of the Future*

Arthur C. Clarke might not remember that he really did "predict" cold fusion, so successful have been his many other predictions of technological and scientific breakthroughs—notably many milestones in spaceflight, including his own invention (in 1945!) of the geosynchronous communications satellite. Yet there it is in my well-worn 1964 Bantam Books edition of Clarke's *Profiles of the Future*.

It appears on many pages, but its most startling form is on page 153: "We must remember, however, that nuclear engineering is in roughly the same position as chemical engineering at the beginning of the nineteenth century, when the laws governing reactions between compounds were just beginning to be understood. We now synthesize, on the largest scale, drugs and plastics which yesterday's chemists could not even have produced in their laboratories. Within a few generations, we will surely be able to do the same thing with the elements." Sorry, Arthur, you were a few generations too conservative—we'll forgive you for that! The catalytic transmutations that you predicted are occurring, in their most primitive forms, in cold fusion cells today.

Following this description in *Profiles*, Clarke describes the conventionally understood catalytic nuclear reactions that occur in the Sun, which convert ordinary hydrogen to helium—the first steps in what he says "might be christened 'nuclear chemistry.'" He continues: "But there are other ways of starting reactions, besides heat and pressure. The chemists have known this for years; they employ catalysts which speed up reactions or make them take place at far lower temperatures than they would otherwise do. . .Are there nuclear, as well as chemical, catalysts? Yes, in the Sun, carbon and nitrogen play this role. There may be many other nuclear catalysts, not necessarily elements. Among the legions of misnamed fundamental particles which now perplex the physicist—the mesons and positrons and neutrinos—there may be entities that can bring about fusion at temperatures and pressures that we can handle. Or there may be completely different ways of achieving nuclear synthesis, as unthinkable today as was the uranium reactor only thirty years ago. The seas of this planet contain 100,000,000,000,000,000 tons of hydrogen and 20,000,000,000,000 tons of deuterium. Soon we will learn to use these simplest of all atoms to yield unlimited power. Later—perhaps very much later—we will take the next step, and pile our nuclear building blocks on top of each other to create any element we please."

Well, we can't have expected Arthur to have predicted that palladium, much less ordinary nickel, would be the initiating catalysts of the cold fusion-transmutation revolution, but they are. He was thinking of exotic catalytic nuclear particles. Yet he did allow that there could be "unthinkable," "completely different ways" of achieving nuclear synthesis.

It is interesting that on his chart of "The Future," on the very last page of *Profiles*, under "Physics," Clarke places the invention of "nuclear catalysts" somewhere between the years 2020 and 2030. (This is on page "235" no less, for those who are fond of numerological coincidences.) Under the "Materials and Manufacturing" column he has "Fusion power," meaning *hot* fusion, of course. Well, hot fusion didn't come in 1990 and will probably never come, because it will not be needed, but then again—a nice coincidence—1990 is just about 1989, the year of Cold Fusion Day, March 23.

Arthur may well have predicted even the *critics* of cold fusion. Concluding these nuclear catalyst passages, he writes: "In this inconceivably enormous universe, we can never run out of energy or matter. But we can easily run out of brains."

On page 19 of *Profiles*, Clarke writes: ". . .even when the existence of atomic energy was fully appreciated—say right up to 1940—almost all scientists would have laughed at the idea of liberating it by bringing pieces of metal together. Those who believed that the energy of the nucleus ever could be released almost certainly pictured complicated electrical devices— 'atom smashers' and so forth doing the job. (In the long run, this will probably be the case; it seems that we will need such machines to fuse hydrogen nuclei on the industrial scale. But once again, who knows?)" There again is Clarke's openness to great possibilities—doubting the notion that a simple fusion reactor could be developed, but holding open the *possibility*. "Who knows?", indeed! Barely a quarter of a century after these lines were penned came Fleischmann and Pons.

On page 143 comes an oblique version of the cold fusion prediction: "Perhaps the forced draft of space technology will lead us fairly quickly to a lightweight power cell, holding as much energy per pound as gasoline; when we consider some of the other marvels of modern technology, it seems a modest enough demand." That remark was in the context of energy storage, not power generation. Furthermore, cold fusion cells will have enormously greater energy storage density than gasoline. Even Dr. Randell Mills' "superchemistry" explanation of cold fusion excess energy has a 200 HP automobile going 100,000 miles on a tankful of ordinary water (see *Infinite Energy* #17).

Even a remarkable technological seer, such as Clarke, can sometimes fall short and pen remarks that contradict his more penetrating visions. He also writes (page 143): "It may well be—indeed, at the moment it appears very likely—that fusion plants can be built only in very large sizes, so that no more than a handful would be required to run an entire country. That they can be made small and portable—so that they could be used to drive vehicles, for example, appears most improbable. Their main function will be to produce huge quantities of thermal and electrical energy, and we will still be faced with the problem of getting this energy to the millions of places where it is needed."

Alas, no one is perfect, but Arthur C. Clarke had nearly perfectly clear vision of how to go about the business of technology prediction—as Jed Rothwell recounts in his more encompassing review of *Profiles of the Future* (not reproduced here).

2001: The Coming Age of Hydrogen Power

Arthur C. Clarke

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

Address to Pacific Area Senior Officer Logistics Seminar (PASOLS) on March 29, 1993, Hilton Hotel, Colombo. The audience included 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. On Thursday, all my friends there 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 PANAM; 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.

Now, 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 which have been largely responsible for the Cold War never becoming a hot one, by creating a transparent world, and vastly reducing 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 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. 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.

However, just four years ago, two scientists named Pons and Fleischmann claimed to have achieved "cold fusion" *at room temperature* in certain metals saturated with deuterium, the heavy isotope of hydrogen. Under these conditions, they reported that they were getting out more energy 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.

But meanwhile, there had been an underground movement of scientists who believed that 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 a Japanese consortium, Technova. Even more significant, Japan's Ministry of Industry and International Trade (MITI) is investing millions of dollars in an effort to commercialize the new technology.

The laboratories of NIT—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 each. I don't know how many of them were snapped up, but that price sounds 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 highlights of the conference have been summarized in a 34-page report by Professor Peter Hagelstein, of MIT's Research Laboratory in 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 now beyond serious dispute that anomalous amounts of energy are being produced from hydrogen by some unknown

reaction. The term "cold fusion"—"CF"—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 *was* 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? Well, they have now been detected—but in quantities far too small to account for the energy liberated. The theoretical basis of CF is therefore still a major mystery—as was the energy produced by radioactivity and uranium fission when they were first discovered.

Now, what are the implications of this? I'd like to give several scenarios.

The first: 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: CF 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.

The next scenario: CF 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 Age of Fossil Fuels has ended. So has the Age of 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.

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 now got into the cold fusion act, and they have just published some startling results in *Physics Letters A*, one of the world's leading scientific journals. They are obtaining about five times their energy input in *gas mixtures,* not solids, and at temperatures of up to 1800˚C. Now 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 planet. 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 round tickets cost about two billion dollars per passenger is a measure of the chemically-fueled rocket's inefficiency.

Well, back to Earth. I'd like to read you a letter which I sent to Vice-President Al Gore last week; it should have reached him by now:

18 March 1993

Dear Mr. Gore,

COLD FUSION (?)

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 Number (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 (26 March). 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% confidence that I'm now doing something very similar.

And this time, it's good news.

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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).

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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).

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Transcript of ABC's "Good Morning America" Program
February 7, 1996 (8:16 - 8:22 a.m.)
Report on Clean Energy Technology, Inc. (CETI) Cold Fusion Device

Charlie (Gibson): "Scientists discover a virtually limitless source of energy." Does it sound too good to be true? Maybe not. Our Science editor Michael Guillen is down in Washington this morning, having an exclusive look at an invention which has the potential of changing our lives. Michael?
(Michael) Guillen: Thanks, Charlie. It's a device that its inventor says produces a hundred times more energy than it consumes. Now let me say right off the bat that lots of ideas come across my desk, that claim to be the energy source of the future, but this one is different. For one thing, the inventor has a distinguished track record. Second, the invention itself has been issued a patent by the U.S. Patent Office. Furthermore, and this is key, independent scientists now claim to have reproduced the results, and major corporations like Motorola are taking a serious interest in it. So, is this potentially the greatest discovery since electricity? Since fire? Good question! Have a look.

[Brief interviews]
James Reding, president Clean Energy Technologies Inc: We've been able to reliably demonstrate a device that produces a thousand times more energy out than you put into it.
Prof. George Miley, University of Illinois: What could it do as far as an electric power plant or a water heater in your home? There are so many applications that the mind can run wild.
Dr. C. Quinton Bowles, University of Missouri: It would be a true source of power for use by the general public.

Guillen: It's hard to believe, but here's what is causing all the commotion. It doesn't look like much—some wires, some salt water, and at the core of it: this container of tiny beads. But these are no ordinary beads, and the man who invented them is no ordinary person.

[Interview with James Patterson in his laboratory]
Patterson: I started making beads back in 1953.
Guillen: Seveny-four-year-old James Patterson looks about as homespun as his device, working out of a large garage in Sarasota, Florida, with more than a hundred patents to his credit. Patterson had always planned on being a chemistry professor but in 1951, while working for his Ph.D. at Berkeley, Dow Chemical made him an offer he couldn't refuse.
Patterson: Dow hired me before I graduated, got my degree. And they paid me more than what I was going to get after I got my degree. So. . .
Guillen: It was during his years with Dow that Patterson invented a recipe for making tiny beads, beads so perfectly round that few people in the world can duplicate them.
Patterson: If I have a claim to fame, [laughs] I'm a good cook for little beads. Well, this is my storage area, and—it's almost like a library of what I've done.
Guillen: Over the years, Patterson's beads have been used in many different ways: in water purifiers, cosmetics, even as the "talcum powder" inside surgical gloves.
Patterson: I'm better than a millionaire.
Guillen: Just because of the money you got from. . .
Patterson:. . . little beads! [laughs] I have converted alchemy. . .little beads into gold!
Guillen: Talk about alchemy! In creating his new energy device, Patterson took his regular beads, and coated them with thin layers of copper, nickel and palladium; a metal sandwich Patterson claims works like magic.
Patterson: This is the guts of it, this is creating heat.
Guillen: So this is water that you have flowing through it. [Feels outlet tube.] Oh, it is hot. Pretty warm! Yeah. And so how much energy is this little cell putting out, compared to what it's consuming?
Patterson: One watt. It's consuming only 1 watt, and it's putting out 200 watts.
Guillen: You know this sounds too good to be true?
Patterson: [Laughs] Well, it may sound too good to be true, but if you'll only look, the scientific evidence is here. I mean, you're looking at it. I mean, you can't disavow what you're looking at.

[Cut back to studio]
Charlie: Michael, alright, we're looking at it, but how's it working?
Guillen: Well, you know, even for a scientist like myself, you can't just tell by looking or even touching it. That's where the other scientists come in, at the University of Missouri, the University of Illinois, and at Motorola. They have tested dozens of these devices, they say they can't get it not to work. Every time you plug it in, the doggone thing just produces all this excess heat.
Charlie: But what's going on, scientifically?
Guillen: Well, that's the big mystery. It's either, you know, an ordinary chemical reaction that's not behaving the way we expect it to, or some kind of a nuclear reaction. But there is no radioactivity that's evident from this thing so it doesn't appear to be a nuclear reaction. It's neither one nor the other, so it really is just a genuine mystery right now.
Charlie: Michael, what you are telling me is you have a scientific experiment that is producing a certain result and you have no idea how it's producing it.
Guillen: Yeah, but that's not unusual. I mean, very often times you run across something in the laboratory and you go, "Wow! Look what it's doing" long before you understand why it's doing that.
Charlie: Michael, this sounds like going back to 19. . .what? 1989?
Guillen: 1989.
Charlie: This sounds like the cold fusion debate again.
Guillen: Yeah. Remember the University of Utah, the whole cold fusion thing? Superficially this looks like cold fusion, in the sense that you have electricity passing through an electrode that is immersed in salt water. But there are essential technical differences. First of all the beads make this cell absolutely unique. That wasn't like the original cold fusion device. The other thing is that the original cold fusion device used heavy water, this uses ordinary water. So, it remains to be seen whether this is just a variation of the old cold fusion experiment or whether this is genuinely a new phenomena.
Charlie: Is there an anticipation that what is taking place here in microcosm can take place in a macro situation where you can produce a tremendous amount of energy?
Guillen: Now *that* is going to be the key question. If the scientists at the independent universities and corporations continue to verify that this device seems to work, the next question is going to be: can you scale it up from this laboratory model into something that can be mass produced, and be cost efficient. Because we have heard other alternative energy like wind power and solar power, they also sound great but they have never become cost efficient. That's going to be the big question in the future.
Charlie: You keep saying "if this works." You are telling me that a number of scientists have been able to make it work. There are also a bunch of other scientists who are saying this is just crazy.
Guillen: Yeah. The scientists are really cautious because of the old cold fusion flap six years ago. They want to be real cautious. The question is here, you have to measure the temperature differences, how much of the heat is putting out. . .is being put out by this device. That requires you to use thermometers of various kinds. They are just double, triple, and quadruple checking those thermometers to make sure they are not misreading them. But they are all saying yes, this seems to work as advertised. So it's potentially historic.
Charlie: Five seconds: are you a believer or not?
Guillen: Uh, talk to me in about two or three months. We're going to be updating this.
Charlie: All right. Michael, thanks. Michael will have more of this on "Nightline," tonight.

ABC News "Nightline" Program Features Patterson Cold Fusion Device

by Jed Rothwell

On February 7, 1996, the ABC late night news program "Nightline" was devoted to James Patterson's cold fusion device, which is being commercialized by Clean Energy Technology, Inc. (CETI), of Dallas, Texas. It was titled "Patterson Power Cell: Fact or Fiction?" A shorter, five minute segment about Patterson, with the same film clips, was shown on the morning broadcast "Good Morning America" (see transcript of the latter adjacent to this article). The show was written and narrated by ABC's chief science editor Michael Guillen. The broadcasts included a long interview with Patterson, and brief interviews with Jim Reding, president of CETI; Professor George Miley of the University of Illinois; and Professor Clinton Bowles of the University of Missouri. The Nightline version included a short question and answer style debate between Michael McKubre of SRI and Prof. John Huizenga, retired from the University of Rochester, who was the head of the DOE ERAB panel that eliminated funding for cold fusion research in the fall of 1989. The program also had brief interviews with Patterson's patent attorney. On the opposing side were Professor Herman Feshbach of the MIT Department of Physics (who totally denies the validity of cold fusion evidence), and Professor Howard Birnbaum of the University of Illinois, another negativist and member of the 1989 ERAB Cold Fusion panel.

In recent years, two serious, hour-long documentaries about cold fusion have been broadcast by the BBC in the U.K. and the CBC in Canada: "The Secret Life of Cold Fusion" (June 1993), and "Too Close to the Sun" (April 1994). The latter was a co-production of the BBC in the U.K. (where it is called the "Horizon" series) and the Canadian Broadcasting Corporation.

This ABC Nightline broadcast is, sad to say, the longest and most serious look at cold fusion in U.S. television since 1989. Guillen did a short segment on Pons and Fleischmann in May 31, 1994, after visiting their IMRA laboratory in Nice, France. NOVA, the main U.S. science documentary program, has totally abandoned its responsibility by having a complete blackout on news of cold fusion since 1990, when it broadcast the atrociously negatively biased "Confusion in a Jar." A member of the NOVA staff—Evcan Haddingham—has discussed cold fusion with Eugene Mallove over the years and has told him that "they know they need to update their cold fusion coverage." Still, nothing has been done by NOVA—not even broadcasting "Too Close to the Sun," which NOVA could readily do if it so chose.

The "Nightline" program was generally positive and informative, although it had little scientific content. It began with a down-home interview with Dr. James Patterson in Sarasota Florida, and tour of his lab, which is, as he put it, "like a library of what I've done." It is filled with old chemicals, obsolete machines and junk. It was messy, but nowhere near as bad as the laboratories I have visited at MIT and the Japanese National Universities. It struck me as an ideal place to do research. Patterson, who is 74, briefly described his career which began at Berkeley and Dow Chemical. He is an expert in manufacturing small, uniform beads for a variety of applications. For example, microscopic beads are used as a man-made replacement for talcum powder in surgical gloves, and larger beads are used in catalysis. The latter application inspired Patterson to try them out as cold fusion cathode material. Patterson holds more than 100 patents for beads and other innovations, including, he told me once, a new type of fishhook. The bead patents have made him a multi-millionaire.

The shots of Patterson in his lab were fun but frustrating. You see him giving an animated, interesting and apparently informative scientific briefing, describing the instruments and methodology. Unfortunately, you cannot hear him because these segments are used as filler, background shots, with a foreground voice-over vapid script from the television writer. Turn the volume way up, listen and watch closely, and you see Patterson had some interesting things to say. If only the producers would say less and let him talk more we might have had some higher scientific content in this program. Even these short segments, and abbreviated glances at the chart recorder, beads, cell, pumps and other equipment convey a lot of information. Patterson claimed that 1 watt was input and 200 watts were coming out. Look carefully, and you can see the return water splashing into the pump reservoir, indicating a high flow rate. At one point Guillen put his hand on the outlet tube and declared: "It's pretty warm." I felt like shouting: "Okay Mike, now put your hand on the other tube and tell us about it." If they had shown a few more details like the input power supplies we could have worked out a ballpark estimate of the excess for ourselves. With the high flow rate and 1-watt input Guillen could not have felt any palpable difference between the inlet and outlet temperatures unless there was massive excess heat.

The program suffered from a curiously amnesic, dreamlike detachment. It is a story told in a vacuum, with no context, no background, no reference to history or other current research. The morning segment barely mentioned the term "cold fusion," except when Guillen said: "Remember the University of Utah, the whole cold fusion thing? Superficially this looks like cold fusion, in the sense that you have electricity passing through an electrode that is immersed in salt water. But there are essential technical differences. First of all the beads make this cell absolutely unique."

That would come as a shock to other researchers who have used thin film and nickel cathodes, and especially to Mills, who was the first to publish reports of excess heat from nickel.[1] Piantelli's gas loaded nickel experiments are unique, but Patterson's technique draws on many previous mainstream experiments.[2] There was no mention of the ongoing MITI/NEDO project, no mention of any work after 1989 (except Patterson's), nothing about the international conferences, and no hint that the literature reports widespread replications. Miley is shown working with the CETI thin film device, but Guillen does not mention that Miley published a paper describing his own thin-film cold fusion cathodes.[3]

Miley has independently replicated the CETI beads from scratch, and he has verified the performance of beads provided to him by CETI. He expressed confidence: "We've consistently measured excess energy coming out of it." Bowles has verified the performance of the CETI cells. He is funded by Kansas City Power and Light. He said: "These Patterson cells seem to be

unique, and somewhat amazing, in the—in their reproducibility. . .We have, in fact, had a total of three cells at different stages over the last nine months, and it's fair to say that all three of them appear to be producing excess power." Motorola's involvement was hinted at, just as it was in the *Wall Street Journal* article.[4] Guillen: "Already, says Patterson, Motorola has tested his cells and offered to buy him out." [Ed. note: Motorola representatives were there in force at the CETI demo at the Power Gen '95 meeting in December.]

The second half of the program was devoted to a depressing debate between Dr. Michael McKubre of SRI International, one of the leading cold fusion researchers, and Prof. John Huizenga, the "Darth Vader" against the field. I felt sorry for Huizenga, who looked old, tired, and nervous. He began by saying: ". . .let me simply say that since Pons and Fleischmann's results were shown to be flawed, there have arisen a whole array of exotic phenomena, including the synthesis of precious metals like gold, which would, of course, be the alchemist's dream, and the light water cells, I think, that are discussed. . .that Mr. Patterson is working on have all been shown not to be producing excess reaction products in the past, and I don't think these people have looked for the reaction products either."

Needless to say, from our point of view Pons and Fleischmann have *not* been shown to be flawed, they have been widely reproduced. But the real issue here is nuclear evidence: neutrons. Whenever the issue of excess heat comes up, Huizenga always evades it and talks about reaction products instead. In point of fact he is incorrect about this particular case. Miley and others are looking for reaction products. But Huizenga claims there are no neutrons and therefore there can be no nuclear reaction, and therefore the calorimetric results must be wrong. When asked for a reason why the calorimetry might be wrong, he always responds as he did here: "I'm simply saying that what I know about these experiments, they're using an open cell and they're not taking account of recombination. . ."

At that point McKubre, who has heard this as many times as I have, could not help interrupting for a moment to say: "That's completely incorrect, completely incorrect." Huizenga went on to cite "many, many errors that they are making that have not been accounted for." He has often cited these many phantom errors, but he has never actually listed one of them. McKubre is quite right: the CETI results are far too big to be explained by recombination. The best results reported to date are 4,000 times beyond the limits of recombination, and furthermore CETI researchers do take account of recombination, with a precision gas flowmeter, so Huizenga is wrong on both counts. Huizenga was not aware of these facts, because, as he admitted, he has not actually seen or read about the CETI experiments. Herman Feshbach also claimed scientific clairvoyance: "I don't know the device, so I don't know what's in it and what's not in it. I can only speak in generalities, and the one thing I can say unequivocally, without any concern, is that it's not a nuclear phenomenon."

This brings to mind Feshbach's famous 1991 pronouncement to Eugene Mallove: "I have had fifty years of experience in nuclear physics and I know what's possible and what's impossible. . . .I don't want to see any more evidence! I think it's a bunch of junk and I don't want to have anything further to do with it."

Evidently, he now decided to make it a rule that he will see no more evidence, and that he can safely pontificate on national television about research he has never heard of.

Huizenga also claimed: "It turns out that mainline scientists have spent hundreds of millions of dollars looking at all of these claims, and no one has been able to verify the cold fusion experiments."

He has often said this, but he never specifies which scientists have spend hundreds of millions. Japanese scientists have spent a hundred million (at least), and MITI has budgeted $100 million more over the next four years, but these examples do not count. The Japanese claim that they have been able to verify the cold fusion experiments. Huizenga is looking for an invisible army of researchers who have spent this kind of money and found nothing.

McKubre closed the debate by justifying continued research: "Well, there is, in fact, no theoretical objection to the existence of a nuclear process occurring in a solid metal lattice. Given the fact that it is not theoretically impossible—and it's not—given the fact that people are observing it in numerous laboratories around the world, I think it would pay us to pay some attention to it, and the amounts of money that are being spent on this research are very, very small."

Video Tapes of ABC's news broadcasts can be purchased by dialing 1-800-913-3434. Transcripts are available from 1-800-255-6397. They can be delivered by e-mail for $10 per copy. This program was "Nightline (ABC) #3838."

Footnotes

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3. G.H. Miley, E.G. Batyrbekov, H. Hora, R.L. Zich, "Electrolytic Cell with Multilayer Thin-film Electrodes," *Fusion Technology*, Vol. 6, p. 313.

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Julian Schwinger—A Fond Remembrance

by Eugene Mallove

Julian Schwinger, whose contributions to quantum physics are legendary, lived to see the difficult birth of the Cold Fusion Age. Born in 1918 in New York City, in his youth he was a boy genius with a rebellious streak. That almost prevented him from receiving his doctorate from Columbia University. Beginning in 1945 he taught at Harvard University and in 1972 he joined the faculty of the University of California, Los Angeles, where he was a distinguished professor of physics. His contributions to quantum electrodynamics eventually led him to share the Nobel prize in 1965 with Richard P. Feynman and Shin'ichiro Tomonaga, both of whom he outlived.

On July 16, 1994, he died after the sudden onset of an illness, working to the very end on physical theories, including his theory of sonoluminescence—which he had metaphorically connected with cold fusion on several occasions. He left behind his wife of 47 years, Clarice Carrol Schwinger, as well as a host of compatriots in cold fusion research. By coincidence, he died on the precise day of the 25th anniversary of a pioneering event: the launching of the first manned expedition to the lunar surface —Apollo 11.

For Julian's great contributions to the cold fusion field—both in theoretical support and in his solidarity with fellow scientists — we can do no less than reprint here one of his great works. We lead with his erudite talk to the Fourth International Conference on Cold Fusion, held in Maui, Hawaii in early December 1993.

I first met Julian Schwinger and his wife Clarice on a memorable evening in Salt Lake City, in March 1990 at the First Annual Conference on Cold Fusion. I next had direct contact with him when in the spring of 1991 while still at MIT as chief science writer at the MIT News Office, I received a welcome notice from John Wiley & Sons, my publisher. The excited voice over the telephone said that Schwinger had read the draft manuscript of my cold fusion book, *Fire from Ice*, and had praised it with characteristic elegance thus, "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." I can't deny that my head spun upon receiving that praise from a Nobel laureate.

Thus began a long friendship through what might be called the first five years of the "Cold Fusion War." I soon learned that Julian had resigned from the American Physical Society rather than put up with some of its members' wanton censorship of his theories about cold fusion. He would subsequently publish his ideas in other journals, and in the Proceedings of the National Academy of Sciences.

Many a time since 1991 I would call Julian late at night (in my eastern U.S. time zone) and Clarice would tear him away from his work so that we could chat—often for an hour—about the latest developments in experiments, theories, and politics. I am proud to say that Julian Schwinger signed a petition to the Congress of the United States—in particular to the House Science, Space, and Technology Committee—urging a Congressional reassessment of research funding for cold fusion. Unfortunately, the Congress did nothing to move the immovable objects at DOE, who lavish funding on hot fusion and ignore cold fusion.

It is impossible to write enough in this short space that would honor Julian to the extent that he deserves. It is best simply to provide below a record of his published contributions to the related fields of cold fusion and sonoluminescence. The cover story of this premier issue speaks of a deep connection between the two enigmatic phenomena.

Schwinger had the deep respect of other theorists, even as they disagreed with some of this ideas. This is what MIT cold fusion theorist Peter L. Hagelstein said of Julian in his tribute in *Fusion Technology* (December 1994): "He was one of my heroes. Even though I did not know him personally, I feel a personal loss as well as a loss to the world of one of the greatest minds of our time."

In one of his cold fusion essays, Schwinger issued a stern warning that is still being ignored by the denizens of the scientific establishment: "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." One of those denizens admits in his obituary on Schwinger that he refused to read what Julian had to say about cold fusion." Physicist-author Jeremy Bernstein, writing in the Spring 1995 *American Scholar*: "The last news I had about Julian, before his unexpected death in July, was that he had become interested in cold fusion. Someone sent me a reprint of a lecture that Julian had given in Japan on the subject. I didn't have the heart to read it."

Julian's spirit lives on and will have the last laugh—that's for sure.

Important Works by Julian Schwinger

- "Nuclear Energy in an Atomic Lattice." *Proc. of the First Annual Conference on Cold Fusion*, March 28-31, 1990, Salt Lake City, pp. 130-136.
- "Cold Fusion: A Hypothesis," *Zeitschrift Fur 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—Causal 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 Natl. Acad. Sci.*, Vol. 89, May 1992, pp. 4091-4093.
- "Casimir Energy for Dielectrics: Spherical Geometry," *Proc. 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; in *Fusion Technology*, Vol. 26, December 1994.

Cold Fusion Theory: A Brief History of Mine

by Julian Schwinger

Nobel laureate Julian Schwinger's talk at the Fourth International Conference on Cold Fusion, ICCF4, Maui, Hawaii, December 1994. Because Julian was not able to attend ICCF4, his presentation was read to an evening session by Eugene Mallove. This was the second talk that Schwinger had delivered to a cold fusion conference, the first having been in Salt Lake City in March 1990 at the First Annual Conference on Cold Fusion.—Ed.

As Polonious might have said: "Neither a true-believer nor a disbeliever be." From the very beginning in a radio broadcast on the evening of March 23, 1989, I have asked myself not whether Pons and Fleischmann are right—but whether a mechanism can be identified that will produce nuclear energy by manipulations at the atomic—the chemical—level. Of course, the acceptance of that interpretation of their data is needed as a working hypothesis, in order to have quantitative tests of proposed mechanisms.

As a long-time nuclear physicist, the knee jerk reaction to the idea of a D-D reaction without significant neutron production brought in words like ^{4}He and Mössbauer effect. I tried, without success, to contact P(ons) and F(leischmann), to the point of sending a letter to the *Los Angeles Times*, which was garbled in the editing process. Finally, with the help of a friend, contact was made in the early part of April and I went to Salt Lake City.

There, I was assured that they knew about ^{4}He, and was shown a peak in a spectroscopic read-out which, I was told, was ^{4}He. Soon after my return to Los Angeles, references to ^{4}He disappeared, to resurface only relatively recently.

I do not have to, but shall-remind you of the two fundamental problems that the acceptance of P&F's excess heat as nuclear in origin entails.

1. What accounts for the absence of particles that are familiar in ordinary hot fusion, such as the neutrons of $D + D \rightarrow n + {}^3He$ and the high energy γ-ray of $D + D \rightarrow \gamma + {}^4He$? Very early in my thinking I added the conventional reaction $p + D \rightarrow \gamma + {}^3He$. Why? Mostly because it would also be there. One cannot produce heavy water without some contamination by light water.

2. Hot fusion relies on achieving enough kinetic energy to overcome the Coulomb repulsion between like charges. How then can cold fusion, operating far below those levels, ever achieve fusion? Incidentally, I have read, and heard, that my solution to the Coulomb barrier problem is to forget it! Not even an absent-minded professor (which I am not) would go that far. Critics should learn to operate within the bounds of sanity.

My first attempt at publication, for the record, was a total disaster. "Cold Fusion: A Hypothesis" was written to suggest several critical experiments, which is the function of hypothesis. The masked reviewers, to a person, ignored that, and complained that I had not proved the underlying assumptions. Has the knowledge that physics is an experimental science been totally lost?

The paper was submitted, in August 1989, to *Physical Review Letters*. I anticipated that *PRL* would have some difficulty with what had become a very controversial subject, but I felt an obligation to give them the first chance. What I had not expected—as I wrote in my subsequent letter of resignation from the American Physical Society—was contempt.

"Hypothesis" was eventually published, after protracted delays, in a 1990 issue of a German periodical. Does it have any significance in 1993? I cite the following excerpts:

> . . .this cold fusion process (of P&F) is not powered by a DD reaction. Rather, it is an HD reaction, which feeds on the small contamination of D_20 by H_20.
>
> The HD reaction $p + d \rightarrow {}^3He$ does not have an accompanying γ-ray; the excess energy is taken up by the metallic lattice of Pd alloyed with D.
>
> . . .concerning the oft repeated demand for a control experiment using H_20, one should note the possibility of a converse effect of the HD reaction: Through the natural presence of D_20 in ordinary water, such control experiments might produce an otherwise puzzling amount of heat.

A following paper, entitled: "Nuclear Energy in an Atomic Lattice, 1," was sent directly to another German periodical, in November of 1989. As of today, the only memorable part is a quotation from Joseph Priestly: "In this business, more is owed to what we call chance—that is, to the observation of events arising from unknown causes—than to any preconceived theory."

The editor thought it necessary to add a total disclaimer of responsibility, ending with: We leave the final judgment to our readers." In my naivity I had thought that was always so. When Part 2 of "NEAL" was submitted, it was simply rejected. The fix was in.

I gave a talk with the same title in Salt Lake City in March 1990. The HD hypothesis—of the dominance of the pd reaction—has the pragmatic advantage of suppressing neutron production at the level of excess heat generation.

To quote from that lecture:

> . . .a well-trained hot fusioneer will instantly object that there must also be a 5.5 MeV γ-ray. He will not fail to point out that no such radiation has been observed. Indeed. . .But consider the circumstances of cold fusion.
>
> At very low energies of relative motion, the proton and deuteron of the HD reaction are in an s-state, one of zero orbital angular momentum, and therefore of positive orbital parity. The intrinsic parities of proton, deuteron, and ^{3}He are also positive. Then, the usually dominant electric dipole radiation—which requires a parity change—is forbidden.

I turn from "missing" radiation to Coulomb repulsion, and quote:

> . . .treatments of nuclear fusion between positively charged particles (usually) represent the reaction rate as the product of two factors. The first factor is a barrier penetration probability. It refers entirely to the electric forces of repulsion. The second factor is an intrinsic nuclear reaction rate. It refers entirely to nuclear forces.

> This representation. . .may be true enough under the circumstances of hot fusion. But, in very low energy cold fusion one deals essentially with a single state, or wave function, all parts of which are coherent. It is not possible to totally isolate the effect of the electric forces from that of the nuclear forces: The correct treatment of cold fusion will be free of the collision-dominated mentality of the hot fusioneers.

To speak of transferring energy to the lattice is to invoke lattice excitations, or phonons. At about the time of the Salt Lake City meeting, or shortly after, I became dissatisfied with my treatment, and began to reconstruct phonon theory. A note entitled "Phonon Representations" was submitted to the Proceedings of the National Academy of Sciences in June of 1990. The abstract reads: "The gap between the nonlocalized lattice phonon description and the localized Einstein oscillator treatment is filled by transforming the phonon Hamiltonian back to particle variables. The particle-coordinate, normalized wave function for the phonon vacuum state is exhibited."

A month later, I submitted a second note with the title "Phonon Dynamics." The abstract reads: "An atomic lattice in its ground state is excited by the rapid displacement and release of an atomic constituent. The time dependence of the energy transfer to other constituents is studied. . ."

The third and last note is called "Phonon Green's Function." Its abstract is: "The concepts of source and quantum action principle are used to produce the phonon Green's function appropriate for an initial phonon vacuum state. An application to the Mössbauer effect is presented."

I remind you that the Mössbauer effect refers to "an excited nucleus of an atom, imbedded in a lattice, (that) decays with the emission of a γ-ray," thereby transferring momentum to the lattice. "There is a certain probability. . .that the phonon spectrum of the lattice will remain unexcited, as evidenced by the absence, in the γ-ray energy, of the red-shift associated with recoil energy."

A casual explanation of the Mössbauer effect has it that the recoil momentum is transferred to the lattice as a whole so that the recoil energy, varying inversely with the mass of the entire lattice, is extravagantly small. As Pauli would say, even to God, "Das ist falsch!" The spontaneous decay of a single excited atom in the lattice is a localized event, the consequences of which flow at finite speed, out into three dimensional space, weakening as they travel. This is a microscopic event, with no dependence on macroscopic parameters such as the total mass of the lattice.

Unmentioned in the abstract, but of far greater importance, is another situation. To quote: "What happens if the momentum impulse. . .is applied, not to one, but all lattice sites?" The reader is invited to "recall that the lattice geometry is not absolute, but relative to the position of the center of mass for the entire system. Thus the injected energy can be read as the kinetic energy transferred to the lattice as a whole." More of this shortly.

In the last month of 1990, I went to Tokyo. The occasion was the 100th anniversary of the birth of a famous Japanese physicist, perhaps most familiar for his part in the Klein-Nishima formula for Compton scattering. On a day that, to my surprise, I found uncomfortably close to another infamous day, I delivered a lecture on: "Cold Fusion: Does It Have a Future?" The abstract reads: "The case against the reality of cold fusion is outlined. 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 on the hypothesis that small segments of the lattice can absorb released nuclear energy."

I pick up the last sentence of the abstract with this quotation from the text:

> 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, that a small, but macroscopic piece of the lattice absorbs the excess energy of the HD or DD reaction. I advance the idea of the lattice playing a vital role as a hypothesis. . .Intermittency is the hallmark of cold fusion. . .Does the lattice hypothesis have a natural explanation for intermittency?. . .a close approach to saturation loading is required for effective fusion to take place. But, surely, the loading of deuterium into the palladium lattice does not occur with perfect spatial uniformity. 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 picture is not inconsistent with the observation of extensive cracking after long runs.)

What answer did I give, just three years ago to "Does cold fusion have a future?" I said: "I have little hope for it in Europe and the United States–the West. It is to the East, and, specifically, to Japan that I turn."

Inspired by good soba and sushi, I dashed off a short addendum that *Progress of Theoretical Physics* received in January and published in April of 1991. The abstract of "Nuclear Energy in an Atomic Lattice-Causal Order" is: "The extremely small penetrability of the Coulomb barrier is generally adduced to dismiss the possibility of low energy (cold) fusion. The existence of other mechanisms that could invalidate this logic is pointed out."

Here are excerpts.

> . . .Implicit in this line of thought (of negligible penetrability) is the apparently self-evident causality assignment that has the release into the surrounding environment, of energy at the nuclear level, occur after the penetration of the Coulomb barrier. One would hardly question that time sequence when the environment is the vacuum. But does it necessarily apply to the surrounding ionic lattice?. . .another reading is possible, one in which the causal order is reversed. Why? Because, in contrast with the vacuum, the lattice is a dynamical system, capable of storing and exchanging

energy.

> The initial stage of the new mechanism can be described as an energy fluctuation, within the uniform lattice segment, that takes energy at the nuclear level from a pd or dd pair and transfers it to the rest of the lattice, leaving the pair in a virtual state of negative energy. . .
>
> For the final stage. . .consider the pd example where there is a stable bound state: ^{3}He. If the energy of the virtual state nearly coincides with that of ^{3}He, a resonant situation exists, leading to amplification, rather than Coulomb barrier suppression.
>
> It would seem that two mechanisms are available. . .But are they not extreme examples of mechanisms that in general possess no particular causal order?

The last lecture on cold fusion was delivered—twice—in the Fall of 1991, to celebrate the birthdays of former students, one of whom is at MIT, a hotbed of hot fusioneers. The cover title: "A Progress Report," injects a bit of my own nostalgia. Not long after the simultaneous arrival of myself at Berkeley and World War II, Robert Oppenheimer gave a lecture with that title. As he explained, it meant only that time had elapsed. That also applied to the first part of my birthday lectures—"Energy Transfer in Cold Fusion"—with one exception: "I note here the interesting possibility that the ^{3}He produced in the pd 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.

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 may be 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.

The second part of "A Progress Report" is entitled: "Energy Transport in Sonoluminescence." What is that? The text begins with:

> The suggestion that nuclear energy could be transferred to an atomic lattice is usually dismissed. . .because of the great disparity between atomic and nuclear energy scales; of the order 10^7, say. It is, therefore, of great psychological importance that one can point to a phenomenon in which the transfer of energy between different scales involves (an) amplification of about eleven orders of magnitude.
>
> It all began with the sea trials, in 1894, of the destroyer HMS Daring. The onset, at high speeds, of severe propeller vibrations led to the suggestion that bubbles were forming and collapsing—the phenomenon of cavitation. Some 23 years later, during World War I, Lord Rayleigh, no less, was brought in to study the problem. He agreed that cavitation, with its accompanying production of pressure, turbulence, and heat, was the culprit. And, of course, he devised a theory of cavitation. But, there, he seems to have fallen into the same error as did Isaac Newton, who, in his theory of sound assumed isothermal conditions. As Laplace pointed out in 1816, under circumstances of rapid change, adiabatic conditions are more appropriate.
>
> During World War I, the growing need to detect enemy submarines led to the development of what was then called (by the British, anyway) subaqueous sound-ranging. The consequent improvement in strong acoustic sources found no scientific applications until 1927. It was then discovered that, when a high intensity sound field produced cavitation in water, hydrogen peroxide was formed. Some five years later came a conjecture that, if cavitation could produce such large chemical energies, it might also generate visible light. This was confirmed in 1934, thereby initiating the subject of sonoluminescence, SL. I should, however, qualify the initial discovery as that of incoherent SL, for, as cavitation noise attests, bubbles are randomly and uncontrollably created and destroyed.
>
> The first hint of coherent SL occurred in 1970 when SL was observed without accompanying cavitation noise. This indicates that circumstances exist in which bubbles are stable. But not until 1990 was it demonstrated that an SL stream of light could be produced by a single stable cavity.
>
> Ordinarily, a cavity in liquid is unstable. But it can be stabilized by the alternating cycles of compression and expansion that an acoustic field produces, provided that sonic amplitudes and frequencies are properly chosen. The study of coherent SL, now under way at UCLA under the direction of Professor Seth Putterman, has yielded some remarkable results.
>
> What, to the naked eye, appears as a steady, dim blue light, a photomultiplier reveals to be a clock-like sequence of pulses in step with the sonic period, which is of the order of 10^{-4} seconds. Each pulse contains about 10^5 photons, which are emitted in less than 50 picoseconds, that is, in about 10^{-11} seconds.
>
> When I first heard about coherent SL (my term), some months ago (June 1991), my immediate reaction was: This is the dynamical Casimir effect. The static Casimir effect, as usually presented, is a short range, non-classical attractive force between parallel conducting plates situated in a vacuum. Related effects appear for other geometries, and for dielectric bodies instead of conductors.
>
> A bubble in water is a hole in a dielectric medium. Under the influence of an oscillating acoustical field, the bubble expands and contracts, with an intrinsic time scale that may be considerably shorter than that of the acoustical field. The accelerated notions of the dielectrical material create a time-dependent dynamical electromagnetic field, which is a source of radiation. Owing to the large fractional change in bubble dimensions that may occur, the relation between field and source could be highly nonlinear, resulting in substantial frequency amplification.
>
> The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different. (So I wrote in 1991.) But they both depend significantly on nonlinear effects. Put in that light, the failures of naive intuition are understandable. So ends my Progress Report.

In the more than two years that have elapsed since the birthday lectures, I have concentrated on the theory of coherent sonoluminescence. Why? Because, of the two physical processes that naive intuition rejects, it is coherent SL that exists beyond doubt. (No, Mr. Taubes, not even you could cry fraud. Too many people have seen the light.) With the advantage of reproducible data, under variable circumstances, constructing a convincing theory for coherent SL should be, by far, the simpler. That, in turn, should supply analogies for theory construction in a domain that is characterized experimentally by "irreproducibility and uncontrollable emission in bursts."

My gut feeling about the Casimir effect, in a dynamical role, first needed some brushing up in the static domain, which I had not thought about for 15 years. My progress in doing that, along with needed simplifications, is recorded in four notes, published in 1992. Two of them share the title "Casimir energy for dielectrics." Each note acknowledges the stimulation provided by the phenomenon of coherent SL. I give only this brief excerpt concerning the action quantity W_o:

> What the static and dynamic Casimir effects share is the reference to the quantum probability amplitude for the preservation of the photon vacuum state: (exponential of iW_o). That the vacuum persistence probability is less than one, in a dynamical situation where photons can be emitted, is expressed by a nonzero imaginary part of W_o: . . .In a static situation where W_o is real, the shift in phase associated with a time lapse. . .identifies E, the energy of the system. . .

In the latter part of 1992, and in 1993, five papers were submitted under the cover title "Casimir Light." The individual ones are called, successively: A Glimpse; The Source; Photon Pairs; Pieces of the Action; and, Field Pressure. The first three notes adopted the over-simplification that the bubble collapse—the source of radiant energy—is instantaneous. "Pieces of the Action" begins "to remove the more egregious aspects of that treatment." The abstract reads: A more realistic dynamics for the collapsing dielectric fluid are introduced in stages by adding contributions to the Lagrangian that forms the action. The elements are kinetic energy, Casimir potential energy, air pressure potential energy, and electromagnetic coupling to the moving dielectric. There are successful tests of partial collapse time and of minimum radius."

This paper ends with a veiled question: "If, as it would seem, a mechanism exists that transfers kinetic energy of a macroscopic body into energy of microscopic entities, could there not be—in a differentcircumstances—a mechanism that transfers energy of microscopic entities into kinetic energy of a macroscopic body?"

What, in 1991, seemed to be only a pairing of two intuitively improbable phenomena ("The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different."), now emerges as related ways of transferring energy between macroscopic and microscopic objects.

"Casimir Light: Field Pressure" begins with a question: "How does a macroscopic, classical, hydromechanical system, driven by a macroscopic acoustical force, generate an astonishingly short time scale and an accompanying high electromagnetic frequency, one that is at the atomic level?"

In response, "I offer the hypothesis that light plays a fundamental role in the mechanism. Provocatively put: The collapse of the cavity is slowed abruptly by the pressure of the light that is created by the abrupt slowing of the collapse."

The hypothesis becomes more quantitative with this supplement: "The conditions for light emission are at hand when the fluid kinetic energy becomes independent of t (time) for a short time interval, and that similar remarks apply immediately after the emission act. In effect, one is picking out the circumstances for spontaneous radiation, from a coherent state of definite energy, to another such state of definite, lower energy."

The equation of motion—along with the conservation laws–that is supplied by the action principle, leads to a picture of what happens during abrupt slowing." Just before that begins, there is no significant field. . .Then the field strength rises rapidly in the vacuum region, giving a positive value to the (outward pressure). . .the slowing has begun. That process will cease when the field, flowing at the speed of light toward the outer dielectric region, has produced the countering pressure.

The somewhat mysterious initial hypothesis has emerged clarified, as an unusual example of a familiar fact—spontaneous emission of radiation by an electric system is a single indivisible act that obeys the laws of energy and momentum conservation."

Now, finally, returning to the 1991 "Causal Order" note, for the example of the reaction

$$p + d \rightarrow {}^3He + \text{lattice energy},$$

one also recognizes this as a single, indivisible act.

So ends this Progress Report.

MY LIFE WITH COLD FUSION AS A RELUCTANT MISTRESS

Edmund Storms

Talk given at the Cold Fusion and New Energy Symposium, October 11, 1998, Manchester, New Hampshire.

Over nine years have passed since many of us were lured into believing that the Pons-Fleischmann effect would solve the world's energy problems and make us all rich. Things have not yet worked out as we had hoped. Each of us have followed a different path through the labyrinth of this expectation. I would like to share with you my particular path and show you how I came to believe that problems of reproducibility are caused solely by the properties of the materials in which the nuclear reactions are proposed to occur.

When the announcement was made, I was working at the Los Alamos National Laboratory (LANL) on rather conventional materials problems associated with trying to design nuclear reactors for use in space. Suddenly, the possibility of making nuclear energy in a Mason jar was the center of attention and conversation at LANL. Meetings were held and memos flowed freely—typical of a government operation. Excitement was intense, causing previously untapped creative juices to flow and an intensity of communication within the laboratory not seen since the War. Dozens of imaginative experiments were started using funds otherwise destined for the design of better atomic bombs. In my case, a search for tritium production seemed to be the most logical approach, because I was located in a building where worked some of the world's experts in the properties and detection of tritium. They know tritium when they see it. So, with the financial backing of the DOE, my future wife (Carol) and I set about trying to verify the claims for tritium production using the Pons-Fleischmann effect.

Our approach was to electrolyze heavy-water in a closed cell designed to collect tritium produced in the electrolyte in a separate compartment from that present in the evolving gas. Because we planned to do many experiments, in order to explore a wide range of variables, the cells were designed to be cheap and simple. As you can see in Figure 1, the evolving D_2 and O_2 pass through a recombiner and the resulting D_2O is collected in an IV bag. For a brief time, we were the major user of IV bags in Los Alamos. The method allowed us to keep a complete inventory of all material within the cell including any tritium. Over 250 cells were studied using palladium from many different sources containing many different introduced impurities. Unfortunately, only thirteen cells produced excess tritium and the amount was rather small. Typical results obtained from active and inactive cells run at the same time are shown in Figure 2. Note the delay in tritium production followed by a rapid onset with bursts. In the process, we made an important discovery. When tritium is produced, it always appears first in the electrolyte, not in the evolving gas. This is important because it indicates that tritium is being produced only at the surface and it leaves the sample before it has a chance to dissolve in the metal lattice. The implications of this behavior will be described in more detail a little later. Unfortunately, we could find no relationship between its production and the nature of the palladium, hence the effect was not reproducible. Being highly controversial, the resulting paper[1] was reviewed by twelve people at LANL and by several more reviewers after

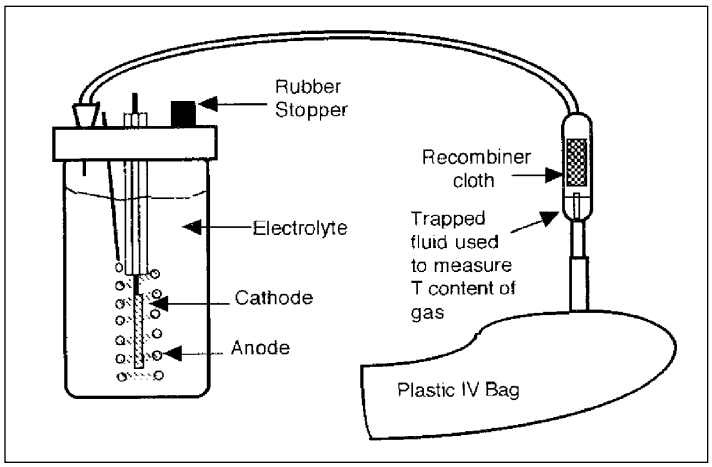

Figure 1. Drawing of closed cell used to study tritium production.

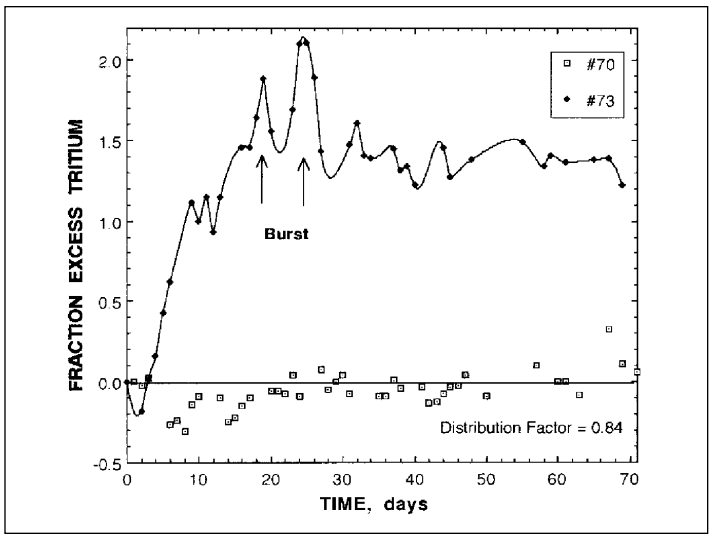

Figure 2. Typical behavior of active and inactive cells in which tritium was measured. Both cells were run at the same time. Note the four day delay before tritium was produced in cell #73.

it was submitted to *Fusion Technology*. Even though this intense scrutiny found no fatal flaws, the results, although published after peer review, were universally ignored. The papers that skeptics publish should be so carefully analyzed.

One of the major rationalizations used to reject such work was the assumption that tritium was already in the palladium or came from the surrounding environment. Consequently, we set about to test these assumptions. We had already made numerous attempts to find dissolved tritium within the initial palladium and within the environment without success. So we chose the opposite approach. We placed cells in an environment known to contain tritium. In addition, we dissolved a known amount of tritium in palladium destined for electrolytic study. If the behaviors seen during the proposed cold fusion production matched those found using known tritium addi-

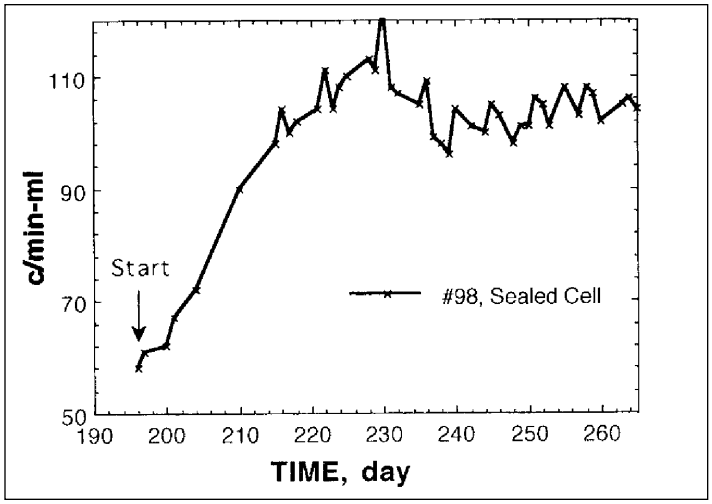

Figure 3. Pickup of tritium in a sealed cell attached to an IV bag located within an environment containing tritiated water vapor.

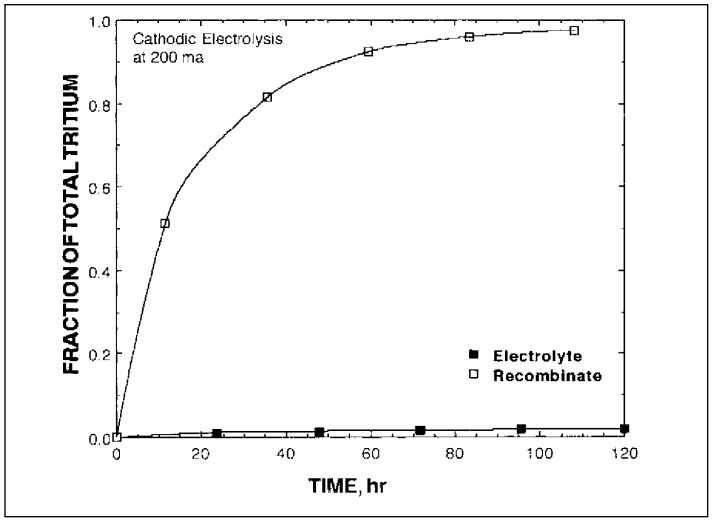

Figure 4. Growth of tritium in the electrolyte and in the evolving gas as a sample of palladium containing tritium was electrolyzed as the cathode.

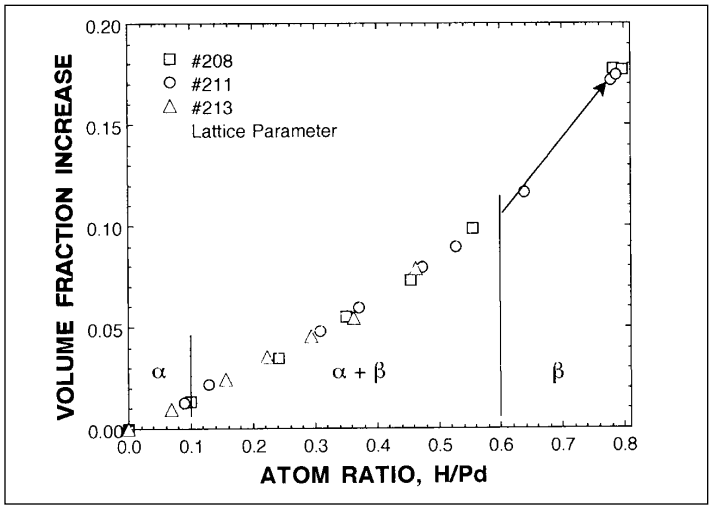

Figure 5. Increase in volume produced by loading palladium with hydrogen.

tions, then an explanation would be obvious.

When a cell was placed in a tritium environment, the tritium content of the electrolyte slowly increased at a linear rate, as expected (Figure 3). In contrast, anomalous tritium was seen to occur only after a delay and then was produced at a high rate over a short time with bursts (Figure 2). In addition, similar cells in the same, clean environment showed no effect while tritium was growing in a neighboring cell. Consequently, the observed behavior of cold fusion production of tritium and that obtained from the environment show entirely different patterns of behavior.

Tritium which was placed in the palladium on purpose always appeared first in the evolving gas, not in the electrolyte, during electrolysis. As can be seen in Figure 4, the contained tritium was released immediately after electrolysis started and continued at a steadily reduced rate as electrolysis continued. Loss into the gas was caused by a first order reaction for which a half-life could be determined. Again, the behavior of tritium claimed to be produced by cold fusion and the behavior of known tritium were different. To us at least, this study demonstrated that neither contaminated palladium nor the environment were the source.[2] Skeptics were forced to propose that tritium released in "cold fusion" cells was not dissolved but was present as isolated impurities to which tritium was tightly bonded. Never explained was the mechanism of its subsequent rapid release after hours of electrolysis. Unfortunately, this work was also completely ignored.

Individual studies always have errors which rightly introduce doubt. On the other hand, a series of studies using different approaches that produce patterns of behavior are much more difficult to reject. In this case, the observed patterns are completely consistent with anomalous tritium being produced within the cell and, more specifically, in the surface of the palladium cathode. Nevertheless, people of a skeptical mindset would propose terribly unlikely and convoluted processes to explain individual results while ignoring the patterns. Amazingly, this method of rejection seems to satisfy many people in academic science these days.

During this study, we began to appreciate how important crack formation was in determining the local concentration of deuterium. Most palladium grows an increased crack concentration each time it is loaded. This crack concentration is measured as a volume increase over that expected from the lattice parameter change, shown in Figure 5. Only a very few samples did not show this effect. Using tritium as a tracer, we were able to quantify the effect of cracks on the loss rate of hydrogen from palladium. In general, the higher the excess volume, the faster tritium left the sample through the cracks as gas, thereby entering the gas rather than being dissolved in the electrolyte as ions. In this case, tritium was also being used as a tracer for deuterium loss. Thus, both tritium and deuterium, previously dissolved in the PdD, left the structure through cracks as gas rather than by ion exchange at the surface. Only tritium produced by the anomalous reaction entered the electrolyte. Consequently, we could conclude that most palladium acted like a leaky bucket which could never be filled. Since we know that high deuterium loading is a requirement for excess energy production, these cracks are apparently a major hindrance in achieving the required high deuterium concentration. Unfortunately, this work had no influence on the skeptics and very little influence on those people who were also trying to reproduce the effect.

About a year later, I wrote a review[3] which was published in *Fusion Technology*. Using this collected experience as an argument to look for heat production, I convinced my Division Leader at LANL to fund a calorimetric study. The calorimeter was sealed, closed, and stirred, all requirements demanded by various skeptics before claims could be believed. A drawing of the device is shown in Figure 6. For our first study, I was given

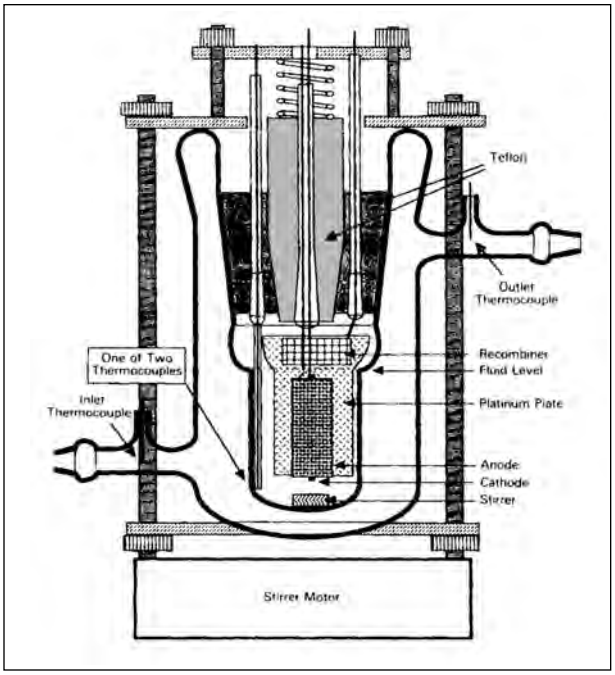

Figure 6. Drawing of the calorimeter used at LANL. The cell was sealed, contained a recombiner, and was stirred. Temperature was measured at two positions within the cell. The deuterium content of the palladium cathode was determined by measuring the change in deuterium pressure.

a piece of palladium from a batch made by Tanaka Metals which had been shown to produce excess energy by Prof. Takahashi in Japan. Amazingly, his sample also produced excess energy in my calorimeter. The most dramatic of the various examples of excess heat production from this sample is shown as a function of time in Figure 7. As you can imagine, this unexpected result was analyzed every which way by numerous people to discover the source of the apparent energy. The calorimeter was calibrated using an internal heater many times during the study and three times during excess energy production, with no apparent change. Unfortunately, the study had to be terminated prematurely because the internal recombiner began to fail. After the calorimeter was repaired, some additional excess energy was seen, but the sample soon died. Because we were sensitive to the influence of cracks, we measured this piece and found very few of the little devils (1.7%). A second piece of palladium from a different batch, but one claimed to have been made in the same manner, was delivered from Japan and studied. This piece was found to produce no excess energy, but contained a high concentration of cracks (13.5%). After informing the Japanese of this fact, a third batch was made. But this time, the conditions were as close as those used to make the first batch as was possible. This material was found to make excess energy, although less than the first batch. Also, it was found to contain a crack concentration slightly greater than the first batch. From these results, it is clear that the crack concentration is an important variable. Table 1 compares the behavior of several samples made in Japan. While crack concentration (excess volume) is important, it is not the only variable having influence on excess energy production.

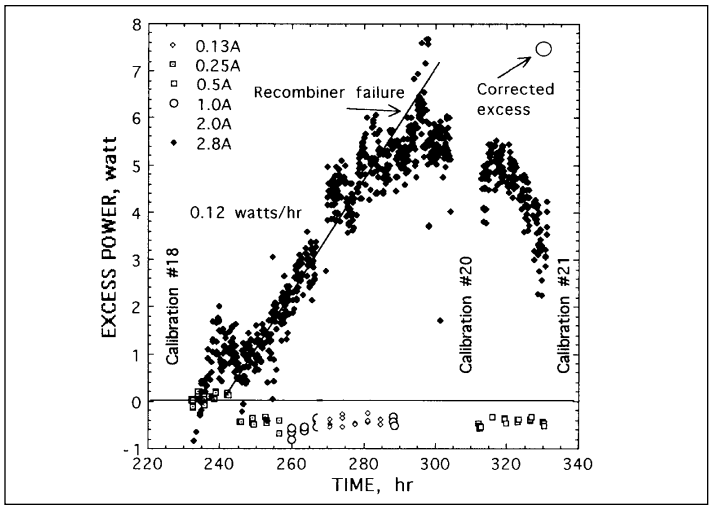

Figure 7. Time variation of excess power production using Takahashi palladium with various applied currents. The calorimeter was calibrated before, during, and after the study. No significant changes were noticed. Excess power was observed only above a critical current. The "correct excess" value is calculated assuming no recombination is occurring at that time.

Table 1. Comparison between measured properties and excess power production.

Sample#	Excess Volume %	Composition D/Pd	Open Circuit Voltage	Excess Power, W (at 3A)
Tanaka 1	1.7	0.82		7.5
IMRA #38	2.8	.....0.875	1.03	3.2
Tanaka 3	2	0.84		2
IMRA #42	1 to 2	0.891	1.25	4.6
IMRA #84	6.7	0.752	1.00	1.5
IMRA #58	4.1	0.833	0.60	0.0
Tanaka 2	13.5	0.75		0.0

Table 1 compares the behavior of several samples made in Japan. While crack concentration (excess volume) is important, it is not the only variable having influence on excess energy production.

Once again, this information, when published,[4] had very little effect on other people's work. It did, however, start me down a path to answer the question, "Why is the effect so difficult to produce?" In addition, this experience started a search for ways to pretest palladium. Meanwhile, conventional science was taking the opposite approach—saying because the effect could not be reproduced at will, it was not real.

About this time several events occurred to give many of us some optimism. Thanks to Eugene Mallove, Rep. Dick Swett (NH) asked me to testify before a congressional subcommittee which was reviewing future funding for "hot fusion."[5] Unfortunately, this effort had no effect on future actions by the government toward "cold fusion." Then, in spite of increas-

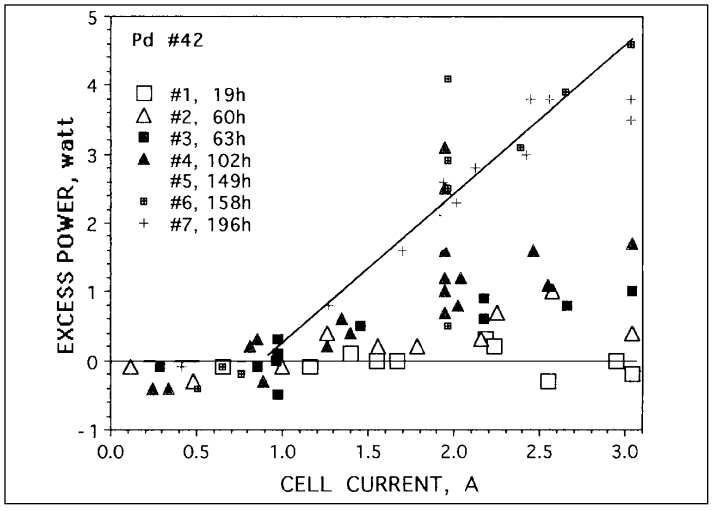

Figure 8. Excess power as a function of applied current after electrolyzing for various times.

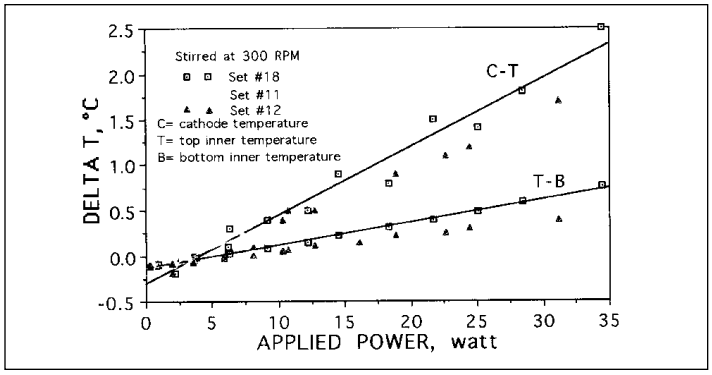

Figure 9. Temperature difference between the cathode and the top of the electrolyte and between the top and bottom of the cell. No excess power was detected during Sets 11 and 12. Excess power was detected during Set 18.

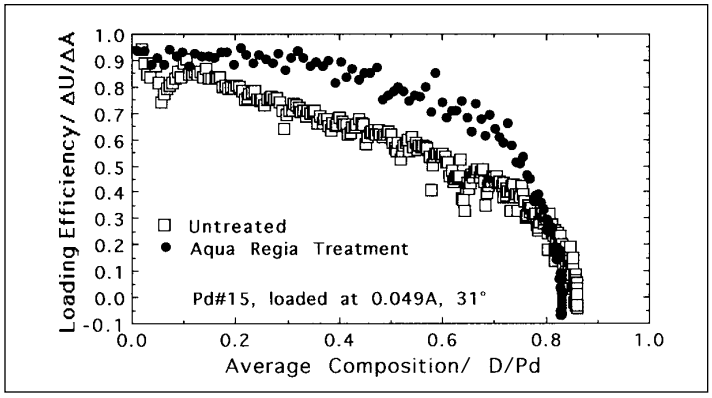

Figure 10. Effect of a surface barrier on the loading efficiency.

ingly vocal opposition to the whole idea, *MIT Technology Review* took the courageous step of asking me to write an article for them.[6] Although this article stirred up some heated debates, it also had no significant impact on the course of the field.

My work in cold fusion at LANL was meeting increased resistance and my new wife and I wanted to build a home in Santa Fe. Retirement began to look very attractive. Amazingly, they were even willing to pay me a bonus to leave. After we finished the house and a laboratory, I began to study ninety pieces of palladium furnished by IMRA (Japan). Some of this material produced excess energy, as shown in Figure 8 and listed in Table 1. In addition, a temperature gradient was found to occur within the cell during excess energy production such that the cathode appeared to be the source of energy, as shown in Figure 9. This sample was found to be unique in that the low excess volume did not grow larger upon repeated deloading-loading cycles. Energy production was very difficult to kill, returning after a short delay even when the surface was removed by Aqua Regia. The study also demonstrated that palladium could be pretested, thereby reducing the growing frustration by eliminating most material from which energy could never be obtained. This study was published in *Infinite Energy*.[7] In addition, the study showed cracking to be a highly variable property and pointed out a number of other variables, besides cracking, as being important. These included the deloading rate after the current was stopped, the open-circuit-voltage, and the loading efficiency. As my laboratory grew, thanks to Dave Nagel (NRL) and Fred Jaeger (ENECO), computer control could be introduced, allowing these variables to be explored in more detail. Unfortunately, the results of this study[8] were held up for about a year by the then editor of *Journal of Electroanalytical Chemistry* (JEAC), apparently because the paper mentioned cold fusion. The work was recently published in *J. Alloys and Compounds* and described at ICCF7.

These studies provided insights into the nature of the regions in which the nuclear reactions actually occur. These regions I call a special condition of matter containing nuclear-active-states (NAS).[9] It is interesting to examine some of these results in detail. In order to produce "cold fusion" or more exactly "chemically assisted nuclear reactions" (CANR), the hydrogen isotope must achieve a very high local concentration. To do this, the atoms must get into the metal lattice though a surface barrier, and the atoms must stay in the lattice regardless of there being many avenues for escape. The "getting in" and the "getting out" are independent variables having wide ranges of values which depend strongly on the nature of the metal. Once regions of high-concentration are achieved, changes must take place such that the atomic and electronic structures are altered to produce unique conditions—a new chemical phase. This transition is also sensitive to a variety of conditions in addition to the hydrogen concentration. In short, two major conditions must be achieved—a high hydrogen concentration and then conversion to a new phase having a composition well above that of normal β-PdD. Each of these

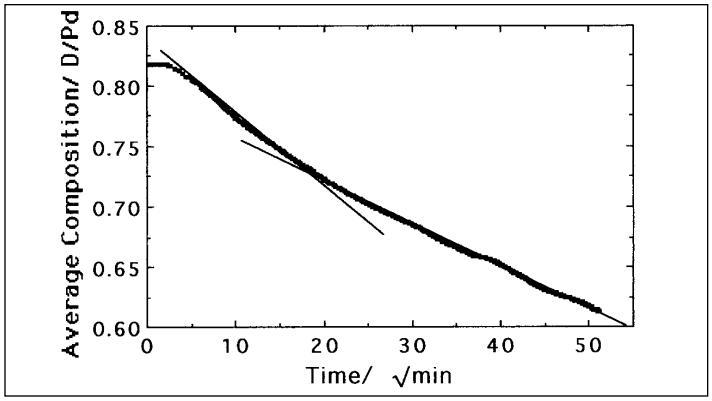

Figure 11. Change in average composition vs. square root of time after applied current is stopped. The initial delay is caused by inertia in the composition measuring system. A break in slope occurs when α-PdD forms on the surface.

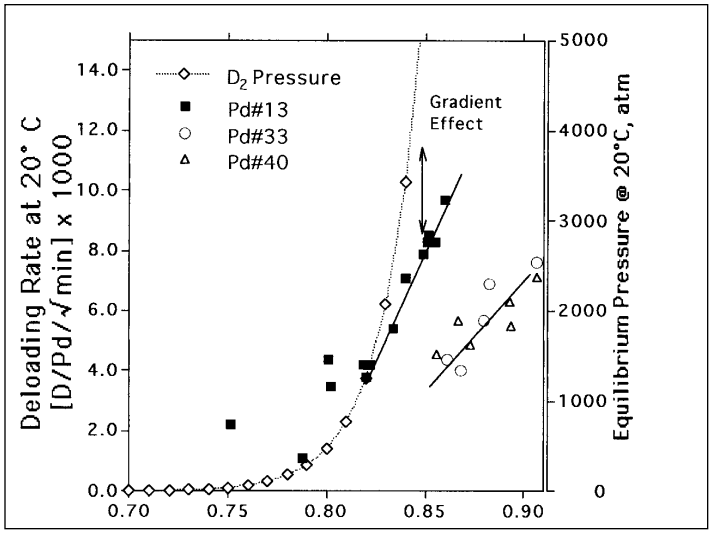

Figure 12. Variation of deloading rate as a function of average composition. Also shown is the equilibrium pressure within voids for the indicated compositions. The gradient effect is caused by a reduced composition at the surface of a crack produced by deuterium loss.

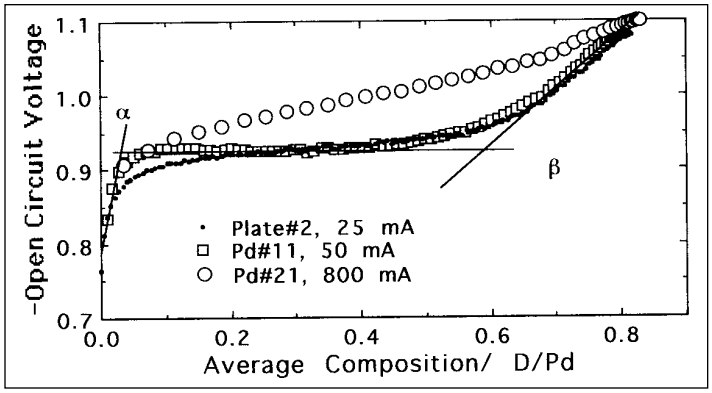

Figure 13. Open circuit voltage measurement during loading at various currents referenced to palladium.

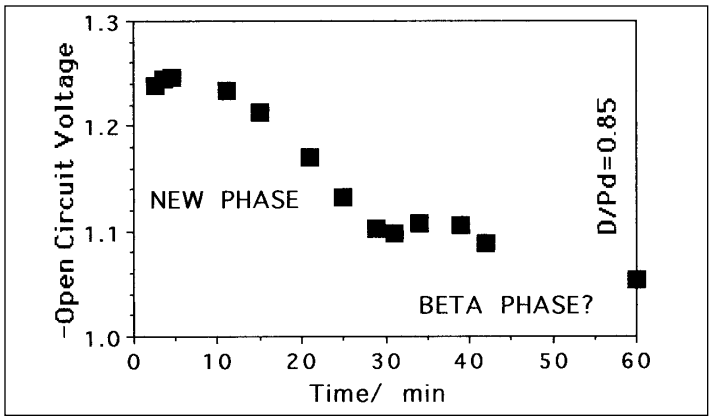

Figure 14. Open circuit voltage during deloading after production of excess energy.

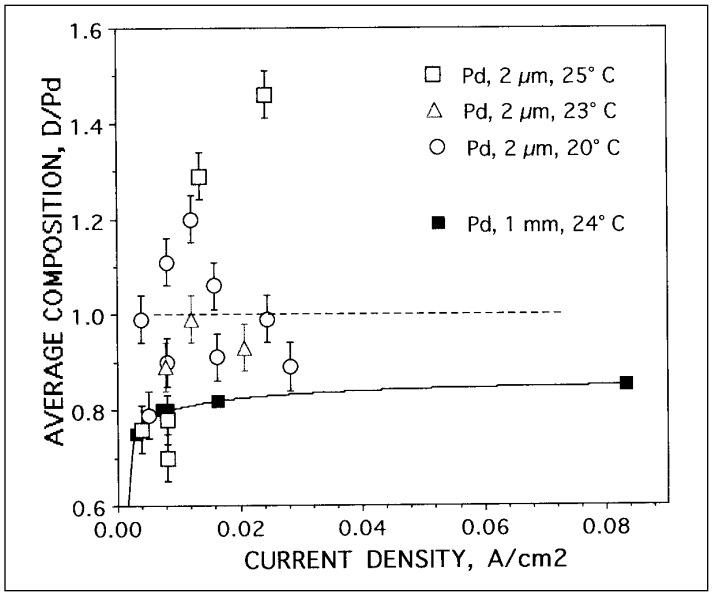

Figure 15. Measured average composition of thin films of Pd plated on Pt after being subjected to different current densities. The behavior of a typical thick plate is also shown.

conditions is affected by dozens of variables. It is little wonder that the effect is difficult to reproduce. The "going in" can be examined using the loading efficiency; this quantity being the ratio created when the number of atoms of hydrogen presented to the surface by the applied current is divided into the number which actually dissolve in the metal, each measured over a five minute interval. Figure 10 shows the behavior of an untreated sample and one whose surface barrier was reduced using Aqua Regia. Needless to say, a poor "going in" rate needs to be avoided. The "going in" rate is also sensitive to applied current and temperature. The "going out" rate can be studied by measuring the deloading rate after the current is turned off. This rate is obtained from a plot of average composition vs. square root of time, as shown in Figure 11. The initial slope is used to evaluate samples. The slopes for numerous samples are compared in Figure 12 as a function of maximum average composition. Two conclusions are obvious: the loss rate increases as the initial composition is increased and some samples have an abnormally low rate even though they have a high composition. From this and other arguments, I conclude that this same loss is occurring while current is being applied. In addition, the rare piece of palladium which is able to achieve very high average compositions does so because it has a low loss rate, in addition to several other features. Consequently, both the loading efficiency and the deloading rate can be combined to quickly identify potentially nuclear-active palladium, in addition to using material having low excess volume.

Unfortunately, the average composition used to obtain these two quantities is not the important variable. The highest composition exists on the surface and it is this composition that determines whether a sample will become nuclear-active. Fortunately, changes in surface composition can be estimated using the open-circuit-voltage (OCV), a value which is sensitive to the chemical activity of hydrogen in the surface. A value is obtained by measuring the voltage between the cathode and a platinum reference electrode while the current is turned off for a few seconds. Figure 13 shows how the OCV changes as a palladium sample is loaded with deuterium. A reverse of this behavior is frequently seen when a sample is allowed to deload after the current is turned off. However, occasionally, especially after very high average compositions have been achieved, the OCV shows a different behavior as seen in Figure 14. This behavior indicates that a new phase has formed which slowly decomposes into normal β-PdD as deuterium is lost. The next question needing an answer is: "What is the actual composition of this phase?" A partial answer can be obtained by studying very thin films of palladium plated on to platinum. As shown in Figure 15, the measured composition of such films is highly variable but can achieve a composition as high as D/Pd=1.5. In this case, the surface region containing the high composition is not as diluted by the smaller interior composition as would be the case if a thicker sample were used, such as shown by the lower curve. Since this particular film was not nuclear-active, the composition of a nuclear-active surface is probably significantly higher. In addition, deloading from a surface is very nonuniform, as can be seen by examining bubble production. Therefore, the maximum composition of such a thin film, or indeed any palladium surface, is well above the average value in addition to being above D/Pd=1.5. This observation means that all theories based on the properties of β-PdD are barking up the wrong tree. I have proposed the actual nuclear-active phase to be PdD_{2+x}.[9]

Meanwhile, the results of this study were used by ENECO in an attempt to convince the Patent Office that the lack of reproducibility was caused by limitations inherent in the chosen material. This argument was completely ignored. In addition, I wrote another review in an attempt to bring all of the better data under one roof so that even an open-mind-challenged person could see the bigger picture. This was published in *J.*

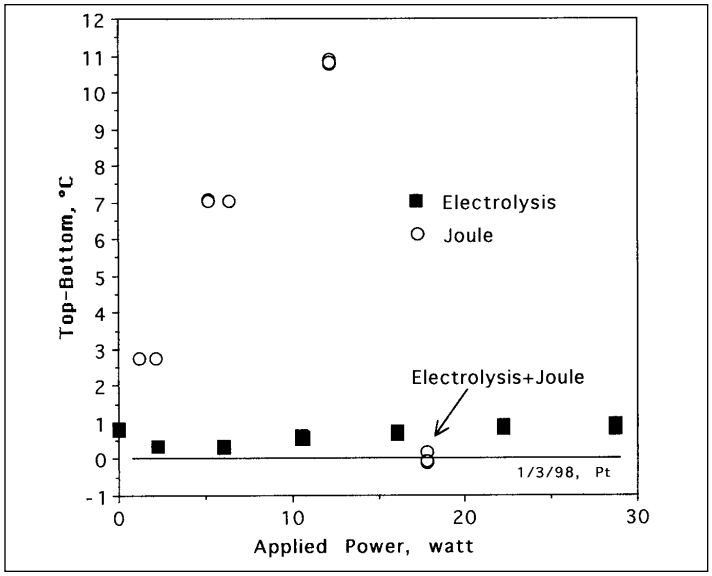

Figure 16. Gradient between the top and bottom of the electrolyte as a function of applied power.

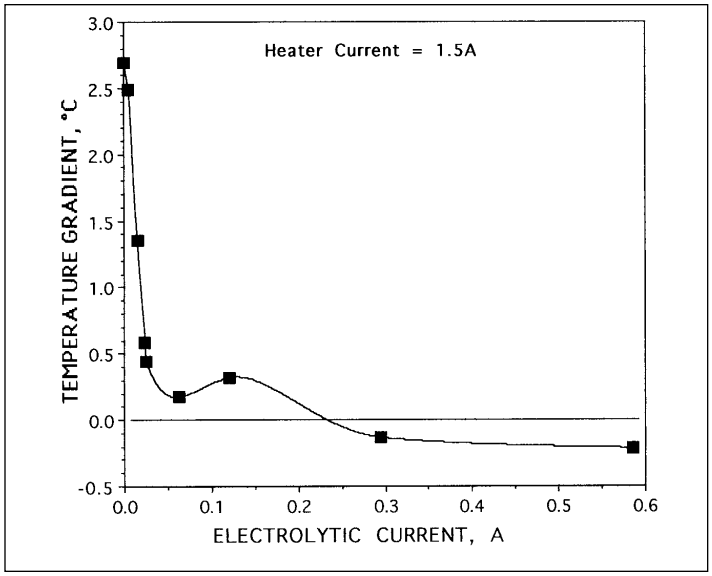

Figure 17. Reduction in gradient as electrolysis current is increased at a fixed heater current.

Scientific Exploration[10] with brief versions in *21st Century Science and Technology*[11] and in an earlier volume of *J. Scientific Exploration.*[12] Again, no change was produced at the Patent Office nor in any other government agency. Recently, a third and final review was rejected by the *International J. of Modern Phys*. even though one of the editors requested the paper. Amazingly, four reviewers rejected its publication for reasons that appeared to me to be based on a bias against cold fusion. This work was published in *Infinite Energy*.[13] You can decide for yourselves whether this review deserved such treatment. My recent experience suggests that many avenues for publication previously available no longer provide the service because reviewers have become even more unwilling to give cold fusion the benefit of doubt. It is depressing to see resistance increase as the experimental results and understanding improve.

Last year, Dr. Naoto Asami sent me some palladium that the New Hydrogen Energy Laboratory (NHE) had made at great expense and to their specifications. Unfortunately, they could detect no excess energy using this material. As expected, my tests showed this material to be flawed—material I would expect to be completely inert. After hearing this, Dr. Asami invited me to Japan, where we were able to discuss the problem in some detail. Apparently, impurities were being introduced into the palladium during manufacture as well as during subsequent annealing. We were able to eliminate the impurities added by annealing. Unfortunately, insufficient time and money were available to change the method of manufacture. Consequently, NHE closed down without exploring the use of proven, active palladium.

Recently, I have been exploring the various errors besetting calorimetric measurements. I have discovered that stirring is not an important variable when electrolysis is used, in contrast to what has been claimed by some well know skeptics. This can be seen in Figure 16, where the gradients produced by an internal heater and by an electrolysis current are shown. A small electrolysis current added to the Joule heating reduces the gradient to insignificant values (Figure 17). On the other hand, the stagnate layer of fluid at the cell wall is important when isoperibolic methods are used. This layer influences the thermal conductivity of the wall and is very sensitive to the amount of fluid convection. Figure 18 shows how the calibration constant changes as the stirring rate is increased. In the absence of mechanical stirring, bubble generation serves the same end although not to the same degree. This study shows that claims for small amounts of heat using unstirred isoperibolic calorimeters may be in error. Pons and Fleischmann largely avoided this problem by frequently calibrating their cell, although the effect may still have some influence on their data especially at the lowest claimed excess power levels. Other studies, especially if they use Joule heating to calibrate the cells, are not so fortunate. In my work, calibration was based on electrolytic heating, with Joule heating being used only to determine whether the calibration constant had changed while electrolysis was ongoing.

I have also built a dual calorimeter (Figure 19) which is being used to study electroplated palladium on platinum and search for evidence of superconductivity in nuclear-active-material. This method greatly reduces the likelihood of misinterpreting apparent excess energy. Electroplated palladium appears to have a much greater likelihood of success compared to bulk material, and it is cheaper.

What can I conclude from this experience? First, the phenomenon claimed by Pons and Fleischmann is real, but it is only a small part of a much larger picture. The reality of this phenomena has an even greater importance to science and technology than was ordinarily proposed. Second, the method used by Pons-Fleischmann is useless for eventual production of commercial power. Active palladium is too difficult to find and conditions are too sensitive to impurities. Nevertheless, it is a very useful and inexpensive method to explore certain aspects of the phenomena. It is unfortunate that these brave and creative thinkers had to take so much pain and be denied the rewards of their discovery by closed-minded colleagues and an incompetent Patent Office. And third, the field is sick and on life support. Major sources of financial support have dried up, many self-funded individuals are moving on, and conventional rejection has solidified. Those of us who would like to see this field grow are encouraged by a few impressive successes, such as Case, Arata, and Stringham, but even these approaches are woefully underfunded and are being studied by a very small number of individuals. We most hope that when the spectacular demonstration demanded by skeptics is found, a way is available to make this fact generally known.

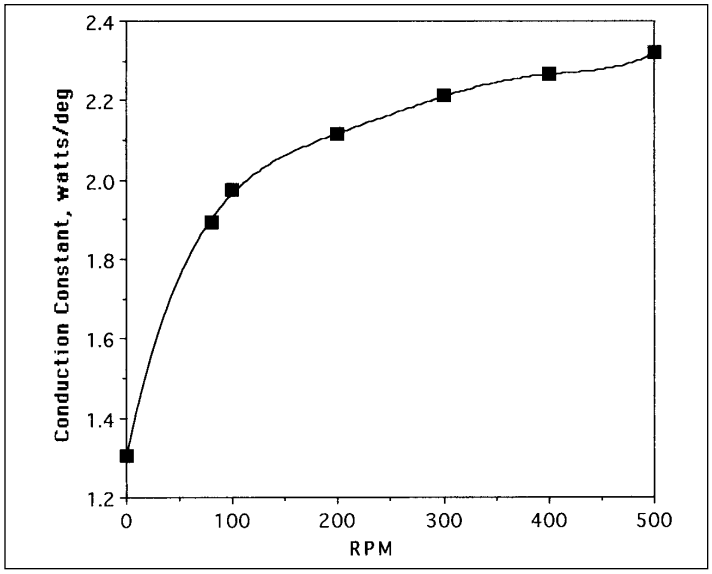

Figure 18. Thermal conduction of the cell wall as a function of stirring speed using an isoparibolic calorimeter.

The amazing claims for transmutation are getting increased attention and are accumulating experimental support, but acceptance is hard to find even in the cold fusion community. These claims require expensive tools to show their reality—money that is not generally available. The only solutions are for the Patent Office to change its approach and/or for individuals or companies to provide funds toward a basic understanding without an immediate guarantee of financial return—several very unlikely possibilities. The most likely possibility is that the entire field, heat and transmutation, will get noticed only after the price of oil skyrockets in the next few years or if there is a major spill of highly radioactive material from a waste dump. Meanwhile, a very objective article in *Wired* magazine[14] will at least wake up the public to this issue. Hopefully, some awareness will trickle down from the public to the scientific profession, the reverse of the usual procedure.

As for me, I will continue to study the effect as time and money permit. However, additional useful knowledge demands sophisticated techniques that I presently do not have available. This limitation makes continued study of this effect a matter of diminishing returns for me. Therefore, I am gradually turning my attention to other problems—such as earning a living and preparing for Y2K. Giving up this mistress is hard for me but very attractive to my wife, as you might expect. Eventually, the field will be discovered by other people, hopefully by someone who can afford to marry her.

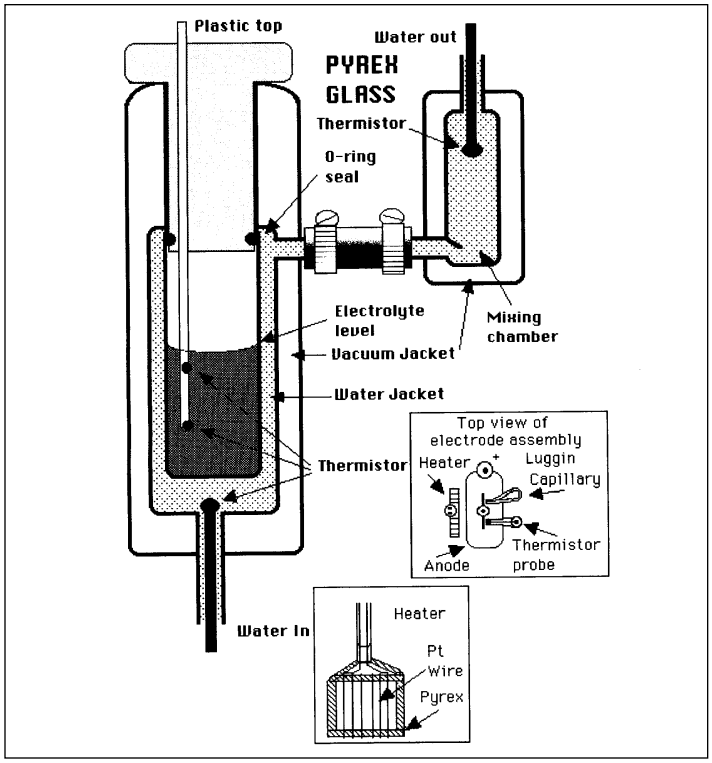

Figure 19. Drawing of the dual calorimeter. The cell is sealed and contains a recombiner and Joule heater. Temperature is measured at three locations within the cell.

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TRITIUM PRODUCTION FROM A LOW VOLTAGE DEUTERIUM DISCHARGE ON PALLADIUM AND OTHER METALS

T.N. Claytor, D.D. Jackson, and D.G. Tuggle

ABSTRACT

Over the past year we have been able to demonstrate that a plasma loading method produces an exciting and unexpected amount of tritium from small palladium wires. In contrast to electrochemical hydrogen or deuterium loading of palladium, this method yields a reproducible tritium generation rate when various electrical and physical conditions are met. Small diameter wires (100 - 250 microns) have been used with gas pressures above 200 torr at voltages and currents of about 2000 V at 3-5 A. By carefully controlling the sputtering rate of the wire, runs have been extended to hundreds of hours allowing a significant amount (> 10's nCi) of tritium to accumulate. We will show tritium generation rates for deuterium-palladium foreground runs that are up to 25 times larger than hydrogen-palladium control experiments using materials from the same batch. We will illustrate the difference between batches of annealed palladium and as received palladium from several batches as well as the effect of other metals (Pt, Ni, Nb, Zr, V, W, Hf) to demonstrate that the tritium generation rate can vary greatly from batch to batch.

1. INTRODUCTION

We will report on our tritium generation results from a palladium wire-plate configuration subjected to periodic pulsed deuterium or hydrogen plasma. This configuration is reproducible within a batch and produces a measurable amount of tritium in a few days. As in other work in this area, it has been found that the output is very batch dependent and sensitive to material impurities that prevent hydriding. As in our previous work,[1,3] all tritium data was obtained from several batches of 100 or 250 micron wire and 250 micron thick plate from J&M or Goodfellow metals. In these experiments most of the tritium data was obtained with on-line tritium gas monitors. Several times, the gas was oxidized and tested with a scintillation counter.

Some have criticized the detection of tritium because the signals seem to be (a) insignificant, (b) tritium is ubiquitous, and (c) the palladium metal is subject to possible tritium contamination. The magnitude of the signals discussed in this paper are multi-sigma and are sometimes over a hundred times the tritium background in the supply gas. Furthermore, the rate of tritium evolution in the sealed system may be the most sensitive and rapid indicator of anomalous nuclear behavior in deuterided metals. As such, it is well suited for parametric investigations. We will briefly discuss the possible avenues for contamination and show that each is negligible, or not a factor, in the experiments described.

2. MATERIALS

For this work we used Cryogenic Rare Gases deuterium 99.995% that has 90 pCi/l of tritium, and research grade hydrogen with no detectable tritium (< 25 pCi/l). The major impurity in the deuterium is H_2 (0.005%) (He <1ppm). A total of 74.2 g of palladium wire/powder/foil was used in plasma experiments described in this paper. Of that amount, 8.6 g was used in various hydrogen or deuterium control experiments. The palladium has been checked for tritium contamination by two independent methods (heating in hydrogen/deuterium and H_2 plasma).

Much of the palladium has been subjected to rigorous metallographic and impurity analysis. The impurity levels for the wires (Johnson Matthey Puratronic, Goodfellow) varied from the specification sheets and were in the 60-150 ppm range (mostly Cu, Fe, W and P) rather than the quoted values of 5-10 ppm. Most wires were used as received, but several wires were annealed in air (at 850°C for 2 hours) or stress relieved (600°C for 4 minutes) in air. Some of the wires (mostly J&M), when wrapped on a white macor ceramic spool and heated (to 600°C) left brown diffuse deposits (50 cm or more in length) or black diffuse spots (1-3 mm in length). The two batches that showed the most tritium did not yield the black spots but did leave light, small amounts of the brown deposits.

Three batches of palladium were used for the plate, the first batch of 220 micron thick foil was annealed at 850°C for 2 hours at 10^{-6} torr before use. A second batch had a different impurity analysis from the first, but was annealed in a similar manner; the third batch was used as received and had a different impurity level from the first two batches (although, all three plate batches had total impurities in the 350-500 ppm range, mostly Pt, Au, Cu and Fe). Wire from five batches (lots W13918, W06528, Z0114, NM 35680, Z0293, GF5140/6) was obtained from Johnson Matthey and Goodfellow Metals and one length of wire was supplied by Ben Bush. Only the Goodfellow batch and J&M (W13918) showed large (8 to 10^2 nCi) amounts of tritium although the other batches of J&M and Ben Bush wire produced small amounts (~1.5 to 6 nCi total per run).

Tritium contamination in the palladium wire and plate was tested by two independent methods: sputtering of the wire in a hydrogen plasma atmosphere and heating of the wire or plate to either 260 or 800°C in deuterium or hydrogen. No evidence of tritium (to within experimental error ~ 0.3 nCi) contamination was found in the heating experiments with hydrogen. The Goodfellow wire was tested for contamination (with null results ~ 0.3 nCi) by heating to 280°C sections (0.1 g) of wire taken between wire samples shown to produce tritium in the experiments. In our previous work[3] we were able to set a limit of 0.005 nCi/g obtained with ^{3}He detection of aged palladium samples from a different lot. Also, in an extensive independent[4] investigation of palladium wire, several hundred wire samples were tested and no tritium contamination was detected. The purity of the wire used in these experiments also weighs against, ubiquitous, intrinsic spot contamination, although the appearance of the black and brown deposits indicates that spot and distributed impurities can be present.

3. APPARATUS

Shown in Figure 1 is one of two stainless steel gas analysis loops containing a 1.8 liter ion gauge and a 310.9 cc calibration volume. The atmospheric, ion gauge and sample pressure

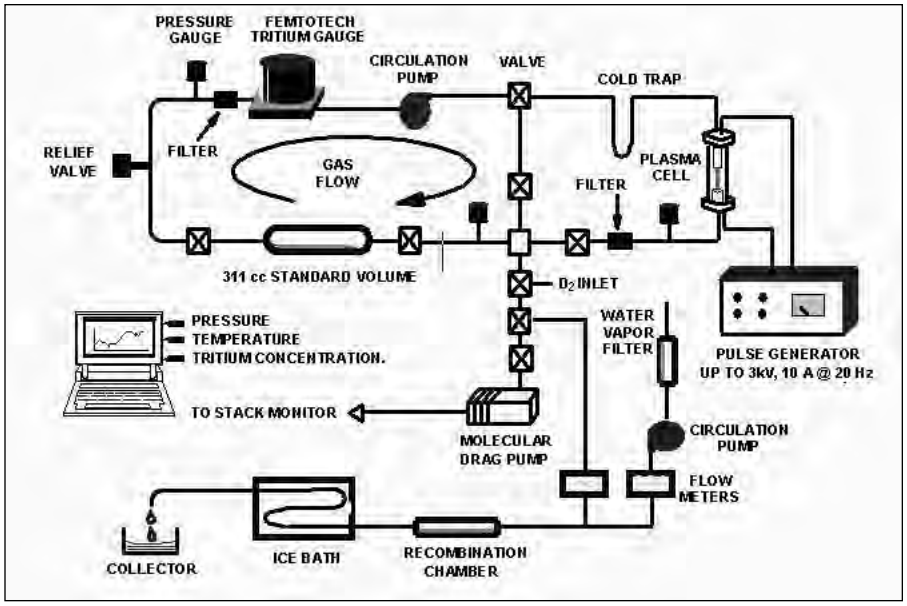

Figure 1. Tritium analysis system used in this study showing the oxidation apparatus.

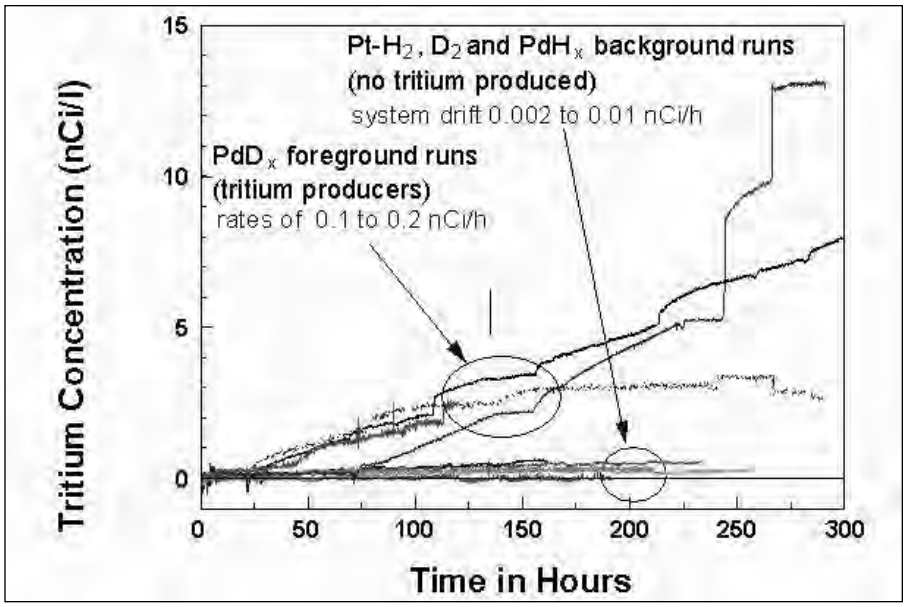

Figure 2. Comparison of background and foreground results with a Pd wire-plate type plasma cell. A Pt wire-plate plasma and a flowing D_2 background is shown for comparison.

(0.2%), Femtotech and room temperatures (0.1°C) are recorded on a computer log at 60 s intervals. The pressure drop during hydriding of the wire and plate is used as an approximate indicator of the stoichiometry of the PdD_x. Both loops have a heater to maintain the Femtotech (-0.03 nCi/l°C) at a constant temperature, an integral cold trap, and there are valves that allow the pressurization of the cell independent of the loop. A two micron filter is installed at the inlet of the ion gauge and at the outlet of the cell to eliminate spurious responses due to particulates. To eliminate the possibility of oil contamination, a molecular drag and diaphragm pump is used to evacuate the system.

The Femtotech ion gauge rejects pulse type radioactive events that effectively discriminate against radon and cosmic ray ionization. The initial background drift rate in the Femtotech was 0.002 nCi/h to 0.006 nCi/h, but after exposure to the cells described in the paper, the drift rate increased, and could reach as high as 0.01 nCi/h. In order to return to the baseline rate, it was necessary to clean the loop tubing and Femtotechs halfway through the study.

A hydrogen oxidation system was built as a backup test for tritium using a scintillation counter (Packard 1600). Calibration D_2 gas with 25 nCi/l of tritium was used to test the two Femtotechs and the oxidation system. The two ionization systems agree to within 5% of each other while the scintillation results are within the experimental error (0.3nCi) of the Femtotechs.

The typical arrangement of the cell allows a wire to sit perpendicular to and a few millimeters above a circular plate. In operation, the plasma is adjusted so that it envelopes the whole wire and contacts the plate at a small spot. Typically, the plasma is light blue (D^{2+}) with areas of pink (D^{3+} or D^{+}). At high currents (> 5 A), a bright pink electron channel forms that extends parallel to the wire from the base of the wire to the plate. Initially, the Pd wire is 25 to 30 mm in length and about one mm from the plate. The plate diameter is 3.0 cm or 1.8 cm.

4. PROCEDURE

The procedure for a plasma run was to first fill the 3.1 liter loop with deuterium gas at 600 torr and obtain a measure of the initial background tritium concentration. With the loop drift rate measured, the deuterium was circulated through the cell to slowly hydride the sample. The pressure in the cell and the loop was then lowered to the operating pressure by pumping the excess deuterium out.

The wire was pulsed negatively, at 20 ms at 50 Hz, with currents between 2 and 5 A, voltages that varied from 1500 to 2500 V, and cell pressure of 300 torr. These conditions reduced the heating in the cell and maintained a cell to ambient temperature difference of less than 25°C to avoid gross dehydriding of the wire and plate. It appeared important to avoid a plasma condition that resulted in either a bright pink electron channel or arcing at the tip of the wire. After a few hours of plasma operation the voltage-current stabilized, presumably due to the formation of small cones (10-20 microns high) all over the surface of the wire. After 20 hours, palladium was visibly sputtered onto the plate. The sputter rate at 300 torr, 3.5 A, was about ~2 Angstroms/s. The cell pressure was monitored, and if it did not drop after 24 hours (indicating hydriding), then a small amount of CO_2 (0.75% by vol) was added, which would initiate hydriding.

At the end of a run the pressure was increased to 600 torr, the gas was circulated, and the system allowed to equilibrate for about 8 hours. If the reading was steady and CO_2 was added, then the gas was circulated through the liquid nitrogen cold trap to collect any water and determine if any tritiated water was present. The system was then pumped out, the cell closed off, and fresh deuterium added to the system after a couple of flushes with either fresh deuterium or air. The difference between the fresh deuterium and the deuterium reading after

exposure to the plasma was used as the measure of the tritium content. In cases where more than 10 nCi were found, the palladium wire and plate were heated separately to over 250°C and the result admitted to the evacuated loop.

5. RESULTS

A total of 65 plasma wire experiments were performed, 12 of these were other than palladium wire and plate. Twenty experiments were run with multiple wires, usually 3 wires bundled together, and eight experiments used different thickness foils 25 to 125 microns thick. The balance of the tests were done with one 250 micron diameter wire and 250 micron thick plate. Three hydrogen plasma experiments were done with palladium plate and wire and two were done with platinum wire and plate. A summary of several background and foreground experiments is shown in Figure 2. The best experiment, produced 10^2 nCi.

Plasma runs 3 and 4 deserve some detailed explanation since these produced the most tritium. First, (see Figure 3) cell 3 was preheated in order to drive off any contaminants. The plasma was then started and the tritium generation rate was 0.15 nCi/h. Near the end of the run, the cell was twice flushed with deuterium, which caused the total tritium (as detected by the Femtotech) to jump up. At the conclusion of the experiment the plate and wire (from plasma 3) were heated, insitu, and released another 5.4 nCi. In order to resolve whether the tritium was originating in the plate or wire, they were separately heated after plasma 4. The wire released about 12.4 nCi of tritium while the plate had no measurable (< 0.3 nCi) release.

A number of Pt and Pd controls were run with D_2 or H_2. Most of these are shown in Figure 2 in comparison with the foreground cells. In general, drift rates with the plasma on were in the 0.004 to 0.01 nCi/h range. Not enough hydrogen and platinum blank experiments have been run to definitely conclude that tritium production is confined to the palladium-deuterium system. We believe, however, that because the hydrogen and non-hydriding metal experiments are low or null, that the rather large results with palladium are unique. We also ran several hydride forming metals other than palladium. In the case of Hf and Zr it was difficult to maintain the plasma, so for most of the run the background drift rate is similar to the cell with D_2 circulating (<0.003nCi/h). Tungsten, vanadium, niobium, and nickel-deuterium were on for about 100 hours, but their rates were still very close to background (0.007 to 0.009 nCi/h). Romodanov *et al.*[5] reported that Nb was more active than W, Zr, Ta or Mo in their gas discharge experiments. We also found small amounts of tritium in niobium (1.1 nCi), and observed a small rate with nickel-hydrogen (0.012 nCi/h). Both of these samples deserve further investigation.

The wire-plate plasma experiments have been very consistent but also very dependent on the exact batch of palladium that was used. We found that the batch, material and material condition are critical parameters. Our first batch of GF5140/6, for example, had an average rate of 0.4 nCi/h, with several rates greater than 0.1 nCi/h. Our second best results came from an arrangement with the second batch of the same wire in which three wires were bundled together. Their rates varied from 0.02 to 0.07 nCi/h.

At the conclusion of two of the experiments, about a third of the deuterium was oxidized and the heavy water and a control were submitted for scintillation counting. The results were 3400 to 213 dpm/ml and were in agreement (within experimental error) with the tritium activity calculated from the drop in reading of the Femtotech. Background activity from the D_2 gas prepared by this method is about 39 dpm/ml.

6. DISCUSSION

The basic premise that the detected ionizing material is tritium is indisputable because, (a) quantitative measurements agree with the scintillation counter, (b) the gas may be transported on a clean palladium bed between different ionization systems and produces an increased reading commensurate with the decrease in tritium concentration noted in the initial system, (c) as the pressure is decreased the tritium signal is seen to decrease (for dry gas) in a manner consistent with the calibration for a known level of tritium in deuterium and finally, (d) the signal shows no diminution over a two week time period, consistent with the half life of tritium.

Three types of contamination of the wire are possible; the first is just surface contamination due to atmospheric or liquid exposure to tritium, the second type might be a distributed impurity, and the third would be a spot contamination. To avoid surface contamination, we thoroughly clean and polish the palladium surface prior to each run. If it were still present, a surface contaminant would be immediately evident when the wire was introduced to the analysis loop and deuterided, but we have not seen evidence for this type of contamination. We attribute the residue and smoke seen from some of the wires to entrained lubricant due to drawing the wire. This lubricant tends to be drawn out and smeared throughout the length of the wire, which implies tritium contaminated oil should be detected in long sections of the wire. However, wires that showed obvious high levels of oil contamination did not show

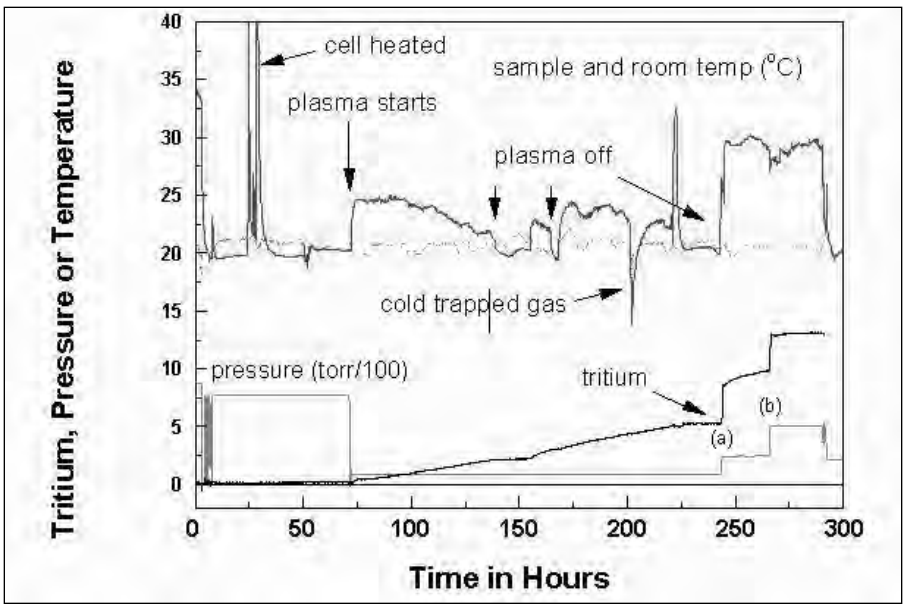

Figure 3. The cell temperature, ambient temperature, tritium concentration and pressure plotted to illustrate the operation of cell 3. When the sample temperature and the ambient temperature are close, the cell is off and the tritium concentration remains constant. When the cell drops below ambient temperature, the cold trap is activated; no significant decrease in tritium level was noticed.

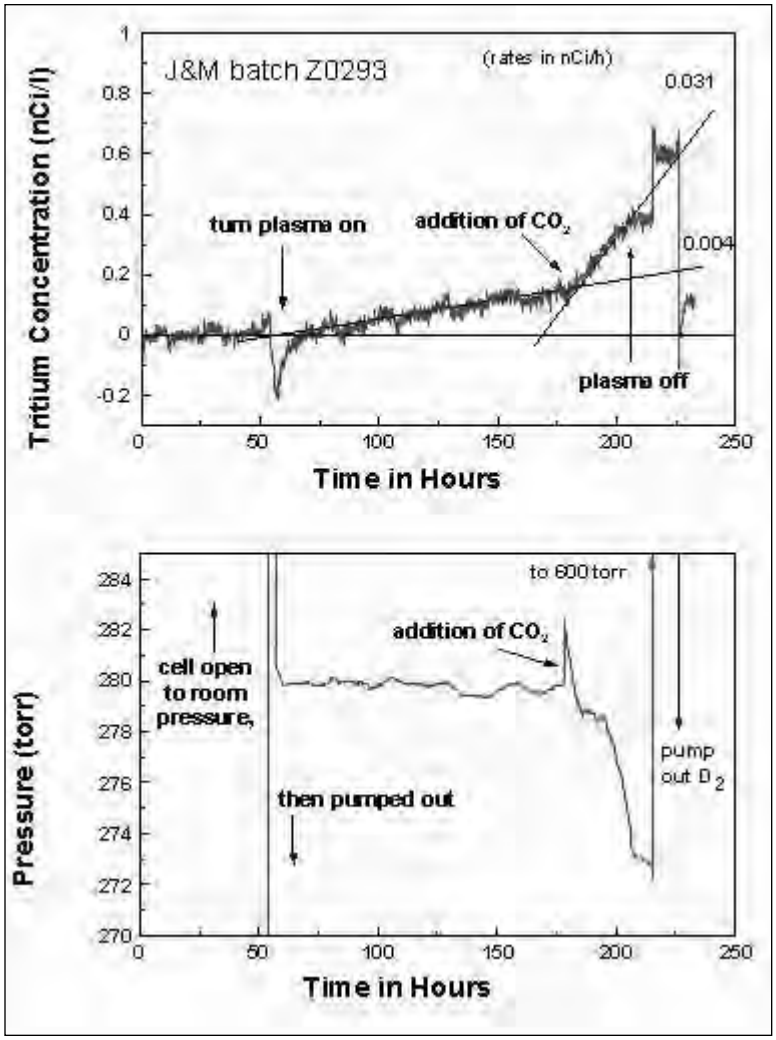

Figure 4. Tritium output from a cell that appeared to be a non-producer until 2 torr CO_2 was added to the deuterium. This caused the plate (and perhaps the wire) to hydride and a coincident rise in the tritium generation rate by nearly a factor of 7 was observed until the wire bent away from the plate.

tritium, and we did not observe a large tritium signal when the wire was heated and the oil suddenly evolved. Similarly, the dark spots are present after simply heating the wire to 600°C but there is no evidence for tritium release at temperatures as high as 850°C.

An indicator that the tritium originated in the cells is that the output was sensitive to the metallurgical condition of the palladium. Palladium wire annealed in air showed a lower output than as received wire. Likewise palladium wire stress relieved but not annealed showed a similar (about a factor of 3) reduction in output. This could be interpreted as a release of tritium if it was contamination, however, then the tritium would have been easily detected in the heating controls. In sample #4, that showed significant tritium output, post heating (250°C) of the palladium wire released 12.4nCi of tritium. This amount of tritium would have been easily detected in the heating control of the same spool of wire (10 cm) taken from the next section of material. Furthermore, the tritium in the gas evolved from the wire during the post heating at 250°C was far (5340 nCi/l) above the equilibrium tritium concentration (31.4 nCi/l) in the gas after the run. The fact that such high concentrations can be left in the palladium suggests that the process is near but not at the surface. The pulse length is sufficient for the diffusion of 200 Angstroms (10 ms pulse length) into the palladium. Then the tritium may be released when the surface layer of the palladium is sputtered by the energetic plasma. This would indicate that the tritium was in a 15 to 30 micron layer on the 250 micron in diameter wire. The fact that a significant amount of tritium shows up as (after the addition of CO_2) TDO is also indicative of a near surface reaction. Dendrites and aspirates (up to 20 microns high) on the surface of the palladium have been suggested[2] as possible tritium formation sites.

When palladium is hydrided it is stressed and, to some extent, work hardened. The wires after hydriding have always shown an increase in grain growth (to 50-100 microns) from the very fine (1-2 microns) microstructure initially observed with these materials. The observed reduction in stress relieved wires indicates that the dislocation density must play a very important role in the tritium production. However, since all as received wires were hard drawn but not all batches of wire showed production, there are other factors that are important, such as the purity and the hydriding.

The purity of the material varies from batch to batch, and within a batch sections of the wire are cleaner than other sections. Thus it could be that the lack of oil, iron or hydrogen impurities is critical or that there has to be an another atomic species present. We believe that the lack of oil or other impurities is important to help the material hydride efficiently. The key mechanism, however, may be associated with another impurity species that need be present only at the sub ppm level at the dislocations.

The importance of hydriding the palladium can be clearly observed in a plot (Figure 4) of tritium output versus time for a sample from batch Z0293 that weakly hydrided. The tritium evolution rate was at the background drift rate (0.004 nCi/l). When (0.75% by vol) CO_2 was added to the system the tritium rate increased to a rate some 7.7 times the background drift rate. Coincident with the tritium increase, the deuterium pressure dropped indicating the palladium plate was hydriding. This decrease in pressure is more than can be accounted for if the CO_2 is totally converted to D_2O. We confirmed this with a platinum control cell in which the pressure only decreased by 2 torr. In another experiment where the pressure immediately dropped, indicating that the palladium had initially hydrided, the tritium generation rate was ~0.02 nCi/h and an addition of CO_2 did not change the rate of tritium production.

The CO_2 may also make it feasible to run at lower pressures where a high loading is more difficult to achieve. An analysis of the ratio of tritiated water to tritium in the gas reveals that most (70%) of the tritium remains in the gas. Additions of CO_2 to Pt runs neither change the rate of drift (tritium) or exhibit large pressure decreases as shown in Figure 4. The CO_2 and CO that is produced within the cell are known to be surface poisons that normally would not allow the palladium to hydride. However, in the presence of a reactive energetic plasma the surface is cleaned of these materials and deuterium is allowed to disassociate on the surface and diffuse in. When the plasma ceases, the surface poison reabsorbs inhibiting deuterium from recombining on the surface.

7. CONCLUSIONS

We have found that the tritium output depends on the temperature, pressure and current applied to the cells. Yet, the tri-

tium yield is most sensitive to the purity and metallurgical condition of palladium used in the experiments. Various tests for tritium contamination confirm that there is no initial tritium contamination in the powder, foil, wire or other materials used in this study. CO_2 additions had a remarkable effect on the production of tritium by these cells and the effect seems to be related to and enhancement of the hydriding of the palladium.

It appears that very pure palladium is more effective than impure palladium in producing tritium. Based on our impurity analysis of the material we cannot identify a difference in concentration of a single impurity that is important to either include or exclude from the palladium. This is partially a morphological or metallurgical issue involving dislocations since we have seen a reduced output from annealed or stress relieved palladium when compared to as received palladium from the same batch. However, palladium that has been hydrided and dehydrided must always be annealed to reactivate it. The fact that most of the tritium is evolved promptly to the gas, yet significant amounts are found in the palladium suggest that the process is near but probably not at the surface.

8. ACKNOWLEDGMENTS

Many people were involved in a direct way with the experiments described here. Some of these were Ken Griechen, Roy Strandberg, and Kane Fisher, who were instrumental in the design and construction of the first few cells. Joe Thompson counted our tritiated water samples. Mike Hiskey and William Hutchinson were helpful in the analysis of contaminants in the vacuum system and on the samples.

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The Wright Brothers and Cold Fusion

Jed Rothwell

Hart O. Berg was an engineer and high tech military-industrial wheeler-dealer. He was an agent for the Charles Flint Company, an investment firm that organized trusts and once sold an entire naval fleet to Brazil. Berg managed the European operations, selling American submarines, machine guns, and electric automobiles. In 1908 he sold the airplane to the world. If he had not done that, Wilbur Wright would have died in obscurity in 1912, carrying many of his secrets to the grave. T.O.M. Sopwith and a half-million other people would not have latched onto aviation and developed it intensively before World War I. The airplane would not have been ready. The allies would have lost without the Sopwith Pup and Camel. Or if they had pulled through, they would have lost the Battle of Britain twenty years later without Sopwith's Hurricane fighters. Berg, the Wrights, and Sopwith together twice saved Western civilization by the narrowest of margins. The high tech entrepreneur Berg changed history by showing the Wrights how to make money.

People often ask: if cold fusion is real, why is it ignored and attacked? I say we have been down this road before. People had to fight to win acceptance for antiseptics, amorphous semiconductors, and even the transcontinental railroad. There are many lessons for cold fusion in the Wright story. Here are a few of them:

History is not inevitable. If the Wrights had not built the airplane, man would not have flown for another ten or twenty years, most experts agree. History is a product of free will. People make decisions, take actions, and shape events. Things do not get invented just because they are needed. We learn to live with awkward machines like the automobile transmission. If Bell Labs had not come up with the transistor, by now we would have computers with a million "vacuum tubes on a chip." (I recall seeing of a photograph of such a chip, fabricated for a special application a few years ago. Technology is flexible; transistors are not the only things you can miniaturize.)

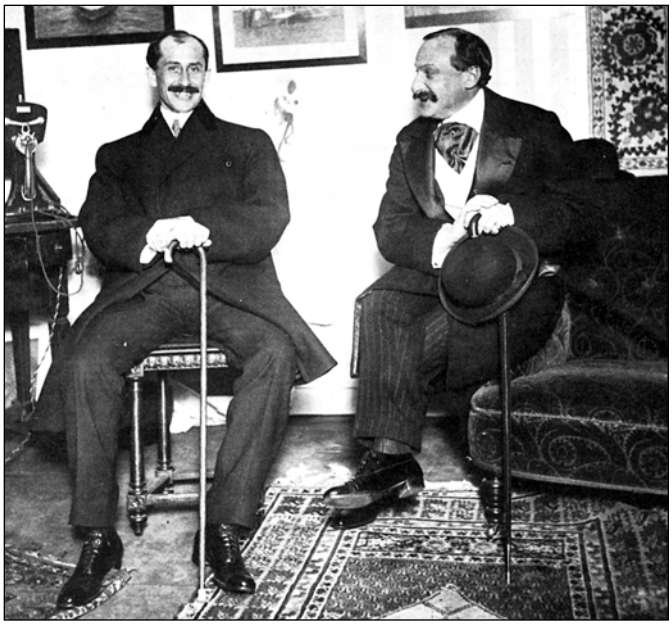
New technology is unpredictable; the only way to get a handle on it is to use it. When something new bursts upon the scene, you cannot predict where it will go or who will be the leading players. The Wrights, Tom Sopwith, Bill Gates, or Michael Dell beat the big guys because they know the technology. The only way to master technology is to get a machine and play with it. In the 1980s IBM lost out because its managers did not use computers. As Paul Carroll of the *Wall Street Journal* put it: "IBM had become like a music-publishing company run by deaf people."[1]

To introduce a new technology you must fight two groups of people: the scientists who oppose it and the scientists who invent it. The Wrights were their own worst enemies from 1906 to 1908. After battling with the establishment for five years, they began acting like paranoid flakes. Some cold fusion scientists are worse.

It is never easy to sell revolutionary technology. Invent a better mousetrap and the world will beat a path to your door, burn your house down, and run you out of town.

The Wright's History

Although it is well-documented, the Wrights' history is not well-known. Myths, misperceptions, jealousy, and revisionist history have obscured the facts.[2] Their achievement deeply embarrassed the establishment. *Scientific American* has been trying to rewrite its buffoonish role in the affair for years, most recently in a 1993 article![3] The Smithsonian Institution denigrated the Wrights for years in a feud over Langley's priority.



Orville Wright with Hart O. Berg, the Wrights' agent in non-English speaking countries.

In 1900 a small band of scientists worked at the fringes of respectability, trying to learn to fly. Some were distinguished men like Alexander Graham Bell, Langley, Maxim, and Chanute. They were old, discouraged, and lonely. Little progress had been made since the death of Otto Lilienthal. Young scientists would not touch the field. That is true of cold fusion today: our champions are the old mavericks like Bockris and Fleischmann. In 1900 there was only one serious, properly funded aviation R&D program in the world. It was at the Smithsonian, where the director, Langley, was trying to scale up his steam driven small models that had flown successfully in 1895. To the vast majority of other scientists, and in all popular journals and newspapers, the issue was settled. A heavier-than-air flying machine was physically impossible. It was an absurdity, a gross violation of the laws of nature. This had been proved mathematically with "unassailable logic" by leading experts in physics, writing in distinguished journals and magazines.[4]

We admire Chanute and Langley, but the fact is they were stuck, just as most cold fusion scientists are stuck today. The field was "moribund," as one expert says.[5] Langley's experiments ended in a fiasco in December 1903. He was lambasted by the press and by Congress for wasting $50,000 of the taxpayer's money. There was, at that moment, nothing left of aviation—not a single research project and seemingly no hope of success, until the Wrights flew two weeks later at Kitty Hawk...in one of history's great ironies.

Going back to 1898, what aviation needed was new blood

and a spark of genius. It got that in Wilbur and Orville Wright. Let me puncture myth number one here. The popular image is that they were small town bicycle mechanics who by trial and error stumbled on a workable design. Nothing could be further from the truth. They were scientific geniuses. They invented the airplane by observation, experiment, database compilation, and analysis. They performed highly complex mathematical modeling of everything from the wings, the fuselage, and propeller to the wind resistance of the pilot's head. Their wind tunnel data was so accurate it was not improved upon until the 1920s. Before they cut wood to build the first propeller, they modeled it, optimized it, and predicted its performance. They got it right to within one percent.[7,8] Their science, engineering, craftsmanship, and experimental technique were beautiful. Their work has the distilled elegance you see in Faraday's experiments and Niklaus Wirth's program code. They were wonderful scientists and lousy businessmen.

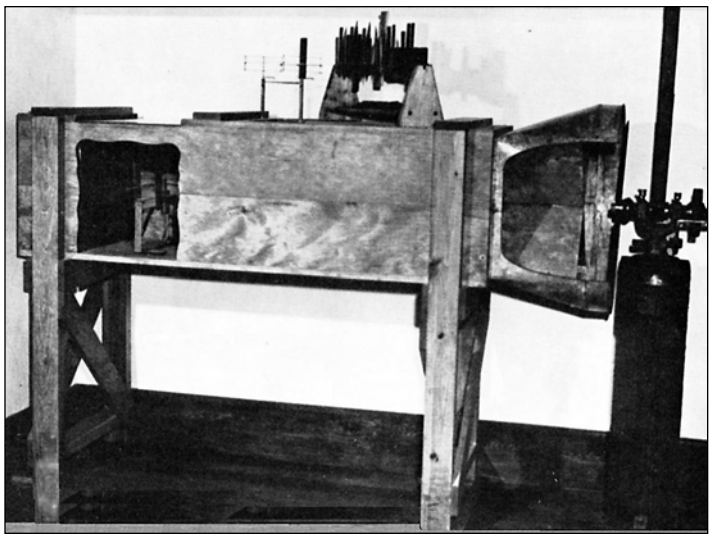
They first flew in 1903. In 1904 and 1905 they flew on Huffman Prairie, next to the trolley car line in Dayton, Ohio, where many people saw them. When the Wrights were later accused of secrecy, they produced a list of more than 60 people who had witnessed flights. They had signed affidavits from leading citizens of Dayton, the city auditor, a bank president and so on. Their longest flight was twenty-four miles.

The Wrights stopped flying from 1906 to mid-1908. They devoted much of this time to pursuing business deals in the U.S. and Europe. They also built improved airplanes and engines. In late 1906 the Charles Flint Company contacted them and agreed to act as their agents.

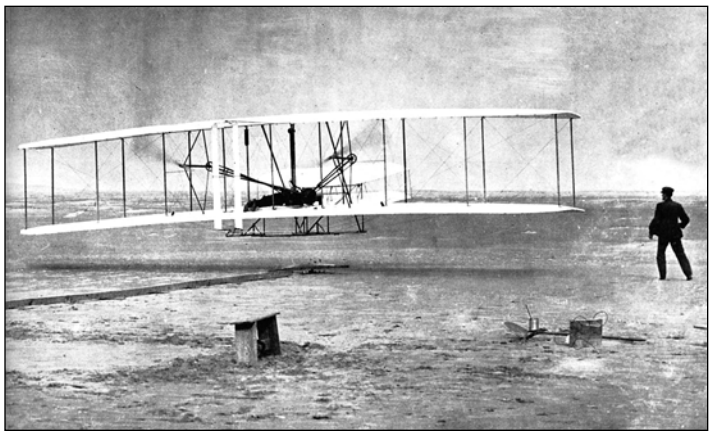
The Wrights spent three years trying to peddle their machine to national governments, getting nowhere. They asked for no down payment, but they demanded a written guarantee that the customer would pay after a successful demonstration. That may seem reasonable, but not to a customer who thinks your machine is impossible and you're crazy. When French and British government agents visited the Wrights in Dayton, they were shown photographs and affidavits, but no flights, because the Wrights would not fly without a contract. The agents reported back to headquarters that the claims must be true. It was not enough. The Wrights should have followed up by sending photographs and documentation. They should have understood that these negotiations required approval at the highest levels, and you cannot ask the French Minister of War to come to Dayton. As Crouch says: "a personal visit to Washington with a handful of the astonishing photos of the long flights of 1904-95, accompanied by affidavits from the Huffman Prairie witnesses, would surely have convinced the [Army] board."[9] The cold fusion scientists make the very same mistakes! They should circulate more data, more photos, and they should perform more demonstrations. They should stop trying to sell their machines to governments and big corporations who do not want them, and find live customers who do.

The French capitalists who were backing a syndicate pushed the Wrights to make a demonstration flight. The Wrights intended to do one eventually, but they procrastinated. If the capitalists had not pushed them, history would have passed the Wrights by and the aviation boom would have been delayed two or three more years. Fortunately, things got moving. In Washington, President Theodore Roosevelt personally intervened to break the logjam. At last, in December 1907 the Army Signal Corps agreed that if the Wrights could do a demonstration flight, carrying a passenger at 40 mph over a distance of ten miles, they would be paid $25,000. The press lambasted the War Department for encouraging crackpots. Newspapers said that if an airplane capable of doing this existed, everyone would already know about it and it would be worth millions, so why would the inventors settle for a mere $25,000? As the *New York Globe* put it:[10]

> One might be inclined to assume from the following announcement, "the United States Army is asking bids for a military airship," that the era of practical human flight had arrived. . .A very brief examination of the conditions imposed and the reward offered for successful bidders suffices, however, to prove this assumption a delusion.
>
> A machine such as is described in the Signal Corps' specifications would record the solution of all the difficulties in the way of the heavier-than-air airship, and, in fact, finally give mankind almost as complete control of the air as it now has of the land and the water. It would be worth to the world almost any number of millions of dollars, would certainly revolutionize warfare and possibly the transportation of passengers. . .
>
> Nothing in any way approaching such a machine has ever been constructed (the Wright brother's claims still await public confirmation). . .If there is any possibility



A Wright wind tunnel.



The first successful flight in history, December 17, 1903 at 10:35 a.m. Orville is at the controls and Wilbur stands to the right.

that such an airship is within measurable distance of perfection any government could well afford to provide its inventor with unlimited resources and promise him a prize, in case of success, running into the millions.

In other words, we shouldn't have a demonstration because we already know it doesn't work because there hasn't been a demonstration. A Catch 22! We hear the same stuff from the cold fusion opposition today.

The story has a happy ending. On August 8, 1908, Wilbur made a flight in front a few hundred people in France. Within days he was a hero on the front page of every European newspaper. He was given gold medals, thousands of dollars in prizes, and contracts in every European capital. Thousands of people flocked to see the flights. He wrote that "princes & millionaires are as thick as fleas." Meanwhile, not a word of the European frenzy reached the American newspapers. So, on September 3, when Orville prepared for his first test flight at Fort Meyer, only a few hundred people turned out to see him. President Roosevelt's son was there. Orville took off, circled the field one-and-a-half times, and landed after a minute and eleven seconds. Years later Roosevelt described the scene:

> [The crowd] went crazy. When the plane first rose, the crowd's gasp of astonishment was not alone at the wonder of it, but because it was so unexpected. I'll never forget the impression that sound from the crowd made on me. It was a sound of complete surprise.

Wilbur during a demonstration flight in France.

The lesson is obvious. People believe what they see with their own eyes. The only way to convince people that revolutionary new technology is real is to demonstrate it in public. Let the whole world see it. Put it into the hands of as many customers as you can, as quickly as possible. Cold fusion scientists today are asking only fellow scientists to look at their data. It is as if the Wrights showed wind tunnel data instead of airplanes, and talked to a few other scientists while ignoring the public.

Secrecy

Another myth is that the Wrights were deeply secretive about their work. This was the establishment's excuse for the five years of official neglect after Kitty Hawk. Here is a wonderful section from the authorized biography:[11]

> Dan Kumler. . .city editor *Daily News*, in Dayton, recalled in 1940. . . that many people who had been on interurban cars passing the Huffman field and seen the Wrights in the air used to come to the Daily News office to inquire why there was nothing in the paper about the flights.
>
> "Such callers," said Kumler, "got to be a nuisance."
>
> "And why wasn't there anything in the paper?" Kumler was asked.
>
> "We just didn't believe it," he said. "Of course you remember that the Wrights at that time were terribly secretive."
>
> "You mean they were secretive about the fact that they were flying over an open field?"
>
> "I guess," said Kumler, grinning, after a moment's reflection, "the truth is that we were just plain dumb."

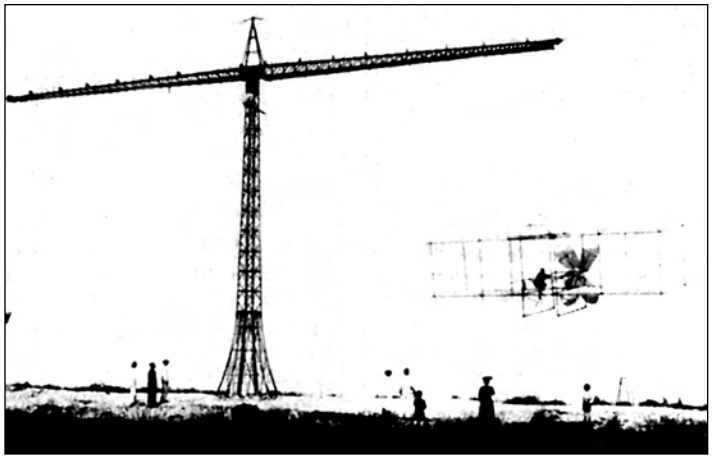
Today, people say we are secretive. I say, "You mean we are secretive about the fact that MITI is sponsoring an international conference next month?" However, it is true that the Wrights and the cold fusion scientists became secretive over time. A few years ago Pons and Fleischmann were showing videos of boiling cells and publishing papers in major journals. Now we hear nothing from them. Even after they got a patent, the Wrights did squirrely things like publishing blurred photographs, to hide details. They made up strange justifications for their strategy, such as the idea that the airplane is more valuable as a secret weapon: the British will pay more if the Germans have not seen it. "The less other governments know, the more it is worth to the purchaser. At present we are able to give positive assurance to any government that other governments have not seen the machine."[12] They gave two main reasons for their secrecy; reasons I have heard many times from cold fusion scientists:

1. Some design improvements were not covered in the 1906 patent. Why didn't they simply file another patent?

2. The competition was far behind, and making little progress in spite of the patent. The Wrights thought this gave them a precious lead they should "conserve." In 1907 they wrote to Charles Flint: "We can furnish governments with practical machines. . .now: no one else can. There is no certainty that anyone else is within three years of us. . .The progress made by others since the announcement of our final success at the end of 1905 is as rapid as could reasonably be expected, but it by no means indicates that others will reach the goal in less time than we required."[13]

Their strategy was predicated on the preposterous idea that you can keep a patented airplane secret. It never seems to have occurred to them that once intense public interest ignites, the quality of replications must improve dramatically. Furthermore, they did not grasp that it is much easier to replicate than it is to invent something in the first place. They should have seen that only third-rate people were trying replicate them during this period, but that in a boom thousands of talented people would soon get to work and progress would be immeasurably swifter. I am distressed to read that Charles Flint agreed with their tactics of keeping the invention under wraps. Many of the entrepreneurs backing cold fusion make this same mistake. They have the same mindset as the scientists.

Many cold fusion scientists want praise and recognition, but they do not want people to steal their ideas, so they play peek-a-boo with their results. Like the Wrights, they are concerned about losing their lead, and like the Wrights I am sure they will lose it a few months after they go public. Nothing becomes obsolete faster than the early models of a new technology. Think of the microcomputers of the 1970s: the TRS-80, SOL, North Star, and Cromemco. It is absurd to worry about being overtaken. You will be competing with every industrial corporation on earth; of course you will be overtaken! If you have no



How not to replicate. French army captain Ferdinand Ferber tests his powered Wright-type glider, suspended from a huge whirling arm, near Nice in June 1903.

patent then you have no protection, so you might as well give the technology away. And if do have a patent, the competition will find out everything there is to know the moment they take you seriously. Either way, there is no point in keeping it secret.

Incremental improvements to established technology must be kept secret. Revolutionary devices that are still at the impractical stage must be made public, or they will never attract the critical mass of people necessary to make them into practical, commercial products. The Wrights' secrecy and their precious three-year lead turned out to be millstones around their necks. They finally began earning money and recognition after they went public.

No Replications and Little Progress Until 1908

News of the Wrights work caused a rebirth of interest in aviation, particularly in France. Yet nobody else flew until 1906, when Santos-Dumont staggered off the ground in barely controlled hops. The French tried some of the innovations the Wrights described in their papers and patents, which circulated widely. But nobody tried all of the innovations in a single careful copy of the patent. For seven years, nobody really tried to replicate. Popular revisionist history books still blame the Wrights because the French did not do their homework.[14]

The photo above shows a famous example of how not to replicate, paid for by the French army in 1902. It built this whirling tower in Nice, France and suspended a biplane built by Captain Ferdinand Ferber. He said it was designed "along the same lines as" the Wright machines. Please note the wings are flat, not chambered. Ferber figured he did not need any fancy wing chambering or warping controls (flaps). He missed the whole point of their work!

You might think that scientists are more sophisticated today and they would never perform such inept "replications." Well, think again. A scientist at a national laboratory once told me that he had done a close replication of the Mills experiment, except Mills used water and he decided to use acid instead. A few weeks ago Barry Merriman at the University of California announced that he had done a replication of the Patterson cell, and he saw no heat.[15] Well:

•Merriman used glass beads. Patterson used plastic. Merriman called that a "minor" difference but for all he knows it could be critical.

•Merriman has no idea whether his beads absorb hydrogen rapidly, as shown in the patents. He has not even measured that parameter; like Ferber, he ignored the most critical point in the published work.

•The man who fabricated the beads never saw the patents.

Many Frenchmen tried to replicate the Wrights, apparently without bothering to read their scientific papers or patent. They thought they knew better than the Wrights. When their machines failed, they blamed the Wrights, saying the design was fraudulent. Today, many scientists who made equally ridiculous mistakes pontificate in the newspapers about how they proved cold fusion is wrong. They do what I call "South Pacific cargo-cult science," where you tie a pine cone to a stick,

How To Do Research Right

Much has been written about how to do science and research and development correctly, and how to develop products on time and on budget. Few people in history have understood the essence of this problem better than the Wrights, and few people have expressed it better than Wilbur did, fifteen days before his untimely death, in a text he was preparing for the Aero Club of America:

> When the general excellence of the work of Lilienthal is considered, the question arises as to whether or not he would have solved the problem of human flight if his untimely death in 1896 had not interrupted his efforts. . .One of the greatest difficulties of the problem has been little understood by the world at large. This was the fact that those who aspired to solve the problem were constantly pursued by expense, danger, and time. In order to succeed it was not only necessary to make progress, but it was necessary to make progress at a sufficient rate to reach the goal before money gave out, or before accident intervened, or before the portion of life allowable for such work was past. The problem was so vast and many-sided that no one could hope to win unless he possessed unusual ability to grasp the essential points, and to ignore the nonessentials. . .When the detailed story is written of the means by which success in human flight was finally attained, it will be seen that this success was not won by spending more time than others had spent, nor by taking greater risks than others had taken.
>
> Those who failed for lack of time had already used more time than was necessary; those who failed for lack of money had already spent more money than was necessary; and those who were cut off by accident had previously enjoyed as many lucky escapes as reasonably could be expected.
>
> Lilienthal progressed, but not very rapidly. His tables of pressures and resistances of arched aeroplane surfaces were the results of years of experiment and were the best in existence, yet they were not sufficiently accurate to enable anyone to construct a machine with full assurance that it would give exactly the expected results. Under such conditions progress could not but be slow. His methods of controlling balance both laterally and longitudinally were exceedingly crude and quite insufficient. Although he experimented for six successive years 1891 - 1896 with gliding machines, he was using at the end the same inadequate method of control with which he started. His rate of progress during these years makes it doubtful whether he would have achieved full success in the near future if his life had been spared. . .

The part about: "Those who failed for lack of time. . ." should be framed and mounted above the workbench of every cold fusion scientist. I am reminded of what Raphael Soyer used to say (and what his teacher told him, at the Art Students League): "You have time, but not an OCEAN of time."

Applied Science, Not Physics

The Wrights did applied science, not basic physics. From their wind tunnel work, they compiled data tables titled "Gliding pressure," "Tangentials, gliding angles, drag: lift ratios" and so on. They used this data to determine the proper shape and chamber of the wings, the separation of the wings, fuselage shape, the design of the propellers and a host of other essential design parameters. They modeled the performance of their machines before building them. Then, based on actual performance of the full-scale machines, revised and refined the models. They could not have done it any other way. It would have taken too much time and money, and flight testing would have been too dangerous. As it was, both brothers were suffered dozens of crashes, some nearly fatal. Regarding the development of theory, Crouch writes (T. Crouch, *The Bishop's Boys*, Norton, 1989, page 175):

> Engineering was the key. The Wright brothers function as engineers, not scientists. Science, the drive to understand the ultimate principles at work in the universe, had little to do with the invention of the airplane. A scientist would have asked the most basic questions. How does the wing of a bird generate lift? What are the physical laws that explain the phenomena of flight?
>
> The answers to those questions were not available to Wilbur and Orville Wright, or to anyone else at the turn of the century. Airplanes would be flying for a full quarter century before physicists and mathematicians could explain why wings worked.
>
> How was it possible to build a flying machine without first understanding the principles involved? In the late twentieth century, we regard the flow of technological marvels from basic scientific research as the natural order of things. But this relationship between what one scholar, Edwin Layton, has described as the "mirror image twins" of science and technology is a relatively new phenomenon. Historically, technological advance has more often preceded and even inspired scientific understanding.

pretend it is a microphone, and you call down results from the sky. Going through the motions is not enough.

These non-replications share another quality with bad cold fusion experiments: more money and attention was lavished on the experimental apparatus than the actual device. Langley spent thousands on the elaborate launch platform built on top of the houseboat. The Wrights did a better job with a monorail costing a few dollars. The French Army must have spent a fortune on the whirling tower. Ferber's airplane looks like an afterthought in comparison. Langley built a similar whirling tower in Pennsylvania that cost many thousands. The Wrights did a far better job with a wind tunnel that cost less than $50. In cold fusion we have seen many splendid calorimeters and ultra high tech neutron detectors hooked to sloppy, ill-prepared electrochemical cells.

Even after Farman, Voisin, Delagrange, and others finally did manage to replicate the Wrights in 1908, they used empirical trial-and-error methods, instead of basing their work on wind tunnel data and engineering analysis. The results were predictable. "It must have been an embarrassing situation, for despite having three and four and even five times as much engine power as was available to the Wrights, the thrust from their propellers gave them less flying power than the first Wright Flyer."[16]

The Aviation Boom

After the Wrights became international media stars, French airmen copied them carefully. Still, many screwball ideas were developed after 1908. Alexander Graham Bell was no fool, but his Cygnet II never left the ground in 1909. In 1910 a Professor Mertz decided that if two wings were good, five wings must be better. But, for every Bell or Mertz there were soon dozens of talented people who got it right. By 1911, *Scientific American* said that a half-million men were working on aviation. Progress over the six years before the First World War was unprecedented. It was free-for-all competition. If you want rapid progress, you must make room for screwballs like Mertz along with geniuses like Sopwith. The boom culminated in 1914 when Igor Sikorsky set a record carrying six passengers for 6 hours 33 minutes in the Ilia Mourometx, a multimotored enclosed airplane that could carry sixteen passengers in comfort.[17]

If the Wrights had not demonstrated the airplane to the world, progress would have limped along the way it did from 1901 to 1908, with just a handful of people. It takes thousands of people to develop revolutionary technology. Each individual works on his own ideas, in chaotic competition. An organized, centrally directed project like MITI's will not cut the mustard. The 1908 demonstrations galvanized the world. Without it, aviation would not have advanced enough to play a significant role in the war. The allies, who depended on a thin edge of technological superiority, might have lost.

Dealing With Geniuses

When Hart Berg met Wilbur, he wrote a wonderful letter to his headquarters describing what it is like working with a stubborn genius. People who get involved with cold fusion must learn to deal with people like this. Here is part of the letter:[18]

> At 12:30 yesterday I met Mr. Wilbur Wright at Euston Station. I have never seen a picture of him, or had him described to me in any way, still he was the first man I spoke to, and either I am Sherlock Holmes, or Wright has that peculiar glint of genius in his eye which left no doubt in my mind as to who he was. . .
>
> The company idea did seem to please him very much, as he first wanted to know himself exactly what the attitudes of the several governments were. After a long talk . . .I believe, please note that I say distinctly "I believe," that I made something of an impression as regards the impossibility of getting any sort of action in the near future from any government. He agreed he did not think the British Government would do any business. He also stated that perhaps it would be very hard to do anything with the French Government. . .
>
> About 5 o'clock in the afternoon, I think, you will distinctly note that I said "I think," I brought about some sort of action in his mind, and think he was on the point, you will note that I say that "I think he was on the point," of veering around from the government to company methods. . .

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1. P. Carroll, *Big Blues: The Unmaking of IBM*, (Crown Publishers, 1993).

2. A. Golin, *No Longer an Island: Britain and the Wright Brothers 1902-1909*, (Stanford University Press, 1984), p. 31-33.

3. Reprint of book review of *The Wright Brothers: A Biography*

Authorized by Orville Wright in the "50 and 100 Years Ago" column, July 1993.

4. F. Kelly, *The Wright Brothers: A Biography Authorized by Orville Wright*, (Harcourt, Brace and Company, New York, 1943), p. 116, describing Simon Newcomb.

5. T. Crouch, *The Bishop's Boys*, (Norton: 1989), p. 231. See also: T. Crouch, *A Dream of Wings*, (Smithsonian Institution Press, 1985) for a detailed history of aviation before the Wrights.

6. H. Combes, *Kill Devil Hills*, (TernStyle Press, 1979), p. 119.

7. Combes, p. 186, and noted by most other biographies.

8. Golin, p. 34. Golin quotes British expert J.L. Prichard's description of the propeller design: "Of such stuff is genius made!"

9. Crouch, p. 305.

10. Kelly, p. 209.

11. Kelly, p. 135.

12. Wright's letter to Flint, April 12, 1907, quoted by Golin, p 221.

13. Golin, p. 243. See discussion of opinions of the Wrights and Flint during this period, pp. 240 250.

14. For example, see: C. Pendergrast, *The First Aviators*, (Time-Life Books, 1981), p. 7, p. 17.

15. On the web, http://www.math.ucla.edu/~barry/CF/CETIX.html

16. Combs, p. 181.

17. H. Villard, *"Contact!,"* (Crowell, 1968), p. 220.

18. Quoted in Golin, p. 245, and in many other biographies.

Early Aviation Time Line

1890s	Progress in aviation "moribund" [Crouch] as Lilienthal and Pilcher are killed gliding, and experiments by Chanute, Maxim and others peter out. Only Langley continues.
1895	The Wrights become seriously interested in aviation.
1899	First kite experiments with wing warping.
1900	Tests at Kitty Hawk.
1901	Invent wind tunnel, compile world's first reliable data on airfoils. First published paper in the Proc. Western Society of Engineers.
1902	Tests at Kitty Hawk.
1903	Second paper in Proc. Western Society of Engineers. Ferber tests a "Wright-type" glider suspended from a huge whirling arm in Nice, France October and December: Langley attempts two flights, which end in disaster. December 17: First powered flight at Kitty Hawk.
1904	Test flights at Simms Station trolley car stop, Dayton Ohio. First turns exhibit complete control. Flights are widely observed. Leading citizens of Dayton sign affidavits. Press ignores or attacks Wrights. British and French government agents contact Wrights, first attempts to sell airplane to governments.
1905	Continued testing. Airplane now "a machine of practical utility" [O. Wright]. Longest test flight 24 miles in 39 minutes. Continued attempts to sell airplane to governments.
1906	Patent granted. In France, Santos-Dumont makes first flight by anyone other than Wrights; nearly uncontrolled 50 meter hop. Europeans become excited about aviation. Continued fruitless attempts to sell airplane to governments. *Scientific American* again attacks Wrights. Wrights publish a list of 17 leading citizens of Dayton who have observed flights. (They have a list of 60 witnesses.) *Scientific American* at last contacts witnesses, and in November sends an editor to meet with Wrights. November, first contact from Charles Flint Company. Charles Flint Company acts as the Wrights agents. December, Scientific American finally retracts and endorses flights. Most other journals continue to ignore or attack them.
1907	Continued fruitless attempts to sell airplanes to governments. President Roosevelt sees clipping from Scientific American, personally orders that the logjam should be broken and the Wrights invited to demonstrate airplane in Washington, but arrangements fall through because the War Department does not have $100,000 budget for purchase of an airplane. In France, Farman makes first shallow, nearly uncontrolled turn. December: After Roosevelt guarantees funds will be found, contract with War Department for payment of $25,000 contingent upon a successful demonstration flight. Press attacks War Department for signing contract.
1908	Continued fruitless attempts to sell airplanes to European governments. Curtiss makes first serious attempt to replicate Wrights, resulting in first flight of over one kilometer by anyone other than Wrights; flight is barely controlled, skirts disaster. June: Wrights fly for the first time in two and a half years with improved airplane, carry passenger for the first time. August, Wrights publicly demonstrate flights in France and the U.S. All skeptical doubts and opposition instantly ends. First fatal airplane accident. Wilbur crashes, passenger Lt. Selfridge killed. Wrights become international heroes; first media stars of the 20th century. Business deals offered by leading U.S. and European capitalists. Thousands of people begin replicating Wrights.
1909	Europeans take the lead in aviation.
1911	Special issue of *Scientific American* devoted to aviation reports that "more than half a million men are now actively engaged in some industrial enterprise that has to do with navigation of the air." Wrights now spending much of their time in court fighting patent infringement.
1912	May, Wilbur dies of typhoid at age 45. European armies begin serious pilot training and acquisition of airplanes.
1914	Igor Sikorsky's Ilia Mourometx multimotored airplane carries up to 16 passengers. It sets a record carrying 6 passengers for 6 hours 33 minutes. German military pilot in endurance test flies 1900 kilometers in 21 hours 50 minutes. World War One begins. From the start, air reconnaissance is crucial.
1920s	Wright wind tunnel data improved upon by others for the first time. First physics theories developed to explain airfoil lift – but the issue is still not completely settled as of 1996.

ZEN. . .AND THE ART OF DEBUNKERY
How to Debunk Just About Everything

Daniel Drasin

So you've had a close encounter with a UFO. Or a serious interest in the subject of extramundane life. Or a passion for following clues that seem to point toward the existence of a greater reality. Mention any of these things to most working scientists and be prepared for anything from patronizing skepticism to merciless ridicule. After all, science is supposed to be a purely hardnosed enterprise with little patience for "expanded" notions of reality. Right? Wrong.

Like all systems of truth seeking, science, properly conducted, has a profoundly expansive, liberating impulse at its core. This "Zen" in the heart of science is revealed when the practitioner sets aside arbitrary beliefs and cultural preconceptions, and approaches the nature of things with "beginner's mind." When this is done, reality can speak freshly and freely, and can be heard more clearly. Appropriate testing and objective validation can—indeed, *must*—come later.

Seeing with humility, curiosity, and fresh eyes was once the main point of science. But today it is often a different story. As the scientific enterprise has been bent toward exploitation, institutionalization, hyperspecialization and new orthodoxy, it has increasingly preoccupied itself with disjointed facts in a psychological, social and ecological vacuum. So disconnected has official science become from the greater scheme of things, that it tends to deny or disregard entire domains of reality and to satisfy itself with reducing all of life and consciousness to a dead physics.

As we approach the end of the millennium, science seems in many ways to be treading the weary path of the religions it presumed to replace. Where free, dispassionate inquiry once reigned, emotions now run high in the defense of a fundamentalized "scientific truth." As anomalies mount up beneath a sea of denial, defenders of the Faith and the Kingdom cling with increasing self-righteousness to the hull of a sinking paradigm. Faced with provocative evidence of things undreamt of in their philosophy, many otherwise mature scientists revert to a kind of skeptical infantilism characterized by blind faith in the absoluteness of the familiar. Small wonder, then, that so many promising fields of inquiry remain shrouded in superstition, ignorance, denial, disinformation, taboo. . .and debunkery.

What is "debunkery?" Essentially it is the attempt to *debunk* (invalidate) new information and insight by substituting scient*istic* propaganda for the scient*ific* method.

To throw this kind of pseudoscientific behavior into bold—if somewhat comic—relief, I have composed a useful "how-to" guide for aspiring debunkers, with a special section devoted to debunking extraterrestrial intelligence—perhaps the most aggressively debunked subject in the whole of modern history. As will be obvious to the reader, I have carried a few of these debunking strategies over the threshold of absurdity for the sake of making a point. As for the rest, their inherently fallacious reasoning, twisted logic and sheer goofiness will sound frustratingly familiar to those who have dared explore beneath the ocean of denial and attempted in good faith to report back about what they found there. So without further ado. . .

Part 1: General Debunkery

• Before commencing to debunk, prepare your equipment. Equipment needed: one armchair.

• Put on the right face. Cultivate a condescending air that suggests that your personal opinions are backed by the full faith and credit of God. Employ vague, subjective, dismissive terms such as "ridiculous" or "trivial" in a manner that suggests they have the full force of scientific authority.

• Portray science not as an open-ended process of discovery but as a holy war against unruly hordes of quackery-worshipping infidels. Since in war the ends justify the means, you may fudge, stretch or violate the scientific method, or even omit it entirely, in the name of defending the scientific method.

• Keep your arguments as abstract and theoretical as possible. This will "send the message" that accepted theory overrides any actual evidence that might challenge it—and that therefore no such evidence is worth examining.

• Reinforce the popular misconception that certain subjects are inherently unscientific. In other words, deliberately confuse the *process* of science with the *content* of science. (Someone may, of course, object that since science is a universal approach to truth-seeking it must be neutral to subject matter; hence, only the investigative *process* can be scientifically responsible or irresponsible. If that happens, dismiss such objections using a method employed successfully by generations of politicians: simply reassure everyone that "there is no contradiction here!")

• Arrange to have your message echoed by persons of authority. The degree to which you can stretch the truth is directly proportional to the prestige of your mouthpiece.

• Always refer to unorthodox statements as "claims," which are "touted," and to your own assertions as "facts," which are "stated."

• Avoid examining the actual evidence. This allows you to say with impunity, "I have seen absolutely no evidence to support such ridiculous claims!" (Note that this technique has withstood the test of time, and dates back at least to the age of Galileo. By simply refusing to look through his telescope, the ecclesiastical authorities bought the Church over three centuries' worth of denial free and clear!)

• If examining the evidence becomes unavoidable, report back that "there is nothing new here!" If confronted by a watertight body of evidence that has survived the most rigorous tests, simply dismiss it as being "too pat."

• Equate the necessary skeptical component of science with *all* of science. Emphasize the narrow, stringent, rigorous and critical elements of science to the exclusion of intuition, inspiration, exploration and integration. If anyone objects, accuse them of viewing science in exclusively fuzzy, subjective or metaphysical terms.

• Insist that the progress of science depends on explaining the unknown in terms of the known. In other words, science equals reductionism. You can apply the reductionist approach in any situation by discarding more and more and more evidence until what little is left can finally be explained entirely

in terms of established knowledge.
• Downplay the fact that free inquiry and legitimate disagreement are a normal part of science.
• Make yourself available to media producers who seek "balanced reporting" of unorthodox views. However, agree to participate in only those interviews whose time constraints and *a priori* bias preclude such luxuries as discussion, debate and cross-examination.
• At every opportunity reinforce the notion that what is familiar is necessarily rational. The unfamiliar is therefore irrational, and consequently inadmissible as evidence.
• State categorically that the unconventional may be dismissed as, at best, an honest misinterpretation of the conventional.
• Characterize your opponents as "uncritical believers." Summarily dismiss the notion that debunkery itself betrays uncritical belief, albeit in the status quo.
• Maintain that in investigations of unconventional phenomena, a single flaw invalidates the whole. In conventional contexts, however, you may sagely remind the world that, "after all, situations are complex and human beings are imperfect."
• "Occam's Razor," or the "principle of parsimony," says the correct explanation of a mystery will usually involve the simplest fundamental principles. Insist, therefore, that the most familiar explanation is by definition the simplest! Imply strongly that Occam's Razor is not merely a philosophical rule of thumb but an immutable law.
• Discourage any study of history that may reveal today's dogma as yesterday's heresy. Likewise, avoid discussing the many historical, philosophical and spiritual parallels between science and democracy.
• Since the public tends to be unclear about the distinction between evidence and proof, do your best to help maintain this murkiness. If absolute proof is lacking, state categorically that "there is no evidence!"
• If sufficient evidence has been presented to warrant further investigation of an unusual phenomenon, argue that "evidence alone proves nothing!" Ignore the fact that preliminary evidence is not supposed to prove *any*thing.
• In any case, imply that proof precedes evidence. This will eliminate the possibility of initiating any meaningful process of investigation—particularly if no criteria of proof have yet been established for the phenomenon in question.
• Insist that criteria of proof cannot possibly be established for phenomena that do not exist!
• Although science is not supposed to tolerate vague or double standards, always insist that unconventional phenomena must be judged by a separate, yet ill-defined, set of scientific rules. Do this by declaring that "extraordinary claims demand extraordinary evidence"—but take care never to define where the "ordinary" ends and the "extraordinary" begins. This will allow you to manufacture an infinitely receding evidential horizon (*i.e.,* to define "extraordinary" evidence as that which lies just out of reach at any point in time).
• In the same manner, insist on classes of evidence that are impossible to obtain. For example, declare that unidentified aerial phenomena may be considered real only if we can bring them into laboratories to strike them with hammers and analyze their physical properties. Disregard the accomplishments of the inferential sciences—astronomy, for example, which gets on just fine without bringing actual planets, stars, galaxies and black holes into its labs and striking them with hammers.
• Practice debunkery-by-association. Lump together all phenomena popularly deemed paranormal and suggest that their proponents and researchers speak with a single voice. In this way you can indiscriminately drag material across disciplinary lines or from one case to another to support your views as needed. For example, if a claim having some superficial similarity to the one at hand has been (or is popularly assumed to have been) exposed as fraudulent, cite it as if it were an appropriate example. Then put on a gloating smile, lean back in your armchair and just say, "I rest my case."
• Use the word "imagination" as an epithet that applies only to seeing what's *not* there, and not to denying what *is* there.
• If a significant number of people agree that they have observed something that violates the consensus reality, simply ascribe it to "mass hallucination." Avoid addressing the possibility that the consensus reality might itself constitute a mass hallucination.
• Ridicule, ridicule, ridicule. It is far and away the single most chillingly effective weapon in the war against discovery and innovation. Ridicule has the unique power to make people of virtually any persuasion go completely unconscious in a twinkling. It fails to sway only those few who are of sufficiently independent mind not to buy into the kind of emotional consensus that ridicule provides.
• By appropriate innuendo and example, imply that ridicule constitutes an essential feature of the scientific method that can raise the level of objectivity and dispassionateness with which any investigation is conducted.
• If pressed about your novel interpretations of the scientific method, declare that "intellectual integrity is a subtle issue."
• Imply that investigators of the unorthodox are zealots. Suggest that in order to investigate the existence of something one must first believe in it absolutely. Then demand that all such "true believers" know all the answers to their most puzzling questions in complete detail ahead of time. Convince people of your own sincerity by reassuring them that you yourself would "love to believe in these fantastic phenomena." Carefully sidestep the fact that science is not about believing or disbelieving, but about finding out.
• Use "smoke and mirrors" (*i.e.,* obfuscation and illusion). Never forget that a slippery mixture of fact, opinion, innuendo, out-of-context information and outright lies will fool most of the people most of the time. As little as one part fact to ten parts B.S. will usually do the trick. (Some veteran debunkers use homeopathic dilutions of fact with remarkable success!) Cultivate the art of slipping back and forth between fact and fiction so undetectably that the flimsiest foundation of truth will always appear to firmly support your entire edifice of opinion.
• Employ "TCP": Technically Correct Pseudo-refutation. Example: if someone remarks that all great truths began as blasphemies, respond immediately that not all blasphemies have become great truths. Because your response was technically correct, no one will notice that it did not really refute the original remark.
• Trivialize the case by trivializing the entire field in question. Characterize the study of orthodox phenomena as deep and time-consuming, while deeming that of unorthodox phenomena so insubstantial as to demand nothing more than a scan of the tabloids. If pressed on this, simply say, "But there's nothing there to study!" Characterize any serious investigator of the unorthodox as a "buff" or "freak," or as "self-styled"—the media's favorite code-word for "bogus."
• Remember that most people do not have sufficient time or expertise for careful discrimination, and tend to accept or

reject the whole of an unfamiliar situation. So discredit the whole story by attempting to discredit *part* of the story. Here's how: a) take one element of a case completely out of context; b) find something prosaic that hypothetically could explain it; c) declare that, therefore, that one element has been explained; d) call a press conference and announce to the world that the entire case has been explained!

• Engage the services of a professional stage magician who can mimic the phenomenon in question (*i.e.,* ESP, psychokinesis, or levitation). This will convince the public that the original claimants or witnesses to such phenomena must themselves have been (or been fooled by) talented stage magicians who hoaxed the original phenomenon in precisely the same way.

• Find a prosaic phenomenon that, to the uninitiated, resembles the claimed phenomenon. Then suggest that the existence of the commonplace look-alike somehow forbids the existence of the genuine article. For example, imply that since people often see "faces" in rocks and clouds, the enigmatic Face on Mars must be a similar illusion and therefore cannot possibly be artificial.

• When an unexplained phenomenon demonstrates evidence of intelligence (as in the case of the mysterious crop circles) focus exclusively on the mechanism that might have been wielded by the intelligence rather than the intelligence that might have wielded the mechanism. The more attention you devote to the mechanism, the more easily you can distract people from considering the possibility of non-ordinary intelligence.

• Accuse investigators of unusual phenomena of believing in "invisible forces and extrasensory realities." If they should point out that the physical sciences have *always* dealt with invisible forces and extrasensory realities (gravity? electromagnetism?. . .) respond with a condescending chuckle that this is "a naive interpretation of the facts."

• Insist that western science is completely objective, and is based on no untestable assumptions, covert beliefs or ideological interests. If an unfamiliar or inexplicable phenomenon happens to be considered true and/or useful by a nonwestern or other traditional society, you may dismiss it out of hand as "ignorant misconception," "medieval superstition" or "fairy lore."

• Label any poorly-understood phenomenon "occult," "fringe," "paranormal," "metaphysical," "mystical," "supernatural," or "new-age." This will get most mainstream scientists off the case immediately on purely emotional grounds. If you're lucky, this may delay any responsible investigation of such phenomena by decades or even centuries!

• Ask questions that appear to contain generally-assumed knowledge that supports your views; for example, "Why do no police officers, military pilots, air traffic controllers or psychiatrists report UFOs?" (If someone points out that they do, insist that those who do must be mentally unstable.)

• Ask unanswerable questions based on arbitrary criteria of proof. For example, "If this claim were true, why haven't we seen it on TV?" or "in this or that scientific journal?" Never forget the mother of all such questions: "If UFOs are extraterrestrial, why haven't they landed on the White House lawn?"

• Similarly, reinforce the popular fiction that our scientific knowledge is complete and finished. Do this by asserting that "if such-and-such were true, we would already know about it!"

• Remember that you can easily appear to refute anyone's claims by building "straw men" to demolish. One way to do this is to misquote them while preserving that convincing grain of truth—for example, by acting as if they have intended the extreme of any position they've taken. Another effective strategy with a long history of success is simply to misreplicate their experiments—or to avoid replicating them at all on grounds that "to do so would be ridiculous or fruitless." To make the whole process even easier, respond not to their actual claims but to their claims as reported by the media, or as propagated in popular myth.

• Insist that such-and-such unorthodox claim is not scientifically testable because no self-respecting grantmaking organization would fund such ridiculous tests.

• Be selective. For example, if an unorthodox healing practice has failed to reverse a case of terminal illness you may deem it worthless—while taking care to avoid mentioning any of the shortcomings of conventional medicine.

• Hold claimants responsible for the production values and editorial policies of any media or press that reports their claim. If an unusual or inexplicable event is reported in a sensationalized manner, hold this as proof that the event itself must have been without substance or worth.

• When a witness or claimant states something in a manner that is scientifically imperfect, treat this as if it were not scientific at all. If the claimant is not a credentialed scientist, argue that his or her perceptions cannot possibly be objective.

• If you're unable to attack the facts of the case, attack the participants—or the journalists who reported the case. *Ad-hominem* arguments, or personality attacks, are among the most powerful ways of swaying the public and avoiding the issue. For example, if investigators of the unorthodox have profited financially from activities connected with their research, accuse them of "profiting financially from activities connected with their research!" If their research, publishing, speaking tours, and so forth constitute their normal line of work or sole means of support, hold that fact as "conclusive proof that income is being realized from such activities!" If they have labored to achieve public recognition for their work, you may safely characterize them as "publicity seekers."

• Fabricate supportive expertise as needed by quoting the opinions of those in fields popularly assumed to include the necessary knowledge. Astronomers, for example, may be trotted out as experts on the UFO question, although course credits in ufology have never been a prerequisite for a degree in astronomy.

• Fabricate confessions. If a phenomenon stubbornly refuses to go away, set up a couple of colorful old geezers to claim they hoaxed it. The press and the public will always tend to view confessions as sincerely motivated, and will promptly abandon their critical faculties. After all, nobody wants to appear to lack compassion for self-confessed sinners.

• Fabricate sources of disinformation. Claim that you've "found the person who started the rumor that such a phenomenon exists!"

• Fabricate entire research projects. Declare that "these claims have been thoroughly discredited by the top experts in the field!" Do this whether or not such experts have ever actually studied the claims, or, for that matter, even exist.

Part 2: Debunking Extraterrestrial Intelligence

• Point out that an "unidentified" flying object is just that, and cannot be automatically assumed to be extraterrestrial. Do this whether or not anyone involved *has* assumed it to be extraterrestrial.

• Equate nature's laws with our current understanding of nature's laws. Then label all concepts such as antigravity or

interdimensional mobility as mere flights of fancy "because what present-day science cannot explain cannot possibly exist." Then if an anomalous craft is reported to have hovered silently, made right-angle turns at supersonic speeds or appeared and disappeared instantly, you may summarily dismiss the report.

• Declare that there is no proof that life can exist in outer space. Since most people still behave as if the Earth were the center of the universe, you may safely ignore the fact that Earth, which is already in outer space, has abundant life.

• Point out that the official SETI program assumes in advance that extraterrestrial intelligence can only exist light-years away from Earth. Equate this *a-priori* assumption with conclusive proof; then insist that this invalidates all terrestrial reports of ET contact.

• If compelling evidence is presented for a UFO crash or some similar event, provide thousands of pages of detailed information about a formerly secret military project that might conceivably account for it. The more voluminous the information, the less need to demonstrate any actual connection between the reported event and the military project.

• When someone produces purported physical evidence of alien technology, point out that no analysis can prove that its origin was extraterrestrial; after all, it might be the product of some perfectly ordinary, ultra-secret underground government lab. The only exception would be evidence obtained from a landing on the White House lawn—the sole circumstance universally agreed upon by generations of skeptics as conclusively certifying extraterrestrial origin!

• If photographs or other visual media depicting anomalous aerial phenomena have been presented, argue that since images can now be digitally manipulated they prove nothing. Assert this regardless of the vintage of the material or the circumstances of its acquisition. Insist that the better the quality of a UFO photo, the greater the likelihood of fraud. Photos that have passed every known test may therefore be held to be the most perfectly fraudulent of all!

• Argue that all reports of humanoid extraterrestrials must be bogus because the evolution of the humanoid form on Earth is the result of an infinite number of accidents in a genetically isolated environment. Avoid addressing the logical proposition that if interstellar visitations have occurred, Earth cannot be considered genetically isolated in the first place.

• Argue that extraterrestrials would or wouldn't, should or shouldn't, can or can't behave in certain ways because such behavior would or wouldn't be logical. Base your notions of logic on how terrestrials would or wouldn't behave. Since terrestrials behave in all kinds of ways you can theorize whatever kind of behavior suits your arguments.

• Stereotype contact claims according to simplistic scenarios already well-established in the collective imagination. If a reported ET contact appears to have had no negative consequences, sarcastically accuse the claimant of believing devoutly that "benevolent ETs have come to magically save us from destroying ourselves!" If someone claims to have been traumatized by an alien contact, brush it aside as "a classic case of hysteria." If contactees stress the essential humanness and limitations of certain ETs they claim to have met, ask, "Why haven't these omnipotent beings offered to solve all our problems for us?"

• When reluctant encounter witnesses step forward, accuse them indiscriminately of "seeking the limelight" with their outlandish stories.

• Ask why alleged contactees and abductees haven't received alien infections. Reject as "preposterous" all medical evidence suggesting that such may in fact have occurred. Categorize as "pure science-fiction" the notion that alien understandings of immunology might be in advance of our own, or that sufficiently alien microorganisms might be limited in their ability to interact with our biological systems. Above all, dismiss anything that might result in an actual investigation of the matter.

• Travel to China. Upon your return, report that "nobody there told me they had seen any UFOs." Insist that this proves that no UFOs are reported outside countries whose populations are overexposed to science fiction.

• Where hypnotic regression has yielded consistent contactee testimony in widespread and completely independent cases, argue that hypnosis is probably unreliable, and is always worthless in the hands of non-credentialed practitioners. Be sure to add that the subjects must have been steeped in the ET-contact literature, and that, whatever their credentials, the hypnotists involved must have been asking leading questions.

• If someone claims to have been emotionally impacted by a contact experience, point out that strong emotions can alter perceptions. Therefore, the claimant's recollections must be entirely untrustworthy.

• Maintain that there cannot possibly be a government cover-up of the ET question. . . but that it exists for legitimate reasons of national security!

• Accuse conspiracy theorists of being conspiracy theorists and of believing in conspiracies! Insist that only *accidentalist* theories can possibly account for repeated, organized patterns of suppression, denial, and disinformational activity.

• In the event of a worst-case scenario—for example, one in which extraterrestrial intelligence is suddenly acknowledged as a global mystery of millennial proportions—just remember that the public has a short memory. Simply hail this as a "victory for the scientific method" and say dismissively, "Well, everyone knows this is a monumentally significant issue. As a matter of fact, my colleagues and I have been remarking on it for years!"

An Interview with Prof. Martin Fleischmann

Conducted by Christopher Tinsley, February 8, 1997

Christopher Tinsley (T): Now that you are retired from IMRA, what do you intend to do? Are you really retired?

Martin Fleischmann (F): I don't suppose I'll ever retire completely. I retired from full-time work at the University of Southampton when I was aged 56, but I didn't "retire." I started a number of part-time projects and, eventually, of course went full-time to IMRA Europe. At the moment I am taking a very careful look at some of the work which we have done in the past. It has been suggested at various times that I should start an operation in the United Kingdom but—bearing in mind my age and medical history—I think this would be not a very sensible way to go forward. So I am now interacting strongly with a group in Italy. I anticipate that we will take a very careful look at what we and other people have done during the past eight years and move on eventually to try to implement some of the work which I have wanted to carry out in the past.

T: You've been giving some assistance to Mr. Evan Ragland with regard to his cell. This cell is of course the one which our magazine is hoping to provide to people as a demonstration device of the basic thermal effect.

F: I think my interaction with Evan Ragland will be principally concerned with the form of the electrodes. I have had this view of the optimization of the electrode design for a long time. Historically we went through various phases in the work and eventually worked on large sheets—very large sheets—of palladium. That work was stopped in March 1988 because of concerns about the safety of the device. At that stage we switched to using rods, which, as everybody knows, we have used because we felt it was very important to be able to reduce the scale rather than to increase it again because of our concerns about safety.

T: Are you thinking here of mechanical safety in the sense of the famous "centimeter cube of palladium" incident?

F: Yes. That was always a big factor. You know, as the work moved forward, it included the work on this cube which disintegrated—unfortunately unobserved, because it happened at night.

Stanley Pons and Martin Fleischmann

T: Perhaps, very fortunately it was not watched under the circumstances?

F: Perhaps fortunately—yes. After that we moved to using sheets under very mild conditions. We tried to reduce the scale of the phenomena. Incidentally, as we were discussing earlier, this included unexplained increases in the temperature of the cell. In March 1988, we decided that we had to take further steps to scale-down the experiments. . .There is a famous diagram which has Stan Pons' and my writing all over it, about these unexplained rises in temperature of the cell. As it happened, I was just recovering from an operation here in the UK. At that stage, we decided that this line of work had to stop and we switched to the rods. However, rods are not satisfactory mechanically because there is a stress concentration in the centre, so it is obviously better to use something like a continuous sheet. That's why I believe that we should now look at tubes.

T: Perhaps with one anode down the centre and another anode as a coil around the outside, so that you make a triode arrangement in that way?

F: Indeed. I think my interaction with Evan Ragland will be along that line.

T: In the matter of the centimeter cube of palladium, the solid block, would you say that the disintegration incident had some effect on you in the way of being a stimulus to your continuing the work?

F: Indeed, yes. It was our incentive to continue with the work but, at the same time, it was a one-off, so you can't really say anything definitive.

T: You don't want to do it again.

F: No, I don't want to do it again. You can specify various things which might have caused it. If you assume that it was a valid experiment, then its disintegration reveals a very substantial part of what has been found since then, including the fact that you can get heat generation at high temperature.

T: You're suggesting it was a thermal runaway of sorts.

F: Yes, you can see that even with a relatively modest enthalpy output and a uniform generation of excess heat in the volume, you would get rather strange conditions in the centre of the cube.

T: Rather like a haystack spontaneously combusting?

F: Yes, it is like that.

T: That although the process producing heat is at a comparatively gentle level, if you do that inside a haystack. . .

F: It'll catch fire. Yes. You can do the calculation on the back of an envelope to show you that this will happen, that it could melt in the middle. It's just strange that people haven't done this. . .you know that people say "pooh, pooh, pooh, it can't possibly be," so the discussion never gets going. . .So if I could just go back now to something which I am sure we should cover here regarding our original scenario: we have, in fact, four ways—four major potential

lines of research. The first was the topic electrodiffusion, I'm sure we shall cover that at some length; the second one was electrochemical charging; the third one was a collection of experiments which really bridged the topics of hot fusion and cold fusion. Interestingly enough—no-one has ever asked us about that, they are not in the least bit interested.

T: They perhaps haven't had the opportunity to ask that?

F: Well, what is so interesting is that no one has asked.

T: Well we are interested.

F: The fourth one was another set of experiments which I may do with my friends in Italy. So there were four distinct lines and, of course, we became committed to electrochemical charging although our real intention was always to work on electrodiffusion. A discursive answer to the first question.

T: We're just a few months on from the ICCF6 Conference. What do you anticipate will happen in the field in the course of the future from now on?

F: It is very difficult to say. I've always said there is the "seven year barrier." Yes, we've passed that. Usually, if you have a new idea, you very rarely break through to anything like recognizable development or implementation of that idea the first time around—it takes two or three goes for the research community to return to the topic. So I thought it would probably all peter out in '96 if we didn't break through, but I don't think it has done that. I think this is one of those situations where although people think it is crazy, the value is so high that they will continue with it. If you think about the meeting in Japan, what was revealed was that if you do the experiment correctly—especially with the correct materials, than you will make successful observations. As regards the materials aspects, I'm very keen on Johnson-Matthey material Type "A" or something which looks like Johnson-Matthey material Type "A." If you use that, you will find it relatively easy to to reproduce the findings in a reasonably short space of time. However, I think that the meeting revealed that there are several research groups entering the field who are doing this. I think that the real success will come from the next phase, which will include experiments in electrodiffusion or combinations of electrochemical charging and electrodiffusion.

T: We are seeing a considerable increase of interest in this whole general area—even in recent months there has been a considerable shift. And yet, of course, Max Planck set his "constant" at twenty years for new ideas to penetrate.

F: Did he? Well, he said that all the opposition has to die out, didn't he?

T: He said that science proceeds by funerals.

F: Yes, yes. There is a lot of truth in that.

T: And yet in cold fusion it's really not been the "young Turks" that have been coming in. . .

F: . . .it's the "old Turks."

T: Exactly.

F: I think that we were starting to talk about that earlier. I think this was a subject for older people who were not afraid to. . .who didn't care about their scientific reputation.

T: But perhaps in the past there have been periods where people have been able to do science without having to worry about their reputations?

F: That's gone now.

T: Perhaps it will come back.

F: Maybe it will come back. I think that at sometime we will want to talk about the general malaise of science.

T: John Bockris has suggested that science had become very rigidified in around 1972. Do you have any comment on that at all?

F: I think there was a very unfortunate development in the 70's, a sort of "anti-Francis Bacon development." People developed a view that a subject is not respectable unless it is dressed up with a suitable overload of theory, and consequently we have had this "top dressing" of theory put on the subject which has tended to make the approach very rigid. Also, the theories are of course written in terms of rather old-fashioned ideas.

T: But we have been seeing a shift in general public attitudes.

F: To science?

T: No. Specifically towards things like towards cold fusion. This may be a kind of pre-millennial tension or something of that kind, but we are finding that companies and individuals are taking the whole field of cold fusion very much more seriously and positively than they were doing even months ago.

F: I think that's probably true.

T: It's a strange thing.

F: I don't think so. I think that that it is a question of economics, I don't know whether you have done your calculations but, about two or three years back, I did a first assessment of what the first successful device would be worth and it came out at about 300 trillion dollars. So, at that sort of value, people are prepared to take a rather high risk on the research. You know, for a long time people have always had a list of the first ten projects. I don't think you should over-emphasize the value of cold fusion necessarily, but if you make your list of the ten most valuable projects, high temperature superconductors will always be on the list; fuel cells will always be on the list. It doesn't matter whether you can or cannot achieve high temperature superconductivity or fuel cells, they will always be on the list because if you could achieve them they would be extremely valuable. So these ideas will keep on coming up. Now, of course, cold fusion is the daddy of them all in a way, in terms of value, so I think that viewed in a social way, from the point of social considerations and economics, it will tell you that this thing will stay around.

T: Do you think that physics and chemistry took something of a wrong turning at some point in the last 150 years or so and started to perhaps head into something of a blind alley? That what we now are seeing—perhaps with cold fusion, and so forth—is that mistakes have been made? We have something that doesn't appear to fit comfortably.

F: I don't think so, you see, I am a very conventional scientist, really. Extremely. I always explain that—I'm really very conventional. We arrived at this topic from various inputs to the subject and, in the end, we could pose a very simple question, namely would the fusion cross-section of deuterons compressed in a palladium lattice be different to the cross-section which you see in the vacuum. Now, I think that was a very simple question—either yes or no. The answer turned out to be different. . .I should explain that what we said was, "Yes, it would be different, but we would still see nothing." That was the starting point in 1983 or whatever, yes 1982-83. Of course, it would be different, but we will see nothing. But it turned out to be radically different to that. Now, of course,

you have to say, "What do we do with such an observation?" Many people—as was shown subsequently—even though they were told what had happened, couldn't believe this and ignored their own experimental evidence. But that is not for us. . .As for taking a wrong turning—well it has in an organizational way. I always say that if you recall Leonardo da Vinci and Michelangelo holding a painting competition in the Town Hall in Florence during the Renaissance then you cannot conceive of that happening in the present age. The early development of science was really a dilettante type of aristocratic preoccupation. . .

T: Lavoisier and company?

F: Yes. You cannot imagine that somebody would now give a latter-day Faraday carte blanche to investigate the interaction of forces.

T: Mind you, for what he cost at the time, we could really afford it. It wasn't that expensive.

F: Nor is cold fusion expensive. One of my theme songs is that if you can't do it in a test tube, don't do it. It is not necessarily true that expensive experiments are not worthwhile doing but there are plenty of rather cheap experiments which are certainly worth doing. So if you haven't got the resources, do think a bit and try the cheap experiments. So has science taken a wrong turning? Well, this is one instance where it has taken a wrong turning, but, of course, there is also this whole overlay of Copenhagen-style quantum mechanics which we have not been able to shake off.

T: You feel that was a wrong turning?

F: Oh, that was a massive wrong turning. Massive wrong-turning, although we have to give credit to Niels Bohr and the Copenhagen school, for a great deal of valuable development of theory. However, that approach should have been abandoned a long time ago. The problem is that replacement of Quantum Mechanics by Quantum Field Theory is still very demanding.

T: Now, how about the difference between, in cold fusion, but perhaps in science generally, the way things are done in Japan and in, for example, the United States? There are obviously significant cultural differences between the countries and this runs into the way they work in every field. A World War II Japanese battleship can't help but look Japanese. Perhaps you could include the UK as examples. How would you characterize the differences?

F: Yes. That's an enormous collection of questions, it's not just one question. I just had an ex-student of mine here, who is now an academic in Coventry. He has a very interesting collection of post-graduate students working on a range of topics. One of these led us to discuss globalization in the context of the difference between Christianity and Islam, and I said, "Well, this is not the question. I think Islam and Christianity can be reconciled but Shinto and Buddha on the one side and Islam and Christianity on the other, that is a much bigger problem." The cultural difference between the Pacific rim and the Greco-Judaeo tradition is going to be a much bigger problem for the world. And, of course, I think that it is very difficult for people to lock into science if they haven't got the Hellenistic tradition.

T: But the Japanese are notoriously fine co-operators. . .

F: Yes, they are very good at retro-science for example, where teamwork is very important, but I don't think their system lends itself to innovative research. I think that many senior people in Japan, who are now unfortunately dying out, realize that Japan will have to take a step towards innovative science, they cannot go on using innovative ideas developed in other countries and develop them themselves. Incidentally, this is one of the problems with the development of cold fusion—they went into it too soon. I think they have a very important role to fulfill, but by stepping in too soon—before the boundaries of the subject had been defined—then this was going to create a great deal of difficulty. So I think that as science is organized in Japan at the moment it will not make a great deal of headway in innovative science. That's my own opinion.

T: But, in Japan, is it not also true to say that they hold in very high esteem persons such as yourself—a Fellow of the Royal Society?

F: Outsiders. A prophet is not recognized in his own land.

T: "A prophet is not without honor save in his own country." But is it not generally true that the Japanese have particularly strong respect for high-powered academics from outside Japan?

F: Yes. But this is because they don't recognize their own prophets. Because they don't fit into the system.

T: But then neither do we. That's a universal problem.

F: Well this has now come upon us. I think this was not true—especially if you take the United Kingdom—this was not true in the past. I mean prophets in other endeavors—politics or the social sciences—might not have been recognized, but in science, prophets were recognized in the United Kingdom.

T: Would that explain the disproportionate role that British science has played?

F: Well yes. I think you know that I classify science as British science, American science, and everybody else. British science has a certain style and, of course, my problem is that, although I was born in Czechoslovakia, I am the archetypal British scientist.

T: You are indeed.

F: I am. I am a caricature of what British science is about in the way I work. American science is much more organized, much more hierarchical than British science has been. I think British science is becoming more like American science—and then there is everybody else, I'm afraid. Is it not true that 55% of R&D, *i.e.* innovative science, since the War been done in the USA and Britain.

T: So, it is extraordinary. . .

F: It is extraordinary and now, unfortunately, we have found ourselves in the position where I think some decisions have been taken by the mandarins in Whitehall that Britain should become a "super Belgium." The fact that we have not been able to exploit our ideas is taken as an indication that we should not do innovative science. When in fact, of course, what has been wrong is that we have not exploited our ideas. Removing the ideas is not going to do us any good whatsoever.

T: That's certainly a fascinating view. You say that science is a highly organized endeavor in the United States, but surely a great deal of innovative and exciting work has been done in the United States as well.

F: However, the cost is very high. It is not a very effective system, though they could afford it, or historically, they could afford it but the cost/benefit analysis of science in America is not very good.

T: Yes, I've always been entertained by chauvinism in science, for example, in this country we have Crick & Watson and in the United States we have Watson & Crick. There's an extraordinary and highly inappropriate chauvinism, is there not, in science or would you say that's only in the public perception?

F: It's in the public domain. I don't think scientists themselves do

that. Scientists are really very conscious of the fact that they stand on the shoulders of an enormous tree of preceding workers and that their own contribution is not so enormous. What I've always said about cold fusion is that "everything I can say about cold fusion can be condensed onto about half a page now and I will know the subject has arrived when it is a footnote." When there is a lot of verbiage then you know you haven't arrived.

T: Is this your comment about from simplicity through complexity back to simplicity again?

F: Well that is part of it, yes, it is a little bit of it. You have to in the end, distill out that which is simple, to think about and re-investigate that which is simple to do.

T: Yes, that's very interesting. Arthur C. Clarke once had a character in a novel comment that the French make the best second-raters at everything in the world.

F: But that's their objective. It's a conscious decision. Historically they have been very good at mathematics, and occasionally you get a peak like Pasteur and they recognize the peak. I think you could hardly ignore Pasteur, but basically the French system also doesn't lend itself to innovative research.

T: And Russia?

F: Well, the Russians have been extremely innovative considering their resource base. So how one should analyze this, why the Russians were so successful? It's a good question.

T: Perhaps they have been in a continually post-diluvial state.

F: Probably yes, I think they could only escape from the system via some sort of profession. You had to hide within your profession. You know, you had to become immune from the political pressures.

T: If you became a Sakharov no one could touch you seriously, though they tried.

F: But if you even go lower down the scale, scientists were left alone, so the clever people who could make it into science hoofed it and made it into science as fast as they could.

T: To return to cold fusion: if you had to do it over again, would you have participated in that Press Conference in 1989?

F: Here again, is an enormous collection of questions. Of course, I was opposed to it as you probably know and I tried to stop it—even the night before—and I failed because there was a key person I needed to contact. . .Stan and I funded the first phase of the work ourselves. It was secret. We reckoned we would get our first answers for about $100,000, which was as much as we could afford to spend. In the Summer of 1988 we reckoned that we would need $600,000 to complete the first phase in about September 1990. We planned to review the question of publication in September 1990. We had at that time, and continued to have all the way through, tremendous hang-ups about whether this work should be published at all. In fact, in '88 we went through several discussions about whether the work should be classified.

T: For reasons of?

F: National security. However, in '88 we had the twin problem that we certainly did not have $600,000 between the two of us to spend on progressing this research properly, and we needed the $600,000. We also had to inform the American Department of Energy in the States, and I had to inform Harwell [Laboratory in the UK—*Ed.*] about this work. So I said let's kill many birds with one stone: let's write a Research Application rather than a patent—which we submitted to the DOE. Initially, it didn't go to the DOE, but it finished up at the DOE in August 1988 and that, of course, brought us into this conflict situation with another scientist who was interested in the subject, who had been interested in the subject previously. He had not done the experiment in a way in which he could possibly have succeeded, mainly because he had used 10% D_2O in H_2O and, of course, he would have had hardly any deuterium in the lattice—and he started to work on this topic again. . .There is nothing wrong with that incidentally, people object to that, but I don't object to that at all. I think that he should have disclosed his intention to restart his work when he refereed our proposal. What was hard for Stan and me was that he wanted to disclose his results. Now Stan and I were still working in secret at that time but, because of this development, we had to inform the University of Utah because we thought that they might need to take patent protection. They said yes, so then the patent became the driving force. And it was the patent consideration which produced the Press Conference, the "prior claim." I was not in favor of that at all, but it was that which produced it. Of course, you might ask if we would have done it any other way. Well, I wished we had carried on for twenty years in a mild way and I wish I had started it in 1972 and done it all myself, quietly and over a long period of time. . .I think the Press Conference was a mistake. But it was inevitable.

T: Can you, looking back, see any alternative to what happened?

F: No.

T: You would have been stuck with the same situation?

F: Given the situation we had and given the results we had, we had to tell the DOE and Harwell. Given the conflict situation which developed we had to tell the University. Given the results we had, the University had to take a patent. It was inconceivable that Chase Peterson and Jim Brophy would have said, "No, we won't take a patent." The only thing which would have changed that would have been the existence of an Ethics Committee to whom it could have been submitted—a National Ethics Committee would have said this is not the sort of science or development which justifies taking a patent, forget the patent, no press conference, no nothing, it would have been OK. But, given the situation in which the Universities found themselves, I think it was inevitable—and it would happen again, and in other fields it will certainly happen again.

T: I for one see no clear objection to what people dismiss as science by press conference. After all, the hot fusion boys do it all the time.

F: I think it is worthwhile to recall Zeta. Zeta [A supposed hot fusion achievement.—*Ed.*] was announced by the Postmaster General in the House of Commons. What can be more outrageous than that?

T: Quite. But I was thinking that, for example, I would very much have welcomed a Press Conference by the French nuclear research people, CEREM, on their full replication of your "boil off" experiment. I'm sure you would have done as well.

F: Hmm. . .hole in the corner.

T: In other words, if you've got it you flaunt it. Did you notice that any mention of the CEREM replication is totally absent from Douglas Morrison's account of ICCF 6?

F: No, I haven't looked at that. But can you imagine something which has been so systematically ignored as that announcement?

T: Surely, but was it announced publicly.

F: Well, Biberian presented this work.

T: Ah, but only at this Conference. Where else has it been announced?

F: I think what is going to happen is that a lot of this work will disappear behind closed doors.

T: For what reason?

F: Three hundred trillion dollars.

T: The energy business?

F: I mean, other reasons as well, just take that as a. . .

T: But you are thinking, in this instance, of the energy implications?

F: Yes. But there are other implications as well. But let's just confine our attention to energy.

T: Quite. You recall the famous sequence of events at MIT, and Mitchell Swartz and Gene Mallove's discussion of that on the BBC/CBC documentary. Have you any comments to make on that particular series of incidents?

F: It was certainly very extraordinary. There were three, possibly you could say there were four, investigations in 1989 that we should have taken notice of. One was the MIT investigation, another one was Lewis at Caltech, the third one was in Harwell, and possibly we should take note of Kreysa and his colleagues in Germany. I think the last is a minor thing—a fairly ridiculous investigation. I think the only half-way reasonable investigation was the one in Harwell, that experiment was well designed but badly executed and, of course, totally misinterpreted. . .However, to their great credit, they made the data sets available for study. This is Harwell.

T: Of course, MIT did that too in a sense, eventually.

F: In a sense, but see what happened. If you take the Harwell data sets, you cannot say that this experiment worked perfectly and that there is no excess heat. You could only say either that the experiment worked perfectly and there is excess heat, or the experiment didn't. And on those two bases you have to do another set of experiments. As regards MIT, all one can do is shake one's head in disbelief really. I mean, again, if you fiddle about with baselines then you have to consign those experiments to the dustbin and start again. The one in Caltech was clearly very strange because there was a redefinition of the heat transfer coefficient. . .I had actually thought of dropping out of this field in '91 and just waiting to see what other people would make of it in order to go back into it in '93 myself, but I was persuaded to go to France.

T: Just one moment, to track back, you were talking about the Caltech experiment—you said something very strange happened and there was a redefinition of the heat transfer coefficient.

F: The heat transfer coefficient. I'd have to refresh my memory, but my own view was that it was much more plausible to re-interpret the MIT and Harwell and Caltech results in terms of the generation of excess heat.

T: Yes, but that experiment was discussed in a paper in the *Journal of Physical Chemistry* some while later, was it not? Did you see the paper there which largely refuted the Caltech experiment and showed there had been excess heat? That was one of Miles' papers.

F: Was it? Well, yes, you know it's not very difficult to show that you get excess heat if you use the right material. Of course, it's a materials science question. If your electrodes crack you will not load them electrochemically. You can load them some other way but not electrochemically.

T: This is all connected with the same period that you were effectively accused of fraud by Parker of MIT. What were your feelings about this?

F: It's insane, really. But Parker had an axe to grind and Parker tried to deny he was the lead author of the paper. I think it was Mitchell Swartz who caught up with him mainly, and Gene Mallove, then Parker somewhere said, "I don't know, nothing has happened," and then someone said, "But you are the author, the lead author!"

T: In fact, the BBC documentary showed Parker in a very poor light.

F: So who called me a fraud?

T: Who indeed?

F: I shouldn't say fraud. Fraud is not an acceptable word, but who created a deception.

T: "Inappropriately interpreted the data."

F: Who inappropriately interpreted the data. Which is very common incidentally, I was recently writing to my Japanese colleagues about misinterpretation of data. Science is full of misinterpretation of data. Because data interpretation doesn't hold a very high priority in science, it is driven by the Research Student Syndrome: "Let me get all these results now and I will interpret them next year." Next year, of course, never comes.

T: One of the first things which convinced me to study the field very much more carefully in the early days was Professor Close's book on the subject. It seemed to me he was tying himself into logical knots to try and explain the results away. I felt that there must be something in it for a man of that calibre to have to go so low. Such comments of his that when heavy water was later found to be contaminated with ordinary water, that this showed that somebody had been tampering with the experiment—and I can't believe a nuclear physicist is unaware that heavy water is hygroscopic.

F: Well, contamination is a big problem, you know. I think this is a very interesting point. If you have a very low level of contamination by light water you will certainly destroy the effects due to deuterium.

T: He was explaining the presence of tritium by saying that it must have been contaminated by tritiated water, because there was tritiated water available and the presence of ordinary water in the heavy water proved that somebody had been contaminating it. In fact it proves nothing of the kind.

F: This thing about the tritium was very interesting for us, because this was something we never wrote up properly at the time, and we have never returned to it because we have got certain hang-ups about this aspect. But, to explain our results with tritium, we would have had to have an isotropic separation factor between deuterium and tritium, this is the ratio in the gas to the liquid phase at about thirteen and a half. You can't get that. There's no way we could have got that much tritium by isotropic separation. So it had to be generated, you see, and other people have found that since. . .But Close, I don't know, I can't understand Close. Frank Close came to see me. I had to return in February 1990 to Salt Lake City, and he wrote to me saying that he wanted to come and talk to me because he had been in Oak Ridge and he had seen the results in Harwell which were negative, and he'd seen the results in Oak Ridge which were positive. There were two groups in Oak Ridge who had positive results at the time that was

Hutchinson and Scott, but there was a third group which had positive results in Oak Ridge and I had just finished with all this calorimetry here. So I said, "Come, I'll discuss it with you." And he came here and it transpired that he really wanted to talk about the gamma rays. I said, "I haven't got those data here, come to Salt Lake City because we've done a new set of measurements there on gamma ray generation," and he never came. He came to Salt Lake but he didn't come and talk to us, so he never had access to all the stuff which is in *Il Nuovo Cimento*. So that's my knowledge of Frank Close.

T: That leads onto another question. Do you feel that there is any further clarification you can give in your answer to the accusations from Close and others about your supposedly unethical shifting of the gamma ray spectrum?

F: There is a whole set of files upstairs and you are welcome to have them in due course. I think that the point about that was that I went to Harwell when I came back from the States, and I used the diagram which I had prepared in February '89 from data which had been given to me by my collaborators. There was something obviously wrong with those measurements. I went from Harwell to Switzerland and I asked for the final version of the diagrams to be sent to me in Switzerland. . .So it's one of these unfortunate things. You can't really say what happened, but the diagram I used in Harwell was a preliminary diagram and when David Williams asked me whether he could have these diagrams and I said, "Yes, if they are for your own study, please don't distribute them; for your own study you can have them." These are the diagrams which Frank Close then got, which he shouldn't have got. I would certainly not have vetoed his use, but I would have wanted to add a word of explanation about how they had arisen. . .There is one important issue here. By March 1989 we had decided that these measurements had to be done with a high resolution Ge-detector, not the low resolution Na-I detector. The results of these measurements were available in Salt Lake City in February 1990. Subsequently, I tried to get these transparencies back to see how they might be related to the material in Frank Close's book. I was told by various people in Harwell that they had been lost. So Frank Close got them and Harwell lost them. The whole thing looks rather doubtful to me. . .In other words, I don't take kindly to being accused of unethical doings by people who clearly have been involved in unethical activities themselves.

T: Speaking of people being accused, what do you feel about John Bockris and his various problems—like horse manure in his letterbox?

F: I didn't know that. Did he have horse manure in his. . .

T: Yes, recently.

F: Really.

T: Well you know he held a conference on low-energy transmutations and had to hold it off-campus. I just wondered if you had any comments, because Professor Bockris is a fine, forceful old gentleman, is he not?

F: Well, he's another one who doesn't care about his reputation. Well, he does, but not to the extent where he would let it cloud his judgment.

T: Yes, that's a very interesting point—the matter of reputation. If one was looking for the world's most highly regarded electrochemists at the time one would have to include yourself and Professor Bockris in a very short list. This is interesting—that both of you have been perfectly happy to take such a stance, rather than resting on your laurels.

F: I think I must interject something here. People said, "Why would you do it?" We can come back to that, but I said in reply to them, "Well it is not clear that it should have been me, but I think it would have been very likely that it would have been an electrochemist who would have done this research." Because of the nature of the subject you see.

T: What is the "nature" of electrochemistry, then?

F: Well, it is the interaction of physical chemistry and theory. You know, it is the combination of knowledge. Your knowledge base which would make you pose the question, "Is it not possible to induce anomalous nuclear reactions from deuterium in palladium?"

T: So, you would say that an electrochemist is rather like someone standing where three countries meet?

F: Yes. A gas-phase man wouldn't think of it all.

T: Are you interested in any other, shall we say, "controversial" areas of science at all? Are there any things which most people would perhaps dismiss, but perhaps you have a less certain view.

F: Yes. Well, cold fusion is part of a much wider area, and I have been really quite uncertain that our theory and understanding of condensed matter is at all satisfactory. However, I'm not interested in some of the more extreme ideas which have been put forward and which interest you, you know in the future of energy.

T: I will say that some of this gravity modification stuff does, in fact, appear to have a theoretical basis as well as some experimental evidence. . .

F: Well, if you think about gravitation, until we have a unified field theory, then you can't be sure what is going to happen.

T: Even Frank Close said that we don't know much about gravity, and anything might happen.

F: We really have an incomplete understanding. This will change, but there are one or two notable exceptions, which I don't want to talk about now. We have no understanding of quantum gravity and until that happens we can't be sure that nature won't play some rather strange tricks. As I told you when we were talking before, we had about four projects which we were working towards, one was to do with gravitation, one was actually to do with the behavior of electrons in metals. We actually started to collect equipment together to investigate the behavior of electrons in metals. . .I have told you there have been certain themes which have run through my work, although they have never really been disclosed. I have often worked on topics where something short of the final answer would nevertheless be quite interesting. When I think about what I have done, I find that I have failed to achieve any of my longer term objectives.

T: A pretty impressive failure, surely?

F: I have been content with what I have achieved, but I have not achieved what I wanted to achieve.

T: Which was?

F: To gain a better understanding of condensed matter. In order to do so, as with the cold fusion story, I find the answers to the global questions have eluded me.

T: Most of the truly exciting science over the last half century has been in condensed matter, you are saying?

F: Yes.

T: In terms of value to humanity, it has been *the* area of science which has been of the greatest benefit.

F: However, there is a lot to be said for working in high vacuum. Curiously enough, I am again extremely interested in the behavior of thermionic diodes. I find I do not understand how a thermionic diode behaves. As I am interested in the interaction of charges in electrolytes, I think about simpler systems, and from that try and understand the behavior of the thermionic diode. I do not understand it; and I don't think that anybody else understands electrons in a vacuum either.

T: In that case, to me, the number of things I don't understand is increasing all the time.

F: I sometimes believe that I don't understand anything.

T: I'm happy to say I'm at least beginning to make some progress in the direction of not understanding. . .

F: Well, you have worked in this field, haven't you. Just think of the space charge around a cathode, you understand that?

T: Well I must admit, to be honest, I've rather tended to take things like the thermionic diode pretty much for granted.

F: Well, before our next meeting try to tell me whether you understand the space charge around a cathode.

T: I will. If something had prevented you from becoming a scientist, is there some other. . .

F: Oh yes.

T: What would you have liked to have done?

F: I could have done many things. Basically, I was more interested in history and English literature than I was in science. It is, you know, very common for chemists to be interested in history and it was really very difficult to choose. Shall I tell you why I became a scientist?

T: Please do.

F: I did not think I could have a rewarding career if I went into Arts.

T: In what sense a rewarding career?

F: Well, an intellectually rewarding career. I decided to do science because I could see. . .this seems a very sort of cold-blooded decision. Well, it was really. A somewhat mature decision for a child of 16.

T: Yes, I can believe that. Staying with your history, what can you tell us about the route your family took to come to England from Czechoslovakia before the Second World War?

F: Well, it was quite sort of accidental, as so many things—really formative things—in one's life are accidental. We had got caught up in the German occupation of Western Czechoslovakia and we managed to get out. I always tell people I had the unique and unpleasurable experience of being arrested by the Gestapo at the age of 11. These things tend to concentrate your mind somewhat, you know, and my father was very badly beaten up by the Nazis. However, we got out. We were driven across the border by a First World War comrade in arms of my father.

T: He had been with your father in the first war?

F: Yes. He was a fighter pilot in the Austrian Army, and my father was an artillery officer, but they were very close friends. They were big heroes locally. He drove us across, he had a taxi firm. He himself drove us across into the unoccupied part of Czechoslovakia. That was the first time we got away, and the second time, it wasn't clear where we were going, we might have gone to Canada or Argentina—or South Wales actually. But we couldn't get any money out. My parents were going to start a factory in South Wales, but this couldn't be arranged, so we lost everything, and in the end my sister was adopted by a Methodist Minister and his wife in Cheadle Hulme and the wife's brother lived in Llandudno and she told him that he had to adopt me. Which he did. He was a bachelor and he adopted me. . .I find this very difficult to talk about. I must say, when Gene asked me about it, I burst into tears—which I am prone to do when I recall this ancient history. At that time, my parents also got permission to come to England, and we all got on the train in Prague and came to the Dutch border and the Germans cleared the train of all refugees and we were in the last coach and my father said, "No, sit tight, don't get off the train," and the train pulled out of the station. So that's how we got away the second time, and arrived at Liverpool Street Station with 27 shillings and sixpence between the four of us.

T: And how were you treated afterwards?

F: Marvelously.

T: This country treated you well?

F: Yes.

T: In what way?

F: In everything. We had the most unbelievable consideration.

T: Because not all people coming into this country nowadays as refugees are so well treated.

F: Well, it's gone.

T: The old spirit has gone?

F: The old spirit has gone. Maybe it was a luxury of the upper classes. Or whatever.

T: You think so? After all, do you not recall the battle of Cable Street when the British fascists were put to rout by the mob in the East End of London?

F: Yes, that is one thing, but the consideration of the refugees I would have thought was a middle class/upper class aspiration, really.

T: So you were set up as it were, in this country?

F: No. My sister went up north and I went the other way to Wales and then my parents were going to start a chicken farm in Sussex, but then my father died and then my mother started this toy firm.

T: Really?

F: Yes. During the War, converting unusable scraps of materials into toys and dolls. The stuff she used would have been burned, you see. And it was her lucky break because her first doll—we used to keep it—resembled Benito Mussolini and she said, "This is the Mussolini doll," and she said the only reason she succeeded was that there was no competition! The dolls improved very quickly. Actually, she had training at the Art School in Vienna so she was a good designer.

T: So things improved for you?

F: Well, at times it was a little touch and go!

T: Moving forward, what are your recollections of meeting with Julian Schwinger in Salt Lake City?

F: Well, I didn't meet him as much as I would liked to have done. Julian Schwinger came to talk to various people in the Chemistry Department, including Jack Simons. Julian Schwinger didn't have such a closed mind, and he could see that such a process in con-

densed matter could not be interpreted in a conventional way. . .I was so preoccupied, I didn't talk to Julian Schwinger as much as I should have done. Subsequently, of course I talked to Giuliano Preparata and that was really a meeting of like-minded people, because he thought of it in much the same way as I did. Of course this may mean that we are both wrong!

T: I suppose that, in a sense, your sort of early experiences, you say that to be arrested by the Gestapo at that sort of age is something that would wonderfully concentrate the mind. It would put the sort of difficulties you have had since 1989 in perspective, I would imagine.

F: I'm sure.

T: When you think of such people as Fred Hoyle, for example, who take this very Yorkshire approach to their difficulties—that's fine, but for yourself I would say perhaps it was based on your past experience? You might have been a Fellow of the Royal Society and everything, but. . .

F: I might have been dead.

T: Yes, somewhere inside yourself you would be the 11 year old boy with the Gestapo, so you just don't take some of these people very seriously.

F: Not really. No.

T: Gene mentions that he's heard that you don't aspire to such things as the Nobel Prize, and I've heard there's a lot of politics in getting the Nobel Prize, but what are your comments on that?

F: I think that's another thing which has gone wrong, you know. I know of quite a few Nobel Prizes which have been awarded to people for work which is manifestly incorrect.

T: Like Millikan, for example?

F: That's an early example, but more recently. . .It's accepted—socially accepted, but obviously flawed. So has it been a positive influence or not—I don't know. First of all there are a whole lot of Nobel Prizes awarded to people for work which is incorrect, or prizes which are awarded which clearly should have been shared between several such workers, and prizes which have been awarded to people who did not do the original work—that is very common.

T: Or not awarded to people like Fred Hoyle, perhaps, for his work on solar nuclear processes.

F: Yes.

T: But should have been.

F: Yes.

T: Because he was not playing the party line.

F: Well, he's had some cranky ideas which has colored the rest. The question is whether Nobel Prizes are judged for an original contribution or are they judged for the totality of the work. Or can the totality of the work detract from the original contribution. Unfortunately, of course, this has happened in recent years.

T: There have been two "branch points" in cold fusion: the nickel/light water thermogenesis or whatever you would call it, excess heat, as particularly exemplified by the Clean Energy Technologies' cell and the work of Mills and of Miley. That is one branching point which the science has taken, and the work in very recent years which points to host metal transmutations, hydrogen/metal fusion. These are two diversions away from the classical process, even if the latter would be more of an alternative explanation or interpretation, whether you look at them as great heresies of cold fusion or great branch points.

F: Well, I have commented on the light water work before. To put it into perspective, Douglas Morrison came to see me when I was in Switzerland and said: "Martin, if I were a man from Mars would you expect me to believe this?" I said, "No, Douglas, no of course not. I realize all the difficulties." So, I realize that there is a credibility problem for d-d processes. I realize that it is much more difficult still to justify H-whatever processes, and then I said I had not done enough work on that myself to express an informed opinion, but that is as far as I will go. I can see there are difficulties with regard to light water, I can see the difficulties. . .Stan and I set down the protocol for the experiment we did so as to exclude as many difficulties as we could: secondary reactions, all sorts of things. Not potassium carbonate, please. We used lithium deuteroxide, the simplest thing, prepared the simplest possible way—the simplest possible system we could set down. No chemical complications. . .I think it would be quite difficult to prove absolutely that there are never any chemical complications in the light water work. Also of course we use high current densities and they use low current densities, so there are always problems with possible side-reactions. But I would never pooh-pooh it because I think that I just don't know whether you might not induce peculiar reactions with protons. I don't know. So that's one thing I would say about that. . .Now the transmutation. Of course, I can think of several ways in which something like transmutation could take place.

T: Any form of nuclear reaction is transmutation anyway. So it's a very very small step.

F: But we do now know that there are high energy X-rays. Gozzi has observed them to over 120 keV.

T: That's a big number.

F: That's a big number, which, incidentally, can't arise from the electrons in k-shells.

T: What is the maximum for that? About 15 keV?

F: Well, whatever it is, but. . .

T: It's a lot more.

F: Yes. It cannot arise from anything in the electronic shells.

T: 100 keV? No way.

F: No way, no way. So this has got to be some peculiar phenomenon. Incidentally, this is a fairly important question because, as Preparata pointed out in Japan, if you have got high energy X-rays coming out—and this goes back to Stan Szpak—lots of people then say, "Well it's soft X-rays," but soft X-rays would never get out of the cells. So they had to be hard X-rays. Those could dump their energy outside the cell, so you can see a lot of the complications with the thermal measurements could be just that people have missed the excess enthalpy with their cell design: the cell is too small, it won't catch the excess energy, and in any case it's only the lower bound that you catch, you must design a cell to trap all the energy in the X-rays. Once you have got the X-rays, you can ask what sort of X-rays, what is going on? Are these coherent X-rays? What would they do? Will they yield some sort of photo-fission processes in the nuclei?. . .So I could think of lots of processes which could be going on, and it will take a long time to sort that out.

T: Would you say that we are talking about systems whose complexity compares with normal nuclear physics in the way that perhaps biochemistry compares with inorganic chemistry? Are we

talking about things that are at a wholly different level of complexity, in a very complex multi-body process?

F: I think that we will find that when people have got some sort of explanation for condensed matter physics, based on single-particle descriptions, they must find it extremely distressing to now get this body of information which cannot be fitted into this framework. And there's much more to it you know, there is a great bag of physics which simply will not fit into the existing paradigm.

T: Could this be some kind of "complexity" effect in itself? That we are now beginning to understand that systems built out of units which individually behave very simply can, in conjunction, produce extremely complex effects?

F: Well, yes. I went through this in the 1947 understanding of the work of Alfred Coehn on electrodiffusion, and understanding at that time that even with the existing understanding of quantum mechanics—which was all that I had at that time, I think all that anybody had—you could then conceive of changing the conditions of deuterium in a lattice so that you would change the fusion process. Then I did a lot of work in the 1960s which led to this idea that solutions really have to be understood in terms of quantum electrodynamics. Not in terms of classical mechanics or even quantum mechanics. It had to be in terms of quantum electrodynamics, and then came all the work on palladium which I have worked over several times in my life. There was one very big slug of work in '67/'70 which convinced me that you could not talk about anything to do with hydrogen or deuterium in palladium in terms of single-body processes. These had to be many-body processes. The explanation for the behavior had to be in terms of many-body effects, and that then triggered the cold fusion work. It's that which convinced me that it was worth going on. . .I still didn't have the whole explanation, in the way Preparata has achieved the whole explanation, I only had 50% of the story. If I'd had all Preparata's insight into this I would have dropped everything else and gone for electro-diffusion, even if I'd had to do it in my kitchen.

T: It's very interesting that you are talking about cold fusion as really being a single aspect of a much larger idea of condensed matter physics.

F: I think, really, that a correct understanding of condensed matter physics in general, and electrolyte solutions in particular, is a prerequisite for taking our next steps in chemistry and biology.

T: Biology as well?

F: Biology especially. And that's going to be more significant than cold fusion.

T: Where would you think this would lead us to, for example, in biology?

F: A totally different understanding of biological processes.

T: Could you amplify on that at all?

F: Well, no, not at this stage. I'm just writing a proposal. But I will talk to you about that in due course.

T: We made up a list of people you might like to comment on—Steve Jones, John Maddox, Huizenga, Frank Close, Mark Wrighton, Gary Taubes, Richard Petrasso, and Doug Morrison?

F: I should explain to you that I have not read Close's book. I have not read Taubes' book. I have, however, read Gene's book.

T: Which was wonderful, of course!

F: Yes, and I have not read all Douglas Morrison's messages and newsletters. People have stuffed various things under my nose which irritated me intensely. But what do I think about them? Let me tell you.

F: Steve Jones? Well, he's an ambitious person. Let's give him some credit. He has some vision, he's very ambitious but I think his ability is not up to the vision he has. That's my comment on Steve Jones. This is not to say that he is a bad scientist, second-rate in science is very good. The problems he wants to do, he just hasn't got the technical competence to achieve them. . .John Maddox? Well, he's a typical establishment figure, isn't he? We have to have people like that. He's out of it now—but I can't understand how his brain functions. Sometimes you are confronted with these people and you say, "What makes you tick? How can you function? I don't understand. . ."

T: "What's your problem?"

F: Yes. "What's your problem?". . .Huizenga? Well, I thought he was just a front man for some organization which the DOE had cobbled together, really. I still think its a piece of disinformation, I think a lot of Frank Close and Huizenga is disinformation. If you could ever get into it, you'd find it is disinformation. . .Up to a point Maddox probably as well. I think Close is a better scientist than Huizenga. A disappointed nuclear chemist who sees his field disappearing; his life's work is disappearing. And could easily be manipulated by people unknown. . .However, I must tell you that at the outset, when Admiral Watkins was in charge of the DOE, I said to Stan Pons, "Stan, what if Admiral Watkins had been me and I had been Admiral Watkins? I would have done to him exactly what he is doing to us." I could not conceive of Admiral Watkins welcoming the notion that the American Universities and goodness knows who else working on nuclear physics in chemistry departments. This is where we came in. "We've got to keep it secret, we've got to have it classified! We don't know what's going to happen!" I think we will be proved right. In '88 we had no idea of the totality of the subject. We proposed to the DOE some things which shall be nameless at this stage, but we had no idea what would happen. We knew what we had got, which I think was sufficient indication it should have been classified. . .And then in 89, of course, we said to the University, "We will go to Oak Ridge or to Los Alamos for two years and see how far we can get." And they said, "Do you really want to work with the Government? Wouldn't you rather work with General Electric?" I wasn't asked that question but my answer would have been, "Yes, I do want to work with the government, thank you very much, I'm off to Los Alamos tomorrow." [prolonged laughter] If they would have had me! . . .Frank Close? I don't understand him either, really. He's a theoretician, not top flight. Well, he's OK, but again I think he has been manipulated. . .Mark Wrighton? I shake my head. He's out of science now, isn't he? He's become a provost somewhere. In days gone by when I used to be asked to referee a lot of material for promotion in the United States, I used Mark Wrighton as a benchmark for excellence, also Rick van Duyne and Al Bard from the older generation, so I obviously thought highly of him as a scientist. Up to a point Nate Lewis too. I used him as a benchmark. . .Gary Taubes? Well, nothing. A second-rate science writer. Primarily, he is a very bad journalist. . .Richard Petrasso I think is a capable fellow, quite frankly. I think he is a capable fellow. . .Douglas Morrison, I think, is another disappointed man. Quite a good analytical mind in some ways, but again I think he is manipulated. I think that if you look at this, you would say Jones can't forgive himself for what he did, so he keeps on trotting out these negative ideas. Jones is in a difficult moral position, and so some of his actions post-1989 had to be, as we said in Czechoslovakia, "holier

than the Pope". . .However, regarding Huizenga, Close, Morrison, I feel that if you really could penetrate behind the smokescreen you'd find that other people have been manipulating them.

T: What sort of people? We're coming close to conspiracy theory. Is this the "Protocols of the Elders of Britain"? What are you really saying here?

F: Well, that there are always groups of people who decide policy, aren't there? For example the Jasons. Lewis is a member of the Jasons, Garwin is somewhere near the head of the Jasons. They advise the Government. So what role do the Jasons have in this? Maybe none, maybe some. Garwin was interested, so was Teller. So who manipulates whom? Or perhaps they do not manipulate, I don't know. I don't think these things are spontaneous.

T: I think one difference in opinion—I suppose an inevitable one because of our unique approach—one difference between ourselves and yourself has been that we have argued and pushed and are, as you know, working for clear public "in your face" demonstrations. I don't know whether you read Rothwell's comments on the Wright Brothers. He mentions for example that these so-called mechanics actually predicted the performance of the first air screw to within a percent before carving it. They were obviously very competent people, but they were beguiled by an idea that they had to do secret deals with all these Governments. They didn't realize that until there would be some general recognition of the existence of flight, you couldn't sell aircraft.

F: This is precisely the point I have made here: "When do you anticipate that the course of public opinion will turn in favor of cold fusion?" The answer is that you have got to get to a demonstration device.

T: Well, that's what we are trying to do, as you know.

F: Yes, and I absolutely agree. We have all the science, we had systems of Type A, systems of electrodiffusion, systems of Type B, systems of Type C which made the link to hot fusion, systems of Type D—very interesting, but I am not prepared to talk about systems of Type D at the present time. Nevertheless, we focussed absolutely at systems of Type B to try and bring this to some sort of demonstration device, because this is the thing which will change people's opinion.

T: But I thought we were in disagreement with yourself in this area—that we were the ones who are arguing most strongly for. . .

F: Absolutely not.

T: Well, I'm very glad. This has surprised me.

F: I have disciplined myself severely and constrained myself in order to try and capture this position, unsuccessfully because I find that people won't take my point. You see, this really takes us along what you should do, you have to say: "I have a sufficient understanding of Johnson-Matthey material Type A. I am going to freeze my design on that. I am going to explore the operating condition of J-M Material Type A. It won't be the best, but it's acceptable to lead to a demonstration." And I find myself in conflict with everybody.

T: So you would be very happy with our going public with a working excess energy machine.

F: Good luck to you.

T: Well, if it doesn't work, I'll be on the phone to you. You can tell me where I'm going wrong.

F: One of the things which was very clear all the way through was that if you followed a certain line of development, which I'll call the Utah branch—the Utah system, you would get to a demonstration, but then the point is why don't people want to do that? The question is: who wants the Utah line to succeed?

T: Well quite, but I would have thought for example that the people at Toyota would see the benefit.

F: They've got their own axes.

T: Yes, but in 1947 the basic idea of making a point-contract transistor was released to the world, and labs all over the world then spent a fair number of years before the very first transistor radio was able to be made. In other words, the thing was out there and everybody was working on it. I would have thought that, for example, Toyota had stuff to sell in demonstration kits, which I think could have been done if the effort had gone in that direction.

F: Well that was very clear. I saw ICARUS I as a stepping stone to a low-cost demo device.

T: Could you define ICARUS?

F: Isoperibolic Calorimetry Research and Utility System. This is the data acquisition system, the whole lab with thermostat tanks and a data acquisition system. . .a data interpretation system. This was all behind Icarus 1, the thing which went to NHE Labs. I saw this as one of the logical developments, to make a low-cost version of Icarus I which people could play with. And more of my foolishness, more of my follies, yes. As I said, I should have called it Daedalus but I couldn't think of a good acronym for Daedalus.

T: The Wright brothers had their arms twisted into giving a public demonstration, and the gasp from the huge crowd who saw them finally fly was the sound of the paradigm shifting. If Toyota had produced and sold demonstration devices—with a great public press conference announced the sale of the Size 1, Size 2, and Size 3 cold fusion demonstrators, things would have changed.

F: But tell me, what interest would they have in that?

T: Well, I think their longer term interest might have been served, do you not think?

F: No, I don't think so. . .Look, if people had said to me how do you develop this thing into a demonstration device, I could tell them. In fact, in '89, when we thought the Select Committee of Congress would come out of Salt Lake City, I dropped everything else in order to try and make a demonstration device, but this is a non-trivial exercise. Very difficult to do. Since then, if people had said to me, "How do you set about making a demonstration device?" I would tell them, but nobody asks me that.

T: But you are happy with our approach, the Ragland cell?

F: Yes, but I have my own view of how I could do it, but I now don't have a budget, so I'm not going to do it. I'd need a lab and I'd need a budget and I haven't got that so I'm not going to do it.

T: Well, fair enough, but you feel our prospects are good?

F: Well, yes.

T: It doesn't really matter what kind of device it is.

F: There will be a hundred different devices.

T: Yes, exactly, we don't really care. We just want to make sure that one of them gets in front of enough people, so that interest will be then taken.

F: If I'd pursued this as a piece of science, I wouldn't have done the research which I have done.

T: It seems to me that you and our group are much closer in outlook than I had thought we were.

F: I think I discussed with you the question of why we did it and, here, I think again if you are doing the transcript I would put this in. This whole thing started off in 1947—at the end of 1947 when I ran across the papers of Alfred Coehn—and I realized that there was a very very big problem here the incompatibility of the dissolution of hydrogen as protons in the lattice, and at the same time, the high diffusion coefficient, the high mobility. The first paper was published in 1929, and in fact Alfred Coehn showed that the hydrogen in the lattice had a unit charge. I knew about the work of Lange on the Galvani potential in the lattice, and the work of Gurney and Butler before the War on interpreting the quantum mechanics of processes at interfaces; and you could stick all this together and come to a conclusion. And at that time there was also a lot of interest in exploding wires and making metal films, so I realized that you could create very strange conditions by applying a field to the wire. But that would have involved rather heroic instrumentation, and I parked it in my head. Then in the early 60s I came to the realization, as I said to you, that we have a very poor understanding of electrolyte solutions. . .So on we go from there, and then I come to the end of the 60s, the beginning of the 70s and realize that the behavior of the hydrogen in the lattice or, of course, at the interface, can only be understood in terms of many-body effects, so now the whole thing is complete; you know—bang-bang-bang now we can go on—there is enough basis to think that we should go on to explore whether the nuclear cross sections are changed. Well, I think that was then the point at which I had decided it was worthwhile starting, but I still didn't start it because I was still in full-time employment and I realized that this research was incompatible with being an academic, it was too outrageous. So, in 1983 Stan and I discussed a number of projects—we had room for one more project in Salt Lake and we had several options, I told you one was the behavior of electrons in metals, one was the strange thing to do with gravitation, and one was cold fusion and there was a spectroscopic one and the spectroscopic one we needed too much money for. In fact, we needed too much money for all the projects except the one on cold fusion. So we decided to do this thing in a rather low level way because we didn't really think it would work, so we had five years of on-off experimentation. But we did actually have four different systems which we defined, which would be interesting, of which the lead one was going to be electro-diffusion; and the reason we did the thing in the particular way in which we did it is a long story. . .So then the results were really totally surprising, and we got into all these other subsequent difficulties. The best result in a way would have been if we had found nothing. You know, historically. I think even we would have been happy, but in the end we had enough information which did not fit non-Poisson distributed neutrons, certainly something in the gamma ray spectrum—goodness knows what—we found the correct gamma ray at the end of '89.

T: And does this not bring in the whole question of the moral dimension in science?

F: Well the question is what do you do with a set of results. The publication was premature, there's no question about that. Our original protocol called for three independent methods of measuring the excess heat, of which we had only done one, so we did not want to publish until we had three independent measurements with as much confirmation as we could muster. But we even had some indication of helium, you know, but that was unpublishable, not even we could be persuaded to publish that. We needed huge resources for that. I don't think one could have done it in '89 actually.

T: Might you consider the helium to be less the result of fusion, more the result of the stimulation of alpha-emission?

F: Well it could be. You'd always have to budget for that.

T: When I asked about the moral dimension, I was thinking less of how well one is fitting into the protocols of science, I'm talking more of the moral dimension to society as a whole, of the individual scientist confronted with an interesting result. There is a problem, is there not?

F: Well it depends on what sort of person you are. I'm sure that most people with the information which we had in '89 would have suppressed it.

T: For good and sufficient reason.

F: Because it just didn't fit in, they didn't understand it. Unfortunately, I think I understood enough about it to realize it was possible.

T: That it wasn't quite as absurd as it looked.

F: No, because that's how we came in.

T: Because you came in from a concept which was rather more sophisticated a concept than simply shoveling deuterium into a lattice.

F: But that's crazy.

T: But people do, as I say, people think of the Wrights as a couple of lucky mechanics.

F: But they were very good engineers.

T: And very good scientists as well.

F: Yes, indeed.

T: Yes but most people see them as a couple of lucky tinkerers and most people see your idea as very naive.

F: This is because they cannot conceive that anybody would ever be able to work something out.

T: Well, I think it's the same reason that Shakespeare can't have written the plays because no glove-maker's son could produce work of such high literary quality.

F: But they don't understand that because they can't do it, that somebody else might.

T: I'm beginning to understand now what you are saying, that you had a vision of solid state—shall we say physics, shall we say chemistry—which included this, and your reasoning for cold fusion was simply as an example of something much more complex.

F: That's true as far as I am concerned, yes.

T: And so the common idea that you were simply thinking in terms of, "Ho, ho, let's squash some deuterium and make it fuse!" is as much of an over-simplification as saying, "Oh, the Wrights were lucky because they happened to have an engine that would pull an airframe."

F: Yes, I have been through this thing before, in a much less extreme way. People have said, "You go in the lab, you fiddle about and get this result and then everybody else finds that you were right, you must have just gone in the lab and fiddled about and got this peculiar result."

Progress in Catalytic Fusion: Birth of a Revolution in Cold Fusion?

To the delight of many at the Seventh International Conference on Cold Fusion (ICCF7) in Vancouver, BC last April, a new approach to cold fusion emerged. Dr. Les Case, an experienced chemical engineer with four degrees from MIT, announced what he is calling "catalytic fusion"—to distinguish it somewhat from the original electrochemical approach. He had concluded that the electrochemical method of Fleischmann and Pons was going to continue to be limited by materials issues—palladium cracking, composition, etc.—and the inherent difficulties of working with electrochemical systems. Furthermore, he wanted to achieve the higher temperatures that are allowed by gas-phase systems.

The story of Dr. Case's discovery of catalytic fusion is exciting, including his travel to Europe and Japan in search of the proper path forward. In the account below, we let Dr. Case tell the Edisonian story of discovery in his own words. It turns out that relatively simple catalysts—off-the-shelf "hydrogenation" catalysts used in the chemical industry—seem to catalyze deuterium (heavy hydrogen) gas to helium-4 in a heat-releasing nuclear reaction that is millions of times more energetic than any conceivable chemical reaction. These catalysts are typically activated carbon that has been doped with precious metals such as palladium. Other catalysts may emerge as a result of this line of investigation, ones that perhaps will not require any precious metals. Unlike high temperature plasma fusion (hot fusion), there is no harmful radiation from the process. Thus, the original promise of cold fusion may now be realized in more robust and repeatable experiments. Ultimately, these could be commercialized in relatively straightforward ways that make use of chemical engineering practice.

At the moment, catalytic fusion studies are proceeding at Dr. Case's own lab in New Hampshire, at SRI International in Menlo Park, California and at the Pacific Northwest Laboratory, (a U.S. Department of Energy lab, under contract with Russ George's Saturna Technologies, Inc.). In our own facility (New Energy Research Lab—NERL) here in Bow, New Hampshire, we saw the positive results of a Case experiment first hand shortly after ICCF7 (see *IE* #19). We are beginning a second round of work to demonstrate the process with a relatively simple calorimetric dewar set up. We hope that these efforts help catalyze new work by others in an area of immense potential.

We are pleased to present the following progress reports on catalytic fusion, in the words of Dr. Case and Dr. Michael McKubre. [In the course of video-taping our documentary about cold fusion ("Cold Fusion: Fire from Water"), our video team visited Dr. Les Case this fall in his basement laboratory in New Hampshire. These are some of Dr. Case's recollections about his discovery and his projections about the future of catalytic fusion technology. The team also visited Dr. Michael McKubre this fall in his laboratory at SRI International in Menlo Park, California. These are some of his comments about the status of his group's experiments to verify the work of Dr. Les Case in the U.S. and Drs. Arata and Zhang in Japan (see *IE* #18 for Mike Carrell's summary of the latter). Though understated and cautious, as befits one of the field's foremost scientists, it is clear from what Dr. McKubre says that much progress is being made.]—*EFM*

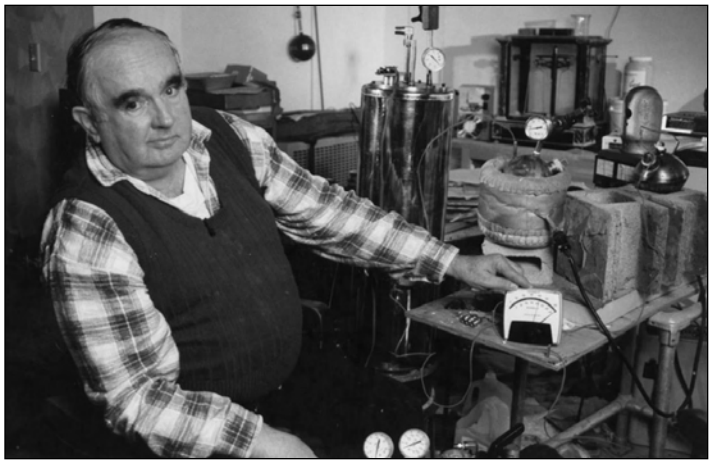
How I Discovered Catalytic Fusion
by Dr. Les Case

Prologue

I was going to be a chemical engineer and then head a large corporation. I went to MIT and I got three degrees in Chemical Engineering through the Sc.D. Also, along the way, I took a side degree in Business Administration. I went to DuPont to their Central Research Station, the Plastics Department, or something of the sort. I worked there and it became clear that they didn't want to do business the same way I wanted to do business, so then I taught school for ten years.

I started my own laboratory, studying improved plastics and polymers and I had, for fifteen or twenty years in Nashua, New Hampshire, my own company and my own building, but it never went commercial. I did a lot of research and development, got a lot of patents, and then my wife got very ill. I spent a fair amount of time concentrating on keeping her well. So the laboratory there went inactive. Then when my wife died in 1987, I had a lot of things to do to get the estate together and so forth. I was then following scientific developments, which were then current. I became quite interested in high temperature superconductivity. In fact, I went to the Beijing Conference on Rare Earths and presented a theoretical paper providing the background, what I thought was the chemical background for the physical phenomenon of high temperature superconductivity. For a while I began to play around with the idea of getting a useful device based on high temperature superconductivity.

At just about that time, the cold fusion hubbub erupted. I followed it with some interest, but I could not see how it would go commercial. The original conception obviously was a scientific curiosity, but it wasn't at any point in the reasonable future heading towards a commercial operation. So I followed that at arms length until I saw some work by Dr. Yamaguchi at NTT in Japan, in which he had obtained an 800°C-plus exotherm [exothermic reaction] with, he thought, big bursts of neutrons. So I went to visit him—actually in Tokyo at his laboratory—and looked at his equipment. Beautiful stuff! Very careful work. Clearly, he had obtained a result which was very, very definite. And, incidentally, at this time, which was about



Dr. Les Case in his basement laboratory.

1993 or so, it was still highly controversial as to whether or not anything related to cold fusion had ever really been seen in a definitive fashion. There was no question that he had seen a very definitive result. He'd obtained 800°C-plus.

Well then, I thought, "OK, this is something that needs to be refined and scaled up." And because he was working with palladium and everybody else was working with palladium and platinum primarily, it became sort of obvious to me that probably some sort of catalytic effect was involved. I am a chemical engineer and chemical engineers use chemical catalysis all the time. Platinum and palladium are the preferred catalytic metals. So I then embarked on trying to follow up Dr. Yamaguchi's work in my own fashion. I was initially concentrating on the neutrons as being something important. I then spent, I think, over a year trying to find a laboratory, equipped to deal with neutrons, which would cooperate with me—in which I could sponsor some work and try to work out my ideas.

Off to Europe

There was no laboratory in the United States that I could find that would work with me. After all, it was cold fusion, or something related to cold fusion and most scientists wouldn't touch it—even for money. I finally determined that because all Eastern Europe is known to be very low wage scale—low price scale—that there were some Eastern European neutron laboratories that were of possible interest. So I got myself a plane ticket to Berlin and took the train going east to Warsaw.

I went to the Department of Nuclear Science or something of the sort in the Physics Department in the University of Warsaw. I met a nice lady there and there was a possibility of doing some work. We agreed to meet a little later on my trip to Budapest, for dinner and further consultation. Then I looked at the train schedule and considered going to Lotz. It's not very approachable, so I skipped directly to Prague, which was a lucky shot. I went to Prague and I knew about Charles University there, which is a very famous old university, and went downtown to the old town square to the main campus and tried to find the Physics Department. It wasn't easy, because I don't speak Czech and many of the Czechs don't speak English. I finally found somebody there and she told me, "Oh, you want to go the Physics Department. That's on the other campus, across the river."

So I got the directions to go to the other campus. It's a tower building there and the Department of Nuclear Science was on, I think, the tenth floor of this tower building. So I had the taxi driver let me off and I went to the tower building, found the elevator, and went up to the 10th floor. I walked out the door and there was a sign that said "Nuclear Science." I went in and there was a very efficient scientific looking gentleman with white hair, sitting there talking to, I guess, the secretary. It turned out he was the Director of the operation.

I explained to him I wanted to do this kind of research and he said: "We'll do it!" I said "Really, who has to approve it?" and he said: "We'll do it!" So I hooked up with the Department; actually it's the Nuclear Center, Department of Physics and Mathematics at Charles University. For I guess over a year, maybe about two years, I was doing experiments in their nuclear laboratory, which is associated with CERN. It's a serious nuclear laboratory. It is by no means equivalent of CERN. . .

Shooting in the Dark

It was empirical work and I was trying to find an effect—the idea was to find some sort of temperature [rise]. I was using the temperature gradient for a catalyst—active versus a blank. I had a big vessel, and I had four samples inside the big vessel. One of these four samples was the blank and the other three were potential candidates. I would change the hydrogen or the deuterium gas over the sample, change the nature of the samples, and look for temperature differences. With neutrons or without neutrons. We also had to measure the neutrons I might be making, so it was empirical. I made a whole bunch of runs—oh, on probably three or four different trips, and with minimal results for maybe the first two trips. One of the times I started with a plated palladium-on-copper tubing, and I thought that might be catalytic, but it wasn't. I tried some titanium tubing, but it wasn't catalytic, and I finally ended up thinking: "If it's catalytic, you better use catalysts." So I ended up scanning through several dozen available samples of catalysts.

Finally, some of these catalysts I was modifying—I actually had some platinum and palladium acetonate, and I was modifying the surfaces—all of a sudden we started seeing temperature differences in one or two of the samples. That is, we were beginning to find active catalysts that would really show a temperature gradient over the inactive catalysts. And I can remember very clearly, one day it was, I think 1.2˚C or 2.1˚C above the background in a particular catalyst sample. The physicist that was working with me was amazed, because as far as physicists are concerned, 1 or 2˚C might as well be a million degrees, because it's clearly an effect and we were measuring it immediately versus an adjacent blank.

He said. "Well, how did you select this material to do this experiment?" And I said: "Because that's the one that works!" This is what happened: I had scanned through with many different experiments through all the various candidates that I had received from three to five different sources of catalyst, until I found a catalyst, a chemical catalyst that was off the shelf, that actually worked to give some sort of effect with deuterium compared to hydrogen and compared to the other blanks. So it was strictly an empirical result, just blindly following my nose. Changing the conditions, changing the pressures, changing the temperatures, and so forth until I finally found a catalyst that gave me a result. . .

What happened was as follows. I have always been very protective of this. Well, not always, but for the last five years or so—very protective of the results—not disclosing them to anybody. I have a series of U.S. patent applications, about eight or ten of them, a basic one which was totally speculative and wrong. I kept filing continuations and amendments to them. Finally, I began to get these results, and then with all of our three or four patent applications prior to my current ones, I began to get results. I kept improving them.

Finally, I got to a set of results which defined the field, basically. With that patent application, I filed for foreign applications and that was published in November 1996. I expected that there would be a very big response when this was published, but there was no response whatever. Nobody was paying any attention. So finally I decided to take the bull by the horns and I appeared at the cold fusion conference unannounced, in Vancouver in April of this year. At this April cold fusion conference, ICCF7, I gave a brief talk, saying that I had developed an experimental procedure for reproducibly generating a heat effect with deuterium and that it's catalytic. As I say, I can reproduce it and I can scale it up. It created quite a stir at the conference, because people were looking. A lot of people were looking for this: some sort of basic real approach, not just playing around, but a concept of something that made

it work reproducibly. The concept I introduced was contacting a certain limited range of standard chemical catalysts with deuterium under standard conditions, and it would work.

Well, there's a little bit more to it than that, but this was new because nobody previously had ever used a standard chemical catalyst. They were always making their own special material and practically nobody thought of a catalyst. It was their particular equipment, and sometimes it was very elaborate. But I was able to buy, off the shelf, standard chemical catalysts which did work. Gene Mallove and I met at that conference. This is how Gene and I came together at the conference in Vancouver.

Latest Experiments

Well the situation basically is this. This is the vessel. It's a modified oxygen tank and in it is a thermo-well, this is a gas inlet and outlet, and this is simply a port for putting solids in or out. Now in the bottom of this vessel, which is heated in this jacket, there are about 40-50 grams of standard chemical catalyst. It's been contacted now with deuterium gas for six or seven weeks and, using hydrogen in this vessel under exactly these conditions, I got a steady state temperature of 181.5˚C. Now, when I switched to deuterium it started off about 180˚C, slowly rose over the space of two or three days, and finally levelled out at about 220˚C, maybe a little bit more than 220˚C. Right now it's about 215˚C, almost 35˚C hotter with deuterium inside than it was with hydrogen. This is excess heat, which is apparently occurring due to deuterium fusing to helium-4.

Catalytic fusion reactor in Dr. Les Case's lab, showing pressure gauge and resistance heater collar.

So, inside this vessel now for six or seven weeks, we have had deuterium fusing to helium-4 and giving this excess temperature of about 35˚C, which is big—a really big effect compared to previous effects of practically unmeasurable temperature increases. This one is now continuing and maybe will continue for some weeks or months still. The idea is to test the reliability of the catalyst. The catalyst must work for some months or it's not a viable commercial process. You have to be able to load up your reactor and have it generate the heat for months without having to re-do the catalyst, because it's expensive and too much of a problem. So this is rather encouraging. It looks like it may be totally stable, or at worst, over the space of many months drop 10, 20, 30% in activity, which is acceptable.

Helium Measurements

Now, when this experiment is concluded for one reason or another, a gas sample is going to be taken off through here and analyzed for helium-4. With any luck, it may even read over 100 ppm of helium-4, maybe 200 or 150 parts per million. It won't be going up to a thousand parts but it's going to 50 or 100 or more. This is very very significant, because the helium-4 content of air is 5.2 ppm. So anytime you get above 5.2 ppm you're *making* it. So this vessel is sitting here making, as we watch, helium-4 at a temperature of 215˚C. Now this is a very novel concept: that you can have nuclear fusion occur at 215°C and one atmosphere pressure. Those are very, very mild conditions compared to what they're doing in plasma fusion and the H-bomb.

I had run this experiment several times before and obtained samples which I had analyzed at the Oak Ridge National Laboratory by the kind people at Lockheed Martin. I had some trouble with leakage and sent some bad samples and one or two fairly decent samples. One sample was contaminated after I adjusted the leakage and measured something like 100 ppm of helium-4. But they were able to analyze a good sample at something like 91 ppm of helium-4. Now the equipment is not ideal, because it's a big magnetic sector instrument and it separates out helium-4 from deuterium, which also has a mass of 4 by a very small difference in mass—something like 1%. That's the only way they do it, they don't trap out the deuterium. Because the helium is at a very low concentration, they see the helium-4 peak as just a bump on the side of the deuterium peak. So it's very iffy.

Now, some of the people at Vancouver [ICCF7], at least, saw this as not particularly reliable, but certainly interesting. They began to try to reproduce this rather quickly in May. Certainly by June other people were trying to reproduce this result. One of the people who tried to reproduce it was a man named Russ George, who has an association with SRI International in Menlo Park, California. He set up their equipment, apparently with permission of the group, and tried to reproduce this. The way he originally set it up, it didn't work. He got no [excess] heat and, of course, no helium. We had a brief consultation about it and I explained to him that you can't run the apparatus that way. I made a couple of suggested changes and it immediately took off with heat generation. Then he used their mass spectrometer instrument to analyze for the helium produced after 24 or 28 days, and he got a helium content up to about 11 ppm, which is far above anything that can be explained from leakage in from the air. And, because it had started at zero and went up to 11 parts per million in a monotonic way, that is, always a rising function, it clearly was coming from inside the vessel and not from contamination.

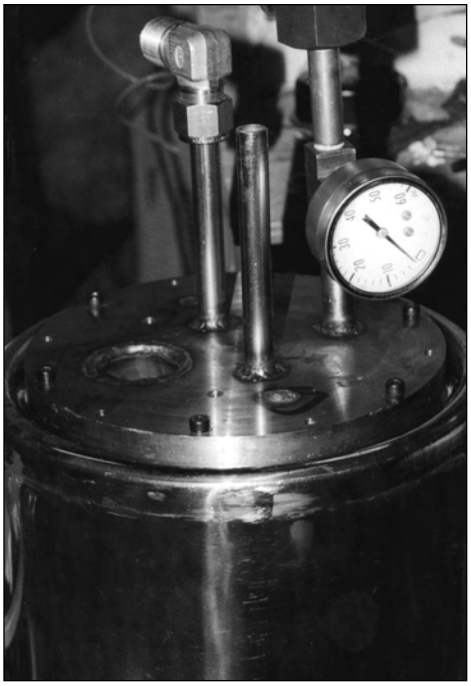
Now, those data aren't considered by the people at SRI to be definitive enough to be published. They are very, very strongly indicative that there is helium-4 generation by this fusion under these conditions. Now that result is going to be re-confirmed by SRI in a much more careful and definitive fashion. When the data are finally very very firm and unassailable, "bullet-proof," they call it, that will be published in a definitive paper saying this is now proof that we are getting helium-4 generated and we get a correlation between the helium-4 generation and the heat output. This clearly is a catalytic fusion, it really is working and, in fact, it is a new branch of physics.

Scale-Up

My objective always has been not to play around scientifi-

cally, because I'm not really a physicist, but to head towards commercialization. I really want to go to a 100-megawatt reactor within two to three years, which is really compressing the time scale, but it may be possible. So the idea is to scale it up. Now I wanted to scale it up, but other people want me to have it so it can sit there and, for instance, unplug this electric heater and it stays hot—self-sustaining heat or, as Gene Mallove says, "Life [sic] after death" [heat after death]. It will stay hot without any heat input from the outside.

Well, I'm trying to achieve both a scale-up and self-sustaining heating by bringing it up to a larger scale. This one has 40 grams of catalyst in it. This is a much larger vessel, this happens to be a modified stainless steel dewar, which is an insulating vessel. In this I will have one kilogram of catalyst, which is 25 times as much as in here. But the heat loss is not 25 times as much as the bigger vessel. The heat loss is maybe three or four times what the smaller vessel has. So if I had three or four times this heat loss and 25 times the heat generation, then presumably this one might self-sustain.



Dr. Case's large modified dewar cell, designed for catalyst beds up to one kilogram and aimed to achieve self-sustaining. Deuterium gas leaks are being fixed.

Maybe I'll get 250, approximately 250 watts of heat output from the catalyst inside this larger vessel. So this is a model scale up of the same reaction in this flask. The stainless dewar is as it came from a cryogenics apparatus. This is the cover and these are steam tubes. This is a heating device. The heat comes into this immersion heater, which is transferred to this aluminum fillet, which is transferred through this inner tube. I call this a "hot finger," the heat is being transferred into the hot finger and then it goes into the deuterium gas. If necessary, I will take some heat out using the steam tubes. There's a pressure gauge here and a gas inlet and outlet. I have two thermo-wells. I can use a thermocouple and stick it into either of these two thermo-wells. One of the thermo-wells is dipping into the catalyst layer, the other is out in the gas phase. However, it isn't that easily constructed. Inside there are some tricks to the way it's been defined and the way it's going to run. But the hope is that this, which will be run within a few days—I finally got it ready to go, work in progress, you know. Within a few days it may reach self-sustaining heating. And then, of course, the idea is: OK, so this is 250 watts, now let's go to 5 kilowatts. Once I go to 5 kW then I'm going to ask someone for some money to design 5 megawatts, or something of the sort.

View inside dewar showing Case catalytic fusion cell mounted on resistance heater.

It is critical the way you have the gas in contact with the catalyst, that's clear. That's been shown by the previous experimenters. With careful scale-up and changing the way the thing is done there's no reason why it can't go to 25 megawatts and 100 and then maybe 1,000 megawatts. I'm going to stop there. A thousand megawatts—that's big enough.

Implications

There are very many implications of this for society. One of them is that there's enough deuterium in the oceans to satisfy all the world's energy needs for a hundred million years. So there's more potential energy in the deuterium in the oceans than there is in all the fossil fuels combined by a factor of, what, a million or something, maybe ten million. But that isn't all. It isn't just that there's an unlimited supply of future energy. This is very cheap energy, because deuterium from the oceans compared to the amount of energy it produces is very, very cheap. The fuel cost is very much lower than fossil fuel. Deuterium as a fuel is surprisingly much cheaper than coal, and this is a big shock to people to contemplate an energy source much, much cheaper than coal. As a matter of fact, it may be more than two orders-of-magnitude cheaper than coal.

That isn't the end of it. The byproduct or, rather the product, of this reaction is helium-4, that's pretty clear. Helium-4 is totally inactive and benign. If you want to you can vent it to the atmosphere. It doesn't make a bit of difference. So this has the promise of getting rid of the greenhouse effect [threat]. When you substitute deuterium fusion for fossil fuel combustion, you start cutting down to the extent that you do that substitution. You cut down on air pollution, you cut down on the greenhouse effect, you cut down global warming. So, ultimately, in ten years or so, we will have totally defeated the greenhouse effect and global warming and air pollution—all at the same time. The public needs to really understand that. It's critical to develop this as quickly as possible to cut down on these horrendous problems of global warming, the greenhouse effect, and air pollution.

Dispersed Power Generation

It is going to be possible, I believe, to design a passive non-moving source to maybe 5 kilowatts or 10 kilowatts, using the technology represented by this, assuming that it works. But it's not going to be possible to scale up to megawatts. It's going to be possible to go to a few kilowatts. Now a few kilowatts is sufficient for a house, and it would make steam and electricity at the same time using a small co-generation unit, or it could be made slightly larger for an apartment or for a location such as a mountain top villa or something of that order. But I cannot conceive of scaling this up, this type of technology, to megawatts. So there will have to be a fundamental redesign of the reactor. I have some strong ideas on how that should be

done. Also, you are going to have to change the catalyst. This depends on palladium or platinum metal. There is a very definite limitation on the amount of palladium and platinum metal that's available for the world. If you were to use palladium catalysts of the type that's now in sight to built a 100 megawatt plant as a small commercial-sized power plant, you need something like 5% of the world's palladium supply in one power plant. You can't build very many power plants a year without severely impacting the palladium market. So there will have to be a change of the catalyst.

Small Case catalytic fusion cell (inside glass dewar, center) set-up for calorimetry calibration and "live" operation. Note: gas cylinder safety cap is used to secure thermal cover on dewar.

I have some far-distant ideas on that. So there will have to be a way to use titanium or nickel or some other metal—a non-platinum group metal as the catalyst—as one scales up and goes commercial. That may take some years, but that clearly is the way for the future.

This is the key to the whole thing. I discovered that using certain standard commercial catalysts, one could get this fusion to occur under reproducible, mild conditions. This is the catalyst that I've set upon as being about the most effective that I currently have available. This is a standard palladium on activated carbon catalyst. One-half percent by weight of palladium loaded on this activated carbon—this is the key. You change this just a little bit and it doesn't work—at all! But if you stay within the approved ranges, it works basically all the time. This is my contribution to find that that specific catalyst, within a certain limited range, operates under these standard conditions.

Comments By Dr. Michael McKubre

The experimental apparatus here is really set up to see whether or not helium can be produced by exposing a carbon catalyst with palladium to deuterium at slightly elevated temperatures and slightly elevated pressures.

This experiment very much follows along the thought process of Les Case and behind me you see five different sets of apparatus. The big vessel here is one of Les Case's, he calls them "footballs," it's a stainless steel vessel—on a heating mantle set up in exactly the arrangement that Les Case himself is doing in New Hampshire.

What we have behind me are four different generations of the Case experiment. There's the original Case experiment in this "football," as he describes it—a cylindrical stainless steel vessel on a heating mantle, a very simple experiment in which you simply put deuterium gas in and monitor for helium production. The first attempt that we had at SRI was formed in these vessels we called "Vessel 1" and "Vessel 2," slightly more sophisticated vessels which you can't see. They are concealed in the stainless steel dewars for heat retention purposes. Originally we had Vessel 1 filled with hydrogen and Vessel 2 filled with deuterium, so we could see whether the helium we were observing was present in the deuterium cell or the hydrogen cell. As it happened this cell Vessel 2 produced something like 11 ppm of helium. Vessel 1 at no stage produced any helium, suggesting that our helium determination process and our leak-tightness was, in fact, satisfactory for this experiment.

The original experiment in Vessel 2, as I said, produced 11 ppm helium. The air that we are breathing in this laboratory now is 5.22 ppm helium, so there is very little opportunity for error. The helium in the vessel, apparently, was produced by some source within the vessel and did not come from the air that we're breathing.

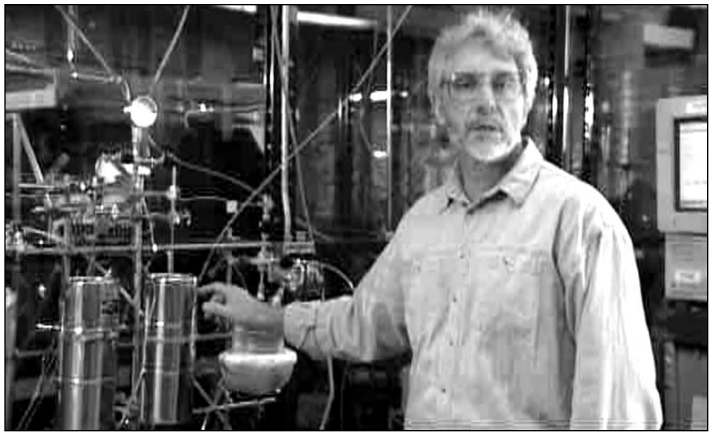
We're running now a second generation of this experiment in these two vessels. It's early stages yet, but we're in the hopeful that we'll be able to reproduce our own result which was, of course, a replication of Les Case's result.

This is a more sophisticated experiment. The question is, does the movement of the deuterium gas play any role in the production of helium. Is convection an issue? Is temperature gradient an issue? In this experiment, which, again, is concealed inside this dewar flask and non-observable, we're simply recirculating deuterium gas over a bed of Les Case's catalyst in a continuous manner and sampling periodically for helium in the deuterium gas. Behind the bullet-proof [transparent] polycarbonate wall here is a high pressure experiment, and this is our most recent attempt to see what the parameter space is for the production of helium from deuterium and carbon catalyst. What is the pressure effect? What is the temperature effect?

Les Case has already explored the temperature dependence somewhat. He finds that the effect occurs in a range of 170˚C up to about 270˚C. We have not explored the temperature domain, and until we get a lot more apparatus we won't do so. But we are able to explore the pressure domain somewhat better than Les Case is able to do because we have somewhat more sophisticated apparatus.

In the vessel on the floor, we have a high pressure deuterium gas at intermediate temperature about 200˚C. This experiment, in fact, just started about two days ago. We have no reason to expect helium production as yet, and the analysis reveals none so far.

All of these experiments are connected to a common gas manifold. What we are able to do is take a sample of the gas from each of these cells periodically. Initially we did it daily, but now we are doing it every two days, in fact three times a



Dr. Michael McKubre in his lab at SRI International with catalytic fusion experiments.

week, so we submit a sample of gas from each of the cells for analysis to the mass spectrometer, a high-resolving, low-mass mass spectrometer. We're capable of separating the two masses of species, deuterium D_2 and helium-4. The sole purpose of this experiment, the sole purpose of this apparatus, is to measure helium-4 in the presence of deuterium D_2.

On the monitor you see displayed, in fact, the mass spectrum from one of these samples. This is a relatively high level of helium-4. The peak here is the helium-4 peak, the deuterium peak would normally appear here; it's completely absent. This particular example shows 10.5 ppm helium. We compare the samples each day that we perform the analysis, we compare the samples of gas from the various active cells and blanks with a sample of room air, which we have measured many, many times and know to be 5.22 ppm. And we have some standards, which we typically use—that is, gas samples of helium in deuterium and argon which we submit to the mass spectrometer for the purpose of calibration.

The mass spectrometer simply sweeps a mass from low mass to high mass, in this case from 3.96 mass units to 4.06 mass units, which encompasses the range in which helium is to be found. In fact, this peak is helium, and deuterium D_2 is to be found which will be found somewhere in this region. We use a liquid nitrogen cooled carbon trap in order to remove D_2 so that we're able to see quite low levels of helium. We're accurate to probably 0.1 ppm helium and we can clearly resolve the presence of deuterium D_2 and helium-4. This spectrum is, in fact, the sum of a number of spectra that the mass spectrometer simply sweeps for the period of time that we pre-program, and this is the cumulative signal representing the integral of all helium which was present in the sample when we submitted it for analysis. To acquire this spectrum takes us about five minutes.

It's clearly not possible to produce helium from a chemical process. If we observe helium in our experiments it's either because it leaked in from the atmosphere—we can rule that out by the blanks that we do and the fact that the helium signal that we have seen is larger than the helium in the ambient. It's possible that the helium pre-existed in the sample and was simply released to the gas phase with long term exposure. We can rule that out largely because we've analyzed the catalyst that we're using and found that it contains no measurable levels of helium.

The only possibility that remains, and remains to be checked, is that the helium is produced by a nuclear process. If the helium is produced by a nuclear process, then necessarily there will be an associated release of heat. Although these experiments were not initially set up to be rigorous calorimeters, we have monitored them with a sufficient number of temperature sensors that we can know, to some degree with some confidence, whether or not heat is being produced and at what time heat is being produced.

From the best of my ability to analyze the thermal record, it appears that, yes indeed, in the vessel that was producing helium there was some evidence of excess heat and that the amount of heat produced was approximately quantitatively correlated, that is, the right amount of heat was produced compared to that of a nuclear process involving deuteron-plus-deuteron producing one helium-4 nucleus which releases 23.8 meV.

I'd like to re-state that the calorimetry was largely retrospective, this experiment was not set up as a calorimeter and, therefore, the calorimetry is not rigorous, but the temperature record quite clearly indicates in these experiments, as it does in Les Case's experiments, that there is an unexplained source of heat and the magnitude of that source of heat is approximately the right value to account for the observed helium.

Part of this generation of experiments is to improve the calorimetry and the central question in the cold fusion field is: "Is there excess heat?" If "Yes," then, "Is that heat the result of a nuclear process?" So the central question that we're all seeking to answer is: "Is there a quantitative and temporal—is there a quantity-related and time-related correlation between the appearance of anomalous excess heat and the appearance of the product of a nuclear reaction such as helium-4?"

So the thrust of our work is very much to find the heat and quantify it accurately and find the nuclear process and quantify it accurately so we can correlate the appearance of these two products.

We have determined that there is excess heat and we have to do a better job of measuring it with accuracy. This laboratory here is really set up to do highly accurate calorimetry. That work has largely been associated with the electrochemical experiments, such as Arata's experiments and our own experiments. So we are quite capable and willing to do the calorimetry. We just haven't applied those skills fully yet to the Case experiment, but this is obviously our plan.

One of the difficulties in the cold fusion field is the apparent lack of replicability of experiments: many people performing the same experiment get apparently different results; different experiments performed in the same laboratory give apparently different results. So it's obvious that if you do the same thing you must always get the same result. What this is telling us is that there are some important parameters of our experiments that are not under our control. Some of them I know and understand, and still [we] can't control some of these parameters we don't know about yet. We just don't know what the process is that we are studying, so we don't know what parameters we need to control in order to yield a consistent result.

An experiment which always gives the same result—can be performed in several different laboratories to yield the same result—would be very valuable to us, in part in helping to convince the remaining skeptical scientists in the world that there is a phenomenon to observe. But, in fact, in order to use the scientific method to observe scientific results, we have to be able to reproduce the results of our own experiments so that we can see what the effects of small changes are on these experiments.

The Arata-Zhang Experiment

One experiment which has been reported to produce consistent and reproducible results is that of Professors Arata and Zhang, both of them are very, very experienced and very well recognized scientists in Japan. They performed a very careful experiment, reproduced it apparently a number of times in their own laboratory—producing both anomalous excess heat in fairly significant levels and helium-4 and, perhaps more interestingly, helium-3. The helium-3 to helium-4 ratio that they observed in their experiments is different from that in the air that we're breathing. [Editor's Note: This isotope ratio is off by a *huge* factor—see the Carrell review in *IE* #18.—*EFM*] Sufficiently different to indicate that there is clearly an anomalous nuclear reaction occurring. The difficulty only with Arata and Zhang's experiment is that it's only been performed by them and only in their laboratory. What we're attempting to do here is to produce their same results with their apparatus and with their help. This is a collaborative effort between Arata and Zhang and the SRI group, to produce in *our* laboratory the

same results as they have obtained repeatedly over the years, which would indicate that we have some degree of mastery over the experiment.

The experiment that we have running here, in fact, is relatively young; it hasn't been operating for very long. One of the difficulties with Arata's experiment is that it requires many, many months to produce a result, and quite literally we're not very experienced with Arata's methods, so we've had some difficulty getting his experiment set up and operational. Certainly, it's caused me to have an increased level of respect for Arata and Zhang's technical competence. They are very, very good scientists. Within a month or two, we hope to have reproduced their experiment faithfully and reproduced their result. And the benefit will be in part sociological. We will demonstrate that an experiment can be transported from laboratory to laboratory and yield the same result. It will also give us something that we can do again ourselves and define somewhat the parameter space in which these experiments yield excess heat and, apparently, helium-3 and helium-4.

I don't know that Arata and Zhang have monitored their experiments for neutrons. We routinely monitor in this laboratory for neutrons at the radiation hazard level. We have a continuously operated neutron detector for personnel hazards. Clearly, this has not alarmed at any time or I would not be standing here right now. Whenever we've made attempts to look for neutrons in active heat-producing experiments, we have not observed neutrons above background level. That indicates simply that the neutrons, if they are produced, are not produced quantitatively with the heat in the same way that a hot fusion process occurs, but we've never had very sophisticated neutron detection applied to a calorimetric experiment producing large levels of excess heat. The problem is a very simple one, the criterion, the conditions necessary to do a first class calorimetric experiment of an electrochemical process—these conditions are incompatible with those necessary to do a high quality neutron determination. So you either optimize your experiment for the electrochemistry and calorimetry or you optimize it for the neutron measurement; you can't do both.

In fact, behind me this large black box is a neutron spectrometer designed for us by [the late] Kevin Wolf who, in my view, is the most able nuclear experimentalist that I have ever met. A first class man with a first class talent for low level nuclear determination. It's a beautiful neutron spectrometer and cost us, or EPRI, perhaps $3,000. We've never used it with serious intent because we've never had an experiment which would make it worth our while mounting and manning this apparatus. The neutrons that are present in these experiments, if they are present at all, are present at very low levels. Levels so low that they can't be directly connected to the heat producing process. They may be indirectly connected, but they are not present in large quantities and therefore they are not very interesting to me.

Advantages of the Case Technology

The Case device is attractive for several reasons. It's simply deuterium gas and carbon catalyst—commercial catalyst—something that can be obtained in 55 gallon drums, and the vagaries of the manufacturing process have already been mastered. So that if the Case experiment works to produce heat by a nuclear process, then it's something that can be very easily scaled up. Most of the work that's been done in this laboratory has been done on electrochemical systems which are very sensitive to handling issues, the metallurgy of the palladium, the purity of the electrolyte, and really only people that have been trained for many, many years in electrochemistry are able to perform electrochemical experiments satisfactorily.

In Case's experiment, you have a gas, an easily accessible temperature, modest pressure in a sealed vessel. This is an experiment which many people can do and facilities exist to perform the experiment and understand its sensitivity to the various parameters and it's easily amenable to engineering scale-up.

The big question, of course, if we do have a heat-producing system, if that system requires significant quantities of palladium then its application is necessarily limited. Palladium is a precious metal. In fact, it's a by-product of the platinum metals industry. But if palladium were to have a use all on its own, its price would go up dramatically. Its availability is scarce so that a commercial system based on Case's concept would require a metal other than palladium or a very efficient way of recycling the palladium. We don't know as yet whether other metals produce the same effect in terms of the helium production. Les Case has studied several of the platinum group metals—palladium, ruthenium and the like, platinum and osmium, and has found that the effect is present with most, if not all, of the platinum group metals. This doesn't help much, because they are all precious, so we really need to find non-precious, non-platinum group metal which produces this effect.

My own view is the attempt to scale up is premature. We need to understand the mechanism, the process that we're studying. Once we understand what the mechanism is we will understand what metals or alloys might be satisfactorily used and perhaps optimized; maybe we'll get a larger effect. And only then can we explore the engineering applications.

This is the first Case vessel which we, in fact, obtained from Les Case in exactly the form in which he is performing his experiments in New Hampshire. This vessel, which we call "Vessel 2" and its twin experiment "Vessel 1" are our attempts to do Case's experiment in a similar geometry but a slightly more sophisticated apparatus.

The experiment over here on the left is an attempt to explore whether the convection of gas, that is the recirculation of gas, affects the rate of helium production.

The important parameters of all of these experiments are being recorded by a computerized data acquisition system are displayed on this screen. . .This is an indicator of one of the temperatures being recorded but, in fact, we are recording anything up to ten or twelve different temperatures in any set of experiments. All of these signals are recorded by computer and we are displaying the most important of them on this computer monitor, that is, the current, voltages and, therefore, powers going into each experiment; the temperatures and the pressures are recorded in each of these experiments. In this present configuration, we are making a measurement every five minutes and recording it to file so that we can analyze it off line to see, for example, whether there is any excess heat, pressure anomalies, leakage and the like.

In an experiment where we are interested in measuring the presence of excess power, we obviously have to record power very accurately and what we use is a Hewlett Packard computer-controlled power supply. Each one of these slots has a different power supply, all of them commanded by the computer to produce either constant current or a constant voltage displayed by the displays here but also recorded by the computer. It's a very stable power supply, very accurate and very constant.

Emerging BlackLight Power: Synopsis and Commentary

Mike Carrell

In January 1999, BlackLight Power Inc. (BLP) announced the discovery of a new family of chemicals with extraordinary properties, and moved their corporate headquarters to East Windsor, New Jersey. East Windsor is near Princeton University, an area with an impressive cluster of high technology laboratories. BLP's new facility was originally built by RCA AstroElectronics for satellite production and testing in 1958. With the purchase of RCA by GE and following mergers into Lockheed-Martin, satellite production was moved elsewhere and the facility was abandoned. Also nearby is the Forrestal Laboratory of Princeton University, site of the U.S. Tokomak project. It faces a bleak future, as Congress has cut funds for the failed hot fusion program.

BLP intends this facility to be a licensed laboratory with a staff of about 100 Ph.D.'s to support development of the power and chemical aspects of BLP, one of the very promising energy technologies of the twenty-first century. The full story of BLP is available at their website, http://www.blacklightpower.com. This site is uncommonly complete, with hundreds of pages of background, test data and theoretical discussion. Some of it requires the Adobe Acrobat reader, which is a free download, available by a click on the BLP pages.

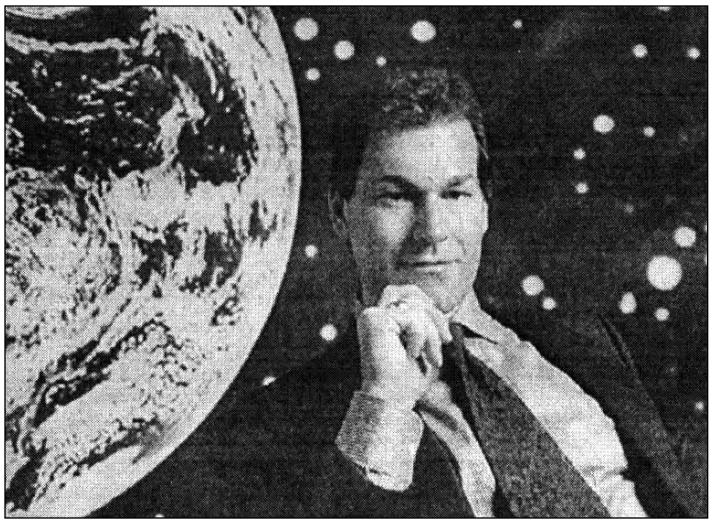
The term BlackLight Power comes from the extreme ultraviolet radiation which occurs in the BLP process. In the BLP process, hydrogen atoms are catalyzed by proximity to ionized potassium atoms, which absorb energy from the hydrogen atom and induce the electron orbit to shrink, yielding energy greater than conventional chemistry, but less than nuclear levels. The shrunken hydrogen atoms are called *hydrinos*. If an electron source is present in the reaction, *hydrino hydrides* can form, which are chemically active and can form the basis of a wide range of novel chemical compounds.

Origins of BLP Technology

In 1986 Randell Mills, M.D. was a graduate student at MIT in electrical engineering. In an inspired moment, he realized that the hydrogen atom and its orbiting electron could be represented in a novel way, an *orbitsphere,* which allowed the application of classical electromagnetism to the calculation of the emission lines of hydrogen from first principles. His theory also predicted that the orbit of the hydrogen electron can be induced to shrink to a lower energy state, with the release of large amounts of energy. The process is catalytic, requiring close proximity to other atoms that could absorb the released energy and remove it. These atoms must present a matching, resonant "energy hole" to absorb the released energy. Otherwise the reaction does not occur.

When the Fleischmann and Pons story broke in 1989, Mills realized that an electrolytic cell might provide an environment in which to test his theory. Mills searched for elements and ionization states which might provide the predicted energy hole. The most feasible was potassium. Mills built a cell with a nickel cathode and electrolyte of potassium carbonate in ordinary water. The cell produced excess heat without the long loading period then experienced in the deuterated cathodes of those following the Fleischmann and Pons system.

In the confusing reports in the "cold fusion" field at the time, it was not widely realized that the Mills cell is fundamentally different in the energy release process at work since the energy release is associated with a reduction in the electron orbit, not a change in a nucleus. Thus the Mills BLP process is a form of super chemistry, outside the realm of nuclear physics or "cold fusion."



Randell Mills, founder of BlackLight Power.

Mills founded Hydrocatalysis Corporation near Lancaster, Pennsylvania. He entered into a contract with Thermacore to build and test cells based on his theories. He also subcontracted work to Penn State University and to Lehigh University for measurements of reaction products in his cells, particularly evidence for hydrinos. He has followed a pattern of subcontracting work to selected university and commercial laboratories to good advantage. The cost of recruiting people and establishing a laboratory was avoided. Further, since the work was done by independent laboratories, their reports gave a growing credibility to the theoretical projections of Mills' theory.

Mills' theoretical base is delineated in his *Grand Unified Theory of Classical Quantum Mechanics,* a book of 557 pages, with a new, expanded edition due in February. In the GUT, he extends his calculations of emission lines from the BLP process and hydrinos to match known emissions from the Sun and deep space which had not been matched to known physical processes. The BLP website contains reproductions of essential theoretical and experimental data from his book.

Gas Phase BLP

While the $H_20 + K_2CO_3$ cells with nickel cathodes produced robust excess heat, and could operate above boiling with pressurization, the power density was not suitable for scale-up to serve the major utility industry. In 1995, he built a small cell to test a gas phase reaction between hydrogen atoms and ionized potassium as a catalyst. His theory predicted that the energy released in the reaction would appear as extreme ultraviolet radiation, so he changed the name of the company to BlackLight Power. It is intended that BlackLight will become a widely recognized trade name through press releases in late 1999 and products in the twenty-first century.

On the basis of the gas phase experimental results, in 1997 BLP raised $10.6 million in private placements, in which sev-

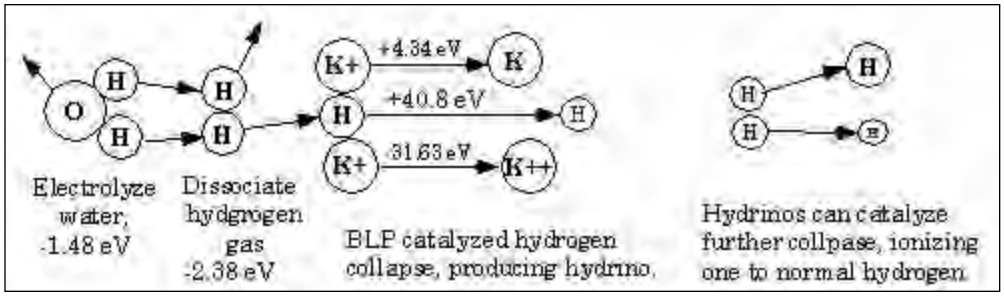


Figure 1. The BlackLight Power process.

eral large utilities participated.

BLP Commercial Strategy

Beyond the private placements, BLP expects to proceed by licensing its technology into various markets. In power generation, for example, there could be licenses for several ranges of power output, each with its particular market niche. For the chemicals, there could again be a range of identified chemicals and target markets. BLP can accept applications and then auction each segment with an up-front fee and continuing royalties. In this way, funds for continuing development may be obtained. Issuance of public stock remains an option, depending on later developments.

A key element in BLP strategy is a rechargeable battery technology based on hydrino hydride compounds. BLP projects that batteries with a specific energy of 600,000+ Watt-Hours/Kg can be realized. The impact of such technology on the transportation, power generation and distribution markets is discussed in the website.

The license route distributes risk and allocates to specialized companies the task of development for markets best known to the licensees. For this, BLP will need the high caliber staff they intend to hire, and will need the license fees and royalties to support such a staff in the Princeton area.

The BLP Process

Figure 1 is a graphical representation of the BLP process, which is most correctly expressed in the equations given on the website and in Mills' book. The energy released is many times that required to produce atomic hydrogen from water. Thus, BLP power generators use water as a "fuel" and release oxygen to the atmosphere.

Experiments and Validation

Physical theories are tested by comparing the numerical results of experiments and observations with calculations from first principles using the theory in question. Where two theories can each account for an experimental result, the theory which encompasses the widest range of observational data with the fewest assumptions is usually accepted. Mills' theory is remarkable, for he claims it to be valid over 45 orders of magnitude, from the nuclear to cosmic scale.

Observations of the sun and cosmos have shown radiation lines which have not been assigned to any known reaction or process. Mills' theory predicts many of the deep-space and solar radiation lines. If the sun were wholly powered by known nuclear reactions, there should be a neutrino flux much greater than has been observed. The energy release of the sun may be principally the BLP process, with nuclear processes playing a secondary role.

A theory which leads directly to a new experiment, which works immediately and robustly, deserves close attention. Such is the case with the electrolytic cell built in 1990. Mills predicted hydrogen collapse as a source of energy and found potassium carbonate as the most promising catalyst (others are mentioned in Mills' Australian patent—reprinted in *Infinite Energy* #17). It gave excess heat immediately, without the long loading period characteristic of the palladium-heavy water system disclosed by Fleischmann and Pons.

Later, electrolytic cells were built whose cathodes were hollow, made of thin nickel, with the interior evacuated and connected to analytical instruments. Hydrinos formed at the cathode diffused through the nickel cathode and were detected by gas chromatography. There are many other experiments showing the existence of hydrinos and hydrino hydrides on the BLP website.

Mills has published a few papers on his work in peer-reviewed journals. Much of the experimental work has been in other laboratories, some under subcontract, with information shared with specific agencies, corporations, and shareholders. Because of the proprietary value of the technology, there was little motivation to expose details until basic patent protection was in place. The business strategy of targeting large utilities as a primary market meant that the audience for the experimental results was a handful of high-level engineers and managers.

BLP then opened its website, with a major update in January 1999. It contains over 200 pages of detailed reports of tests of BLP cells and new compounds formed in the cells. Apparently taken from patent applications, the reports are detailed with respect to the laboratories used, the method of sample preparation, the analytical instruments used, and the test results. There is too much to reproduce here, but a sample will illustrate the character of the material.

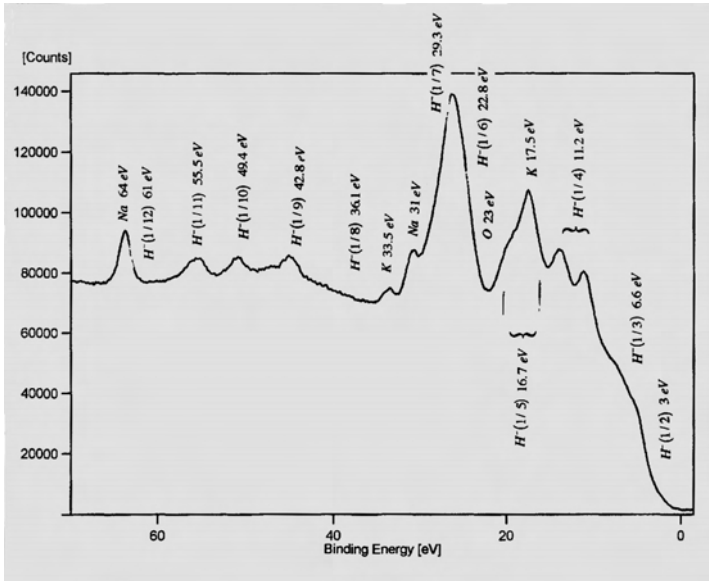


Figure 2. The 0 to 70 eV binding energy region of a high resolution X-ray Photoelectron Spectrum (XPS) of a glassy carbon rod cathode following electrolysis of a 0.57M K_2CO_3 electrolyte and storage for three months (sample #3).

Hydrino Hydride Compounds

Hydrinos are electrically neutral. If an electron source is available to the reaction, hydrino hydrides form which can combine with other elements to form novel compounds.

Look at the plot (see Figure 2) of the binding energy (energy necessary to liberate an electron by X-ray bombardment) from a glassy carbon rod which had been the cathode of electrolytic cell with 0.57 Mole potassium carbonate electrolyte for three months. The bumps in the curve are associated with binding energies calculated for hydrinos in various stages of collapse.

The laboratories and techniques used in this work include:

Laboratory	Analytical Test Performed
Lehigh University	X-ray Photoelectron Spectroscopy-XPS
Virginia Tech	Raman Spectroscopy
Charles Evans & Assoc. East	TOFSIMS, XPS, EDS, Scanning Electron Spectroscopy
Charles Evans & Assoc. West	TOFSIMS
Xerox	TOFSIMS, XPS
Physical Electronics, Inc.	TOFSIMS
Spectral Data Services	Proton & K NMR
Surface Science Associates	FTIR
IC Laboratories	XRD
Ricerca, Inc.	LC/MS
PerSeptive Biosystems	ESITOFMS
INP	EUV Spectroscopy
Galbraith Laboratories	Elemental Analysis
Franklin & Marshall College	XRD
Pennsylvania State Univ.	Calvet Calorimetry, XRD
TA Instruments	TGA/DTA
Northeastern University	Mossbauer Spectroscopy
M-Scan Inc.	FABMSMS, ESIMS, Solids Probe Magnetic Sector Mass Spectroscopy
Micromass	ESITOFMS
Southwest Research Inst.	Solids Probe and Direct Exposure Probe Magnetic Sector Mass Spectroscopy
BlackLight Power	EUV Spectroscopy, Cryopgenic Gas Chromatography, Thermal Decomposition/Gas Chromatography, Solids Probe, Quadrupole Mass Spectroscopy, MS of Gases, Calvet and Heat Loss Calorimetry

See Table 1 for a list of calculated binding energies of hydrino hydride ions for the first 14 stages (*n* parameter) of hydrino collapse. The text mentions *n* values up to 200.

Hydrides may lead to high value chemicals in the fields found in Table 2.

BLP Power Technology

BlackLight Power cells can be grouped into four categories (see Table 3). The website has drawings of test setups for each of these cells.

Commentary

Among the initiatives in the new energy field, Mills and BLP have made a lot of the right moves to attract financial backing and to proceed toward a potentially commanding market position with long-term pay back to the investors. He may eventually be ranked among Eastman, Land, Carlson, and Edison as a technical entrepreneur who built an industry on a personal invention. These built on existing scientific knowledge, but Mills has done something more in his Grand Unified Theory.

Table 1.

Hydride Ion	Binding Energy, eV	Hydride Ion	Binding Energy, eV
H^- (n = 1/2)	3.047	H^- (n = 1/9)	42.83
H^- (n = 1/3)	6.610	H^- (n = 1/10)	49.37
H^- (n = 1/4)	11.23	H^- (n = 1/11)	55.49
H^- (n = 1/5)	16.70	H^- (n = 1/12)	60.97
H^- (n = 1/6)	22.81	H^- (n = 1/13)	65.62
H^- (n = 1/7)	29.34	H^- (n = 1/14)	69.21
H^- (n = 1/8)	36.08	H^- (n = 1/15)	71.53

Table 2. Applications of Hydrino Hydride Compounds

Batteries (Automotive and Consumer), Etching Agents, Optical Coatings, Polymers and Synthetic Fibers, Masking Agents, Optical Filters, Isotope Separation, Semiconductor Fab., Agents to Dopants, Fiber Optic Cables,Refining Methods, Negative Ion of Electrolyte of a High Voltage Electrolytic Cell, Superconductors, Xerographic Compounds, Photoluminescent Compounds, Explosives, Propellants and Solid Fuels, Magnets and Magnetic Computer Storage Media, Photovoltaics, Cathodes for Thermionic Generators, Corrosion Resistant Coatings, Photoconductors, Industrial Cutting Materials, Heat Resistant Coatings, Phosphors for Lighting, Light Weight, High Strength Structural Materials, Proton Source, Thermionic Generator, Light Source

Such theories are ambitious and apt to be dismissed by critics, but its predictive power should draw the most careful attention, even though it may be modified in the future.

At present BLP has shown evidence of hydrinos, hydrino hydrides, intense energy release, and UV radiation in small scale tests. All this is significant. Commercial success of BLP requires scale-up to large and replicated reactors.

Originally BLP's strategy was based on power generation, retrofitting the boilers in existing utilities and designing smaller units for transportation. At present in the U.S. and many of the developed nations, fuel is cheap and there is little incentive to undertake the capital costs of a transition to a whole new power system.

Discovery of the hydrino hydrides changed all that, particularly the potential of the battery technology. If the theoretical promise can be realized, then the philosophy of power generation and distribution could change. For example, electric automobiles using BLP batteries might outperform internal combustion engines and run 1000 miles on a charge.

These systems will require extensive engineering. Much is in place, but the production and distribution of the necessary equipment will require some years of work.

The power yield of a given BLP reactor is strongly dependent on the autocatalysis sequence, where hydrinos catalyze other hydrinos to still lower energy levels. This is quite new, yet it must be managed to sustain the expected power levels. For example, the gas phase reactor operates at low pressure to minimize recombination of hydrogen atoms to molecules. Low pressure also reduces the probability of catalytic encounters between H and K^+ atoms. It also reduces the energy density and limits transfer of heat to the outside to radiation. Optimizing these factors could be the basis of future patents or trade secrets to maintain a controlling position after the basic patents expire.

In the chemical case, similar process control problems exist. The reactors can be expected to produce hydrino populations with a spread of degrees of collapse. These will have different

Table 3.

Power Cell Type	Catalyst	Dissociator	Operating Temp. Range	Typical Power Density	Application Areas
Electrolytic	K_2	Ni cathode and Ni or Pt anode	25 - 100 C	1 mW/cm2	Space Heating Long term production of hydrino hydrides
Low Temperature Gas	KNO_3	precious metal	250 - 350 C	50 mW/cm^2	comparable to a nuclear power plant, lower maintenance, no radioactive waste
High Temperature Gas	KI	transition metal, precious metal, or refractory metal	700 - 1200 C	100 mW/cm^2	fossil fuel boiler, hydrino hydride reactor
Ultra-high Temp. Gas	KI	refractory metal	1200 - 2000 C	in progress	hydrino hydride production, self sustaining operation

binding energies and different chemical properties. Separation and purification of such a mix is a classical chemical engineering problem, with a whole new chemistry to deal with. The toxicity, if any, of these new compounds may come into question if widely used.

The potential rewards of the BLP technologies will justify the combined efforts of BLP and members of alliances it may form with various industries. BLP's announced intention to hire 80 to 100 Ph.D.'s in the next two years shows an awareness of this problem. They will be needed.

Mills has maintained a careful distance from the "cold fusion" field, maintaining that the BLP process is exclusively chemical, having nothing to do with nuclear phenomena. In the current website text, "cold fusion" is dismissed as a failed effort.

Readers of *IE* are well aware of the problems associated with replication of the Fleischmann and Pons effect, the variability of cathode materials, and the difficulty of replication and scale-up. Yet, through the work of Arata and Zhang, Case and others, it is clear that a second set of energy processes exist which do not seem to involve the BLP process—catalysis and orbital collapse. In the A&Z cells, deuterium ions participate in the reaction. In the Case cells, no catalyst with the specified energy hole is present (unless specific catalysis conditions exist at the fractured surfaces of the palladium film). The energy densities in these reactions can be very high, with evidence of micro-explosions and melting in cathode materials.

Mills has staked out a claim to this territory in his Australian patent by noting that deuterium can also form hydrinos. Being smaller, and electrically neutral, the deuterons can approach closely with increase in the fusion probability. He terms this process Coloumbic Annihilation Fusion. It should be noted that if catalytic orbital collapse can occur for hydrogen, it should occur for other elements, leading to uncharted areas of chemistry.

The extensive family of transmutation reactions which are collectively known as Low-Energy Nuclear Reactions, or Chemically Assisted Nuclear Reactions, appear to lie outside the BLP universe.

The emergence of novel products and devices which demonstrate the new knowledge are keys to kindling a paradigm shift in physics to usher in the twenty-first century.

The Correa Invention: An Overview and an Investigation in Progress

Mike Carrell

This is a follow-up to the feature story on the Correa Pulsed Abnormal Glow Discharge reactor in *Infinite Energy* #7. Because of time limitations, one patent and parts of another and a laboratory report by the Correas were published in that issue. The patents and report are informative, but require careful study to extract the data of most interest to readers of *Infinite Energy*. In preparing this article, the author had the benefit of several long conversations with Dr. Correa, access to the full text of three patents, and some recent data taken with digital instrumentation.

What the Correa Reactor Does

The Correa's reactor produces short, repetitive pulses of electrical energy of multi-kilowatt magnitude which can be utilized to drive electric motors and charge batteries. The energy released is tens to hundreds of times that needed to excite the reactor. Sustained self-operation has been demonstrated.

In the course of development work on X-ray tubes, the Correas noticed some anomalous behavior in glow discharges. Further experimentation and literature search disclosed that under certain conditions, large bursts of energy are released in cold cathode discharge tubes. The bulk of the patents are devoted to means for reliably producing and enhancing the energy release, plus circuits for extracting and utilizing the energy. There are extensive and detailed test data, as well as a theoretical discussion.

The Source of the Energy

There is no obvious source for the energy bursts observed. In one of the patents, the Correas state:

> Any apparent imbalance in the electrical energy input to the system and withdrawn from the system by the its operator must be considered in the context of the entire continuum in which the system operates, within which it is anticipated that accepted principles of energy balance will be maintained.

In other words, the reactor is not a "perpetual motion" machine in a thermodynamically closed system—which is impossible. It is, rather, an "open" system, open to the active vacuum, aether, ZPF, or whatever name will be given to the energetic substrate of the universe. Dr. Harold Aspden has devoted many years to the development of an alternative physics which is relevant to the Correa's invention as well as other developments of interest to readers of *IE*.

The Essential Phenomenon

The Correa's reactor is simple: a partially evacuated tube with two or three electrodes, as shown in Figure 1. The cathode area is large—128 square cm in some test samples; the area of the anode and the probe are less important. Other electrode configurations, such as a cylindrical cathode with an axial anode, are possible. Electrode spacing from a few centimeters to 20 cm are useable.

Impressing several hundreds of volts between the anode and cathode sets up an electric field which will accelerate any stray electrons sufficiently to ionize gas molecules. Electrons, being negative, are attracted to the positively charged anode. The positive ions, are attracted to the negatively charge cathode, but being much heavier, move more slowly. Conventional discussions of the gas discharge phenomena focus on electron behavior. The phenomena in the Correa's reactor are much more complex and will be discussed below.

Limiting the current flow and allowing it to increase, while measuring the voltage across the reactor, produces the curve of Figure 2, which illustrates typical behavior of the cold cathode discharge as it is generally understood. In regions where the voltage increases with current, the reactor exhibits positive resistance and its operating condition is stable. In regions where the voltage decreases with increasing current, the reactor exhibits negative resistance and is unstable.

In the Normal Glow Discharge region, the cathode becomes covered by a glow which is characteristic of the gas in the tube. This glow is commonly seen in neon indicator and decorative lamps; in the decorative lamps the current is limited so the glow does not cover the cathode and it flickers unstably.

When the current is allowed to increase, the glow covers the cathode, and then has nowhere to go. The voltage increases rapidly and the pinch effect begins to concentrate the ion flow into a smaller region. What usually happens is that the ion bombardment causes local thermionic heating of the cathode, releasing a flood of electrons and the glow collapses into the Vacuum Arc Discharge region. This is seen in fluorescent lamps, advertising signs, and high intensity flood lamps.

The Correa's patents show how to avoid the arc discharge

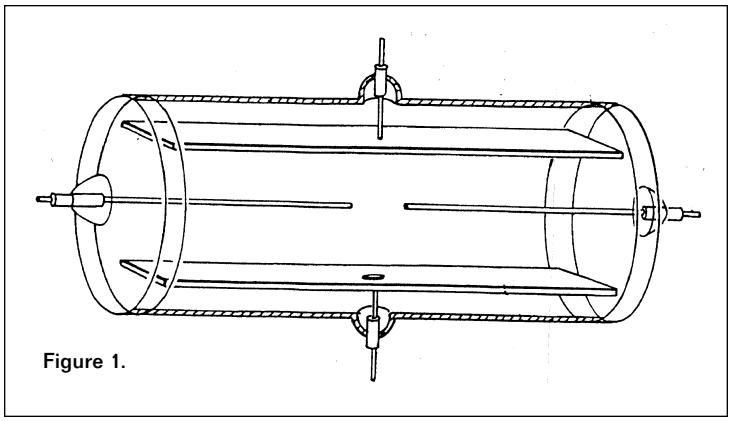


Figure 1.

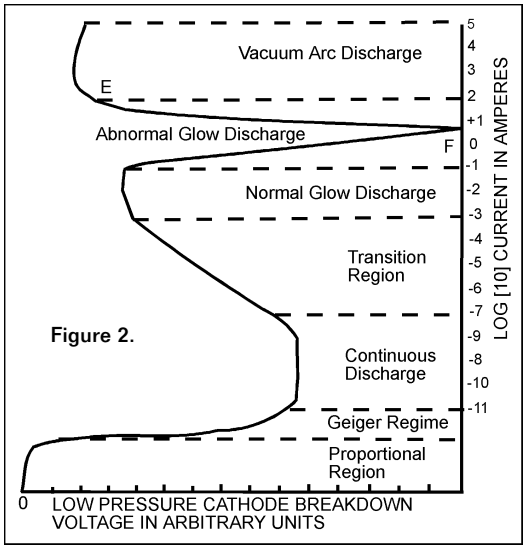


Figure 2.

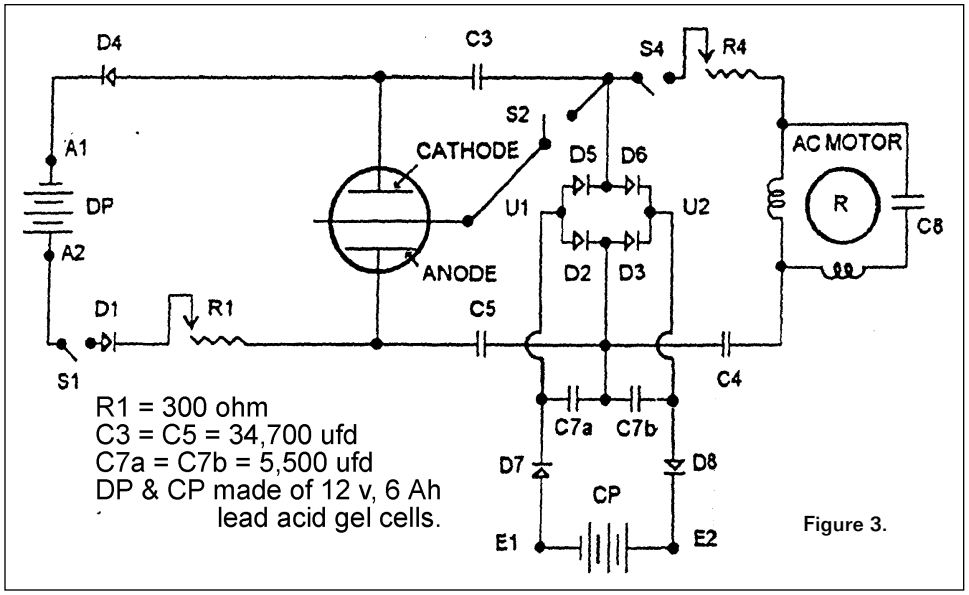


Figure 3.

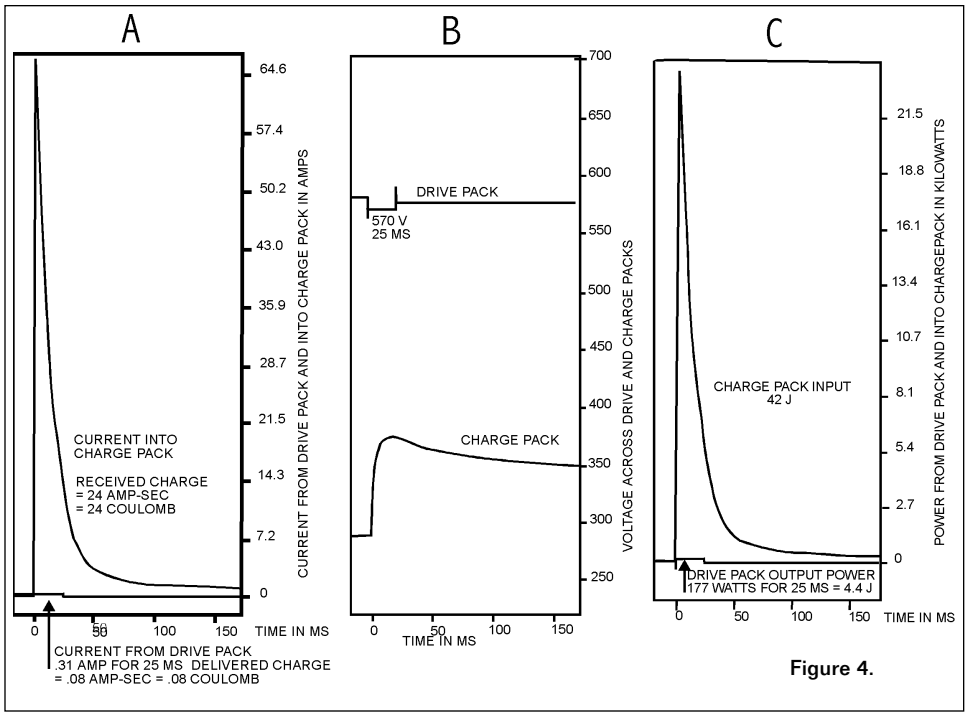


Figure 4.

and operate in the Abnormal Glow Discharge region, with currents in the range of 0.1-10 amperes. It is in the region from F to E that the Correas discovered the energy burst which is the foundation of their invention. With proper construction and operating conditions, the energy bursts are repetitive and self-extinguishing and the reactor is quiescent between bursts.

The Pulsed Abnormal Glow Discharge, PAGD: Is There Anomalous Energy Production?

The Correas present several kinds of evidence. Tests made with the circuit of Figure 3 show that a PAGD reactor can charge batteries and run motors, using less energy from the exciting battery than is delivered to the loads.

There is, in addition, self-sustaining operation in which net energy is produced without external input.

PAGD Utilization

The PAGD energy burst is electrical, so means are necessary to set up the PAGD conditions and capture the burst energy for external utilization. The Correas have invented a number of electrical circuits of which Figure 3 is representative, being used in the tests documented below.

A stable source of DC to set up the PAGD is provided by the Drive Pack (DP) battery with terminals A1 (+) and A2 (-). The current flowing from the Drive Pack is limited by the resistor R1. Diodes D1 and D4 prevent current from flowing into the Drive Pack from the energy burst in the PAGD reactor. Capacitors C3 and C5 couple the energy burst to the load while preventing any continuous discharge of the drive Pack into the load. Diodes D2, D3, D5, D6 comprise a full wave rectifier which charges capacitors C7a, C7b. Diodes D7 and D8 allow current to flow only into the Charge Pack battery, CP.

An auxiliary circuit containing an AC motor can also be driven by the PAGD reactor by closing switch S4 and choosing appropriate values for R4, C4, C8. Patent '391 has extensive information on test results with motors.

The reactor can be operated as a diode, or as a triode by closing switch S2.

Dr. Correa has furnished the author with several oscillograms taken with high performance digital instrumentation since the patents were filed. Three of these have been scanned and carefully traced using CorelDraw and reproduced as Figures 4a - 4c. In these, Voltage In is across A1 and A2, and Current In is that going into A1. Similarly, Voltage Out is across E1, E2 and Current Out is into E2. The power curves were calculated for each sample from the raw data.

The sampling interval was 80 ms. Essentially, the energy burst charges C7a and C7b, which then discharge into the Charge Pack. Capacitors C3 and C5 reach full charge in about 3.2 ms, which suggest that the peak energy in the burst is much higher than shown in Figure 4. When the reactor extinguishes at the end of 25 ms, charge stored in C7a and C7b transfers smoothly to the CP. The values for Coulombs of charge transferred and Joules of energy were obtained by careful reading and graphical integration of the original plots, which are detailed enough to show the values for each of the samples.

Figure 4 clearly shows substantial over-unity performance. It also indicates the difficulties in study, documentation, and utilization of the PAGD phenomenon. The data of Figure 4 are for one specific set of conditions, with a pulse rate of 0.5 pps. For higher rates, the peak values are less.

One develops a burning curiosity about the voltage and current waveforms at the reactor itself, but these, alas, remain the proprietary information of the Correas.

Measurements in the '989 Patent

Without the present instrumentation, the energy bursts could be observed, but not directly measured. And for practical

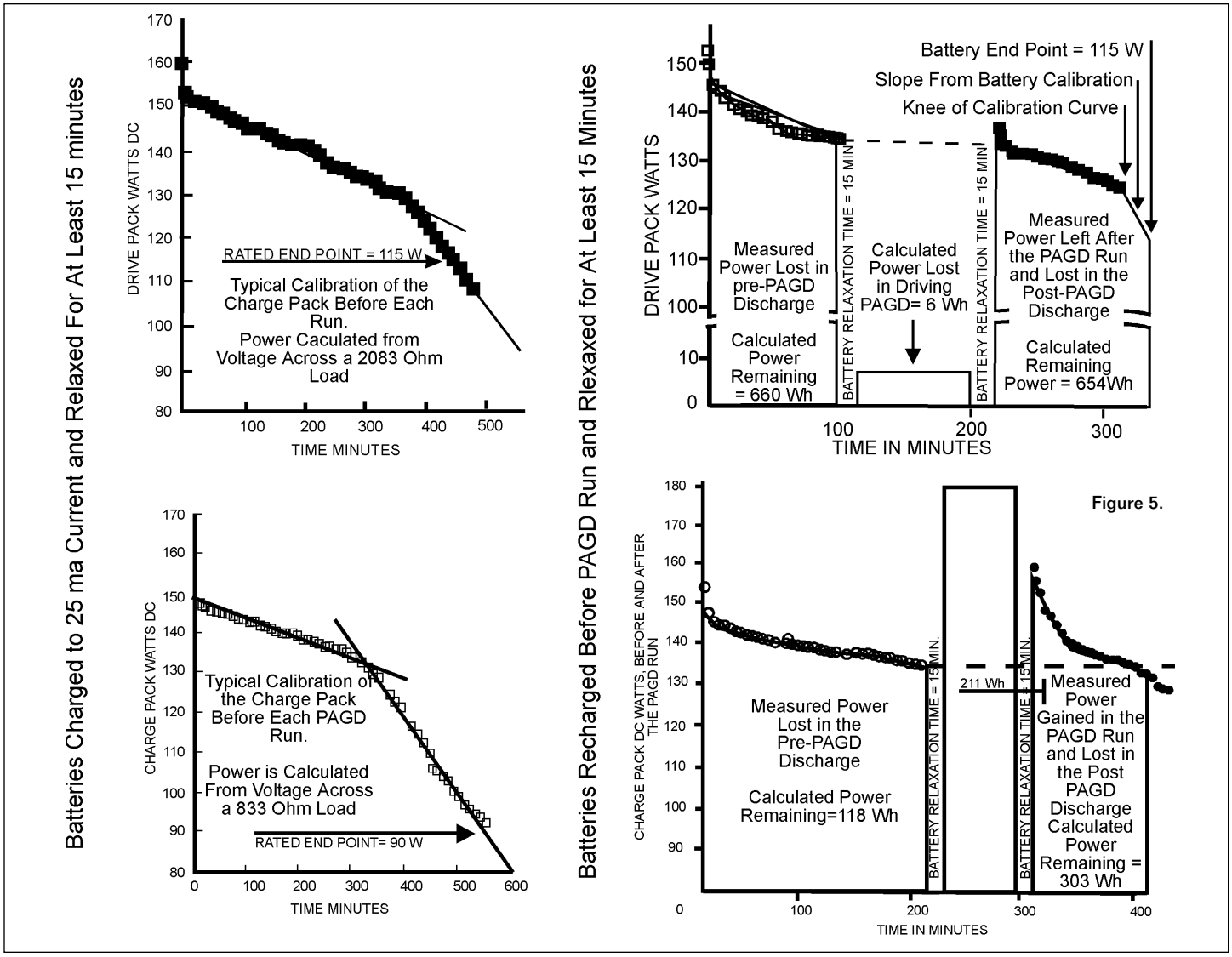


Figure 5.

utilization, sustained runs were necessary. The Correas used calibrated batteries for the Drive and Charge Packs. (Newman cited the extended performance of batteries driving his Energy Machines as a proof of the unusual characteristics of his developments. Newman used primary cells, introducing many uncertainties in evaluating his results.)

The Correas are well aware of the problems in measuring energy with batteries. The '989 patent contains an extensive discussion of four different strategies and their weaknesses, resulting in an experimental protocol which is illustrated in Figure 5. This illustration is a composite of scans of the patent illustrations, with some additions and changes to clarify the protocol.

Pre Charge: The Drive and Charge Packs consisting of 12 V, 6 Ah gel-cells, are each charged in a normal fashion. Full charge is taken as the point where the charge current drops to 25 ma. The Packs are are allowed to relax for a minimum period of 15 minutes, but extended for experimental convenience.

Pre-Run Charge: The batteries are charged again as before.

Pre-Run Discharge: The batteries are again partially discharged for over an hour, taking enough data points to establish each battery's characteristic against its immediately previous calibration.

Test Run: During the test run, the Drive Pack will lose energy and the Charge Pack will gain energy. The batteries are allowed 15 minutes to relax from the stress of discharging or charging.

Post-Run Discharge: The load resistors are again connected and voltage readings taken until the batteries' discharge characteristic tracks the previous calibration curve. It is then possible to estimate the energy lost by the Drive Pack and the energy gained by the Charge Pack, and calculate an efficiency as shown in Table 8 of the '989 patent. Figure 5 is based on Run 3 of that table, and the battery calibration curves of Figure 5a and 5c were taken the day before Run 3.

One difficulty with the above protocol is that the measure of the energy loss of the Drive Pack amounts to the difference between large numbers, and appears in the denominator of the efficiency calculation. The result is thus vulnerable to measurement errors.

The presentation of data in Figures 16 and 17 of the '989 patent has a number of difficulties, which become more apparent with careful examination. Indeed, sparing the reader that difficulty was the motive for reformatting the data as seen in Figure 5.

A compensation for the need for elaborate calibration procedure is that the measurements are all DC, with no uncertainties from power factor, phase, and rise time as seen in Figure 4.

"Videographic" Data

Figure 20 of the '989 patent shows battery power on a running sample basis. It is reproduced in simpler form as Figure 6.

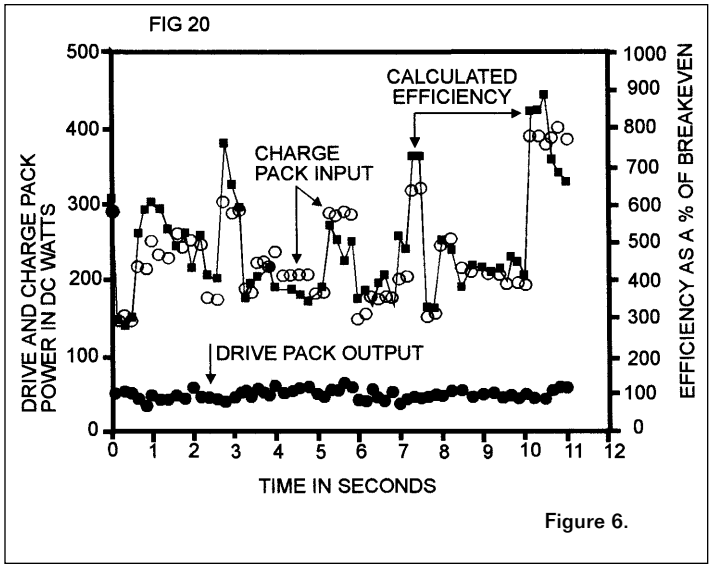


Figure 6.

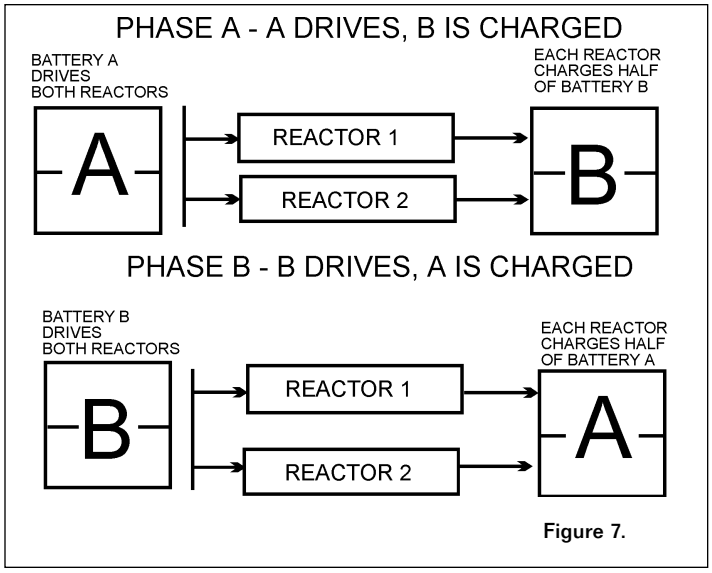


Figure 7.

A bank of Beckman RMS multimeters were set up to measure the voltage across and current through each Pack. The meters were then photographed with a video camera. Played back in a stop frame mode, it was possible to read the meters and perform power calculations at 1/30 second intervals. In Figure 6, the points a the bottom, clumped into a irregular line, are the power input from the Drive Pack. The circles are power to the Charge Pack, and the black squares the calculated efficiency for each set of measurements.

The Beckman multimeters utilize a RMS module from Analog Devices. The module contains a precision full-wave rectifier and a logarithmic squaring circuit, followed by a low-pass filter for averaging. The instrument will indicate true RMS within a range of input waveforms.

The illustrated tests were for Run 6, and the waveform data, it is probable that the Beckman multimeters were not giving accurate readings because of the low duty cycle of the pulses. The errors would affect the input and output measurements in similar ways, so Figure 6 can be taken as an interesting illustration of another aspect of the over-unity performance of the PAGD reactor.

While there are a number of criticisms which could be made of one or another aspect of the protocols, a honest study of the patents will show a thorough awareness of the uncertainties in the use of batteries, and careful, systematic characterization of the batteries at hand.

Self-sustaining Operation

When discussing over-unity performance, endless measurement is no substitute for self-sustaining operation with no apparent external input. The Correas have achieved this with two PAGD reactors and a battery-swapping procedure. The circuit arrangement is given in the '989 patent, with a schematic summary in Figure 7.

The Charge Pack must always be at a lower voltage than the Drive Pack. Two center-tapped battery packs are used. The full pack is used to drive the reactors, each of which charges half of the second pack. The roles are then switched.

In one test cited in the patent, the battery swapping was continued for eight hours, with both packs gaining charge. There was no external energy input. Dr. Correa indicated that this is done automatically in more recent implementations, not covered by the available patents.

Is PAGD Just a Strobe Oscillator?

The circuit of Figure 3 bears a superficial resemblance to an ordinary strobe lamp, where a capacitor connected across a discharge tube is charged to the breakdown potential, initiating a Vacuum Arc Discharge in which the peak power can be very high. In the circuit shown current from the DP flows through, and charges, the CP as the capacitors C3, C5 are charged. When the discharge occurs, a portion of the charge in C3 and C5 is again transferred to the CP, by virtue of the full-wave rectifier D2, D3, D5, D6.

Considering this hypothesis, in a first approximation the voltage of CP opposes that of DP, and R1 is 300 ohms. Using voltages from the example in Fig. 5, the available charging current is (580-300) / 300 = .93 A. Using the 70 minute test run of Figure 6, about (.93)(300)(70) / 60 = 326 Wh would be transferred from DP to CP, which is greater than the 211 Wh calculated for the test illustrated in Figure 6. This hypothetical scenario would produce flashes in the reactor tube and charge the CP from the DP, but would not show over-unity performance.

What Actually Happens in the PAGD Run

Actual measurements of the current out of the DP and into the CP in Fig. 5A show that no current flows out of the DP or into the CP except from the PAGD energy pulses. In the hypothetical case proposed, an average current of .93 A should be seen flowing out of the DP at all times, and would be easily seen in the instrumentation plots.

What is seen is a current pulse of .31 A amplitude and 25 ms duration, coincident with the energy pulse in the reactor. In that same time period, a current pulse peaking over 65 A goes into the CP. At the end of the 25 ms pulse, the current out of the DP drops to background levels.

The hypothetical strobe oscillator mode described might occur in the absence of the specific conditions of the PAGD. However, in PAGD the energy eruption drives the nominal cathode both positive and negative, and may leave C3 and C5 temporarily charged so that D1 and D4 block current flow from DP.

Thus the hypothetical strobe lamp mode does not actually occur, and the evidence points to over-unity operation.

The PAGD phenomenon, with its energy yield, can be evoked without C3, C5 or any of the attached circuitry—all of that was developed simply to couple the energy burst to useful external devices. What is essential is field-effect emission from the cathode.

Comments on the PAGD Phenomenon

The Abnormal Glow Discharge is well known, as are anomalous forces and energies associated with plasma discharges.

In 1969, Manuel patented a coating process utilizing the AGD, with external controls to prevent the AGD from entering the VAD region. It did not generate energy, nor were the pulses self-triggered.

The PAGD phenomenon is complex. In addition to ions and electrons originating in the gas, cold-cathode auto-electronic (field effect) emission from the cathode contributes a substantial electron flow. The ions are attracted to the cathode, and the electrons to the anode, but there is a third flow of atoms to the anode, effectively neutralized by the electron stream.

This third flow was observed as far back as 1930, by Kobel and Tanberg in published reports on forces reacting on cathodes in Vacuum Arc Discharges in Physical Review. Tanberg measured a vapor velocity of 16 x 10^6 cm/sec.

Aspden, in a paper "The Law of Electrodynamics" in the *Journal of the Franklin Institute* in 1969, notes that where charge carriers differ markedly in mass—as with ions and electrons in plasmas—very strong longitudinal forces can appear.

These ideas are developed more fully in a privately published Energy Science Report No. 8, *Power from Space: The Correa Invention*. He proposes a radial separation between the ions and electrons at the cathode which sets up strains in the aether, releasing substantial energy.

Aspden's reasoning is consistent with the appearance of spherical or conical plasma balls on the cathode with each energy burst, shown in photographs in *IE* #7.

The cathode is eroded by the PAGD process, some portion of it being vaporized. In a sense the cathode material is a "fuel" consumed by the process. It is more likely that this is a result of the energy release, rather than the cause of it.

The cathode pits have been measured by Correa. The material removed is not adequate to produce charge carriers for the output current pulse.

In patent '391, the Correas refer to their reactor as a transducer of energy, which is an appropriate description. In the form illustrated in *IE* #7, the PAGD reactors are laboratory prototypes, built around 1992. Since then, significant advances have been made in smaller and larger configurations which address the cathode erosion problem to extend the working life of the devices.

A current development target is a reactor 80 cm long, 10 cm dia. with a power output of 5 kW and a operating life of two to three years. At present, the Correas are at about a 1 kW level. There is reason to believe that the reactors can be made smaller and the operating voltage reduced. They are having discussions with potential licensees.

Dr. Paulo and Alexandra Correa.

The Correa PAGD Reactor: Errata and Supplement

Mike Carrell

Introduction

Issue 8 of *Infinite Energy* contained an overview of the Correa invention, based on three issued U.S. patents and discussions with Dr. Correa. The readership of *Infinite Energy* includes subscribers to an Internet listserver called Vortex-l, constituting an informal discussion group for the range of topics included in *Infinite Energy*.

The group includes many professionals in a variety of disciplines, who accept the possibility of new energy phenomena, but vigilantly examine each new device or process. The author is indebted to members of Vortex-l for pointing out some errors in the previous article and areas where more data and clarification is needed. In particular, Dr. Mitchell Swartz found errors by the author in Figure 4 of the previous article, and showed a need for clarifying some points concerning Figure 5.

The author is indebted also to Mark Hugo, Bob Horst, Michael Schaeffer, and others for a spirited discussion. The following material is the responsibility of the author, and carries no implied endorse-ment by Dr. Swartz, Dr. Correa, or others.

Errata, Figure 4, p. 11, *IE* #8

The three curves and scales are faithful copies of original data provided by the Correas. The author made three errors in supplemental calculations done for the convenience of readers.

In Issue #8 Figure 4a, the "Delivered charge (from the Drive Pack)" should be 0.008 amp-sec = 0.008 coulomb. The "Received Charge" (by the Charge Pack) should be 1.2 amp-sec = 1.2 coulomb. Corrected numbers now appear in the adjacent Figure 1. *The charge out/in ratio is 150.*

In Issue #8 Figure 4c, the "Charge Pack Input" energy should be 445 joules. Again, corrected numbers now appear in the adjacent Figure 1. *The energy out/in ratio is 101*. This ratio is not the same as the charge ratio because the DP delivers its charge from a source at 570V and the CP receives its charge as a sink at about 380V.

Batteries as Energy Integrators

Figure 5 of the previous article and its related text outlined a procedure used by the Correas to integrate the energy input and energy output of the reactor to test the performance of various reactor configurations. There are many uncertainties in using lead-acid batteries for this purpose, but the gel-cell construction used for the Correa tests is recognized by the industry as being the most stable, repeatable form of the lead-acid battery. Figure 5 is a graphical illustration of the procedure used, but certain points were left unclear.

Before each PAGD run, the DP and CP batteries are each calibrated by fully charging, then discharging through fixed resistors, with the battery output power measured at frequent intervals and recorded in graphical form.

Just before a PAGD run, the batteries are again charged and partially discharged, using the same load resistors as before. This is, in effect, a new partial calibration.

The PAGD run is then performed. In the case of Figure 5, the run was 70 minutes. Any apparent differences are due to the

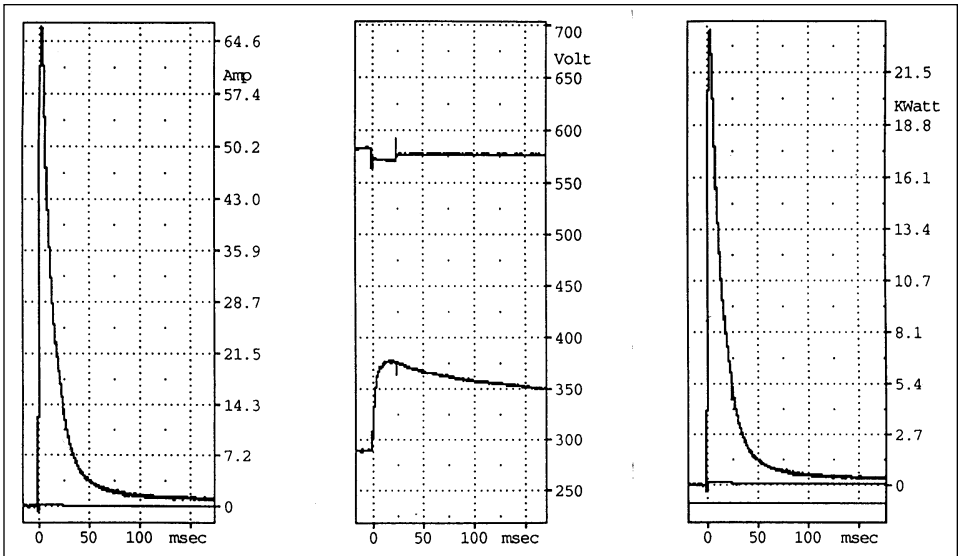


Figure 1.

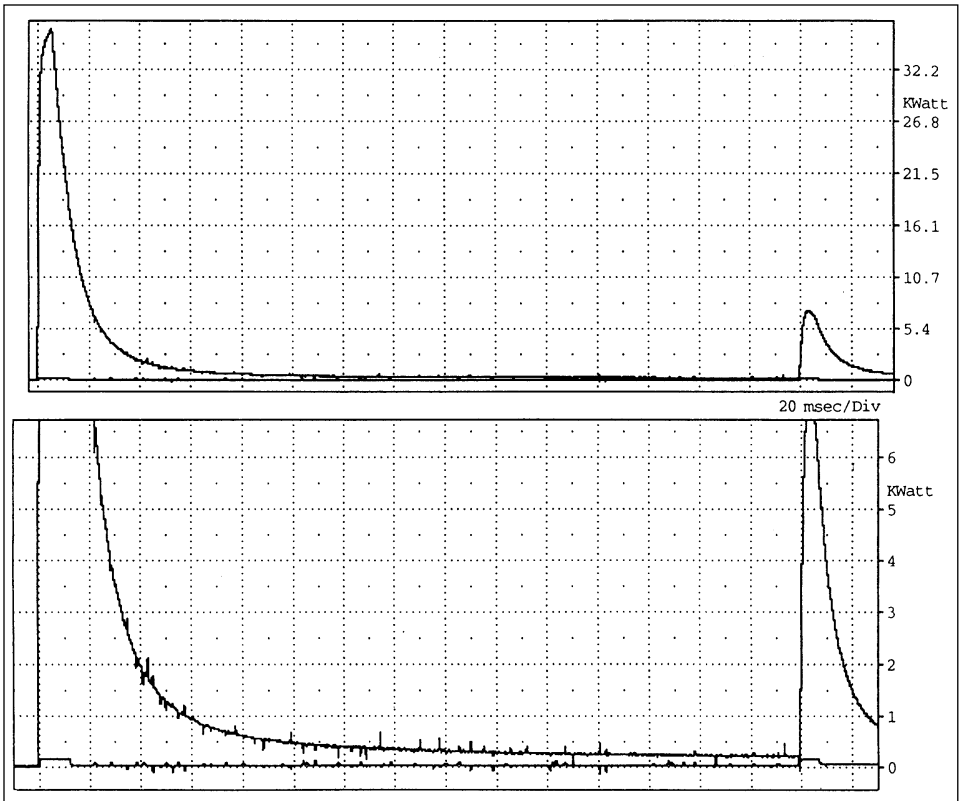


Figure 2.

different scale factors and slight errors in constructing the plots.

After the PAGD run, the batteries are again connected to their resistive loads and the discharge continued, with power measured at frequent intervals.

The discharge data taken just before the PAGD run can then be mapped against the data taken in the previous complete calibration to establish the state of charge of each battery. There is no requirement that the preliminary discharge times be the same; in the case of Figure 5, the DP is discharged for about 100 minutes and the CP for about 220 minutes. It is only necessary that enough points be taken to compare with the previous calibration curve to establish confidence in the estimation of the charge state of the battery.

The discharge power data for each battery taken after the run is mapped against the previous calibration curves to establish the new charge state of the batteries. Again, it is not necessary to discharge each battery fully, only to obtain enough points that the mapping can be done with confidence. In each case the batteries were discharged for over an hour.

With all these precautions, the calculation of energy out/in is sensitive to errors involving differences of large numbers, such as the determination of the 6 Wh energy loss of the DP. Table 8, p. 38 of *IE* #7 contains summaries of six runs using different reactor configurations. The energy out/in ratios range from 4 to 34. OIF these, the run illustrated in Figure 5 has the least energy spent and the greatest gained, and the greatest sensitivity to measurement errors. But all show substantial over-unity performance.

The four curves in Figure 5 of the previous article *all deal with one experiment*, although features of the several curves could make it seem that unrelated measurements are grouped together. In particular the two curves at the right depict the separate calibrations performed before and after the PAGD runs, as noted above. The time scales are intended to indicate proportionate durations of the elements of the calibrations and runs, not clock time. The two battery packs have their own histories of charge and discharge, which are coincident in clock time only during the PAGD run. The rest times are indicated as a minimum of 15 minutes, as dictated by good practice, but there is no definite maximum time, which is unrecorded and unrepresented in the graphs.

Closed Loop Tests

A crucial test of o/u claims is the capability to operate the device without external or stored (battery) power input, while still producing tangible work.

The previous article described a test the Correas performed using two reactors and two center-tapped batteries, illustrated schematically in Figure 7 of that article. Numerical data from the test is not publicly available, but Dr. Correa told the author that it was run for eight hours, with the batteries switched hourly by operation of a single switch. During this time, both batteries gained energy, as measured by power into a load resistor.

Why Batteries?

Many observers express dissatisfaction with batteries as integrators in quantitative measurements. The author has discussed this with Dr. Correa on many occasions. One answer emerged from early tests with the characterization tests

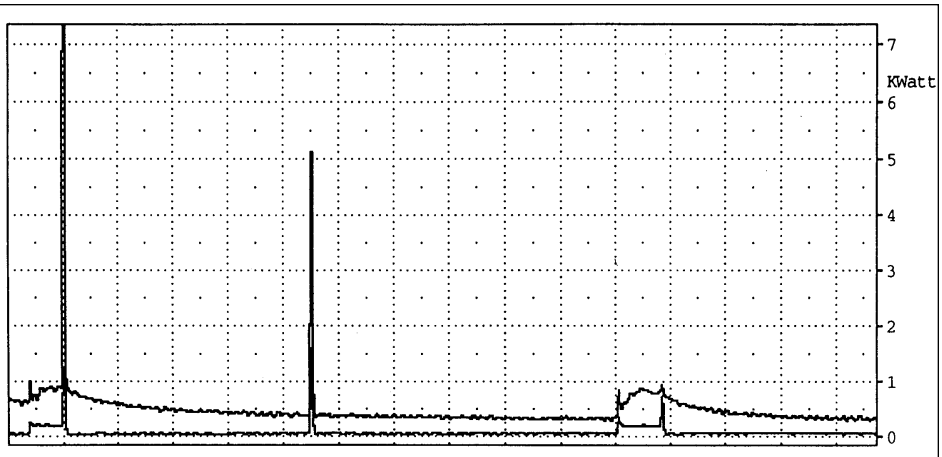


Figure 3.

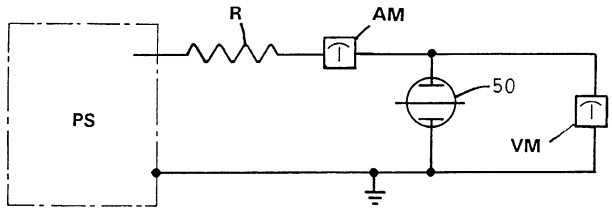


Fig. 20A

Figure 4.

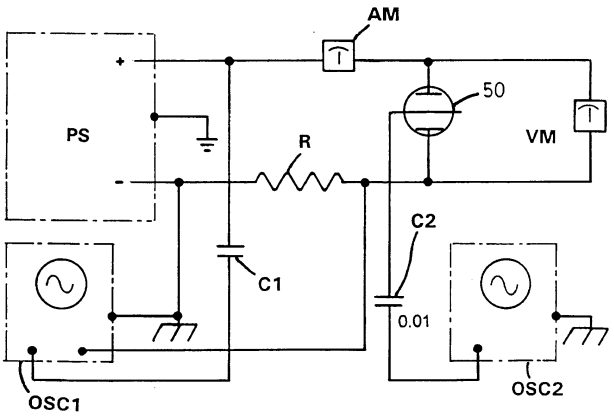


Fig. 20B

Figure 5.

described below. When the PAGD event occurs, a very powerful burst of energy is released within the reactor space and more than one electrical power supply and regulator have been burned out. Batteries are rugged sources of energy.

Relaxation Oscillator?

When claims are made for a new phenomenon, particularly in the search for o/u performance, it is proper to first search for a conventional explanation for the effects seen.

Some readers have noted a similarity between the test circuit shown as Figure 3 of the previous article, and the well-known discharge tube relaxation oscillator. In this circuit, a capacitor shunts a discharge tube and is charged through a series resistor. When the capacitor charges to the tube's breakdown voltage, the accumulated energy is discharged through the tube. Repeated powerful flashes, as in a strobe lamp, result.

In this case, the energy is first stored in the capacitors, then released in the discharge tube. The test circuit shown as Figure 3 of the previous article bears a superficial similarity to a relaxation oscillator, but it does not operate as such nor does the PAGD phenomenon depend on it.

Evidence Against "Relaxation Oscillator"

The evidence is three-fold. First is a brief recapitulation of the analysis in the previous article. Second is a discussion of two oscillograms provided by the Correas, not included in the previous article. Third is a review of the extensive characterization of the PAGD phenomenon in circuits which are clearly not relaxation oscillators.

Refer now to the analysis in the previous issue. If the test circuit of Figure 3 (of the previous article), were a relaxation oscillator, a current of .93 A should flow from the DP, charging C3 and C5 with the energy to be released in the reactor flashes. A current of .93 A from a source of 570 V is 530 W. Referring to Figure 1 of this article, the power from the DP is the lower curve in each graph, and the power to the CP is the upper curve. The bottom graph has an enlarged Y-axis, with major divisions representing 1 kW.

If the circuit were operating in a relaxation oscillator mode, the 530 W power from the DP would be clearly visible. It is simply not there.

Figure 2 shows two pulses of a three pulse set of data for a run which begins with "No plasma discharge; background levels for input and output" (Correa notes). The total data set duration was 780 ms. The repetition rate was 2.8 pps. Energy in from the DP was 48.4 joules and energy out to the CP was 1071.9 joules (Correa data).

In the lower graph, the minor divisions represent 125 watts of power on the Y-axis and 2.5 ms of time on the X-axis.

The curves have the same general aspect as Fig. 4c of the previous article. The power from the DP is represented (for the first event) by a pulse of about 200 W lasting for about 12.5 ms.

Immediately after the event, the DP power output drops into the noise floor of the instrumentation. The presence of noise spikes shows that the instrumentation is active and sensitive to powers in the tens of watts. Just before the second event, Correa notes "No plasma discharge, input background level." After the second event, the power From the DP does go immediately to background levels, but decreases with time (Correa comment), until the next event.

Figure 3 shows another example taken from a longer run. Again, the upper curve is the power delivered: to the CP and the lower is the power extracted from the DP. Once again, the instrumentation is sensitive enough to detect noise, and there is essentially no power drain from the DP between pulses.

The three sets of data, two from this article, and Figure 4 of the previous article, suggest that the energy events take many forms, depending on operating conditions, but there is a consistent aspect of over-unity performance. Also illustrated is the difficulty of measurements which would unequivocally show o/u performance.

PAGD Characterization

U.S. patent #5,502,354, issued 26 March 1996 to the Correas contains data from early work in characterizing the PAGD phe-

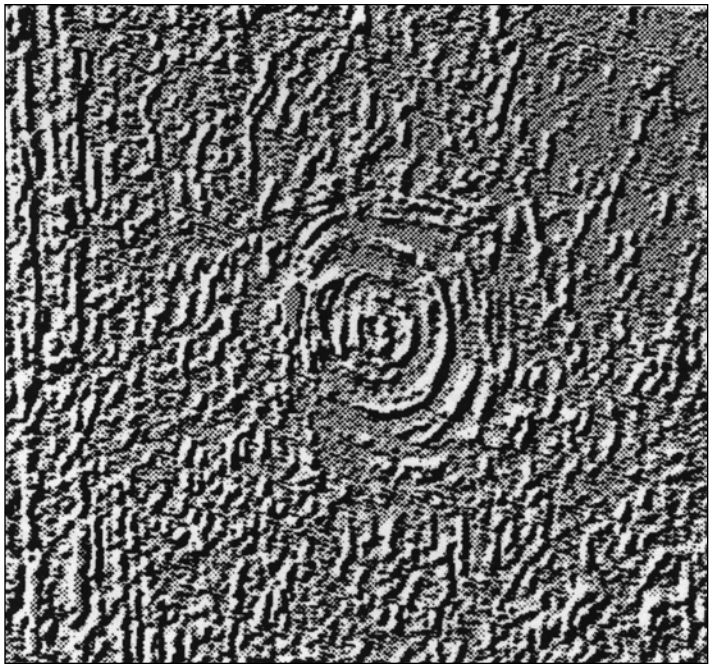


Figure 6. Erosion of an aluminum PAGD cathode.

nomenon itself. For the purpose they used two test circuits, which are reproduced here as Figure 4 and Figure 5.

The only shunt capacitor is in the power supply output, and the ballast resistor R. in the range of 10^3 to 10^6 ohms, isolates it from the reactor. This is not the traditional relaxation oscillator circuit, where the capacitor shuts the discharge tube itself.

The patent contains 15 graphs and 17 tables in 38 pages, showing the effect of various parameters on the performance of the PAGD reactor. Salient features of the discussion are noted below, emphasizing the nature of the PAGD phenomenon as distinct from the prior art.

PAGD Characteristics

1. Repetitive, self-extinguishing, energy-producing discharges with only a stable voltage source and current limiting resistor as external circuit support.
2. Pulse rate a function of electrode area and spacing, reactor gas pressure, drive current, electrode composition, source voltage.
3. Static capacitance of reactor in the range of a few pF. Pulse rates in the range of 0.01 to >5,000 PPS. Series resistors used in tests in range 10^3 - 10^6 ohms. Such numbers are inconsistent with assumptions of relaxation oscillator action.
4. Strong field emission is necessary and is observed at field gradients lower by a factor of 10^5 than that predicted by theory.
5. Absence of thermionic emission from the cathode.
6. Pulse energy out/in ratios ranged from 1.5 to >50 for examples tested. Power output ranged from 25 to 400 watts.
7. Based on charge production and source voltage a dynamic capacitance for different reactors can be estimated at 100 mF to 80,000 mF.

Cathode Erosion

Aluminum alloys are a preferred material for the cathodes and are eroded by the discharges, as illustrated in Figure 6.

Measurements of the material ejected from the cathode indicate 5.8×10^{-8} g, or 1.3×10^{15} Al ions per pulse for one case. The kinetic energy of the ejected material is more than three orders of magnitude greater than found by Tandberg for a Vacuum Arc Discharge.

Reaction Forces against the cathode in excess of 300 dynes have been observed in PAGD experiments.

The erosion suggests the presence of energies only three orders of magnitude less than Aspden's estimate of the energy priming the vacuum. The energies are of the same order as those found in water-plasma arc explosions by the Graneaus.

This erosion leads to questions about the working life of a PAGD reactor. Estimates in the patent suggest a lifetime energy output of as much as 40 megawatt-hours for a reactor. While the electrode material is eroded, there is no suggestion that the energy source is a nuclear reaction in the electrode.

PAGD Phenomena

The pulse repetition rate depends, among other things listed above, on the time it takes charge carriers to redistribute within the reactor volume after a discharge event.

The event begins with the driving current enabling a glow to cover the cathode and go into saturation. Beyond that, the voltage across the electrodes rises and the field effect emission begins. The glow is attenuated and a cone-like discharge column appears with a faint glow elsewhere on the cathode.

Within the discharge column, very intense energy is released, resulting in the pitting mentioned above, but also releasing electrical energy which is captured by the output circuit of Figure 3 of the previous article.

Examination of the discharge column suggests the presence of a vortex of energy, which is also suggested by the photomicrograph of a crater in Figure 6. The work of Aspden indicates a segregation of positive and negative charge carriers, and the development of strong longitudinal forces within the discharge column.

The above comments only point to the existence of a very complex phenomenon discovered by the Correas, which warrants study in its own right.

THE REALITY OF PERPETUAL MOTION

Harold Aspden

Introduction

Having been appointed a new member of the Scientific Advisory Board of *Infinite Energy*, I have been asked by the Editor-in-Chief, Dr. Mallove, to submit an article "synthesizing from my previous work." That is a rather daunting task, but I will do my best here to provide a general outline of the theme I have followed over the years.

I will have much more of a specific nature to say quite soon in future writings, but think it best here to introduce how I view the "free energy" field.

Based on simple Newtonian mechanics, perpetual motion within a purely mechanical device is impossible. However, once one understands the true electrodynamic nature of the force of gravity and how interactions are set up which involve energy transfer between electric charges, then perpetual motion, subject to machine wear and tear, is just a matter of exercising one's ingenuity. This is because electric charges in motion interact as a function of motion relative to one another and by virtue of their interaction with the all-pervading subquantum medium that some call a frame of reference, but which we will here refer to by its proper name, the aether.

There are three ways in which one can contemplate building a "perpetual motion" machine, which is really what we are all talking about when we use terms such as "free energy." They are:

1. Build something you imagine might work and then pray for a miracle as you try to set it running.

2. Be attentive to claims made by others who say they have built something that does perform such a miracle and then try to replicate it from the gist of what you can find they have disclosed.

3. Study the detail of the mechanism of an existing very large perpetual motion machine, which you know does work and to which you have access, and see if, by first probing the physics of that mechanism, you can devise a way of tapping into its energy activity, just as the alternator draws electrical power from the engine in your automobile.

The middle course above is the one normally adopted and it has its excitement but is very frustrating. I have chosen the third track, and even though that has had its frustrations I have advanced relentlessly. I believe I now understand the physics needed to access that hidden energy and so can help to build the new energy technology.

If too many would-be venturers in this field go along that second track each travelling separately and they try to replicate what they think others may have done, a state of chaos can result. However, even amongst that chaos, which is now becoming so very active, we are witnessing a kind of clustering as order is beginning to emerge at certain focal points. Thus we see "cold fusion" or "plasma discharges" or "permanent magnet motors" as giving some of us the feeling that this is all real and that the era of new energy technology can now begin. There are still those, the vast community of scientists who sit on the side lines and watch, confident in their knowledge that all this effort is a waste of time. I say "watch," but it is more correct to say that they look the other way and avoid all thought of getting something for nothing, at least in the "free energy" sense.

My interest in this scenario has an unusual background. Being well brought up in the disciplines of science and engineering, I could never have dreamt that "perpetual motion" would be something I would ever write about, far less get involved in experimentally.

Indeed, my Ph.D. thesis was more concerned with the mysterious loss of energy that was observed in the electrical steels used in power transformers and in the alternators used to generate power.

I became interested career-wise in inventions and my knowledge of magnetism and its related technology was applied to secure patent protection for a major engineering company in U.K. before moving on to IBM at year-end 1959 as their U.K. Patent Manager. Then from 1963 to 1983 I was the director in charge of IBM's European Patent Operations.

I do not recall ever seeing an invention disclosure in IBM that could be classified as "perpetual motion," yet even in the 1960s IBM's patent function was processing several thousand invention disclosures per year. I remember, however, from my pre-IBM years discussing with an elderly German visitor his offer of a "free energy" machine. He declared it would replace the locomotive diesel engines manufactured at one of our plants. As his credentials he said he was chief engineer involved in U-boat design during World War II and then surprised me My saying, "You know Albert Einstein?," then allowing me a very brief glance at a letter addressed to him and signed by Einstein. He followed that by letters from Max Planck and Werner Heisenberg and thereafter went into a display of copious design details of thrust forces in a closed cycle gas turbine having no fuel input.

When I later studied his calculations I found that he had omitted the reaction forces which arise when gas goes around a bend and had only worked out the axial forces on the compression and expansion sets of turbine blades in his proposed engine. He had in fact not done a full analysis and was in breach of Newton's third law of motion, concluding that action and reaction were not in balance. He was wise enough to know that if one can breach that law then one can contemplate the perpetual motion machine.

I was, in those days, part of the conventional world that knows that "perpetual motion" is impossible, but, unlike many in that world, I was attentive to alternative opinion. On the pure science front, as opposed to engineering proper, I was already rebelling against the Einstein philosophy, which ran contrary to my research findings on magnetism. I must say that, in view of that scientific interest, the names Einstein, Planck, and Heisenberg made that little episode of the mid-1950 era stick in my memory. I did know enough German to know whether those letters were supportive, but was not allowed to read anything other than the signature, so you can draw the appropriate conclusion.

IBM Glueballs and Patent Power

I have said above that, as far as I remember, IBM did not

process any invention disclosures related to "perpetual motion" and I now mention two news items from the Spring 1966 issue of "Helpware" sent to me by IBM. They read:

> **Patent Power:** IBM has topped the US patents table for the third year running. With a record 1,383 patents in 1995, IBM received 27% more patents than any other company.
>
> **Glueballs:** Quarks have strangeness and charm, but IBM has glueballs. Three IBM scientists working on the fundamental properties of matter have figured out the properties of the things that stick all the other particles together.

The latter item made me wonder if "glueballs" could have something to do with "cold fusion," because "glue" has its hot and cold forms and there was a time when hot bonding techniques were used in manufacture, whereas today we also see cold-bonded resin technology.

IBM has doubled its patent filing rate since the 1960s and I wonder how many of those 1,383 U.S. patents granted in 1995 are beginning to touch that forbidden boundary which we see blocking the field of "cold fusion"? There will surely be some that are invasive and traverse the boundary set by the second law of thermodynamics, as I will explain below when I refer to superconductivity.

As a European Patent Attorney, I was well aware of the facts of patent law which declare, unambiguously, that an invention must be capable of industrial application. Under this heading comes the issue of "perpetual motion."

Inventions are excluded from patentability if the article or process is alleged to operate in a manner clearly contrary to well-established physical laws, a specific example being a "perpetual motion machine."

Patent law is applied to reject such inventions by the use of a double-barrel gun. The Patent Examiner fires the first barrel if the claim specifies the intended function or purpose of the machine to be the generation of that "free energy" we talk about. The second barrel is fired if the claim does not declare the intended function and merely specifies the construction of the machine, it being implicit somewhere in the specification that the objective really is to cover a "perpetual motion" machine.

We therefore confront the chicken-and-egg argument of which comes first, (a) demonstrating actual industrial application of a technology that can be categorized as based on "perpetual motion" inventions so as then to contest a change of patent law or (b) getting scientists to accept that some of their "well established physical laws" are open to challenge and so can become disestablished. Without patent protection, R&D funding is not forthcoming. Even investors not interested in patents will not rely on what they see might work. They cover themselves and their co-investors by taking independent scientific advice, and that implies need for demonstration of a fully tested operable machine, which then takes us back to square one.

I retired early from IBM in 1983, expressly to get back to university and continue my private research on the third track introduced above. I had no idea in 1983 that IBM would later take up research aimed at calculating the masses of protons and neutrons, a subject I had written about years before at some length.

You see, if we can understand how protons are created we can understand how energy was shed to create the universe. If that understanding shows us that energy radiated into space can be absorbed, stored for a while and then, by some statistical pattern of events, remolded into protons, we have an insight into the operation of that perpetual motion machine we inhabit and which we call the "universe."

From 1952 onwards I had been dissecting and probing the operating system inside that perpetual motion machine, the aether that pervades the universe, recognizing that it had a demonstration model of itself locked in the domain structures we see inside iron crystals and other ferromagnetic substances.

"Free Energy": 1988 and Before

It was in 1988 that I switched to that second track, knowing enough by then of what I needed to know about Nature's own perpetual motion machine. That was just before the discovery of "cold fusion," but just after the discovery of "warm superconductivity."

1988 was a year in which a paper of mine entitled "The Proton Factor and its Unknown Effects" was published. Readers of *Infinite Energy* (#7) may have noticed a letter by Dr. Paul E. Rowe, in which he refers to the aether as a source of energy. The writing of that 1988 paper of mine was inspired by the experimental discoveries of Dr. Rowe concerning his finding that protons were actually being created in high energy electrical discharges in his gas discharge tubes. Here, at last, was some practical evidence that seemed highly relevant to my theory of proton creation.

Dr. Rowe's research was showing that high voltage discharges can add hydrogen into the gas in the discharge tube and it seemed not to originate in hydride decomposition of the electrodes. If the discharge meant that the aether was disturbed in a way which made it shed energy then the options are "excess heat," anomalous electrical EMFs in the electrode circuit , and/or creation of matter (hydrogen). I was interested in all this, but my main interest was the creation of protons from aether energy.

Meanwhile IBM scientists, in 1985, had announced that they had put together at their Yorktown Research Laboratory their GF11 parallel computer using 576 floating point processors to engage in the largest computing task ever confronting a physicist, that of calculating the mass of the proton and the deuteron. So far as I know, it is still working on that problem. Yet ten years earlier I had been co-author on a paper published in the mainstream literature which deduced the proton-electron mass [ratio] as being 1836.152 and, in the event, the leading scientists later measuring the proton-electron mass ratio to very high precision declared:

> The value that they (Aspden and Eagles) calculate is remarkably close to our experimental value. This is even more curious when one notes that they published this result several years before direct precision measurements of this ratio had begun.

It was "curious" because of the method used, which relied on energy in an aether ever-striving to create protons and only winning if it had spare energy to shed from its equilibrium requirements. The word "aether" was something one normally did not mention in a scientific paper appearing in a mainstream physics journal. So the "curious" note implies that information based on aether theory is something one should look for in the "Old Curiosity Shop." [It is in London, not too far from Chancery Lane and the Patent Office Library, the lat-

ter being a better search place for aethereal curiosity.]

Other IBM scientists working in their research facility on the outskirts of Zurich in Switzerland were studying the electrical conducting properties of perovskites and, as we know, that led to the technology of "warm superconductivity." Here in fact was a breakthrough into the "free energy" world, though it was not seen as such at the time.

One can, of course, patent chemical compositions which display unexpected properties having useful industrial application. One can do that even though what is involved seems contrary to well established physical law. Whatever happened to Ohm's law? "Warm superconductivity" defies that law.

I authored a book entitled *Physics without Einstein* and published in 1969. It explained the nature of the neutron and deuteron, as well as pointing out that there is no neutron in the deuteron, but it also explained that a magnetic field acting on a metal locks the energy of that field into a thermodynamic reacting state inside that metal. The book was read by Dr. Jaggi, an IBM scientist at that Zurich laboratory and, on one of my visits, he drew my attention to his experimental discovery, also published in 1969. He had found that there was a curious saturation condition and size-dependent non-ohmic behavior occurring in germanium and silicon when the magnetic energy equated to kinetic energy within the conductor. See page 124 of my 1972 book *Modern Aether Science*.

The point I am making is that when warm superconductivity was discovered the scientific world should have realized that energy shed as heat owing to ohmic resistance loss has a way of regenerating electricity in the metal!

I did not know that when I did my own Ph.D. research and discovered that the eddy-current losses in electrical sheet steels could, at certain stages in the B-H magnetization cycle, be as much as six times greater than theory predicted. What I did not then realize was that the energy shed as heat was regenerating EMFs which enhanced the current to levels far above the normal ohmic value. In short, what I am saying is that the power transformers in use today, though very efficient, are not as efficient as our natural physical laws say they should be, simply because they are doing what physicists say is impossible and regenerating electrical power from the heat they shed.

Ask yourself: "How can the eddy-currents in electrical sheet steel ever be 6 times greater than they should be according to the rigorous calculations which all academic electrical engineers learn about to pass their university examinations?" Later independent reports of measurements made with the magnetization at 90° to the direction in which the electrical sheet steel had been rolled showed a 10-fold energy loss over theoretical prediction! The latter is textbook fact of some 30 years ago, but facts reporting experimental findings that have no explanation unless we accept the "impossible." There is regeneration of electricity from heat, in defiance of the second law of thermodynamics!

What I see in the discovery of "warm superconductivity" is the corresponding discovery that heat released by ohmic heating regenerates EMFs in certain materials having an appropriately tuned response.

I am now saying that by understanding this fully we can break through new energy technology barriers and develop efficient ways of converting low grade heat into electricity in breach of the second law of thermodynamics.

Space, the Vacuum, Its Anisotropy and Its Energy

It was on pages 29 and 30 of my book *Physics without Einstein* that I declared it might be possible to pump energy from the aether. The proposal involved a very powerful magnetic field such as can now be set up by a superconductive solenoid and it involved a ferromagnetic core. It was based on theory which some scientific critics then rubbished by declaring that space would need to reveal a preferred magnetic axis, which they said would have been discovered if it existed. I now draw attention to the item "Testing Over-Unity Devices in Germany" on p. 7 of the Issue #7 of *Infinite Energy*, in which we were told that high ranking authority in Germany was now ready to move forward and pay serious attention to what might be offered on the "free energy" front. Professor Gruber received a response to this from an institutional source in Moscow. It was from Yu.A. Baurov, declaring that an engine running on physical vacuum energy has been developed and tested with a "free energy" output of 0.5 kW. As back-up information Baurov refers in that communication to his co-authored paper in *Physics Letters A*, 162, pp. 32-34 (1992) entitled "Experimental Observation of Space Magnetic Anisotropy." The paper says that experiments in which test bodies are suspended in a superconductive solenoid display a preferred magnetic direction in space. The level of magnetic induction used is that we see in ferromagnetic materials.

Here is evidence of an aether that can do work, an aether which fits my theoretical prediction, the same aether that creates protons. And I had good insight into its way of working, because I had decoded how it determines the proton-electron mass ratio.

However, the shattering effect of the dawn of "cold fusion" made 1989 the year when interest in the new energy field escalated. I was interested immediately, owing to my commitment to a theory which was based on proton creation and deuteron formation without neutrons. I was interested because I was already involved in energy anomalies in metal. I was interested because I had written about anomalous forces acting on cathodes in discharge devices.

Indeed, I was interested enough to file patent applications on "cold fusion," securing patent grant in U.K. but was soon to realize that I was up against a Patent Examiner who reads the *Wall Street Journal* and, if the *Wall Street Journal* says that cold fusion is a non-runner then the US Patent Office bows to that superior authority! I am being a little cynical here, but have noticed the "More Garbage from the U.S. Patent Office" item on p. 60 of the March-April 1996 No. 7 issue of *Infinite Energy* and, well, I will not comment further at this time, save to say that the *Wall Street Journal* will be appropriately authoritative in duly reporting the eventual business success in Japan as "free energy" takes off.

The Secret of the Creative Vacuum

1989 was also a year in which a book entitled *The Secret of the Creative Vacuum* by John Davidson was published. That book, now in its second printing and soon to appear in a German translation, was too early to refer to "cold fusion," but it well reflects the status of the "free energy" theme at that time. The caption under the title was "Man and the Energy Dance" and that is very apt because, when we shed energy as waste heat and it is radiated away into space, the sub-quantum aetherial world is, in fact, engaged in a kind of rhythmic waltz and that spent energy is captured and obliged to join in the rhythm of the dance. Order comes from chaos and from that order energy can be packaged into proton-electron form or even released by ferromagnetism or gravitation, the dance for-

mation being quantized in its waltzing energy pattern and the latter all being "phase-locked" to its universal rhythm.

So, in 1989 I read pp. 245-259 in Davidson's book which refers to the spiral-turbine experiments of Viktor Schauberger. I could not believe what I read: "If water is rotated into a twisting form of oscillation a build up of energy results, which with immense power can cause levitation. . ."

I could believe what John Davidson wrote on page 255:

> Looking at things from first principles, we have to understand that motion is the essence of manifestation. Everything we perceive is an energy dance. At the physical level this subatomic dance is spun out of the energy of space or the vacuum state energy field. And the nature of the motion of these spinning, whirling energy vortices, which we call subatomic particles, is of the utmost importance, for it is a patter which gives rise to all the macroscopic forms we perceive with our senses and allied instrumentation.

Now, a physicist or engineer not tuned in to the "free energy" scenario will see this as mere words with no scientific logic. I was not fully "tuned-in" in 1989, but was attentive. I can now see that by centrifuging water, which comprises positive hydronium ions H_30^+ and negative hydroxyl ions OH^-, one can separate the ions slightly and set up a radial electric field about a spin axis. You will presently see why this is relevant to the generation of "free energy," though I am still thinking about the antigravity aspect.

Reverting to the task of "converting" the scientific community so that that barrier of "well established physical law" does not unduly obstruct the scope for securing patent grant, I note that there are only three physical laws that we need to consider. The first law of thermodynamics, otherwise known as the law of conservation of energy, is well-established and cannot be breached. It was well-established at the time scientists believed in the existence of an aether. Energy is conserved in all exchanges between matter and the aether. I cannot recall any part of my university education when I might have been told that magnetic field energy, which I knew could be stored in a vacuum, was something that escaped the law of conservation of energy. Whether one uses the word "field" or "aether" one is talking generally about the same substance. However, a "field" cannot be a substitute for the "aether," because a "field" is a vector and "field energy" is a scalar, whereas the "aether" is that something in the vacuum that organizes itself to accommodate that energy—and there is far more energy in it than is explicable by our perception of "field" effects in the laboratory.

Today there is a foolish sector of the scientific community living in an imaginary world of virtual reality and unable to see an aether in their 4-space picture. Their opinions can carry no weight in the evaluation of the physics of "free energy." We conserve energy when it is transferred between aether and matter! One cannot "establish" a physical law at a time when everyone believed in the aether and then change its territorial jurisdiction without revising that law. Take away the aether and the law is no longer valid!

I have heard it said that if the aether were to create energy on its own account it would go out of control and we would all be blown up. The simple answer is that it is already in equilibrium with matter but protons, believe it or not, decay to shed their energy which then feeds the aether with a surplus from which it recreates new protons. All we have to do is to stick our finger in the pie while this ongoing cycle of events takes place and capture energy that has climbed to a higher potential but do that before it gets back to the proton creation stage. See my above and later references to "phase locking."

Laws and More Laws

Secondly, there is the second law of thermodynamics. This is well established and cannot be breached so long as one keeps within its limitations. It concerns heat engines, as such, by which is meant engines that run on heat as fuel. Heat goes in at one temperature and comes out at a lower temperature, doing mechanical work en route. There are two temperatures. Gas molecules have a temperature according to their kinetic (mechanical) activity. What may I ask is the temperature of a photon? Indeed, what is the temperature of electricity? What is the temperature of magnetism? You see, if I can input heat at one temperature and it is transported through metal by electrons subjected to a magnetic field, I can divert those electrons off course and tap some of that heat to generate electricity. Now ask yourself a simple question. Is this a heat engine? Does the fuel (the heat) have a temperature? Does the working substance heated by that fuel all flow out, as in a heat engine, through the one exhaust at a low temperature? Well, no, because there is something different here. We are not talking about the MHD (Magneto-Hydro-Dynamic) technology of the 1960s, where as many atoms of gas emerge as output as enter as input. Those electrons in the metal do not really all flow from the hot temperature input to the cold temperature output. If they did they would carry current along with the heat flowing through a metal conductor and we do not see such a flow of electricity. The metal subjected to that magnetic field has a way of developing the electric power output transverse to the heat flow, without demanding any net electric current flow along the heat path. So, I say I know how to build a device for converting heat into energy without it being a heat engine within the scope of the second law of thermodynamics. It is bound by the first law only. I know this because I am a co-inventor of a device which generates electricity from melting ice placed on its top heat sink surface and then freezes water on that same surface when fed with a.c. electrical power input.

I note that if one can build two devices, one which defies the second law of thermodynamics and can convert heat into electricity with an efficiency much greater than the Carnot level set by that second law and couple that with a conventional heat pump complying with that law, then, in their back-to-back operation, one has not only a free energy generator, but refrigeration as well. The aether is not involved in this technology, which requires little more than a laminated assembly of thin films of nickel interleaved with a suitable dielectric material. My reports on that technology will soon be issued so I will say no more here.

The third law considered is not the third law of thermodynamics, which relates to the impossibility of cooling matter down to absolute zero of temperature in one action cycle. That law is connected with the name Nernst and textbooks on thermoelectricity term the effect described above as the *Nernst effect*. However, it needs a good measure of ingenuity to apply the Nernst effect to practical technology, but such technology is now in sight.

No, the third law I mention is the law of electrodynamics, which I see as the basis for Newton's third law of motion, but if I delve into that then this article will become too long and I must now curtail my commentary somewhat.

The "Fairyland World" of "Free Energy"

Before I conclude, I want to refer to the cover article of Issue #7 of *Infinite Energy*, the article on the Correa technology, and to introduce a book I have just published entitled *Aether Science Papers*.

In publishing a work under this title, I am redeeming a promise I made to myself when I stated on the back cover of my book *Modern Aether Science*: "A mathematical extension of the new ideas presented in this work will be published separately under the title of *Aether Science Papers* and will be available from the same publishers."

That promise is redeemed after 24 years. The reason for this delay is the fact that that 1972 book was branded *Physics in Fairyland* by a key reviewer. That "fairyland" world is now our "free energy" world.

It was on page 67 that I quoted Dirac as declaring that the universe may contain as many negative protons as positive protons and as many negative electrons as positive electrons, it being all a question of which stellar domain regions were considered. We happen to belong to a region with positive protons and negative electrons. On pages 44-45 I quoted a French cosmologist Alexandre Veronnet as presenting a vision of the aether which warranted attention, owing to its connection with magnetism and in particular the unit we term the Bohr magneton which gave the aether a quantum feature and provided the link with ferromagnetism that I exploited. This was no "fairyland" but it was a world of energy filling space, and I was explaining how the universe was created from that energy.

More important, at the end of my book I urged that attention should be paid to an experiment performed by H.A. Wilson which bears upon our "free energy" interest.

It had been suggested that a body in rotation might develop a magnetic field as a gravitational phenomenon. It had come to be known as the Schuster-Wilson hypothesis. If the mass of a body is multiplied by the square root of the constant of gravity and the result is assumed to be the measure of electric charge, that body in rotation should develop a magnetic field. This idea worked qualitatively and quantitatively using data for the two bodies, our Earth and the Sun. So Wilson set about experimenting. He found magnetic fields induced in this way in iron and could not get rid of them, suggesting therefore that here was something very fundamental.

That dates from 1923, but eventually in 1947, the year before he won his Nobel Prize, Blackett drew attention to the fact that the Schuster-Wilson hypothesis applied equally well to a star some ten billion times more massive than the earth. He then set about trying to test the hypothesis in a laboratory, this time by contriving to use a very large gold cylinder which was located in a shed in a remote location. He sought to measure the magnetism seated in this gold cylinder as induced solely by its rotation with the earth. This required an enormously sensitive magnetometer, but the tests proved negative and so the Schuster-Wilson hypothesis stood rejected.

Now, when my aether theory showed me how rotating aether sets up a magnetic field I found that, if one assumed aether coextensive with the earth was rotating with it, then it gave the correct value for the geomagnetic moment. I knew then why the Blackett experiment had not worked. He had used an object which concentrates mass but not one that concentrates aether. In short, as the aether must extend to ionospheric altitudes and so pervade our atmosphere above ground, the gold cylinder would reveal no change of magnetic field by its presence or absence at the test location.

The aether was, however, seen by that critic who reviewed my book as something one could only relate to "fairyland." So, I will now come directly to the point of all this by declaring that the aether is "phase locked," which means that if we try to rotate a sphere of aether there will be constraints asserted upon that spherical form. These constraints are asserted by the enveloping aether owing to that "phase locking." Analysis shows that this will develop a radial electric field centered on the spin axis. Conversely, if we can set up a radial electric field about an axis of spin, then the aether coextensive with the range of that field will develop a spin. Since this comes about by a constraint asserted from that enveloping aether environment, the latter must contribute the energy needed to keep that "phase lock" condition. So, for every joule put in as electric field energy to set up the spin, the aether delivers one additional joule as kinetic energy. That is the source of the "free energy" I see at work in the Correa technology.

Can you now see why I referred above to the findings of Viktor Schauberger? By setting up that centrifugal separation of positive and negative ions in water he was setting up a radial electric field about the spin axis. There would be an inflow of free energy and if pockets of air in the pipework he used could make that flow pulsate in some way, setting up oscillatory effects, then that "free energy" could be replenished over and over again.

As to how stars get their magnetic field, the answer is that there was a cooling down of aether activity which allowed the aether to crystallize into a form that introduced gravitation. As in a ferromagnet, when cooled through the Curie temperature, domains form as magnetism appears. The corresponding phenomenon in the aether is what we term gravitation. The protons that existed could then coalesce under gravity and, owing to their mutual gravitational attraction giving an acceleration 1836 times that set up between electrons, the initial state of stars thus nucleated would have a positive charge. This set up the radial electric field as powered by gravitational energy. The radial electric field in turn induced the spin condition of the aether, further powered directly by aether energy, which eventually transfers to the matter in the star, but which also sets up the star's magnetic field.

In my book *Modern Aether Science* I related this to the creation of thunderballs by lightning discharges, because a discharge that concentrates positive ions (as in the Correa technology or in the discharges of Rowe's experiments) must develop radial electric fields. We get inflow of "free energy" supplied by the aether. That materializes either in a useful form or, as in the thunderball (or even the tornado), in a form that can be quite destructive. Yet all this is said to be "Physics in Fairyland."

In the case of the homopolar machine with a permanent magnet rotating to induce EMFs in a cylindrical disk, we have exactly the same scenario. The "free energy" potential is there but we have to know how to extract the energy, as by setting up pulsations. In a practical "free energy" device, one needs to recover the priming energy of 1 joule for every "free energy" joule delivered by the aether as a dividend. The energy capital invested has to be deposited and withdrawn repeatedly because the pay-off is a one-shot response which doubles the investment each time the radial field is reestablished.

This applies to the "aether spin" method of tapping that "free energy," as in the Correa apparatus where capacitative components have a feedback role, a 5:1 power gain in the device itself then being feasible. Once external storage and

feedback is provided with this technology to close the loop, a power gain factor of this kind will not be of overall relevance. It will simply be a question of the amount of start-up power input needed and its limited duration to set the system in operation and then the performance will be judged by the specification of its continuous power output.

The Aether, "Cold Fusion," and the "Supergraviton"

As already indicated, the aether can shed energy by creating protons, but whether this may have practical consequences remains a matter for speculation. In the meantime, the "cold fusion" theme is of primary interest. Aether theory concerns also the creation of neutrons and deuterons and can help to explain the absence of neutrons in the fusion reaction.

It seems, furthermore, that the aether will shed energy in responding to electrodynamic action by an action quite distinct from that associated with a radial electric field. The latter transfers angular momentum and related energy from the aether, but the electrodynamic reaction induces the precisely opposite response. In responding to electrodynamic interaction between two charges in motion, the aether will not develop an out-of-balance reaction as a couple or turning moment. It can, however, develop an out-of-balance linear force, which means a breach of Newton's third law of motion, coupled with the delivery of "free energy" from the aether. This accounts for the anomalous cold-cathode reaction forces found in the researches of Correa and others, but these forces do useful work in compressing positive ions into a plasma ball which then can set up the radial electric fields which tap the primary input of energy from the aether.

Then there is the still undeveloped physics of magnetic actions in metals which offers enormous promise for the "free energy" theme. There are ways of setting up non-linear electric field gradients inside a metal, given the trigger of an initial temperature gradient. Such a gradient means that sources of electric charge originate inside the metal. In other words a surplus of negative charge can exist and yet not be detected. A surplus of electron charge in a metal, and sitting amongst free protons or deuterons which are free to migrate inside the cathode and so are subject to the anomalous electrodynamic forces accounting for cold cathode reactions as just mentioned, can draw on aether energy. Such forces are accentuated and escalate in strength if the mass of the interacting charges differ, which is the case once we consider migrant protons or deuterons. That can trigger the merger of two deuterons, because the electrons are in surplus and an unexpected energy fluctuation is at hand. We may then see the makings of a fusion reaction resulting in excess heat.

In deliberating on this scenario, I have also to consider the involvement of the "supergraviton." My theory explains gravitation but requires the presence of a dynamically reacting "graviton" system. The supergraviton has a mass slightly greater than 102 atomic mass units but is only present in dense matter. A palladium cathode constitutes dense matter, because its atomic nuclei are of mass commensurate with the supergraviton. Deuterons entering the cathode in the cold fusion cell are associated with a retinue of normal gravitons of much lower mass. The aether sorts this out by converting gravitons into supergravitons and that process sheds some energy as heat. This is another factor to consider, because that heat energy will be concentrated as motion imparted to deuterons in amounts sufficient to trigger fusion.

If the reader wonders what I mean by "supergraviton," then take note that a typical warm superconductor perovskite La_2CuO_4 has a molecular mass of 407 atomic mass units, based on copper isotope 65. This means that it is highly tuned to dynamic resonance with the supergraviton, being close to four units of 102 atomic mass units. Such dynamic resonance means that electron collision with the molecules of such a material will not shed much energy as heat. It is as if the impact is transferred to the dynamic mass centre so that the energy is stored conservatively by a spin about that centre, only to be shed by being returned to electrons driven out of the molecules as the system reacts to conserve angular momentum.

The "supergraviton" plays a key role in my interpretation of the warm superconductivity phenomenon. It cooperates with the energy stored by magnetic induction to sustain that electron current flow, even though that means slowing down the thermal motion of those molecules. In other words, there is superconductivity because heat of molecules is converted into electrical power. That means that scientists who discovered warm superconductivity also discovered a regenerative energy process which defies the second law of thermodynamics. It becomes a matter for technological development to harness that discovery to serve a "free energy" purpose in generating electrical power from ambient heat, before the aether gets to work and packages that energy into protons and, of course, accompanying electrons!

Onwards to that Source of "Infinite Energy"

The "supergraviton" is a catalyst that can do the work of the Maxwell demon, but before you learn about "supergravitons," you need to understand something about "gravitons." These are the ghost particles in the heartland of aether energy.

I therefore invite readers to refer to my new book *Aether Science Papers*, which will be the forerunner of my writings on the detailed operation of the specifics of the new energy technology in which I am interested. In that ongoing effort I will be referring to *Aether Science Papers* as the full explanation of the energy source being tapped. Essentially, apart from a 62 page commentary which relates the subject to the new energy field, the book comprises copies of fourteen published papers plus bibliographic references to many of my other published work, including the one in which the supergraviton mass is derived.

See the following summary, as published on its back cover [not reproduced here]. The book is targeted at the theoretical physics community and attacks them on their own ground, where they are weak for not having the good sense to search the aether for those energy sources that have been signalling to us and telling us that "free energy" is within our immediate reach.

They will, of course, not fend off such attack, but just wait to see experimental proof of "free energy" machines in operation. That means their surrender once those machines trample over their undefended territory. They do not know how gravitational potential gets its energy, nor do they know how matter is created from the so-called zero-point energy of the vacuum. Once we have that energy in our sights, we can intercept some of it, and the aim of *Aether Science Papers* is to illuminate the aether energy field so that those amongst us who have not already staked a claim can set to work tapping that energy.

Why Does Lightning Explode and Generate MHD Power?

Peter Graneau

Presented at *Infinite Energy's* Cold Fusion and New Energy Symposium on October 11, 1998 in Manchester, New Hampshire.

Abstract

The talk given at the Manchester symposium reviewed the long history of thunder research and proposed, for the first time, that it is arc liberated chemical energy from the air which explodes the lightning channel. Regardless of the cause of thunder, the ejection of ions from the atmospheric arc through the strong encircling magnetic field should generate MHD (magneto-hydrodynamic) power. This is expected to augment the flow of discharge current in the lightning stroke. Experimental evidence provided by laboratory arcs of lightning strength supports current augmentation by MHD action.

The Cause of Thunder

The best known electric arc in air is the lightning stroke. It explodes and sends a shockwave through the surrounding air, which is known as thunder. Thinking man has observed thunder and lightning for thousands of years. This spectacle plays a role in both Eastern and Western mythology. Thunder is recognized as one of the oldest riddles of recorded scientific inquiry. At the end of the twentieth century we are still questioning what makes the lightning channel explode. Remillard[1] published an excellent review of thunder research from Aristotle to 1960.

In the middle of the present century it was firmly believed, but poorly substantiated, that it was the thermal expansion of the lightning plasma which set up the shockwave in air. Then in 1961 Viemeister[2] published his findings with regard to "cold" and "hot" lightning. He wrote:

> Cold lightning is a lightning flash whose main return stroke is of intense current but of short duration. Hot lightning involves lesser currents but of longer duration. Hot lightning is apt to start fires while cold lightning generally has mechanical or explosive effects.

In the 1980s we proved at MIT[3] with photography and other means that the shockwave emanating from a short air arc of less than one centimeter length and carrying current of lightning strength, between metal electrodes, did not propagate with a spherical front, as it should have if random thermal collisions between air molecules provided the driving force. Instead the explosion was found to be a distinctly radial blast. The expanding air plasma disk, of a thickness equal to the arc length, was ablated by the environmental atmosphere and formed a supersonic edge.

By Viemeister's definition, this was a cold arc. A sheet of newsprint stretched across the arc gap was mechanically torn, but did not catch fire, so long as it did not touch the electrodes, which exhibited surface melting. No charring or any signs of heating could be detected on the paper.

If not heat, what is it that propels the radial arc explosion? The process of gas breakdown and ionization absorbs rather than liberates energy. Arc plasmas are charge neutral and have never exhibited Coulomb force implosions or explosions. Fifteen years ago we thought the forces which drove the arc ions apart had to be of electrodynamic origin, that is they had to be ponderomotive magnetic forces between current elements. Measurements[4] confirmed decisively that the explosion strength increased with arc current in conformity with an electrodynamic explanation.

Unfortunately, according to conventional electromagnetic theory, the dominant electrodynamic force on the arc should be the Lorentz pinch force. This could cause an arc implosion but it acts in the wrong direction for the observed explosion. The Newtonian electrodynamics[4] with Ampere's force law agrees with the Lorentz pinch force but, in addition, predicts strong axial pressure in the arc column. Without a containment tube, the axial pressure will break out in the radial direction. Ten years ago this appeared to be the most likely cause of thunder and air arc explosions.

Intense research of high current arcs at MIT and Northeastern University did, however, reveal that the Ampere forces were too small, by at least a factor of ten, to create the measured arc pressures.[4] This research also involved water arcs in which the explosion pressure was a hundred times that which could be justified with Ampere forces. Then it was discovered that the water arc explosions were the result of the liberation of internal chemical energy.[5] This led to a complete change of the understanding of the dynamics of pulsed arc explosions.

Liberating Chemical Bond Energy with an Electric Arc

All substances owe their existence to chemical bonding. The bonds involve largely electrical forces of attraction and repulsion. In the bonding of any two particles, the attraction must be balanced by nuclear or atomic repulsion, otherwise matter would collapse and fuse. Forces of repulsion are said to store positive potential energy, while forces of attraction store negative potential energy. If negative potential energy were to annihilate positive potential energy, there would exist no stored bond energy, no bonding, and no matter. We are driven to the conclusion that both these energies must be able to exist side by side.

What is known is how much heat it takes to break a bond. This should be—and sometimes is—described as bond dissociation energy. There is no reason to believe that bond dissociation energy must be equal to the stored bond energy. In fact, a given bond may be broken in an electric arc without heating and the dissociation energy is then likely to be very different from the thermal dissociation energy. Bond energy tables actually list thermal dissociation energies. In general, we do not know what the stored potential energies of bonding are.

Water arc experiments have shown[4,5] that a small amount of electrodynamic energy can unlock a much larger amount of stored intermolecular bond energy, which then causes an explosion. It is not unreasonable to suspect that a similar arc-triggered bond energy release is responsible for the explosion of lightning channels. That lightning and arcs in atmospheric air are responsible for chemical reactions has been known for a long time. In fact, electric arcs are used commercially to convert N_2 and O_2 molecules of air to NO, that is nitric oxide.

A considerable body of knowledge exists regarding the heat

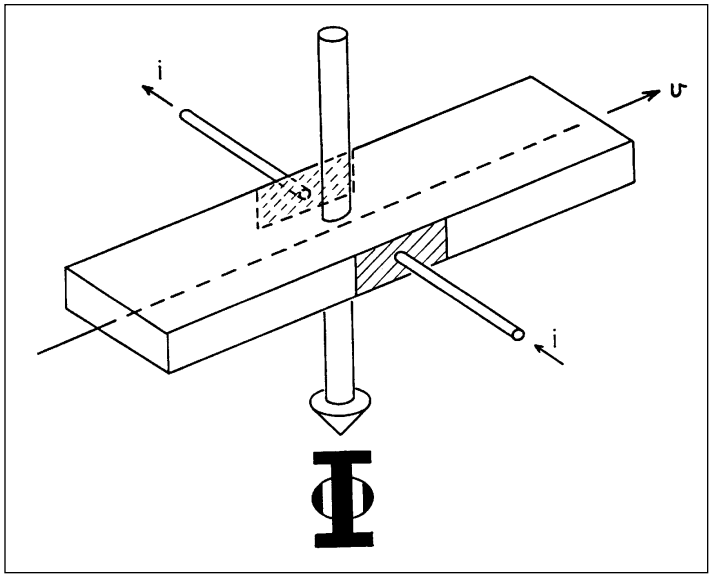


Figure 1. Conventional MHD generator.

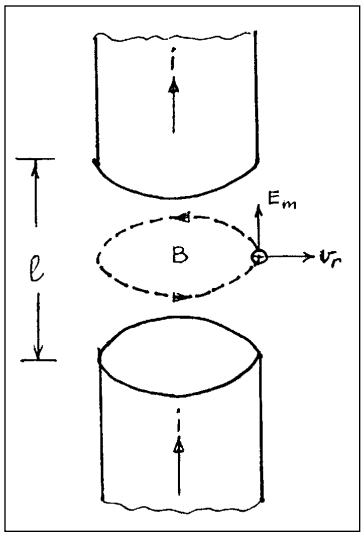


Figure 2. The arc as an MHD generator.

required to break up the strongly bonded N_2 molecule, but there exists no information indicating how much potential energy is stored in the molecule. When the N-N bond of the N_2 molecule is deprived of its attraction force by a small electrodynamic force, or action, in the arc, the remaining large repulsion force between the two atoms may instantly drive the atoms apart in an event which could be described as an explosion.

In view of the fact that more than two thousand years of research have not found the cause of thunder, it now looks entirely possible that this cause is the unknown amount of chemical bond energy stored in N_2 and O_2 molecules.

The Exploding Air Arc as an MHD Generator

Ordinary MHD generators, used in military and space applications, employ a rectangular plasma duct of heat resisting dielectric material. This duct is shown in Figure 1. Typically the plasma traveling at high velocity v down the duct is the flame of an oil burner. Electromagnets are usually employed to set up the magnetic flux F across the duct which intersects the plasma stream. The ion motion at right angles to the magnetic flux induces the electromotive force (emf) in the direction perpendicular to plasma flow and magnetic field.

As shown in Figure 1, metal electrodes are built into the duct wall so that electrons accelerated by the MHD emf can flow from the electrodes through an external load. This current i represents the electrical energy output of the MHD generator.

In the electric arc, the current creates an encircling magnetic field of considerable strength. Now consider vertical electrodes with an arc gap between them. The magnetic flux circles then lie in horizontal planes. Explosively driven ions, moving radially outward from the arc gap, cross the magnetic flux lines and induce a vertical MHD emf, or the field E_m, in the expanding plasma. Electromagnetic theories are found to demand that the induced emf acts in the direction of arc current flow, as shown by E_m in Figure 2. Hence, the electric arc has to behave like an MHD power generator.

The motionally induced emf in the direction of current flow is a forward emf associated with the conversion of mechanical energy, derived from chemical sources, to electrical energy. The reverse process of the conversion of electrical energy to mechanical energy, as in a motor, produces a back emf. For example, if an electric current is forced to flow through the electrodes of the MHD device of Figure 1 and the plasma is replaced with liquid metal, the apparatus becomes a liquid metal pump, which is the linear motion equivalent of a rotating motor. The motionally induced emf then opposes current flow and therefore is a back emf.

The back emf per unit current has the dimension of a resistance. It adds to the resistance of the current circuit through the electrodes. Similarly, a forward emf per unit current subtracts from the resistance. Hence, we may argue that in the MHD generator the current is increased by a reduction of the internal resistance of the arc plasma.

Experimental Indication of the Presence of MHD Energy in an Air Arc

The relevant circuit theory and diagnostics for measuring arc currents, voltages, resistances, impedances, action integrals, etc., are outlined in Reference 4. The air gap between metallic electrode rods is broken down with a capacitor bank charged to a high voltage, usually in the range 10 - 50 kV. The energy stored in the capacitors then drives an oscillating current i through the arc and discharge circuit, which may be written

$$i = I_o e^{-t/T} \sin\omega t, \qquad (1)$$

where I_o is the current intercept of the exponential envelope ($e^{-t/T}$) of the positive and negative peaks, t is time, T the decay time constant and w=2pf is the ringing frequency. Figure 3 is a typical oscillogram of the decaying arc current.

The solid curve plotted on Figure 4 represents the experimental values of

$$I = I_o e^{-t/T}, \qquad (2)$$

as a function of time. Every positive and the magnitude of every negative current peak lies on this curve.

The MHD power generated has to be very dependent on the current i, which determines not only the strength of the encircling magnetic field B but also the ion velocity v_r as a result of the current-dependent strength of the chemical explosion (see Figure 2). The MHD effect will be zero at t = 0 and approximately zero after a certain time when i has fallen below some threshold value.

The solid curve of Figure 4 refers to a particular arc experiment and has been plotted for the positive and negative current peaks of Figure 3. This curve has been labeled "with MHD." From the I_O value and the tail of the curve (I<800 A) the exponential $e^{-t/T}$ can be computed from Equation 2. Using this exponential, the rest of the curve can be plotted. This is the broken curve labeled "without MHD." It is seen to fall below the experimental curve, just as expected if MHD energy is generated over the middle portion of the curve. The difference between the two curves of Figure 4 represents the MHD augmentation of the arc current.

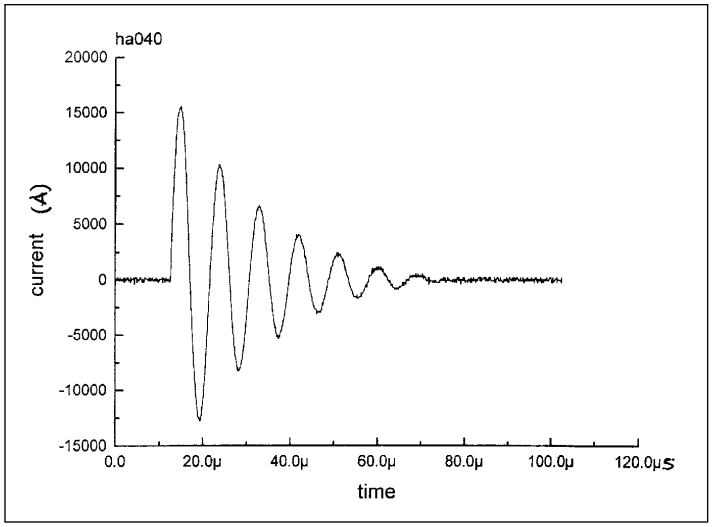


Figure 3. Discharge current oscillogram.

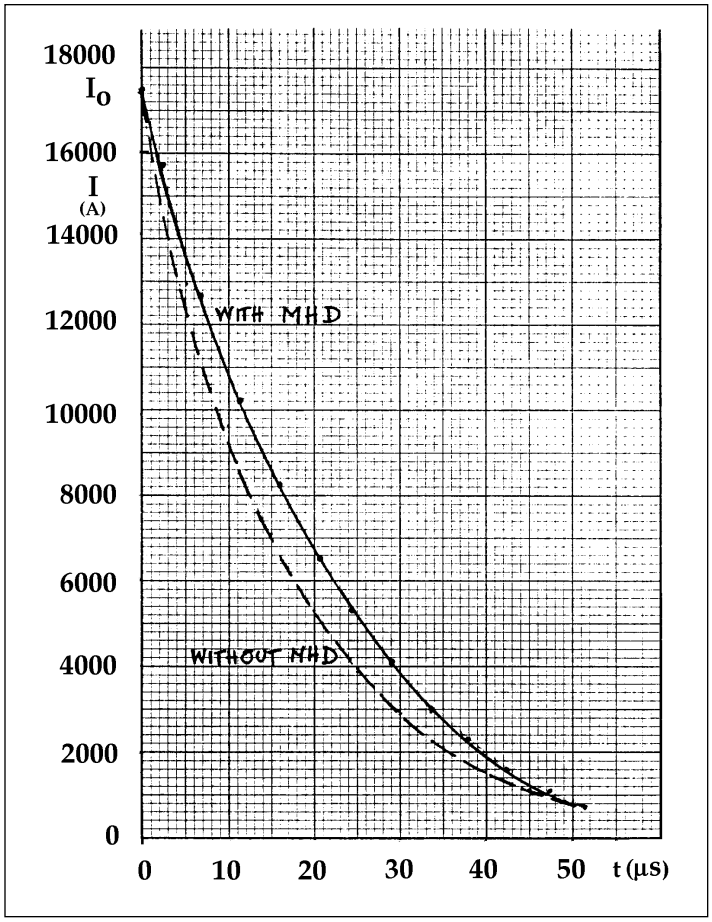


Figure 4. Plot of Equation 2.

Conclusion

The cause of thunder has eluded scientists from Aristotle on right up to the end of the twentieth century. All explanations advanced in this long search have been disproved with laboratory experiments involving atmospheric air arcs of lightning strength. Now a new explanation has been put on the table. It claims the explosion of the lightning channel is due to the impulsive liberation of chemical bond energy stored in the diatomic molecules of nitrogen and oxygen. It will take time before this suggestion is widely confirmed or rejected.

The lightning channel undoubtedly explodes and thereby shoots air ions through the magnetic field of the lightning current. This should result in the generation of MHD power and an augmentation of the arc current. It now appears that notice of this fact has escaped arc scientists. Maxwell's field theory and the Newtonian electrodynamics agree that the effect should exist, but it remains to be established if it is of significant or negligible magnitude. The first experimental findings suggest that it is significant.

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Nuclear Transmutation Reaction Caused by Light Water Electrolysis on Tungsten Cathode Under Incandescent Conditions

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Abstract

When a tungsten cathode is electrolyzed at high power, it exhibits an intense reddish-purple glow discharge, and emits radio frequency (RF) electromagnetic waves. In some cases powerful excess heat, ranging from 60 to 140 watts is generated, and substantial amounts of new elements are formed, including Fe, Cr, Ti, Ca, Ni, C, Re, and Pb. This has been observed with many different electrolyte solutions including Na_2SO_4, Na_2CO_3, $NaClO_4$, K_2CO_3, KNO_3, Rb_2CO_3, Cs_2CO_3, $Ba(NO_3)_2$ and $Ba(ClO_4)_2$. Based on the heat and apparent transmutations, we conclude that some form of nuclear reaction occurs. We propose several different nuclear reactions that might plausibly account for the phenomenon.

Introduction

In recent years, experimental results achieved by a number of independent researchers support the finding that nuclear reactions can occur under room temperature conditions in metal hydrides, but these reactions are not necessarily conventional deuteron-deuteron fusion reactions. They may be fission or fusion reactions involving metallic elements in the electrode, and not just hydrogen or deuterium.[1-17] In an Au/H_2O electrolysis system, considerable amounts of Hg, Kr, Ni, Fe and, in some cases, Si and Mg were produced on and in the electrode, apparently as a result of the nuclear fusion or fission reactions involving the Au electrode material.[7-8] This suggests that the excess heat reaction might be enhanced by employing as electrode material a metal with a large atomic number, since it will have relatively small nuclear binding energy. In this respect tungsten (W) would be one of the most favorable electrode materials because it has a large atomic number and resistance to high heat. For this reason, we selected W as the working electrode material.

In the present study, strong excess energy generation, new element production, and two other noteworthy phenomena—glow discharge and RF electromagnetic wave emissions—were observed. We report these results and propose some nuclear transmutation reactions that might explain the phenomenon.

Experimental

The electrolytic cell is made of quartz glass. It is simpler than the vessels used in previous experiments, with a design intended to facilitate excess energy measurements (Figure 1). The total volume is 240 ml. The working electrode is a rectangular W foil (0.5 cm^2 nominal area, 0.1 mm thick). The surface is scraped with a glass shard which has been cleaned in warm aqua regia and washed with pure water. A lead wire of W or Ni (0.4 mm diameter) is connected to the center of the rectangle. A W lead wire is used in experiments intended to quantify transmutation products. The counter electrode is a Pt mesh (1 cm x 7 cm, 80 mesh) connected with a Pt wire (0.4 mm diameter). The lead wires are covered with Teflon. Before the working electrode is installed in the electrolytic cell, the cell is cleaned in a hot acid solution (1:1 H_2SO_4 + HNO_3), then rinsed with Milli-Q water several times and placed in an ultrasonic cleaner in a bath of Milli-Q water.

Electrolysis was performed in 0.5 M Na_2SO_4, Na_2CO_3, K_2CO_3, $NaClO_4$, $Ba(ClO_4)_2$, and $Ba(NO_3)_2$, 0.25 M Rb_2CO_3 and Cs_2CO_3, and 0.16 M Cs_2CO_3 solutions prepared with Milli-Q water. The Na_2SO_4, Na_2CO_3, and K_2CO_3 reagents are Merck sprapur grade and other reagents are analytical pure grade. We also tested an alloy of 50% Mo - 50% W in electrolytes of 0.5 M K_2CO_3, H_2O, and W in 0.5 M K_2CO_3, D_2O. The volume of the electrolyte solutions ranged from 120 to 170 ml (usually 150 ml). Impurities in the 0.5 M Na_2SO_4 solution included: Ba (0.2-0.1 ppm), Si (0.1-0.01 ppm), K (0.01-0.001 ppm), Li, Mg, Ca, P, Sr, Cr, Fe, Co, Ni, Cu, Zn, As, Hg, Pb (0.001-0.0001 ppm), Ti, Mn, and Cd (< 0.0001 ppm). Impurities in the Pt mesh included: Rh (18 ppm), Pd, Cr, Si (2 ppm), Cu, Fe, B, and Cd (< 1 ppm). The amounts of Fe and Cr contained in the W electrode material are < 1 ppm. The electrolysis cells were placed in a constant temperature air chamber, thermostatically controlled at 20±1°C. A regulated DC current/voltage supply with 160V maximum output was used.

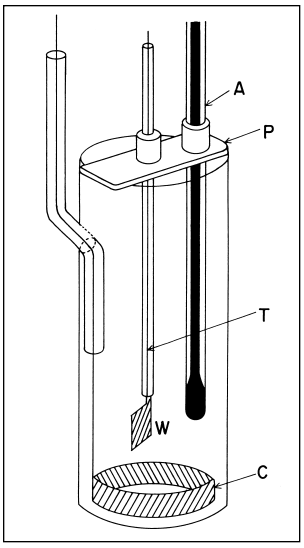

Figure 1. Electrolytic cell: (A)-alcohol thermometer, (C)-Pt counter electrode, (P)-plastic plate, (T)-Teflon tube, and (W)-working electrode.

After electrolysis, transmuted elements on the surface and in the bulk of the electrode were identified and quantified with an energy dispersive X-ray analyzer (EDX), electron probe micro analyzer (EPMA), and secondary ion mass spectrometer (SIMS). The isotopic distribution of the product elements was determined by SIMS.

Results and Discussion

Incandescence of the Electrode and Glow Discharge

Under electrolysis with the current below 2.5A, the working electrode did not incandesce, and the only chemical reaction to occur was H_2 evolution. However, at current densities above this, the electrode began to incandesce and emit an intense reddish-purple glow (see *Infinite Energy* #20, Figure 1, p. 20). The input voltage jumped from ~20V to 160V and the current fell to 1~0.8A and then decreased gradually down to 0.7~0.5A. When the glow discharge began, strong RF electromagnetic emissions began, which disrupted the thermocouple, so the solution temperature was measured with an alcohol thermometer (Figure 1). We assume the glow is caused by the plasma, and the particle acceleration within it induces the strong RF noise. The incandescent state of the electrode continued as long as the electrolysis conditions were maintained.

Excess Energy Generation

The temperature of the solution just before the initiation of

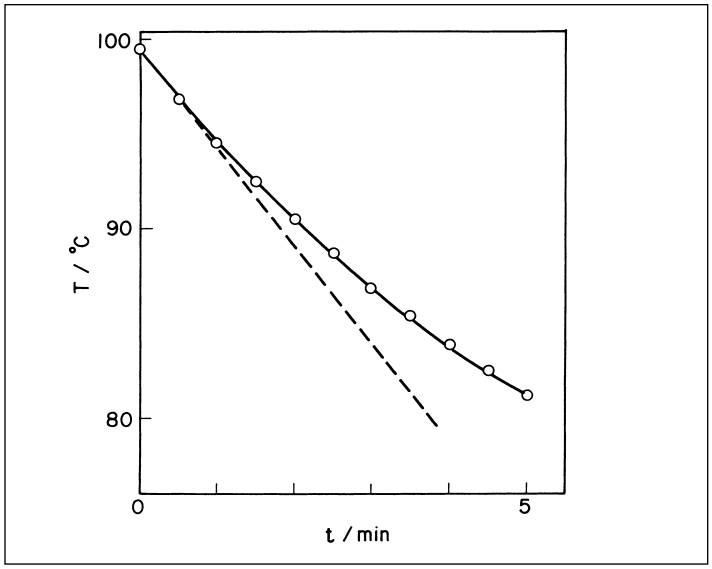

Figure 2. The cooling curve of the electrolyte solution after it reached boiling and power was turned off. The solution nearly filled the cell.

incandescence was 80 to 85°C. After initiation, the temperature increased sharply and reached the boiling point within 80 seconds. Excess energy measurement was made mostly for 15 to 30 minute periods under incandescent conditions, using the following equation:

$$E = W_{vap} + W_{sol} + W_{cell} + W_{wall} - W_{in} \quad (1)$$

where: W_{vap}: heat required to vaporize light or heavy water
W_{sol}: heat required to raise the temperature of the solution just before the incandescence of the electrode up to the boiling point
W_{cell}: heat required to raise the temperature of the container of the cell
W_{wall}: heat effused through the wall of the cell during the measurement
W_{in}: electric power applied.

The W_{wall} was determined from the time derivative of the temperature, $(\delta T_s/\delta t)_{t=0}$, at a starting point (*ca.* 99°C) of a natural cooling curve (Figure 2) of a boiling electrolyte solution which nearly fills the electrolytic cell. This is represented by the following equation,

$$W_{wall} = C_s M_s (\delta T_s/\delta t)_{t=0}\, t_m + C_q M_c (\delta T_s/\delta t)_{t=0}\, t_m \quad (2)$$

where: C_s and C_q: specific heats of the electrolyte solution and quartz of cell material
M_s and M_c: masses of the electrolyte solution and the cell
t_m: measurement time

The excess energies obtained in thirty-one electrolysis systems are listed in Table 1, which shows that large amounts of excess heat were generated in every test, the yield being virtually the same whatever electrolyte was used. Apparently, alkali metal and alkali-earth metal cations and carbonate, sulfate and perchlorate anions play little or no role in any of the nuclear reactions generating excess energy. Figure 3 shows the distributions

Table 1. Excess energies and energy efficiencies of various electrolysis systems.

Solution		Volume (cc)	Time (min)	Current (A)	W_{vap}	W_{sol}	W_{wall} (KJ)	W_{cell}	W_{tot}	W_{in}	Excess Energy (W)	Efficiency (%)
W-Electrode												
0.5M	K_2CO_3	170	30	0.48	133	18	179	4	334	138	108	242
	K_2CO_3	170	30	0.54	159	18	179	4	360	155	114	232
	K_2CO_3	150	30	0.53	125	13	179	4	321	153	94	210
	K_2CO_3	150	30	0.53	86	18	179	4	287	153	75	188
	K_2CO_3	120	15	0.65	87	9	90	2	188	94	105	200
	K_2CO_3	120	15	0.60	64	10	90	2	166	86	88	193
	K_2CO_3 (*)	150	30	0.56	133	11	179	4	327	160	92	204
	K_2CO_3 (*)	150	30	0.43	49	13	179	4	245	124	65	198
	K_2CO_3 (**)	150	15	0.73	93	8	90	2	193	105	97	183
	Na_2CO_3	150	30	0.63	99	14	179	4	296	179	65	165
	Na_2SO_4	170	30	0.67	193	16	179	4	392	193	111	203
	Na_2SO_4	170	30	0.70	161	17	179	4	361	201	89	180
	Na_2SO_4	170	30	0.65	156	14	179	3	352	187	92	188
	Na_2SO_4	120	15	0.55	67	12	90	4	173	79	66	165
	$NaClO_4$	140	15	0.70	85	5	90	3	183	101	92	181
	$NaNO_3$	150	6	0.63	36	13	37	4	90	37	141	243
	$NaNO_3$	150	23	0.63	110	14	135	4	263	137	93	192
	KNO_3	150	30	0.58	144	13	179	4	340	167	96	204
	KNO_3	150	30	0.64	139	13	179	4	335	184	83	182
0.25M	Rb_2CO_3	170	30	0.59	218	18	179	4	419	170	138	246
	Rb_2CO_3	120	15	0,78	81	11	90	3	185	112	80	165
	Rb_2CO_3	150	30	0.58	102	15	179	4	300	167	74	180
	Rb_2CO_3	150	30	0.43	70	14	179	4	267	124	78	215
0.16M	Cs_2CO_3	170	30	0.47	95	12	179	3	289	135	85	214
0.25M	Cs_2CO_3	150	15	0.59	84	14	90	4	192	85	119	226
	Cs_2CO_3	150	30	0.44	86	17	179	4	286	128	88	223
	Cs_2CO_3	150	30	0.62	100	17	179	4	300	179	68	168
0.25M	$Ba(ClO_4)_2$	170	30	0.49	121	16	179	4	320	141	99	226
	$Ba(ClO_4)_2$	170	15	0.52	74	11	90	3	178	75	114	237
	$BaNO_3$	150	30	0.44	73	16	179	4	272	127	81	214
W50%-Mo50% Electrode												
0.5M	K_2CO_3	150	30	0.59	133	14	179	4	330	170	89	194

(*) W wire electrode (**) in D_2O solution

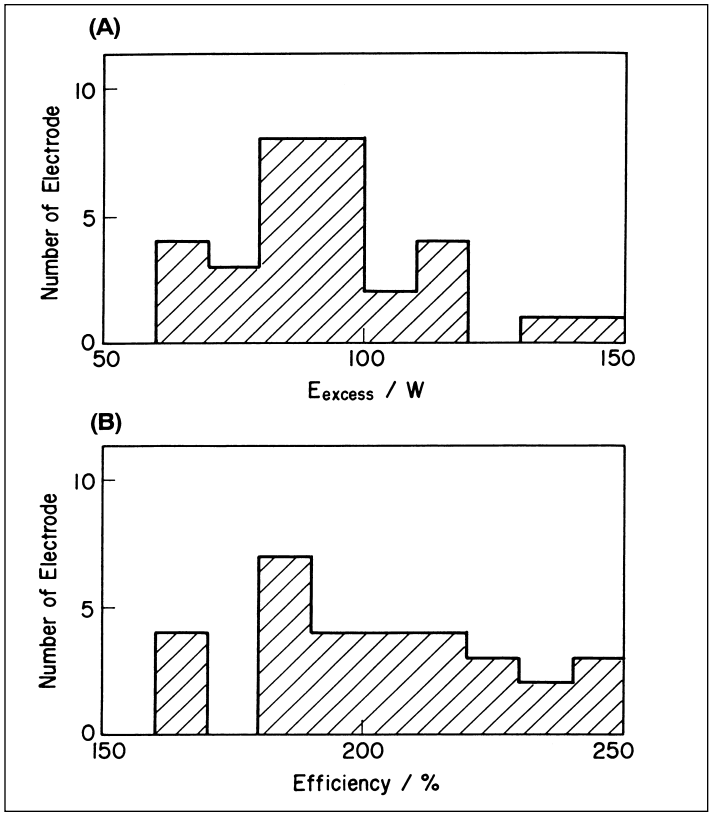

Figure 3. Distribution of excess energy and efficiency in the Au / H_2O electrolysis systems; (A) excess energy, (B) efficiency.

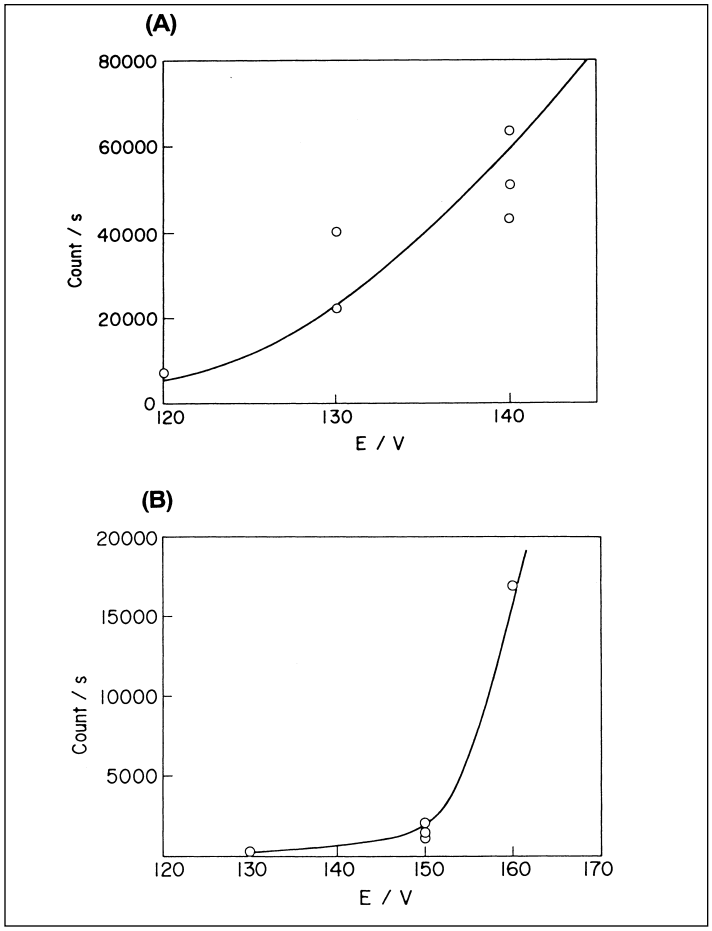

Figure 4. Intensities of the emission from the incandescent electrode counted by the REM neutron counter; (A) in Na_2SO_4, and (B) in K_2CO_3.

of the excess energies and the energy efficiencies obtained in all of the electrolysis systems listed in Table 1. Excess power was between 60 and 140 watts, centered mainly in the range 80 to 100 watts. Energy efficiency, output as a percent of input, was 150 to 220%, mainly in the range of 180 to 200%.

Electromagnetic Wave and Neutron Measurement

Under incandescent conditions at the electrode, strong emissions were detected by the neutron counter. However, the REM neutron counter used in this study detects electromagnetic waves as well as neutron activity, and it cannot distinguish between them. Judging by the fact that the electrical noise was so intense it interfered with the functioning of the thermocouple, we conclude that most of the signal detected by the neutron counter must have been caused by electromagnetic emissions rather than neutrons, although there is no denying the possibility that neutrons may also have been detected.

Figure 4 shows the intensity of the emissions plotted against input voltage in 0.5 M Na_2SO_4 and 0.5 M K_2CO_3 solutions. The emission intensity tends to increase nearly exponentially with voltage, depending on the nature of the electrolyte. This suggests that the emission is not induced from the electric power supplies or circuits but from the inner part of the cell, being caused by some form of charged particles (plasma) produced by the glow discharge from the incandescent electrode.

Transmuted Element Production

After glow discharge electrolysis, remarkable anomalous structures were found on the electrode surfaces. Figure 5 shows

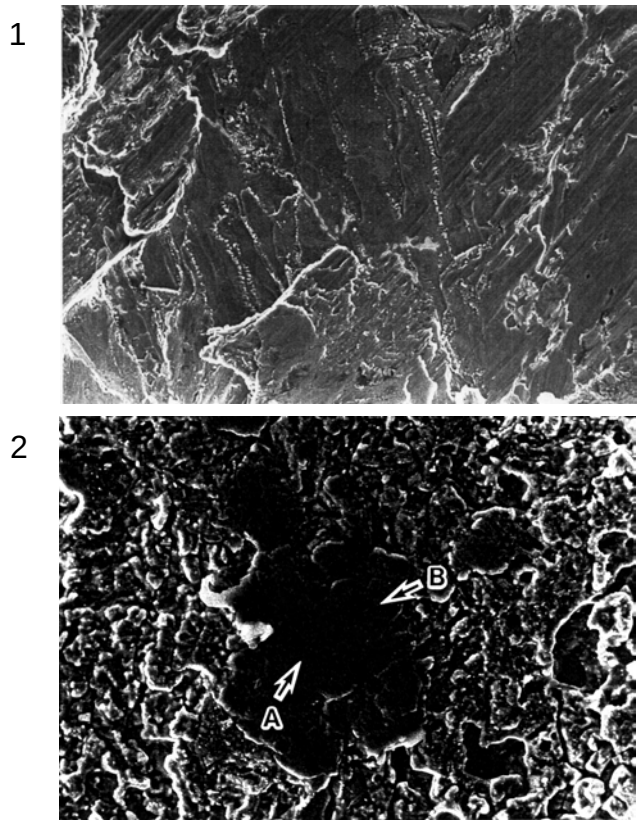

Figure 5. SEM images of the W-Type I electrode surfaces before and after the electrolysis; (1) before electrolysis, (2) after electrolysis. The duration of the glow discharge of the electrode is ten minutes. Arrows A and B in the image (2) show the measurement spots of EDX spectra in Figure 8.

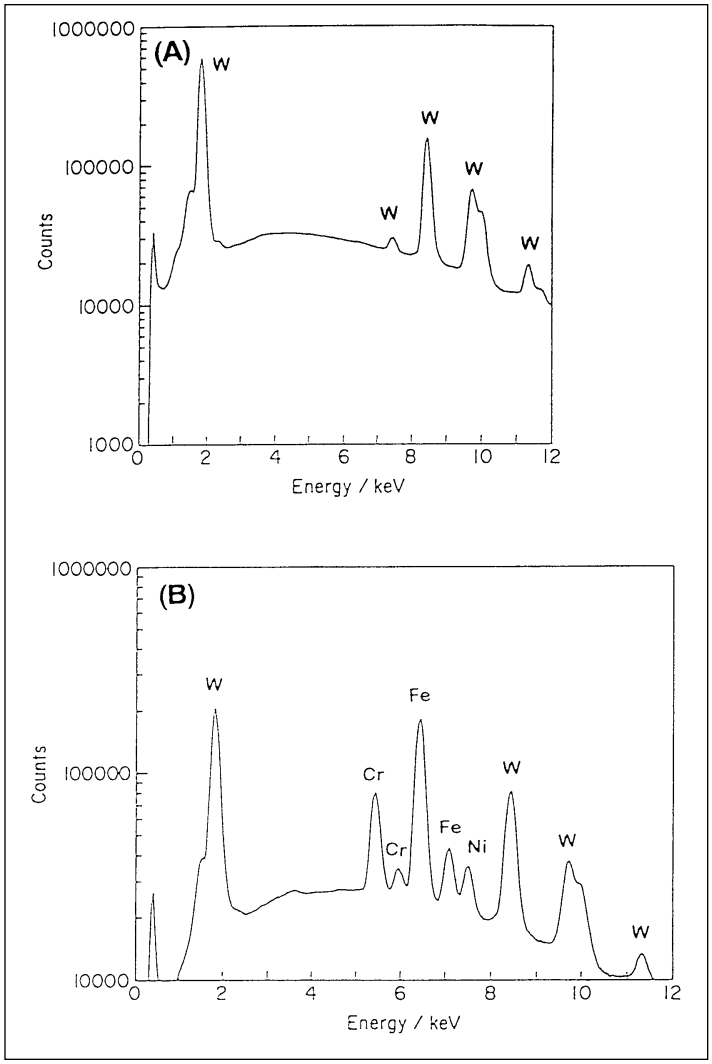

Figure 6. Typical EDX spectra in the W-Type I electrodes before and after the electrolysis; (A) before the electrolysis, (B) after the electrolysis.

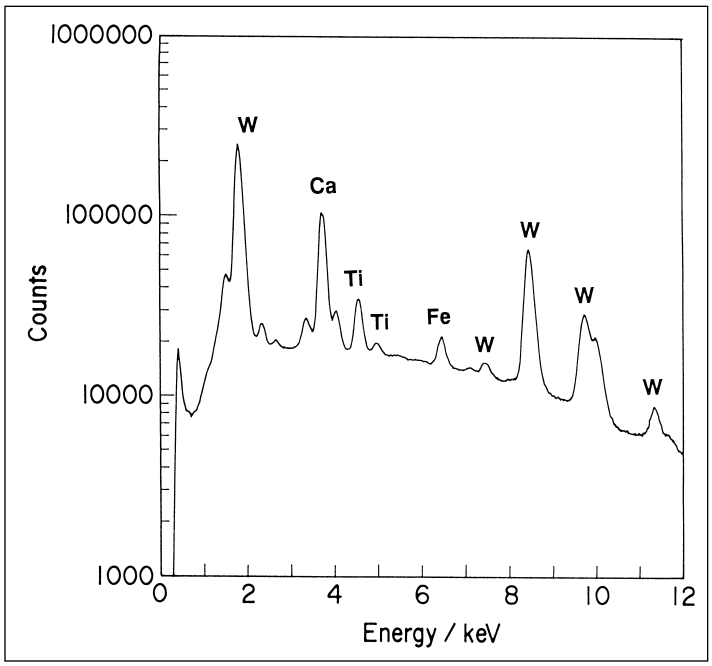

Figure 7. Typical EDX spectra on/in the W-Type II electrodes after the electrolysis.

scanning electron microscope (SEM) images of the electrode before and after glow discharge electrolysis. As seen here, the whole surface of the latter electrode is covered with a rugged structure not unlike that deposited by a volcanic lava flow. The W layers near the electrode surface have melted. This indicates the temperature may have risen to 3400°C or more. In addition, one can see a crater-like structure at the center of the melted area. Considerable amounts of several unexpected elements were detected in this area. The EDX spectra of freshly prepared electrode surfaces before electrolysis are shown in Figure 6a. Typical EDX spectra of the electrode surface after glow emission in 0.5 M Na_2SO_4 solution are shown in Figure 6b, with pronounced Fe, Cr, and Ni signals. Cathodes producing this spectra will be referred to hereafter as W-Type I. A few cathodes produced spectra like those shown in Figure 7, with strong Fe, Ti, and Ca signals. These will be referred to as W-Type II. The signal strengths of both Fe and Cr in Figure 6 (W-Type I) and Fe and Ca in Figure 7 (W-Type II) are comparable to those of original W electrode material before electrolysis. As described above, the amounts of Fe, Cr, and Ti found as impurities in the cell materials prior to electrolysis are infinitesimally small. The amount of Ca should not be large enough to give such a strong EDX signal even if all of the impurities in the entire cell were deposited on the electrode. In addition, the fact that two completely different types of EDX spectra (*i.e.* W-Type I and W-Type II) were obtained in the same kind of electrolysis system supports the hypothesis that these elements are not impurities but reaction products.

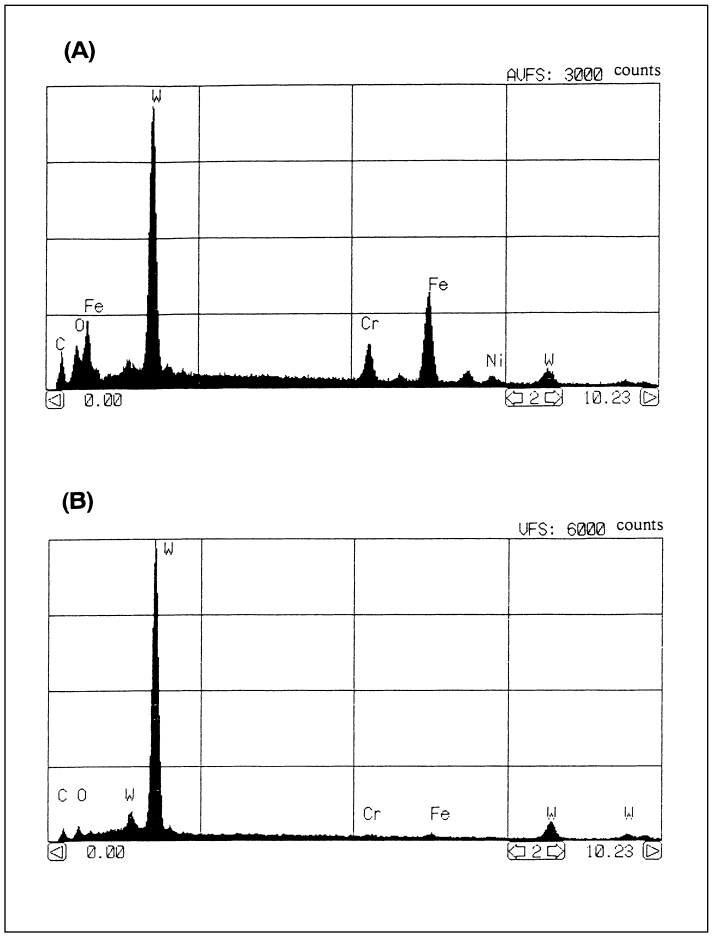

Figure 8. EDX spectra at points A and B of the crater-like structure shown in Figure 5.

Figure 8 shows the EDX spectra observed at two different spots on the crater-like structure shown in Figure 5 (which is W-Type I); one is a spot at the center part (point A) and the other is a spot near the edge (point B). In the spectrum from point A, strong signals of Fe and Cr and a moderate signal of Ni are observed, from which the contents of Fe, Cr, and Ni are estimated at 67.4, 16.9 and 7.9 at.% (atomic percent), respectively. The content of W is only 7.8 at.% in that location. However, the contents of Fe, Cr, and Ni decrease markedly with increasing distance from the center of the crater. As a result, Fe, Cr, and Ni at point B are reduced to 6.7, 1.9 and 0 at.%, respectively. This result is also obtained with EPMA analysis.

Isotopic Distribution

Besides Fe, Cr, Ni, and C, small amounts of Pb (0.6 at. %) and Re (0.3 at.%) were detected in the outermost layers of the W-Type I electrode to a depth of 160 Å. The isotopic distributions of Fe, Cr, Ni, Re, Pb, and W present in the outermost layers and in the bulk layers ranging between 1760 and 2400 Åare shown in Table 2 together with their natural isotopic abundance. In this calculation, the isotopic contents of ^{54}Cr, ^{54}Fe, ^{61}Ni, ^{62}Ni, and ^{64}Ni are assumed to be equal to their natural isotopic abundance for convenience because there is a suspicion that their SIMS signals are overlapping with those from other metal oxide ions. Consequently, the isotopic distributions for Cr, Fe, and Ni, exhibited in Table 2, is not strictly accurate.

For W, the ratio of the lightest isotope, ^{182}W, is slightly lower than its natural value at the outermost layers of the electrode, but elsewhere the isotopic distribution is normal. For Cr and Ni, slight deviations from the natural isotopic abundance are observed. For Fe, the deviation is very small. In contrast, for Pb and Re, the deviation is notable. Although the isotopic deviation for Fe scarcely occurred in the W/H_2O electrolysis systems, it was quite pronounced in the Au/H_2O,[1,3,5-8] Pd/H_2O,[18] and Pd/D_2O[Refs. 2,4] electrolysis systems. Similar isotopic deviation was observed for Fe detected in the electrodes after the electrolysis in Au/H_2O[Ref. 13] and Ni/H_2O[Ref. 14] electrolysis systems.

In view of the overlapping of Fe, Cr, and C at a center part of the crater-like structure of the W-Type I electrode surface there would be little doubt that the ^{56}Fe, ^{52}Cr, and ^{12}C atoms were produced simultaneously in that location by some sort of nuclear reaction. These reactions would be responsible for the generation of enough excess energy to push the W electrode to emit the plasma glow and the electromagnetic waves at input power levels too low for this to happen normally.

Table 2. Isotopic content of new elements produced on/in the electrode and tungsten of the electrode material.

Element	Isotope	Isotopic content within 160 Å from the electrode surface (1st run) (at.%)	Isotopic content within 1760-2400 Å from the electrode surface (11-15th runs) (at.%)	Natural isotopic abundance (at.%)
Cr	^{50}Cr	6.1		4.31
	^{52}Cr	79.1		83.75
	^{53}Cr	12.4		9.55
Fe	^{56}Fe	91.0		91.66
	^{57}Fe	2.9		2.19
Ni	^{58}Ni	63.0		67.98
	^{60}Ni	30.8		26.23
Re	^{183}Re	48.4		37.1
	^{185}Re	51.6		62.9
Pb	^{206}Pb	38.5		25
	^{207}Pb	55.0		22
	^{208}Pb	6.5		52
W	^{182}W	25.3	$26.6^{+0.6}_{-0.5}$	26.3
	^{183}W	14.9	$14.5^{+0.3}_{-0.4}$	14.3
	^{184}W	30.4	$30.5^{+0.6}_{-0.5}$	30.6
	^{186}W	29.0	$28.2^{+0.8}_{-1.2}$	28.6

Plausible Nuclear Reactions

The results obtained in the present study, *i.e.* excess energy generation, glow and electromagnetic wave emission, transmuted element production and the isotopic deviation observed in some of these transmuted elements, forces the conclusion that some sort of nuclear reaction must have occurred during electrolysis. From the SIMS analysis, the volumes of Na, B, Mg, and Li at the outermost layers of the electrode were found to be 1.9, 1.18, 0.06, and 0.0008 at.%, which was far less than the levels for Fe or Cr. The isotopic distributions of B and Mg are not much different from their natural isotopic abundance. In addition, Li and Mg were not detected in the electrolyte solution after electrolysis by the ICP analysis. This fact suggests that the major nuclear reactions are not induced by nuclear fusion reactions involving only deuterons and protons. If the new elements, *e.g.* Fe, Cr, etc., are produced as a result of the combinations of some nuclear fusion reactions starting from deuteron-deuteron or deuteron-proton reaction, fairly large amounts of new elements lighter than Cr, at least comparable to the discovered quantities of Fe or Cr, should be detectable in the W electrode or in the solution after electrolysis.

It is well known that a very strong electric field exceeding 108 V/cm is formed across the electric double-layer at the electrode/solution interface. Under such strong electric fields,

electrons passing through the double-layer by tunneling may, perchance, be captured by protons or H atoms as follows,

$$H^+ + e^- \rightarrow \text{neutron} \quad \text{(A)}$$

although an overwhelming majority of the electrons are consumed by the hydrogen evolution reaction. With an increase in the electric field the probability of the reaction (A) would be enhanced rapidly, which might make the neutron emission possible (Figure 4). The reaction (A) is endothermic so that the neutrons produced should be thermo-neutrons with a low kinetic energy. Consequently, the greater part of the neutrons thus produced would be captured by the W electrode material, for example in the following reactions:

$$^{182}W + \text{n neutron} \rightarrow {}^{182+n}W \quad \text{(B)}$$
$$^{182}W + \text{neutron} \rightarrow {}^{183}Re \quad \text{(C)}$$
$$^{184}W + \text{neutron} \rightarrow {}^{185}Re \quad \text{(D)}$$

A slight decrease in the content of ^{182}W on the outermost layers of the electrode (Table 2) supports the possibility of these reactions. The $^{182+n}W$ produced should be unstable, disintegrating to form Fe, Cr, and C, for example, for W-Type I electrodes as

$$^{184}W \rightarrow {}^{56}Fe + 2\,{}^{52}Cr + 2\,{}^{12}C + 12\beta^- \quad \text{(E)}$$
$$^{184}W \rightarrow 2\,{}^{56}Fe + {}^{52}Cr + 5\,{}^{4}He + 12\beta^- \quad \text{(F)}$$
$$^{184}W \rightarrow 3\,{}^{56}Fe + 4\,{}^{4}He + 12\beta^- \quad \text{(G)}$$

or likewise to form Ti and Ca, for example, for W-Type II electrodes as

$$^{184}W \text{ Æ } 2\,{}^{48}Ti + 2\,{}^{40}Ca + 2\,{}^{4}He + 14\beta^- \quad \text{(H)}$$
$$^{184}W \text{ Æ } 2\,{}^{56}Fe + {}^{40}Ca + 8\,{}^{4}He + 14\beta^- \quad \text{(I)}$$

Perhaps this is why the production of Fe, Cr, and C were closely linked. These nuclear transmutation reactions are assumed to occur locally in the W electrode. This would promote the development of the crater-like structure seen on a W-Type I electrode or the fine crater structure formed on an Au electrode in the Au/H_2O electrolysis system.[3,7,8] Perhaps these reactions occur at the rim of micro-cleavages and edges of the fractured face of the electrode, since it was confirmed that the craters are developed mainly at those areas. Tunneled electrons and protons and/or H atoms may become highly concentrated at the cleavages and edges of the fractured face, forming an anomalous phase. We suspect that the probability of reaction (A) is enhanced when this occurs. Recently, in this context, the idea that the formation of electron clusters on the electrode surface promotes the nuclear transmutation reaction was proposed by Hal Fox.[19] The nuclear transmutation reactions, (B) to (I) would release γ-rays, the energy of which would be expended on the relaxation by the surrounding W atoms of the electrode. As a result of this, X-rays would be emitted from the electrode, as have been detected by some researchers in Ni/Li_2SO_4, H_2O[Ref. 14] and in Pd/LiOD, D_2O electrolysis systems.[17-20] In the present study we observed that the clear reddish-purple glow extended into the bulk of the solution, completely enveloping the incandescent electrode. This means that the generation of the plasma occurs even in solution separated by the distance of *ca.* 2 cm or so from the electrode.

Thus, strong evidence of low temperature nuclear transmutation reactions was obtained. However, the mechanism appears to be very complicated. The reaction schemes we propose here are not conclusive and may only be a few of the many nuclear reactions that might occur simultaneously in this electrolysis system. In addition, it remains unclear why two types of W electrodes exist, showing different spectra, presumably caused by different reaction schemes. To clarify these issues, more detailed investigations in various fields will be necessary, including spectroscopic analysis of the glow and electromagnetic wave, clarification of the product elements and their distribution in the electrode, clarification of the product elements in solution, detection of X-ray, γ-ray and neutron emissions and their characterization, morphology of the electrode surface, etc.

Nevertheless, we believe that it would not be so difficult to scale-up in output power levels of 1,000 to 10,000 kW by utilizing the low temperature nuclear transmutation reactions occurring in the W/H_2O electrolysis system, judging from the fact that the excess power of 200 watts was generated from a W electrode of only 0.5 cm^2.

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The Perennial Challenge of Anomalies at the Frontiers of Science

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Abstract

The history of science is full of rejections of novel discoveries that challenged the dominant paradigm. The reasons for rejection on the part of the scientific community are discussed. Nonetheless, scientific anomalies are crucial to promoting continuous scientific innovation and breakthroughs. The most challenging anomalies are recognized only after a reasoned explanation has been offered and accepted. Many of these anomalies come from the "frontier sciences," areas of scientific inquiry that are not yet mainstream. The extraordinary obstacles that frontier scientists face are elaborated. In light of this, strategies are offered toward progress, particularly for research in homeopathy and low dose bio-effects.

Several frontier areas of research in biology and medicine relate to the subtler features of life that seem to defy explanation by conventional molecular mechanism. Frontier scientists working in the areas of consciousness studies, epigenetic inheritance, certain topics in bio-electromagnetics, and homeopathy and other low dose bio-effects offer science the gift of new questions that go beyond the dominant paradigm of mechanical reductionism. Often regarded as isolated anomalies by the mainstream, the results of their investigations taken collectively show the need for a larger paradigm to accommodate them. Biology appears to be entering a crisis. Whereas conventional science maintains that biological information is stored and transferred via biomolecular structures such as DNA, the novel frontier findings suggest that other informational signals not attributable to discrete chemical structures may elicit biological effects. These signals may be interacting with a more subtle bioregulatory system that is a property of the whole organism, such as its endogenous electromagnetic field.

Introduction

Scholars have documented the resistance to novel scientific discovery by various groups, such as economic and religious groups. However, there has been less attention given to the resistance of the scientific community itself to challenging scientific discoveries.[1] Nonetheless, we find it in the history, philosophy, and sociology of science and especially in the writings of scientists who have personally suffered obstacles due to this resistance. The scientific community believes that it deals with novel controversial discoveries in a rational manner, yet this is rarely the case.

The history of science, medicine, and technology is full of rejections of novel discoveries that seemed anomalous in their time. Contemporary scientists laughed when Benjamin Franklin proposed that lightning was a form of electricity. Semmelweiss, a Viennese physician who documented that washing one's hands before obstetrical assistance would prevent childbed fever, was scorned and rejected by his contemporaries. William Crookes, the noted British scientist and member of the Royal Society who discovered the element thallium, was bitterly attacked by his scientific colleagues for his research in parapsychology. Lord Kelvin said that X-rays were a hoax. Helmholtz, who was not a physicist, but a physician who formulated the theory of energy conservation and who was opposed by the physicists of his time, noted how the "greatest benefactors of mankind usually do not obtain a full reward during their lifetime."[2] Lister warned medical students against blindness to new ideas in science, such as he had encountered against his own theory of antisepsis. Long after their time, many of these scientists whose ideas were rejected were regarded as formative thinkers who made significant contributions or even launched new scientific paradigms.

The Scientific Paradigm

In 1962 Thomas Kuhn published a seminal work, *The Structure of Scientific Revolutions*,[3] which addresses the manner in which science advances. Kuhn's main thesis is that science is not a slowly growing body of knowledge approaching a true description of the world. Instead, science is characterized by periods of quiet research activity leading to a crisis, which may last for years to decades. During this transition period, scientific problems appear that cannot be resolved within the given paradigm. Scientific anomalies, experimental results that cannot be reconciled with current theory, may occur. Such anomalies are critical to progress in science. In fact, each new major advance in science starts with an anomaly that is unacceptable at first.[4] Therefore, anomalies are valuable because they inspire new ways of thinking. Conventional scientists attempt to explain the anomalies within the framework of the dominant paradigm, while a smaller, usually younger group of scientists develop an alternative paradigm. The crisis is resolved by a dramatic change of perspective, a paradigm shift. A struggle typically ensues that may result in the overthrow of the old paradigm. After the triumph of the new paradigm, the old paradigm eventually disappears in a time frame necessary to provide stability and confidence in the new paradigm. What was an anomaly earlier now becomes the expected result. Textbooks are rewritten in such a way that they even disguise the very existence of the revolution that generated them. Eventually, new research uncovers problems with the new paradigm. Then the process repeats itself.

Kuhn noted how unconsciously ingrained the dominant paradigm is. He wrote: "Scientists often work from textbook models acquired through education and through subsequent exposure to the literature without knowing or needing to know they are accepting a community paradigm."[5] They work to fit their data into the ruling paradigm. The usual peer review process in science provides an adequate forum for evaluating new ideas and discoveries, but this is only true if those ideas and discoveries do not challenge the paradigm. As was mentioned previously, those considered incomprehensible or too challenging to current scientific understanding are typically rejected. Michael Polanyi, in defending this conservative nature of science wrote: "There must be at all times a predominantly accepted scientific view of the nature of things, in the light of which research is jointly conducted by members of the scientific community. Any evidence which contradicts this view has to be disregarded, even if it cannot be accounted for, in the hope that it will eventually turn out to be false and irrel-

evant."[6] Although the neglect of other possible conceptual categories is not malicious in intent, it can become malicious in effect because the dominant paradigm discourages, and is intolerant of, competitors. That is, scientists prefer their work to appear as an integral, growing body of knowledge under the auspices of a single paradigm. Perhaps this is because scientists are encouraged to demonstrate what they know rather than to raise truly novel questions that challenge what they think they know.

Kuhn recognized an "essential tension" within science because it must preserve its accumulated knowledge by acting cautiously and conservatively and on the other hand remain an open system ready to take in novel, potentially revolutionary data and concepts.[7] This balance is maintained in a number of ways. In the first place, science places the burden of proof on those who claim to discover scientific anomalies or otherwise make revolutionary scientific claims. Secondly, the proof must be commensurate with the claim; that is, extraordinary claims require stronger than usual proof. (This relates to the principle of parsimony in science, in which the simplest adequate theory is the most acceptable.)

Each new major advance in science starts with an anomaly that is unacceptable at first.

It is interesting to note that Kuhn believes that science generally progresses in a positive direction,[8] but that some paradigm shifts have reversed concepts such that aspects of an even older paradigm may return in the form of new input, reshaping old models.[9] It is a common conviction that the world is progressing in one direction scientifically and socially, but as Kuhn points out, very often the clock is turned back with new scientific developments. For example, relativity and quantum theory, two of the most significant scientific paradigm shifts in the twentieth century, both turned back the clock in certain ways. The gravitational aspects of Einstein's general relativity reflect back to Newton's predecessors, and quantum mechanics has reversed some of the methodological prohibitions that had occurred in the earlier chemical revolution. Needless to say, the reshaping of older views into a new paradigm would have significance for homeopathy and low dose bio-effects. Many scientists today have the attitude that these phenomena from an era predating modern molecular biology have been overthrown, or that at best they represent a placebo effect. These scientists are victims of historicism who refuse to accept anything from an earlier time as bearing any modicum of truth.

Scientific Anomalies

According to science sociologist Marcello Truzzi, an anomaly is something that:

- actually occurs (that is, something both perceived and validated),
- is not explained by some accepted scientific theory,
- is perceived to be something which is in need of explanation,
- contradicts what we might expect from applying our accepted scientific models.

I would suggest that the anomaly's lack of fit with accepted theory is the necessary element common to any real anomaly. It is a fact in search of an explanation.[10]

In the field of anomalistic observations, or anomalistics,[11] that is, enquiry into anomalies and their role in science, there are different types of scientific anomalies, at least in retrospect. There are those that are recognized in their time by the scientific mainstream and become the subject of legitimate research activity, and those that go ignored by the mainstream because they are apparently too threatening. Many of the latter come from the "frontier sciences," that is, whole areas of scientific enquiry that have not yet been incorporated into conventional science. These areas are ignored or even considered irrelevant by the mainstream, in some cases because they are often residues of older systems of knowledge that have been denounced as pseudoscience as, for example, parapsychology and astrology.

The history of science shows that the most challenging anomalies, those that seriously challenge the dominant paradigm, are ignored by the scientific mainstream until they are explained, and only then are they recognized in retrospect. The term retrorecognition has been given to this type of recognition which is given only after there is a compelling explanation for the anomaly.[12] Such anomalies make the scientific community uncomfortable, as it likes to think of science as an integral body of knowledge that is nearly complete. These unexplained facts are either ignored, reduced in importance, or merely accepted as "givens." Several factors are behind this attitude, such as the sheer intellectual difficulty of recognizing anomalies, the tendency to ignore a problem that cannot be easily solved, and the conservatism of science. But there is something more. The recognition of what were once anomalies under an older paradigm only after they are reconciled with a new paradigm clearly shows that the scientific community is unable to live with ambiguity and cognitive dissonance (psychological inconsistency). However, frontier scientists whose work challenges the paradigm appear to be of a different psychological makeup, with a higher tolerance for ambiguity and cognitive dissonance. It is interesting to note that such tolerance correlates highly with creativity scores in psychological testing.[13] Furthermore, frontier scientists may be working from dimensions other than rationality and logic, for Kuhn has written, "The man who embraces a [new] paradigm at an early stage must often do it in defiance of the evidence. . .A decision of that kind can only be made on faith."[14]

The Role of Skepticism

Indeed, it is rare to find scientists who are true skeptics, that is, without prejudice, open, and tolerant of uncertainty. It is unfortunate that the term "skeptic" is being used by many who are disbelievers or debunkers whose aim is to remove the anomaly, rather than true nonbelievers.[10] This appears to be particularly the case for organized so-called skeptics groups such as the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), which sponsors unusual critiques and other activities to discredit anomalous scientific claims, undermining the usual processes of replication attempts and peer review. In some cases this has involved members outside of the scientific community, such as professional magicians, in a process analogous to inquisitors for a dogmatic church.[15] Unfortunately, this has the effect of creating fear among those who would have an interest in trying to replicate the anomaly, thereby blocking real scientific enquiry.

Where there are anomalies and frontier areas of science that seriously challenge the paradigm, the scientific community is

often polarized into two categories: believers and disbelievers. Although the scientific community may consist largely of disbelievers, sometimes the frontier scientists or proponents of an anomaly act as "true believers." In some cases there are societies of "true believers" centered around maverick scientific claims that do not welcome open dialogue. In my opinion, they are no better than some of the mainstream scientists they criticize. Sometimes the discoverer of a challenging fact overstates his claims, jumping to conclusions about the importance of his discovery without adequate data. On the other hand, the "essential tension" of the scientific process renders it very difficult to find the right balance in reporting anomalous claims. If the discoverer understates his claim, it may go ignored; if he stresses its revolutionary character, it may gather more attention and resources for further study. From my own work aiming to facilitate new research and greater open-mindedness in frontier areas of science, I find that it is difficult to stand firm on the fine line that separates the believers from the disbelievers. In my opinion, this is the best viewpoint to encourage an attitude of nonbelief that stimulates new questions and further experimentation. Apparently this viewpoint is not well understood or liked by most, as I am often accused of being "the enemy" of one group or the other. However, openness and a healthy level of skepticism are crucial in order to avoid pathological science.

The Power of New Questions and Approaches in Science

Scientists must approach nature by asking questions of her, and it is impossible to pose a question without some expectation or anticipation. Clearly, from the analysis of Kuhn and numerous other scientific historians and sociologists, science is not context-independent. Scientific objectivity does not reside in theory-free perception. It lies in the flexibility to reject a cherished theory when an anticipated observation cannot be confirmed, and a contrary event or fact is perceived instead. Scientists may say that they see the data with their own eyes, but in fact, they see it through their brains. They cannot bypass this central focus and filter full of biases, products of both evolution and society. It is very difficult to "see" scientifically beyond the context of theory or expectations.

As an example, consider the following. Before Darwinism, the paradigm that preceded evolutionary theory was natural theology, in which each creature was considered to be perfectly adapted to its environment and designed for full functionality. While natural theology dominated, no one noticed that some organisms were less well-adapted to their environment. Natural theology would not permit such questions. Ducks with webbed feet that could not swim, birds with wings that could not fly, and bats with eyes that could not see, went unnoticed. Darwin asked new questions and noticed that some animals were less well-adapted for their environment. He explained these anomalies on the basis of natural selection, an ongoing evolutionary process. The point here is to show the power of asking new questions that take us outside the present scientific theory or paradigm. These offer the possibility of a breakthrough to a new way of seeing nature. As physicist Werner Heisenberg noted, "What we observe is not nature itself, but nature exposed to our method of questioning."

Another historical example of this goes back to microscopy of the seventeenth and eighteenth centuries. The great microscopist van Leeuwenhoek and his contemporaries claimed they saw minute forms of complete babies inside sperm under the microscope. Their observations were shaped by the 2000-year-old idea that women contributed nothing to conception but the womb as an incubator. In this case, too, preconceived ideas determined what was scientifically observed.

In another historical example involving microscopy, different methodological approaches of observation based on different philosophies led to a scientific debate. In the 1940s the bacteriologist Adrianus Pijper maintained that bacterial flagella are not true motor organs, but are essentially insignificant, being merely cell wall by-products of bacterial motility.[16] From his observations of live bacteria under the dark-field microscope, he claimed that he saw small changes in the body forms of the bacteria, a slight undulating motion, which he proposed as a theory of bacterial motility. As it turned out, his view was unpopular because he was far outnumbered by those who fixed and stained dead bacteria for light microscopy or electron microscopy, which was newly introduced at that time. The majority of scientists then claimed that flagella were indeed the organelles of motility and showed evidence via microphotography of sites of flagellar attachment to the cell body. Pijper rejected these physical approaches, emphasizing that studies on the living state itself were critical to understanding cellular motility, and that the approaches using dead cells might yield artifacts. This led to an ongoing debate, as both schools refused to "see" any evidence beyond their own viewpoints. In the end, Pijper lost the debate. His refusal to acknowledge the "superiority" of the electron microscope was held against him by the scientific majority.

Antoni van Leeuwenhoek

Beyond the specifics of this historical debate, the latter case is important for us to consider because it reveals the perennial struggle between the naturalist and the mechanist in biology. It shows how naturalists' observations of living systems were replaced by a modern biology tightly linked to physicochemical reductionism as new powerful, expensive, prestigious, technological tools came into being. These new physical methods require an often insensitive manipulation of organisms that distorts or even kills them in order to study them. The naturalists' approach came to be regarded as old-fashioned and even reminiscent of vitalism by the new biologists, who were led by several physicists-turned-biologists in the 1940s and 1950s. These were the people who ushered in a new scientific era, the revolution that became the dominant paradigm of molecular biology and biotechnology in recent decades.

Resistance of Scientists to New Discoveries

Studies on the psychology of science suggest that scientists have a resistance to acknowledging data that contradict their own hypothesis.[6] In one study on falsifiability, a simple experiment was set up to compare the performance of a group of scientists and a group of clergymen. A false hypothesis was given to all the participants. The means was provided for them to test the hypothesis, which they did not know was false. The results showed that most of the scientists refused to declare the hypothesis false, clinging to it longer despite the lack of evidence. The clergymen, however, more frequently recognized that the hypothesis was false. This and other studies show that scientists are at least as dogmatic, authoritarian, and irrational as non-scientists in resisting unexpected findings.

The historical examples cited earlier illustrate only a few reasons why resistance to novel discoveries in the scientific community occurs. Analysis of many other examples shows numerous ways in which scientists resist discoveries that are old paradigm breaking and new paradigm making. One of these mentioned earlier is the loathing of ambiguity. Most scientists prefer to elaborate what they think they know rather than focus on what they do not know; perhaps this is simply human nature. Along with that is fear of novelty. New discoveries require restructuring older ideas and ways of doing science. Change, whether it is personal, social, or intellectual, is difficult and may even cause a lifetime of work to become unimportant and obsolete. Related to this is the fact that older scientists have a tendency to resist the novel work of the younger. Innovative "outsiders" may also be rejected by the "insiders," especially if the new discovery comes from outside the field, as in the case of cold fusion.[17] There is also a faithfulness to old models, reflecting a belief in scientific concepts or simply conservatism. When Thomas Young proposed a wave theory of light, the scientific community remained faithful to the older corpuscular theory for some time. This tendency sometimes reveals a dogmatism or scientism. Paul Feyerabend accuses contemporary science of being a "church" in which scientists play a role that is in many respects similar to the role bishops and cardinals played not too long ago.[18] Another mode of resistance, also illustrated by the example cited earlier of van Leeuwenhoek and his colleagues, is blindness due to preconceptions. It is extraordinarily difficult to "see" what may lie beyond one's paradigm, which delimits all questions posed of nature and ways of perceiving her. Anomalies without "causes" or an adequate explanatory model are rejected because they do not fit neatly into the body of science. If an anomalous claim pertains to an area reminiscent of mysticism, religion, older paradigms that have been overthrown, or pseudo-science, this may be grounds for rejection by those who feel threatened by these associations. Occasionally conflicting personal religious ideas may be another reason for rejection. That was the case for both Galileo and Copernicus, and it also appears to be a factor in the debate between creationists and evolutionists. Scientists evaluating an anomalous finding sometimes take into account the relative professional standing of the discoverer as well as the number of prestigious followers of the new claim, and these are primarily political concerns. Concerned about their reputation, scientists are reluctant to take the lead in helping to advance a new claim. In relation to this, publications about the new scientific claim in other than the most prestigious peer-reviewed journals are taken less seriously and may be grounds for rejection or simply neglect. Finally, and perhaps most important to contemporary science, it is true that where substantial funding is involved, patronage to those ideas endorsed and funded to the exclusion of others is overwhelming.

Copernicus

Galileo

Today, because of large economic interests in science, biomedicine, and technology, and the increasing overlap between academia and industry, the resistance to new discoveries or ideas that challenge the dominant paradigm goes well beyond ideological concerns. Challenging ideas can be seen as threatening to big business interests, including the interests of industries waging war against cancer or AIDS. Anyone who is a proponent of ideas that threaten large-scale economic interests can expect even harsher backlash from the scientific community, which in mainstream biology and medicine is now closely linked to pharmaceutical and biotechnology firms. Surely that is one of the most significant reasons for rejection of novelty in biology and medicine today. Moreover, the many different fields of biology with their varied orientations to life that existed before big business science are presently extinct, at least in the U.S.

It is simply taboo to offer a serious challenge to the dominant paradigm, and those who propose such maverick ideas or findings suffer extraordinary obstacles. Similar to the acceptance of novel discoveries, the obstacles are especially severe for those whose work threatens big economic interests that are now coupled to mainstream science.

Obstacles Faced by Scientists Who Challenge the Paradigm

There are a number of serious, even extraordinary obstacles that scientists presently face as proponents of paradigm-challenging discoveries or where their reputation becomes associated with research on unconventional topics. These obstacles are not characteristic of a particular culture; they appear worldwide. These are:

- difficulty in obtaining funding, as there are simply no usual sources
- difficulty in publishing, and there is no *real* peer review
- loss of camaraderie (colleagues fear a loss of reputation by association with a scientist who is deemed an outcast)
- loss of reputation in the scientific community regardless of one's stature
- obstacles to promotion, retention, and tenure
- possible critical backlash from the scientific community
- possible loss of employment and future employment opportunities

The pursuit of research in frontier science areas such as homeopathy and extremely high dilution bio-effects, novel medical therapies or diagnostics, new energy technologies, and consciousness studies—research in any area that challenges the dominant paradigm—presents extraordinary hardships for scientists. Merely expressing an interest in these can affect one's reputation as a serious member of the scientific community. Whether one is a post-doctoral researcher, a junior professor, a member of a distinguished national academy of science, or a Nobel laureate, essentially the same obstacles remain. For those who have seemingly overcome these hurdles, publication of challenging scientific results may bring about unforeseen backlash in the form of discrediting the discoverer or the claim without really disproving it, prohibiting it from being tested by others. Moreover, this may prevent consideration of similar challenging claims in the scientific literature, textbooks, and education. The proponent of the anomalous claim is thus isolated from further debate and interaction with the rest of the scientific community.

Many people associate such repressiveness with earlier times, but there are living examples today. One illustrious

example—a case where big economic interests in biotechnology and medical testing are threatened—is that of Peter Duesberg, Professor of molecular biology at the University of California at Berkeley. His work identifying the first oncogene to cause cancer and also decoding the first retrovirus genes earned him an outstanding international reputation as a molecular biologist and virologist. However, because of his recent criticism of the oncogene theory of cancer and especially his criticism of HIV as the cause of AIDS, he has essentially been silenced by the scientific community. No one will debate his arguments either in writing or in person. Duesberg is unable to publish in prestigious peer-reviewed journals, not even the *Proceedings of the U.S. National Academy of Science,* despite his stature as a member of the National Academy, because they rewrote the rules especially to prevent him from publishing. He lost his annual $300,000 Special Investigator Grant from the U.S. National Institutes of Health, which was expressly for the purpose of asking novel questions, and, as a result, his students and technicians have had to leave. Duesberg has been excommunicated from the scientific community. Needless to say, the review panel who refused to renew his grant included scientists who earn their living from the theories that Duesberg is undermining, and many others in the mainstream also earn their living from these theories.

The usual peer review process in science provides an adequate forum for evaluating new ideas and discoveries, but this is only true if those ideas and discoveries do not challenge the paradigm.

Strategies Toward Progress in the Frontier Sciences

With all those obstacles and resistances, how can we help to facilitate rational, objective criticism and fair peer review of anomalous claims? What strategies can we implement to bring progress to a frontier science area such homeopathy and low dose bio-effects?

•We must recognize that there is no single critical experiment that can prove an anomaly. This is ridiculous from the scientific viewpoint, as the history and philosophy of science has shown that there is no such thing as a critical experiment.

•More empirical studies need to be undertaken by more researchers, and we need to work together at least to provide peer review of each others' work, if not outright collaboration. All too often, the work of pioneering frontier scientists represents isolated, individual efforts. By contrast, most quality science involves collaborative efforts. It is important to build on one another's work. Just as cooperative or collective phenomena in nature have unusual stability, there is also a strength in collective scientific efforts that is harder to dismiss.

•An interdisciplinary approach to anomalies is absolutely necessary, because we do not know ultimately where an anomaly will fit. In the case of homeopathy or high dilution bio-effects, interdisciplinary group collaboration with experiments performed in tandem on the same high dilution would be worthwhile, because for the first time it would reveal physical, chemical, and biological information about a single preparation. This could develop into an international task force, a global cooperation, to address the problem.

•We must produce well-designed experiments that are well-communicated in the scientific literature, which will presumably continue to demonstrate the effect in a wide variety of biological systems.

•We must show replication of phenomena, especially by skeptics.

•We must also discover and document where no such anomalous effects are observed, so that the boundary conditions of the effect are clear.

•Conceptual work toward achieving a theoretical explanation for the effect is crucial for its recognition.

•We must keep communication flowing between those working in the field who don't agree on the details. A diversity of opinions is extremely important because it drives the formation of new questions. Good science requires good and effective criticism. Furthermore, failures in communication from splinter groups in frontier areas of science only weaken the case, as their presence makes a statement to the scientific community that there is weakness or irrational behavior associated with the anomaly.

•One of our best strategies would be to serve as mentors and inspire younger scientists to conduct research in novel areas of science. For one, it is most likely that presently established scientists will have to retire before a paradigm shift is completed, and most of them will not change their viewpoint. As physicist Max Planck sadly noted, ". . .A new scientific theory 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."[19] Niels Bohr put it somewhat differently: "Science advances—funeral by funeral."

•Retired scientists, who have less to lose in terms of their reputation or funding, are occasionally more open to new ideas or discoveries. Moreover, they may still wield political power in the scientific community. Therefore, communications with or other involvement of retired colleagues may be a viable strategy.

•Another strategy that may be used to advance scientific recognition of a challenging anomaly is to identify and align with social, political, or economic interests that would very much like this particular piece of scientific unorthodoxy to be true, or at least to be highly interested in resolving the issue. When Robert O. Becker, medical researcher in bio-electromagnetics, had the unorthodox idea in the 1970s that electromagnetic fields from power lines might be a health risk, he found no sympathetic ears in the scientific community or the electric power industry. However, he communicated the issue clearly in his popular writings and launched a public campaign in which the people demanded unbiased research to test his ideas. Within less than two decades, substantial U.S. government funds became available for this purpose.

•Another approach related to this strategy is to develop a successful application of the anomaly that will bypass the scientific community altogether. Once the application is adopted, scientists will be naturally drawn to the fundamental discovery underlying it.

•Finally, we should attempt to foster true skepticism–neither denial nor disbelief, but a balanced state of openness. The best way to do this is by personal example, by maintaining a level of healthy skepticism ourselves, with an emphasis on further questions. This is crucial to keeping science an open system of inquiry.

Role of Homeopathy and Low Dose Bio-Effects in the Future of Science

The observations of low dose biological effects challenge the dominant paradigm of mechanical reductionism, of viewing life as a collection of biomolecules responding to molecular stimuli. The enhanced potency of very low doses, as in homeopathy, appears to challenge molecular theory, one of the pillars of modern chemistry. On the other hand, it may demonstrate that something else is occurring at these very low doses that does not involve molecules. Biological effects of low doses have been demonstrated in a growing number of studies worldwide, and we are now in the midst of a paradigm struggle. As Kuhn predicts, an intellectual and emotional battle is occurring: there have been nasty editorials, tenure battles, debates and arguments, splinter groups, the rejection of papers, frequent denial on the part of the scientific community, and many questions that have been raised for further research. From an historical perspective, the accretion of anomalies or numbers of anomalous observations in themselves are not enough to product a paradigm shift. Further effort is required. Conceptual work toward new theories and a paradigm that would reconcile them is critical to their recognition by the scientific community. No one other than the proponents of the anomalies will accomplish this. It remains for us, the frontier scientists, to design the theories, elaborate the new paradigm, and show how they explain our anomalies.

One of the best examples of a conceptual revolution is found in a nineteenth century science fiction classic, E.A. Abbott's *Flatland*.[20] The inhabitants of Flatland live on a two-dimensional surface and have no concept of our third dimension. When a sphere visits Flatland, he is perceived as an anomaly: a circle that first grows bigger and then smaller. The sphere then lifts the leader of Flatland into the third dimension where he can see his whole world. This novel perspective not only clears up the anomaly, but offers a new perspective for everything. We need a similar major conceptual breakthrough for homeopathy and low dose bio-effects. When it occurs, it may reframe our ideas of matter, energy, life, and information in a radically new perspective.

Presently the greatest challenge to those working on homeopathy or low dose bio-effects is to develop a proper theoretical context for their observations. We need a theory of very high dilutions in the context of the organism. This would enable us to form testable questions that move the research from an accumulation of anomalous observations to a sequence of facts that fit together like pieces of a puzzle. It is becoming more apparent that molecular theory offers nothing but conceptual limitations for this field of inquiry, and that an alternative that goes beyond it must be sought. Moreover, I anticipate that a breakthrough toward a radically new view of chemistry is in the making, and it is long overdue. Quantum chemist H. Primas, wrote:

> The richness of chemical phenomena renders it impossible to discuss them exhaustively from a single point of view. The molecular view is just one of these views and has no privileged status. . .While the molecular theory fell on fertile ground, the further development of a theory of chemical substances was deprived of intellectual incentive. Even today, chemical thermodynamics and chemical kinetics are still in a rudimentary state of development achieved at the turn of the century. . .The molecular idea flourished and degenerated into a dogma, requiring unqualified faith.[21]

He also wrote, "Our vision of the world will be severely limited if we restrict ourselves to the molecular view. Molecular theories describe some aspects of matter, but it is not wise to think that they give us a description of reality 'as it is.' If questions of a different kind can be asked, nature will then respond in a new language."

As to the future of science, research on homeopathy and other low dose bio-effects offers the gift of new questions to the great scientific community—not only for homeopathy and solution chemistry, but for the entire theory of condensed matter with ramifications for biology, chemistry, and physics. Chipping away at the molecular dogma and raising uncertainty about what scientists thought was bedrock truth should be seen as healthy for science. As physicist Louis de Broglie warned us, "The advances of science have always been frustrated by the tyrannical influences of certain preconceived notions that were turned into unassailable dogmas, and for that reason scientists must periodically re-examine their basic principles." Research on homeopathy and low dose bio-effects may lead to a revision or a refinement of molecular theory, or it may show that something other than molecular theory is involved at these low doses.

There is theoretical work in physics toward a new theory of matter that may hold promise for application to homeopathy and low dose bio-effects. Del Giudice[22] and Preparata[23] propose a novel theory of condensed matter based on quantum electrodynamics in which collective or cooperative phenomena are critical to its structure and properties. They show that conventional molecular theory works well for gases, but falls short in explaining the phenomena of liquids and solids. A system of molecules kept together by purely static forces becomes dynamically unstable beyond a certain density threshold. Therefore the system enters a lower energy configuration where molecules oscillate in tune with a self-produced coherent electromagnetic field. The energy gain is proportional to the particle density, and then matter is forced to condense. The theory predicts the appearance of coherence domains in solids and liquids such as water. Because the living cell and its structural subcomponents have dimensions of the same order of size of the calculated coherence domains in liquid water, it is expected that electrodynamic coherence may be relevant to the living state, in terms of enhanced stability and novel energy and information transactions. Such novel energy and information transactions, if they exist, may be relevant to homeopathy.

The results of many low dose experiments suggest new features of matter such as information that may be conveyed by more subtle properties of matter than molecules. It comes as no surprise that living systems, which are well known to involve many levels of order and different types of informational exchange, appear to be sensitive to what may be "informational" properties of very high dilutions of bioactive substances. Experiments from another frontier area of biology suggest that there may be subtle non-chemical bio-informational

transfer in cellular systems.[24,25] Still other experiments suggest that the zero point energy of the quantum vacuum may be involved in subtle informational transfer in biology.[26] Perhaps an appropriate explanation for low dose bio-effects awaits us in a biophysics that is yet to be invented.

Whereas conventional science maintains that biological information is stored and transferred via biomolecular structures such as DNA, there is some indication that more subtle informational signals may elicit biological effects. In bio-electromagnetics there are many observations that extremely low-level, non-ionizing electromagnetic fields whose energy content is below the physical thermal noise limit can produce biological effects sometimes robust. There is no agreed molecular mechanism for these effects. It has been postulated by some that they may act on the organism in such a way that they affect the organism's endogenous electromagnetic field, which may be bio-regulatory. That is, they act at the level of the whole organism to provide bio-information or disrupt it rather than at the level of energy or power intensity directed to molecular receptors. Furthermore, it is possible that several other phenomena that elicit biological effects, such as very high dilutions, homeopathy, healer treatments, acupuncture, and other types of "energy medicine," may mediate their effects by means of coherent excitations, forms of electromagnetic bio-information that might interact primarily with the organism's endogenous fields. Endogenous electromagnetic fields, which are properties of the entire organism rather than of specific biomolecules, may be involved in self-regulation of the whole organism, and sensitive to a variety of subtle informational signals from the environment. These speculations not only challenge the concept of molecular mechanisms, but also the dogma that mechanical reductionism is the fundamental principle underlying the living state. However, much work needs to be done to develop these speculations into testable hypotheses and theories.

There are a number of other attacks on the mechanistic view of life which those working on homeopathy or low dose bio-effects should be aware of. Richard Strohman, a leading molecular biologist and Professor Emeritus at the University of California, has recently presented some serious challenges to the genetic paradigm. He argues that the information for cellular activity is not in the individual genes, but is holistically located.[27] In his view, biological research is presently lacking this integral program. The creativity of the organism, which is perhaps life's most salient feature, involves the interplay of the integral design and function of the organism with its environment. Strohman raises the argument for an epigenetic rather than a genetic view of life, whereby environmental interactions produce hereditable changes. This means that interaction between the organism and its environment is nonlinear, with the temporal sequence of events determining the complexity that unfolds even in the simplest organism. Of course, it is much easier to ask questions within the mechanistic reductionist framework by studying the fragments of a dead organism. It is much more difficult to study the interaction of genetic and environmental factors in a living organism and develop a science of life at this level. However, most biologists fail to see the limitations of their paradigm and the importance of aiming for this larger context.

There is a popular anecdote based on a Sufi story of a drunk who lost his keys somewhere in a dark street and was groping for them only under the street lamp. Asked where he lost them, he replied that he didn't know, but he was looking there because the light was good. Similarly, the dominant paradigm of mechanical reductionism has prevailed because the biology community has asked only the questions where the "light is good," and the results are clear-cut and reproducible. Biologists explore, for the most part, those dynamic possibilities for life only where organisms "obey" the paradigm.

They have missed the enormous creative potential of life in its subtle interactions and interrelationships. Furthermore, the genetic approach has not permitted "other" questions to be addressed, which, in fact, challenge the conventional approach and the dominant paradigm. Moreover, there is a terrible confusion in contemporary biology between the ontology of life, its epistemology, and the methodology. That is, the methodology used (mechanical reductionism) has frequently been equated with life itself or the model of how it functions. This is particularly true in the U.S. where higher education in science does not typically include course work in the history or philosophy of science. The whole organism may be a biological fundamental that cannot be reduced to its parts, the whole may be self-governing by virtue of its long-range electromagnetic fields that are the summation of many electrically charged component species and their interactions. This is reminiscent of the words of Claude Bernard, "The vital force directs phenomena that it does not produce; the physical agents produce phenomena that they do not direct."[28] In 1839, when Bernard wrote this statement, the "vital force" was taken to mean a metaphysical concept beyond the scope of science. However, the "vital force" may indeed be a property of the whole organism, a time-varying electromagnetic field summation of all the electrically charged molecular events occurring within it. Subtle biological effects may be mediated through this subtle informational network at the level of the whole.

Conclusions

The dominant paradigm of mechanical reductionism that shaped science for the past few centuries, but was overthrown by developments in modern physics earlier this century, still governs modern biology and medicine. Mechanical reductionism, which was developed for the inanimate physical world, determines the scope of questions that can be posed for living organisms, and conventional biology is the collection of theory and results based on those questions. However, frontier scientists are exploring other features of life by asking new questions that go beyond the dominant paradigm. Their questions come from various frontier areas of science and medicine such as epigenetic heredity, bioelectromagnetics, homeopathy, and low dose bio-effects. The results of their investigations, which may be regarded as individual anomalies by the mainstream, may be taken together as evidence for the need of a bigger paradigm to accommodate them. Biology, it appears, may be entering a crisis.

Not only do these "anomalies" challenge our present view of life, but collectively they point to the necessity for a holistic view of life to complement the reductionist view. Whereas conventional science maintains that biological information is stored and transferred via biomolecular structures such as DNA, the anomalies show that other informational signals *not* stored in chemical structures may elicit biological effects by possibly altering the subtle informational signals involved in biological regulation of the whole organism.

Major changes in science have never been brought about by isolated experimental findings, but by collective evidence. Thus, it is crucial for scientists who dare to venture into tribu-

taries of the mainstream or into uncharted terrain to come together to enter into dialogue and share their data, to find that what may seem as isolated anomalies fit together to form the rudiments of an emerging paradigm. It is important to look at the problems of our science and the gaps in our knowledge. We must continually ask new questions and never be satisfied with the old ones, nor with the answers that have come to pass. Scientists must continually be motivated by the "mother" of all questions: what facets of nature remain undiscovered because what we consider to be theoretical certainties prevent the posing of new challenging questions?

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Earth Day! Not Again?

Remy Chevalier

Editor's Note: In this Invited Opinion, we are pleased to present the heartfelt frustrations of a dedicated environmental activist with the traditional green movement. Though we may not agree with his every sentiment, we applaud Remy Chevalier's wake-up call to mainstream environmentalists to consider fairly the prospects of cold fusion and new energy. They should examine the scientific evidence, ignore the negative propaganda, and—to steal from a phrase from the sixties—give infinite energy a chance! — *EFM*

If former U.S. Senator Gaylord Nelson was the father of Earth Day, Denis Hayes was its engine. Thirty years ago they managed to get thirty million Americans out into the streets to protest the abject corporate impact on the environment. In 1990, these same corporations became Earth Day sponsors, lavishing this country in a sea of greenwash. It had come around full circle. Little progress had been made. For every Band-aid solution, ten other disasters loomed ahead with no quick fix. The Reagan administration took its toll. Denis went from being director of the Solar Energy Research Institute in Denver, back then stripped of all funding, to being president of the Bullitt Foundation in Seattle, another one of those very wealthy green tax-shelter organizations with top-heavy objectives and nebulous results.

I met Denis in 1989 when I booked him as a speaker for an Eco-Saloon at Wetlands, a New York nightclub fronting direct-action groups. We talked about the role the military could play in the environmental movement. Pentagon procurement eventually became a major supporter of Eco-Expo. Then in 1996, I contacted Denis again about work I was doing trying to bring attention to "new energy" research. I did call him an ill-advised icon for not taking these possibilities into consideration. But I was stunned by his reaction. He lumped "new energy" research with "hopes to power humankind with Swedish Stones and alchemy," adding he had "no tolerance for conscious fraud."

We have since lost touch. Earth Day degenerated into Keep America Beautiful. Where I live, in the Connecticut suburbs, the roads are still littered with Budweiser cans and now Snapple bottles. Nothing has changed very much. Yes, there is a photovoltaic industry, but it's a blip on the radar screen of the oil companies that own them. General Motors just recalled most of its few electric cars because of some convenient fire hazard. American rivers might be a bit cleaner than they were twenty years ago, but in Europe and elsewhere, they are dead or dying, like the Danube. Earth, from space, shows the deserts gaining ground and the ice caps melting. Humanity has decided immediate survival is more important than long-term ozone concerns. The Rio and Kyoto agreements were not worth the paper they were printed on.

So what can another Earth Day do? In the spirit of *50 Things You Can Do To Save The Planet,* published back in 1990, and its three dozens variation on the theme, Denis has written a new little booklet: *The Official Earth Day Guide To Planet Repair.* Island Press publishes academic environmental books. Their titles rarely reach super-bookstores. I can't understand why this book which was meant as another how-to for Earth Day activities, had a publishing date of March 15, only a month before the event! Shouldn't this book have been out over the summer, giving a chance for people to plan things out?

The aspiration this year is that the Internet will make it a much more vibrant and immediate event. But in the wake of the WTO Seattle riots, I'm sure Earth Day organizers were not ready to really rock the boat. They lavished the distinction of celebrity spokesperson Leonardo DiCaprio, a guy who, let's face it, is a great actor, but a spoiled brat. He has spent the last two years partying on the rave beach scene of Thailand under the pretext of shooting a movie. Interestingly enough, the crew was blamed for trashing its location. Whether that accusation is true or not, knowing Union workers, I wouldn't put it past them. Hollywood has two green think tanks, Earth Communications Office and the Environmental Media Association. Both are great at producing public service announcements, but have a hard time cleaning up their own house.

Leo went on TV with Denis this winter with a never-got-up-this-early haze, pontificating about his love for the environment. He followed this stellar performance with some incoherent rant in a *Rolling Stone* interview about having to choose between eating beef or tuna. What's going on here? What has all this degenerated into? We are faced with biosphere problems of cataclysmic proportion. Europe has just experienced the worse storm ever recorded, which destroyed entire forests, including ten thousand trees in Versailles. An oil spill worse than the Exxon-Valdez on France's Atlantic coast three months ago was totally ignored by American media. A flood in Mozambique is right now killing hundreds of thousands of people. Same in Bangladesh and dozens of other places around the world where water levels are rising at an alarming rate. Venice's monuments are flooded six months a year.

Yes, Al Gore wrote a book called *Earth in the Balance,* from which he's laid low ever since. A few years ago he was caught on camera explaining to a woman how eating beef was bad for you and then had to apologize to cattle ranchers. Did we expect Earth Day 2000 to be a "Gore For President" political rally? Wouldn't you be afraid this might backfire? People in this country are sick and tired of celebrity endorsements. Tired of holier than thou attitudes on the part of icons who keep falling flat on their face in tabloid scandals? Nothing is sacred anymore, and my stomach starts to turn when I see those whose art I respect, like R.E.M. or U2, fall prey to mindless politicians wearing blue suits and red ties.

I've been an environmental activist since I had an epiphany at sixteen in 1969 looking out over the ocean sky one night on a Florida beach. I was overwhelmed by the knowledge that the planet was alive, a real live organism. The Gaia Hypothesis had not been invented yet. For hours I stood there embracing the vastness of space as I could feel the earth cry out for help. To quote the bad guy in the movie "The Matrix": "man is a virus." Well, maybe I wouldn't go that far. So I became torn between a need to curb humanity's lifestyle and my gut response to my planet's perceived cries for help. This feeling later gave rise to sentiments of ecofascism on the part of those who never had these experiences of unification with the elements. Back in my youth when every religion swarmed in my brain, I finally understood how some fundamentalist Christians could see the environment as a satanic concern. Only Lucifer would care about Earth's fate since this was his only home. Christ and those he saves will just magically restore the Kingdom. This explained a lot of inertia. A lot of Americans were just living in that hope, at the expense of harsh realities. Six billion mouths to feed, bodies to warm, 1000 million smog-spewing vehicles

on wildlife-destroying asphalt roads! A Crumb cartoon nightmare.

I started looking for solutions, "real" solutions. What could do away with the internal combustion engine and provide us with clean, unlimited energy? I worked on behalf of solar power for two decades until it became apparent to me it would never make a dent in the power structure. I started discovering the work of renegade inventors who had ideas about tapping energy from the primordial field, "CHI," but who didn't have any more financial resources to make it happen than I did. The conspiracy of energy invention suppression is all too real. So when a man like Denis Hayes, whom I looked up to when I was a teenager back in the 1970s, described my aspirations at organizing a Manhattan Project-styled program to bring all these inventors together a "conscious fraud," it really, "really" hurt me! How can a man who was such a supporter of solar energy be so opposed to something that might finally solve all our problems, twenty years later? Like the oil companies invested in oil, he was now invested in solar. That's why.

His *Guide To Planet Repair* is just that, "repair," like the patch kit you buy for bicycle tires. The Bullitt Foundation is worth $100 million dollars. The Energy Foundation, on whose board Denis also sits, spends $20 million a year promoting and sponsoring "renewable" energy projects. Yet, there's been no effort on the part of any of these green mega-groups, another being the $1.7 billion Packard Foundation, to make the slightest effort to investigate the research regularly reported by periodicals like *Infinite Energy* magazine or *New Energy News*. They are trying to solve old paradigm problems with old paradigm technology. It's doomed to fail. Earth Day has become a corporate fest, when in fact it should be a return to certain Pagan rites of respect for the astral body that gave us life. A lot of astronauts have come back down to Earth with the same conviction. How can we ever hope to bring Earth back in the balance if we don't tune into what it is trying to tell us?

When self-appointed saviors of our environment refuse to even look at work done by those who propose radical departures from conventional thinking, then these people become part of the problem. Are Denis Hayes, Leo DiCaprio, and Earth Day part of the problem? I think they mean well and try as hard as they can with the tools of comprehension at their disposal. But I also think it's time they made way for people who have a lot more forward vision, and who can now let in those who for years think they have a better answer, but were never given a chance to make their case known. It's the same old story all over again. The military for years has been playing with classified technologies deemed too dangerous to let loose on the population. These same technologies used to build mysterious weapons are the same technologies that could save the world. If you are a civilian inventor trying to implement these ideas in a peacetime manner, you are ignored by those whose role should be to give you a public forum.

So it was Earth Day again. More helium balloons. More sweeping the hemp legalization issue under the rug. More buy electric cars that don't exist because Detroit will never mass-produce them. And come April 23, it's back to business as usual, until there isn't one old-growth tree left standing in Oregon, until the Mediterranean is as dead as Long Island Sound, and a multitude of other calamities befall us. Why? Because $300 billion is spent every year on national security instead of international security, and because we've erected a force field so high around our borders, America has forgotten it is part of the same globe as everyone else. ❑❑❑

Table-Top Antigravity?

Chris Tinsley

Some of our readers may have seen some references to "antigravity" in the press recently and, since I have been keeping an eye on the subject, our Gentle Editor has asked me to make a short summary of the story so far.

It's all rather embarrassing because all this has been going on under our noses for several years, and yet we were not really paying much attention until one of the best science journalists in the UK, Robert Matthews, covered it in the *The London Sunday Telegraph* (September 1, 1996, page 3).

The article had two authors: Matthews, and Ian Sample—who is an editor of *Journal of Physics-D: Applied Physics*. This reputable journal had peer-reviewed and accepted a paper by Eugene Podkletnov and Petri Vuorinen of Tampere University in Finland, a paper which claimed that if a disk or ring of superconducting material were spun while supported by a magnetic field, the weight of an object placed above it could be reduced by as much as 2%.

So far, so good. But Tampere promptly responded to this article by saying that Podkletnov was not really an active member of the University, and a Petri Vourinen also responded by denying that he had co-authored the paper! All was not well in this affair, it would seem. Matthews investigated all this, and got some rather odd statements from Podkletnov—including the assertions that there was some confusion over the name of his co-worker, and that the work reported did in fact date back to 1992.

Newspapers are rarely enthusiastic about going back over their more sensational articles, just as in the case where a news magazine accused Dr. John Bockris or his graduate students of fraud over his CF tritium results—but did not want to report on the 120 papers which later confirmed his work. So, *The Sunday Telegraph* did not print Matthews' follow-up article, and instead he published a report in *New Scientist* (September 21, 1996 page 7). By this time, Podkletnov had withdrawn his *J. Phys. D.* paper, claiming that his financial backers did not want it published.


News: Analysis & Commentary

SCIENCE

TAKE THAT, ISAAC NEWTON

researcher in Finland claims to have defeated gravity

WEIGHTLESS BATTLE Scientists, including those at NASA, take the report seriously and are trying to replicate the feat with a spinning superconducting disk

Business Week then published an article (September 30, 1996, p.42.) by Otis Port, summarizing the situation to date.

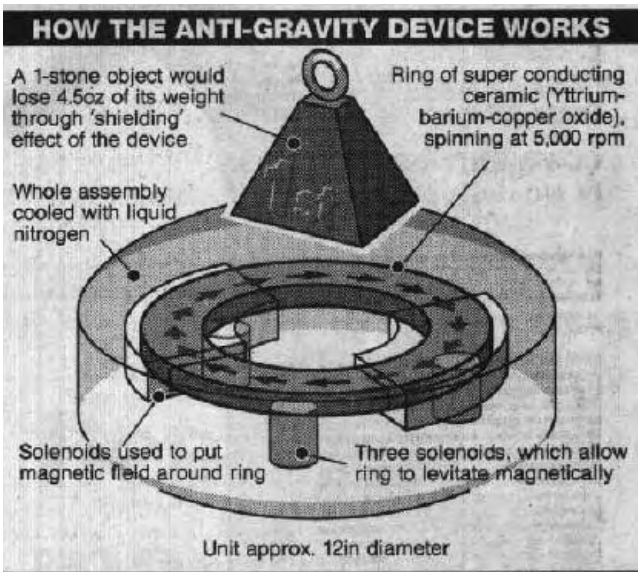
All heady stuff! Trying to pick through the fog of accusations, muddled thinking, dubious experimental technique, false assumptions and the impenetrable thicket of mathematics in the theory papers has been quite a task. Just about all I can do to help is to examine the actual published papers on the subject. But, before doing that, I am reminded of what Professor Frank Close, Head of Theoretical Physics at the Rutherford Appleton Laboratory said on the recently shown BBC TV series "Future Fantastic" (made in collaboration with The Learning Channel, so it should be available soon outside the UK). Nobody could describe Dr. Close as

being any friend to unconventional science, yet on the subject of antigravity he was surprisingly cautious. He said that possibly antigravity was closer to science fact than one might think, and that in some theories gravity had two components—the one with which we are all familiar, and "a little bit on top, which is like antigravity."

But what of the papers? I have seen discussions of what is termed the "Hooper Effect," which may be an early precursor to this work, but the first paper I have is from Podkletnov and Nieminen (Physica C *203* (1992) 441-444). This journal is apparently one for superconductor research, and—frankly—this paper isn't a very good one. One serious criticism is that there are no clear explanations of how the "weight loss" was measured, nor are there any error bars associated with the claim of 0.5% weight loss of small samples placed above the device. Another problem is that there seems to be a lot of irrelevant talk about superconductors, but the worst error is the assumption that this is a form of "gravity shielding." Perhaps this is deliberate, nobody wants to use the "'A' word"! The paper begs far more questions than it answers, but perhaps I can summarize the "good bits."

The test involved a six-inch disc of superconductor ($YBa_2Cu_3O_{7-x}$), about one-quarter inch thick. It was raised above a liquid helium bath using solenoids, and was spun in the same way. During the time it stayed cold enough (below 70K) it reduced the weight of a sample of silicon dioxide (glass or quartz) by 0.05%. Various precautions were taken to prevent error, but it isn't really clear how good they are.

While we do not have a copy of the paper accepted by *J. Phys. D*, we do have a 1995 report (Tampere University Press) from Podkletnov and Levit (MSU-95 chem). This reports an essentially similar experiment, one which claims an effect of 1.9% - 2.1% at maximum. In this case, a torus was used, inner diameter 80mm, outer diameter 275mm and thickness 10mm. Test materials included metals, glass, plastics and wood; typically of mass 10 to 50 grams. The effect was noted at heights above the apparatus of between 25mm and 1500mm. That latter figure is most interesting, because it appears to show that whatever is happening it is not gravity shielding as we would imagine it. A paper by Unnikrishnan (*Physica C* 255, 1996, pp. 133-137) argues (correctly, I feel) that shielding would affect only a short cone of space immediately above the gadget. Unfortunately, he makes the classic error of taking the experimenters' possibly-flawed theory, trashing that, and concluding that the results cannot be real.



The Tampere report continues:

> The levitating superconducting ceramic disk revealed clearly visible shielding effect against the gravitational force even without rotation. The values of the weight loss for various samples were within the range 0.05% - 0.07%. As soon as the main solenoids were switched on and the disk began rotating in the vapors of liquid helium, the shielding effect increased, and at the speed of 5000 rpm, the air over the cryostat began to raise slowly up to the ceiling. The particles of dust or smoke made the effect clearly visible. The boundaries of the flow could be seen clearly, and they corresponded exactly to the shape of the toroid. The weight of the various samples decreased no matter what material they were made of. The loss of weight depended on the shape and the position of the sample. The maximum loss of weight could be reached if the sample was oriented with the flat surface parallel to the surface of the disk, so that the projection of the sample had the maximum area."
>
> Podkletnov claims that vertically stacking *two* 2%-reduction devices leads to a 4% reduction in weight of objects above.
>
> Moving to the theory papers, there are several by Torr and Li which appear in reputable journals such as *Physical Review*. These appear to lend support to the experimental results.

Our conclusion? Well, this has been described as "high risk, infinite reward" physics. Certainly it is not anything which we would recommend as an investment opportunity! However, this magazine can do as it wishes with its tiny funds, so we have made a no-strings grant of $1,000 to one researcher who is diligently attempting to reproduce and perhaps even improve on the Podkletnov results. In fact, four known groups—even one at NASA's Marshall Spaceflight Center—are trying to duplicate the Finland experiment. Whit Brantley, chief of NASA's Advanced Concepts Office in Alabama, is on record in *Business Week* with full support of efforts to replicate antigravity.

Watch this space.

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❑ Harold Aspden, "The Theory of Antigravity," *Physics Essays*, Vol. 4, No. 1, 1991, pp. 13-19.

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The Mysteries and Myths of Heat: A Brief History of Hot and Cold

Eugene F. Mallove

Our knowledge of heat is as old as the history of contemplating whether atoms, "smallest units of matter," exist. Much of what we know—or think we know—about heat came about in the nineteenth century, but thinking about what heat really is goes much further back. Primitive peoples clearly knew that rubbing sticks together could make heat and then fire, but connecting the idea of atoms to this "heat" was beyond even the imaginative ancient Greeks.

A brief perusal of Isaac Asimov's *Biographical Encyclopedia of Science and Technology*[1] unearthed this ancient background of atomic and pre-atomic theory: Greek philosopher Anaximander (610-546 BC) imagined "a formless mass that was both the source and destination of all material things." His name for this unobservable substance was *apeiron*, translation: infinite. Indeed, the precursor of later nineteenth century theories of the *aether,* and their present emergent forms after their twentieth century Einsteinian demise, traces that far back. It will most likely be determined in the affirmative—after many more bloody battles—that an energetic aether gives rise to matter and is also the repository of its localized extinction. This aether, forming a universe perhaps infinite in time, is nearly certain to vanquish the unsupported myth of Big Bang cosmology.

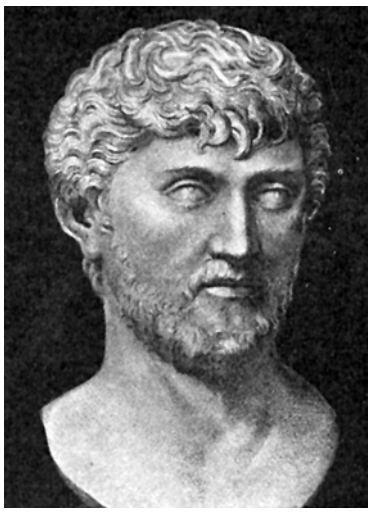
Another Greek philosopher, Leucippus (born 490 BC), is generally regarded as the primary author of "atomism." Greek philosopher Democritus (440-371 BC), a student of Leucippus, put forth the idea of a void in which atoms moved and interacted. Finally, influenced by this early Greek thinking, atomism was codified and elaborated by Roman writer Lucretius (Titus Lucretius Carus—95-55 BC) in his work "DeRerum Natura"("On the Nature of Things"). Atomism continued to play a role in scientific thinking into the Second Millennium, but since no one had seen atoms or knew their nature, it was possible even for some leading scientists, *e.g.* Ernst Mach (1838-1916), to doubt their existence into the second decade of the twentieth century. With kinetic theory of gases theorist Ludwig Boltzmann listening in January 1897 at the Imperial Academy of Sciences in Vienna, Mach had loudly announced, "I don't believe atoms exist!"[2]

Democritus, Greek philosopher

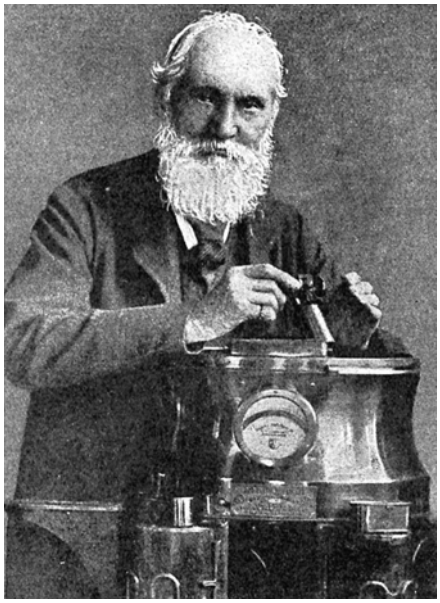
It is fascinating that the first known heat engine (a machine that converts heat to work) was also of ancient Greek vintage—the primitive *aeolipile* of Hero (sometime in the first century AD, about year 75, some think), which used the jet action of steam to produce the rotation of a sphere. In a remarkable example of how an invention can arise and then disappear if it is not manufactured and then used widely, it was not until the seventeenth and eighteenth centuries that heat engines came into being as utilitarian devices, initially to drive crude water pumps. A fascinating story of their development is told by John F. Sandfort in *Heat Engines*.[3] In the process of developing the early heat engines, few people seem to have given much thought to what was this "heat" produced from burning wood or coal. The so-called "father of chemistry," French scientist Antoine Laurent Lavoisier (1743-1794), is perhaps most identified with the invisible fluid concept of heat, which acquired from him the famous name *"caloric."* It was supposed that driving this caloric out of material by rubbing, or by combustion, produced the manifestations of heat—caloric *was* heat. That led to the obvious question: how much caloric could be contained within a given mass of material?

Lavoisier in his *Elementary Treatise on Chemistry* (published posthumously in 1798) listed the then known "elements"—even though the very reality of atoms was still at issue. In that list of elements Lavoisier included, believe it or not, *light and heat!* Now as Asimov remarks, "He had eradicated one imponderable fluid, phlogiston, but it was only partly through his influence that caloric, just as false, remained in existence in the minds of chemists for a half a century." We might add that Lavoisier's dogma of the non-transmutability of "elements"—as he then knew them—has also endured. This two-hundred year-old dogma combined (in the late twentieth and early twenty-first centuries) with modern theories of atomic structure to deny experimental proof of low-energy nuclear reactions. Strong myths and dogmas, once begun, have rather long lives.

The caloric theory of heat was surprisingly enduring. It survived far into the nineteenth century, despite many experiments which showed that caloric, if it existed, had no weight. And there were theorists who founded the kinetic theory of gases, James Clerk Maxwell (1831-1879) and Ludwig Boltzmann (1844-1906), whose theories provided very strong support for atomism. Even the convincing experimental work of Benjamin Thompson (1753-1814), an expatriate from England's American colonies (what are now Massachusetts and New Hampshire) who became Count Rumford in Bavaria, could not kill the idea of caloric. In his work in the late 1790s boring brass cannon barrels for his German patron, Rumford determined that the metallic shavings from this horse-driven boring appeared to have the same heat capacity after the drilling action as



Titus Lucretius Carus



William Thomson (Lord Kelvin)

before. He suggested that the supply of heat in matter was *without limit*—an exceedingly revolutionary concept that contradicted the caloric theory. He wrote: "The more I meditated on these phaenomena *[sic],* the more they appeared to me to bid fair to give a farther insight into the hidden nature of Heat; and to enable us to form some reasonable conjectures respecting the existence or non-existence of an igneous fluid: a subject on which the opinions of philosophers have, in all ages, been much divided. . .It is hardly necessary to add that anything which any insulated body, or system of bodies, can continue to furnish without limitation, cannot possibly be a material substance: and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited or communicated, in the manner the Heat was excited and communicated in these Experiments, except in MOTION." (quoted by J.F. Sandfort[3]).

Today a scientifically literate person understands that the excited, chaotic motion of atoms and molecules creates in our bodies or in measuring instruments a sensation of hot or cold. But this concept of heat is relatively modern—an outgrowth of the work of Rumford and other knowledge developed in the nineteenth century, in particular the work of James Prescott Joule (1818-1889). According to Isaac Asimov, earlier scientists had conceived of heat as a form of motion, among them Francis Bacon (1561-1626), Robert Boyle (1627-1691), and Robert Hooke (1635-1703), but caloric endured, until Maxwell, it is said, finally killed it off.

It is astonishing to realize that many modern conceptions (or "laws") in the science of heat—thermodynamics—arose during the nineteenth century, a period of utter confusion about the fundamental nature of heat. How could it have been otherwise, given that the very existence of atoms was still in question! One sees the shakiness of the claim that the laws of thermodynamics had reached a state of "near perfection" in the twentieth century (see Von Baeyer[4]), when they in fact rested on this very flawed foundation.

Much before the nineteenth century there was only a very weak conception of a relationship between heat and energy. So it is not surprising that the important paradigm of the conservation of energy, which later became known as the First Law of Thermodynamics, was long in coming. The name firmly associated with introducing the conservation of energy are German physicist Julius Robert Mayer (1814-1878), who predated both James Joule's and Hermann Ludwig Ferdinand von Helmholtz's (1821-1894) statements of the conservation of energy. Mayer in 1842 had published a paper on the general equivalence of all forms of energy and he gave the first estimate of the mechanical equivalent of heat. Because Mayer was not of the scientific establishment, his then heretical concept of the conservation of energy was not accepted. It was James Joule who performed the definitive exhaustive series of experiments that showed the convertability of mechanical action to a heat equivalent. Though Joule began lecturing about and publishing his work in 1843, it was not until a critical meeting at Oxford University on June 27, 1847 at which he lectured that his ideas began to receive acclaim. There, man of the establishment William Thomson (1824-1907), already well-published by his then age twenty-three, became impressed with Joule's solid work on the mechanical equivalent of heat. (William Thomson was knighted as Lord Kelvin in 1866, by which name he is more commonly known.)

But for three years after that meeting there continued a deep confusion in Thomson's mind, based on the earlier work of French engineer Nicolas Léonard Sadi Carnot (1796-1832), with which he was also impressed. Carnot in 1824 (the year Thomson was born) had published a remarkable paper, which mathematically defined the upper limit in efficiency of steam engines of the time—and, by extension, the maximum efficiency of all heat engines. Carnot stated that the most general heat engine required a high temperature input reservoir (at T_{high}) and it had to exhaust its wasted heat to a lower temperature reservoir (at T_{low}). His formulation that the maximum efficiency of a heat engine was $(T_{high}-T_{low})/T_{high}$ later became enshrined as dogma in both physics and in practical engineering. A heat engine that could convert heat to work at 100% efficiency from a single temperature reservoir would be deemed impossible under this Carnot restriction. This is the basis for contemporary mockery of attempts to make what are called "perpetual motion machines of the second kind," of which Xu Yelin's device (see p. 31) is one type.

So what was William Thomson's problem? Thompson in 1847 was still a firm believer in the caloric theory! After all, Carnot had been too, and Thomson firmly believed Carnot—Thompson in fact had rediscovered Carnot's obscure paper and had promoted Carnot's ideas. But Carnot had developed his efficiency limitation on heat engine performance from the perspective of the caloric theory. So here James Joule was presenting in 1847 material that was equally convincing to Kelvin, but energy conservation flew in the face of the caloric theory. Just as Thomson's ideas on resolving the paradox were jelling three years later, German mathematical physicist Rudolf Clausius (1822-1888) published the solution to the paradox in May 1850, "On the Moving force of Heat and the Laws of Heat Which May be Deduced Therefrom."

Nicolas Leonard Sadi Carnot, French engineer

In one fell swoop Clausius "scooped" Kelvin and cast into precise form *both* the First and Second Laws of Thermodynamics—energy conservation, and the limitation of Carnot efficiency. The actual form of Clausius' statement of the Second Law is: "It is impossible for a self-acting machine, unaided by an external

agency, to convey heat from one body to another at a higher temperature." In 1851, Thomson would claim independent discovery of the Second Law. His statement of it would be: "It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects." Both the Clausius and Kelvin statements are said to be equivalent. Clausius' collected thermodynamic theory was published in 1865; it included introducing the seminal concept of entropy, a measure of disorder that, it is said, stays constant or inevitably increases, but never decreases in a closed system.

Rudolf Clausius, German mathematical physicist

From that time forward, physics moved in lock-step with the presumed inviolability of the Second Law. It is true enough that the Second Law, in general, mandates that heat cannot *spontaneously* flow from a cold body to a hot body (but be aware, there *may be* exceptions even to this connected with "advanced Maxwell's Demons"). Generations of students had this Second Law and Carnot's maximum efficiency formula "proved" to them by a mathematical demonstration that is nothing short of circular reasoning: *If* Carnot's principle concerning the maximum efficiency of a reversible heat engine were violated in such and such system (elaborately diagrammed in colorful and expensive thermodynamics texts), that would violate the Second Law. Ergo, Carnot's efficiency limit is supposedly proved by *reductio ad absurdum*. The proof is used the other way around too—to prove the Second Law from Carnot! Isaac Asimov, for one, is embarrassingly clear in admitting the circular logic that is implicit: "It is possible from Carnot's equation to deduce what is now called the Second Law of Thermodynamics and Carnot was first to be vouchsafed a glimpse of that great generalization."[1]

Sad to say for the physics establishment and the technology establishment, that turned out not to be the case. For the sake of Humankind, it is very good news indeed that this almost two hundred year old dogma will now come crashing down. As Maurizio Vignati in his exhaustive book[5] and Xu Yelin in his experiments show (and in the work of others still to come no doubt), the Second Law is simply this: *A limitation based on the belief that no macroscopic violation of that limitation had ever been seen or would ever be seen.*

As we will see in the paper Dr. Paulo and Alexandra Correa published in this issue, another much more serious challenge to the Second Law of Thermodynamics has arisen. It appeared in January 1941, as I have outlined in my editorial, when Wilhelm Reich attempted, in vain, to get Einstein to "look through his telescope" to see a persisting temperature anomaly that was in direct violation of the Second Law.[6] Einstein, in effect, refused to "look through that telescope" and we have been suffering delayed awareness of an energetic aether and sound thermodynamics ever since. But now a pathway to a much greater understanding of fundamental physics has opened. We have barely begun to reformulate the theory of heat that will extend far beyond the useful but highly limiting concepts we inherited from the nineteenth century.

Through new physical descriptions of the energetic aether and other emerging understandings of the flaws of classical thermodynamics, all the textbooks will need to be rewritten. If anyone thinks this will be easy, given the behavior of the scientific establishment since the discovery of low-energy nuclear reactions, think again. As with cold fusion, to get the ossified scientific establishment even to listen will require irrefutable devices embodying these principles. It is now certain that these will come.

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Commentary on Maxwell's Equations and Special Relativity Theory

William H. Cantrell

Abstract

The importance of Einstein's Special Relativity Theory (SRT) is discussed in the context of our modern technology and the progress of science. Historical reasons are given for the development of SRT, and the problems it attempted to solve concerning The Holy Grail of Science: *Maxwell's Equations*. (See definitions of technical terms in display boxes.) The justification for SRT, that of making Maxwell's Equations covariant to inertial translation by using the Lorentz-transformation, is discussed. This, in turn, creates problems, paradoxes, and logical flaws, which are enumerated herein. SRT is challenged by at least three alternative theories from various researchers. These theories merit attention because they do *not* require the concept of length and time to be modified (tampered with) to obtain correct answers.

Introduction

There can be no doubt that Einstein's Special Relativity Theory (SRT) has had a profound impact on the physics of the twentieth century. His theory has enjoyed a series of brilliant successes for some ninety-five years now. Yet there is a sizeable community of scientists who *reject* it outright. And there is a far larger group who harbor a pronounced distaste for the theory, but know of no feasible alternatives to it. This widespread dislike stems from the fact that Einstein's theory is codified in the Lorentz transformation, and this transformation tampers with two foundational concepts of physics, length, and time measurement, in order to *force* the speed of light to be constant for all observers. When thoughtful, skeptical people are asked to abandon time-honored definitions of space and time, they sense a logical flaw, a sleight of hand. Given a clear alternative to SRT, these mavericks would *jump* at the chance for a substitute theory. Of course, there are the vast majority of engineers and scientists who have no need for SRT, for it plays no role in their profession. At best, it has a specialist role to play in a few branches. At worst, it may have stifled creativity and retarded the rational development of science.[1]

To admit a fundamental mistake in such a hyped-up theory would be an embarrassment, not only to the physics community at large, but also to the memory of a man whose portrait hangs in nearly every physics department in the world.

So why has Einstein's theory remained popular for so many years? I believe there are at least five reasons. First, the alternative theories have never been given much attention nor taught at any university. Second, the establishmentarians have invested a lifetime of learning in maintaining the *status quo*, and they will act to protect their investment.[2] (For the readers of this magazine, there is no need to elaborate on this point.) Third, Einstein's theory, being rather vaguely defined and self-contradictory by its own construction, allows some practitioners to display an aura of elitism and hubris in their ability to manipulate it. There is an exclusive quality to the theory—like a country club, and that is part of its allure. Fourth, to admit a fundamental mistake in such a hyped-up theory would be an embarrassment, not only to the physics community at large, but also to the memory of a man whose portrait hangs in nearly every physics department around the world. And fifth, contrary to popular myth, Einstein was very good at *public relations*. During the Great Depression, the popular-culture was actively seeking "heroes" from all walks of life to bolster public morale. Einstein actively courted the press as the stereotypical brainy scientist. In response, Hollywood *loved* it and promoted the image—and *that* helps to explain the many portraits.

The Holy Grail of Science

Some of today's "popular" books on science imply that the most important theory to come out of the modern age is Einstein's Relativity. This perception is incorrect. Go back in time to the Age of Steam, and then imagine the *giant* technological leaps about to occur—the creation of the electric light, the telegraph, the telephone, and that most magical of inventions, the *wireless*. Sparkling with mystery and intrigue, its very name was completely descriptive. It captivated and tantalized the world's imagination like nothing else. Indeed, it must seem difficult to believe that the invention of radio took on an atmosphere of excitement and *glamour* exceeding that of the Internet and the communications revolution of today.

Radio, computers, and in fact the whole foundation for our electro-technical civilization is canonized in *Maxwell's Equations*. These equations, published in "rough form" in 1864 by James Clerk Maxwell,[3] were based upon the pioneering experimental work of Coulomb, Ampère, Faraday, and others. The theoretical genius that he was, Maxwell developed a unifying set of equations for electricity and magnetism. From "The Maxwell Equations," as they are sometimes called, the wave equation can be derived for electromagnetics. This equation predicted the existence of radio waves, and it fell to the theoretical and experimental genius, Heinrich Hertz, to prove their existence in 1888. Make no mistake about it, the Maxwell Equations are *The Holy Grail of Science*, second in importance only to the work of Sir Isaac Newton.

So where does SRT fit into the picture? Well, it turns out that there is a problem with the Maxwell Equations. The hallmark of a good theory is that it is *invariant* to the Galilean transformation. Unfortunately, the Maxwell Equations are not invariant to this transformation. As a matter of fact, they're *not* invariant to the Lorentz transformation either. (They are

*co*variant to the Lorentz transformation. Like most branches of science, electrodynamics has its own jargon. See below and the following page insets for some helpful definitions, especially that of invariance versus covariance. *Most textbooks get this wrong.*)

So the dilemma facing late nineteenth century physicists was that of a very successful set of equations with an obvious flaw. The equations successfully predicted radio waves. They pointed Hertz toward their discovery in his laboratory. They were (and are) simple, elegant, and beautiful in the mathematical sense. Yet they flunked a most important test, that of Galilean invariance. A scientific crisis had erupted. What to do?

Special Relativity Theory

Enter Einstein in 1905 with his famous paper on SRT.[4] Contrary to popular belief, he was not attempting to address the 1887 Michelson-Morley (M-M) null-result, and was reportedly *unaware* of it at the time.[5] In effect, he borrowed, and then redefined, the Lorentz transformation in order to address the invariance problem with the Maxwell Equations. He also stated two postulates.

Einstein's First Postulate was a restatement of the *principle of relativity* (not to be confused with the *Theory* of Relativity). The principle of relativity is the anti-Ptolemaic epitome of enlightenment and common sense, and was known during the time of Galileo, if not earlier. It was first stated by Newton in his *Principia*[6] over three hundred years ago. In essence, it says that *the laws of physics, when properly formulated, remain equally valid in all (inertial) frames moving with uniform velocity with respect to each other.* All physicists accept this principle without reservation.

Einstein's Second Postulate states that the velocity of light is independent of the state of motion of its emitting source. Actually, this is not at all an unusual proposal, and it would be expected of a medium-based (aether) theory. The analogy to sound waves is irresistible. When a train whistle blows, the speed of sound is independent of the speed of the train, but *not* of the velocity of the wind carrying the sound to the observer. Here, the air molecules are the medium, and they play the equivalent role of the aether for electromagnetism. But SRT is *not* an aether-based theory, at least not *directly*. (General relativity uses the concept of "curved space-time." Matter supposedly causes the curving of "space," and "curved space" causes the bending of light rays. From a metaphysical standpoint, curved space-time is just as audacious and arbitrary as the nebulous aether of the nineteenth century.) And although not explicitly stated by Einstein, some thought will convince the reader that the Second Postulate, when combined with the principle of relativity, results in a velocity of light that is also independent of the receiver (the observer), because the emitting source can take on any velocity including that of the receiver. So here we have a prediction resulting in a sharp departure from our everyday experience.

It has been stated that Einstein independently *derived* the Lorentz transformation using his two postulates, but this is not true. Taken alone, they are insufficient to derive the Lorentz transformation *uniquely*. Further assumptions must be made to do so; otherwise, several alternative transformations can be derived from them.[7] Nevertheless, in 1905 this was a tentative start toward a salvage operation of the Maxwell Equations for observations at high speeds, and physicists were willing to overlook the smell of covariance, and the use of an *ad hoc* transformation, if it would rescue the Holy Grail. As Phipps has pointed out,[8] this was a bizarre turn of events, for there *were* competing theories at the time, theories capable of remedying the situation without the need to tamper with the time-honored concepts of space and time. This is all the more poignant, considering the fact that the Lorentz transformation intermixes and scrambles space and time, something that is unfounded and erroneous.

Problems and Paradoxes of SRT

Before we present alternative theories that could be used to *replace* SRT, it is important to discuss the serious problems and paradoxes associated with the *use* of SRT. The full impact of this most unusual theory is rarely discussed, and the first warning

What is a moving inertial frame?

With respect to a stationary coordinate system (usually a Cartesian coordinate system in the laboratory), we define a *second* coordinate system in uniform motion at constant velocity. For simplicity, this second reference frame is normally aligned with the laboratory in x, y, and z, and has its direction of motion parallel to the x-axis. This moving reference frame does not speed up or slow down, nor does it rotate. A typical application would be to place a moving electric- or magnetic-field detector in this second reference frame.

What is a Galilean-transformation?

Between moving frames of reference, time is unchanged and velocities add linearly as expected. Galileo explained that a stone dropped from the crow's nest of a ship would strike the deck at the same distance from the mast as viewed from the shore, even though it appeared to drop vertically aboard the ship and [along a parabolic trajectory] from ashore. The transformation equations are shown below for *any* velocity v along the x-axis:

$$x' = x - vt$$
$$t = t$$

What is a Lorentz-transformation?

There are no examples from everyday experience to rely upon. Both time and length are changed by this transformation, and the two become intertwined. The transformation equations are shown below for a velocity $v < c$ along the x-axis:

$$x' = \gamma(x - vt) = \frac{x}{\sqrt{1 - \frac{v^2}{c^2}}} - \frac{vt}{\sqrt{1 - \frac{v^2}{c^2}}}$$

$$t' = \gamma\left(t - \frac{vx}{c^2}\right) = \frac{t}{\sqrt{1 - \frac{v^2}{c^2}}} - \frac{vx}{\sqrt{c^4 - v^2c^2}}$$

$$\text{where } \gamma = \frac{1}{\sqrt{1 - \left(\frac{v}{c}\right)^2}}$$

signs of trouble appeared shortly after the 1905 paper was published. For simplicity, a single Lorentz transformation is normally applied along one coordinate axis. But what about the more general case of *two* linear, but non-aligned (non-collinear) translations? You might assume that this merely adds complexity to the calculation, but not so—the theory breaks down. The results depend upon the *order* in which the two translations are applied. For example, apply two transformations in succession, with the velocities of the two systems pointed in different directions, say, along the *x*-axis and then along the *y*-axis. Next, repeat the calculation by reversing the order, *e.g.*, the *y*-axis and *then* the *x*-axis. The result is different. Hence the Lorentz transformation fails to obey the commutative law of mathematics, (a+b = b+a) and a definitive answer eludes us. This is a paradox, an absurdity.

Thomas Rotation

Theoreticians were brought in to resuscitate SRT and they administered the so-called "Thomas Rotation" ("Thomas Precession," when applied to the electron). This is a rotation of coordinate axes introduced to compensate for the error between the two results mentioned above. It might seem a bit odd that an inertial system, that is, a moving system with fixed velocity and no acceleration nor rotation, *should begin to rotate!* Furthermore, where did this rotational energy come from? Is this a real effect, or is it some sort of mathematical artifact, some indication that the Lorentz transformation (and hence SRT) is in error?

Some years later, a theoretical prediction[9] for Thomas Rotation was published in the British journal *Nature*, and an experiment was conducted by Phipps to determine whether it really existed. The experiment produced a null result, and despite the photographic evidence, it was refused publication in *Nature*. Consequently, the results were published elsewhere.[10,11] There is also a theoretical basis for refutation of the Thomas Rotation.[12]

The Ehrenfest Paradox

If phantom rotation wasn't bad enough, the next disease to afflict SRT was the rigid body problem, otherwise known as the "Ehrenfest Paradox." Einstein had originally intended SRT to apply to rigid bodies only. Ehrenfest asked the question, "What happens when we have an idealized rigid disk, and we set it into rotation?"[13] The outer edge of the disk can be divided into infinitesimal segments such that they appear to be moving with a linear velocity with respect to an observer in the laboratory. How would the Lorentz contraction affect such an object, as viewed by the observer? The radius must contract somehow. In 1910, the first generally accepted answer (now called the Herglotz-Noether theorem) said that, since the disk was rigid, *it could not rotate*! Of course, there were those who decried this sort of non-solution solution, so the next answer to bubble to the surface said the disk *could* rotate if it was somewhat *elastic*, and *not* made of rigid material. So once again, we have a radical departure from everyday experience. Newtonian mechanics has absolutely no problem with rigid spinning bodies, while Einstein's approach expressly forbids them on purely mathematical grounds.

The Lorentz Contraction

There is also the pole-vaulter paradox, the lever paradox, and a host of other variants. What these all have in common is the Lorentz contraction. But do atomic particles shrink in the direction of their motion, as viewed by an external observer? Do macro-objects really shrink? The author of the Lorentz transformation considered the contraction effect to apply *only* to a deformable electron based on an aether-stress theory.[14] He never intended for his mathematical-equivalence contraction formula to be extrapolated *to all matter* as a reality. This might explain why H.A. Lorentz was adamantly opposed to SRT until his death in 1928. He *intended* it to explain the electron's non-uniform concentration (bunching) of electric field lines perpendicular to its direction of acceleration. In electrical engineering, this is similar to the "skin-effect" at macroscopic levels.[15, pp.149-151] But length contraction plays no role.

During the first half of the twentieth century, physicists were eager to put the Lorentz contraction to the test, and see if the phenomenon really existed. Several experiments were per-

What is invariance?

An equation (or set of equations) is said to be *invariant* under a particular transformation of its independent variables, if the form of each term of the equation is unaltered by the transformation.[8,p.116] Obviously "Lorentz-invariance" is an oxymoron.

What is covariance?

An equation (or set of equations) is said to be *covariant* under a particular transformation of its independent variables, if the form of each term of the equation is not left unaltered. That is to say, each term is altered by the transformation in the same manner.[8,p.118] For the case of the Lorentz-transform, this is best illustrated by quoting H. Minkowski of four-vector fame: "Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows." (Space and time are scrambled—like eggs.)

What are the Hertzian-modified Maxwell Equations?

Shown here are the equations (using rationalized MKSA units) in free-space, where the constitutive equations have been used to replace *D* and *H* with *E* and *B*:

$$\nabla \cdot \boldsymbol{E} = \rho/\varepsilon_0$$

$$\nabla \cdot \boldsymbol{B} = 0$$

$$\nabla \times \boldsymbol{E} = -\left\{\frac{\partial \boldsymbol{B}}{\partial t} + \boldsymbol{v}\cdot\nabla(\boldsymbol{B})\right\}$$

$$\nabla \times \boldsymbol{B} = \mu_o \boldsymbol{J} + \frac{1}{c^2}\left\{\frac{\partial \boldsymbol{E}}{\partial t} + \boldsymbol{v}\cdot\nabla(\boldsymbol{E})\right\}$$

$$\nabla \cdot \boldsymbol{J} = -\left\{\frac{\partial \rho}{\partial t} + \boldsymbol{v}\cdot\nabla(\rho)\right\}$$

The Continuity equation has been added at the bottom. Here the partial derivatives ($\partial/\partial t$) in Faraday's and Ampere's Laws have been replaced with time derivatives (d/dt), and expanded using the chain-rule. Hence, a new velocity term, *v*, appears for use with moving reference frames. These equations are *invariant* to the Galilean transformation.[8]

formed,[16-18] but no variation in length was observed. Recently, a modern space-based test has been proposed by Renshaw.[19] To date, *no **direct** experimental verification of relativistic length contraction has ever been measured.*

The Twin-Paradox

The twin-paradox (also known as the clock-paradox) is without a doubt the most famous paradox associated with SRT. Once upon a time, there were two twin brothers. One twin ventured into outer space at relativistic speeds, while the other twin stayed home. As extrapolated by SRT, the Lorentz transformation causes time itself to slow down, not just for moving subatomic particles, but for atoms and molecules, and for bigger objects, say, *people*!

To continue with the story, the space traveling twin eventually *reverses* course to return to Earth. Upon his arrival, he discovers that his Earth-bound brother has aged many years, while he has aged only a few. This is considered to be a paradox because each twin (each observer) can claim that it is the *other* who moves at high speed as viewed in his own reference frame. So in SRT, how can one age more than the other? The symmetry-breaking event is alleged to be the fact that the space traveling twin must reverse course in order to return. This causes him to undergo accelerations and decelerations that the other does not experience. No clear explanation is given as to why this would break symmetry and slow the aging process, especially over a many year period, where the actual time involved in acceleration could be quite small.

In reality, there is little doubt that both twins would age at the *same* rate. So there really is no paradox because there is no time-dilation. This myth is perhaps the greatest extrapolation of elementary observation in the history of science. Let's take a careful look at the evidence supporting the time-dilation aspects of SRT.

Time-Dilation

The alleged proof for time-dilation is claimed to be among the most confirmed experiments in physics. Yet a careful dissection of these experiments reveals an equally plausible alternative explanation, one that does not require time to be a dependent variable.[20,21] There are three types of experiments that address this issue, the rate of radioactive decay of high-speed mesons in linear motion[22] and in circular orbit,[23] the transport of atomic clocks around the globe,[24,25] and (indirectly via) the relativistic Doppler formula.[20] As Beckmann has pointed out, in all cases the experimenters have failed to ask, let alone answer, whether *time itself* is dilated, or whether internal processes are simply slowed by moving through a gravitational field.[15, pp.77-81] To date, *no **direct** experimental verification of relativistic time-dilation has ever been measured.*

Do we know what the "innards" of a high-speed meson consist of? Nope. Do we know what causes natural radioactive decay? Not really. It can be characterized mathematically by a Poisson distribution, but the actual internal "trigger" for a particular decay is unknown. Could the rate of decay be affected by angular accelerations, or by traversing a gravitational potential? Think about the last time you rode an amusement park ride. Weren't *you* affected?

In 1761, the British Royal Navy awarded John Harrison a cash prize of 9,000 £ (over $2,000,000 in today's currency) for inventing a navigation-quality timepiece with enough accuracy to withstand the pitching and rolling seas of the Atlantic. The magnitude of the residual error has diminished over the centuries, but the basic problem remains. We cannot construct an *ideal* clock using actual materials, even if we use cesium atoms *by definition*. To emphasize this point, let's take a look at a grandfather clock. If we transport such a clock eastward around the globe, it will slow down. But if we transport it westward, it will *speed up*. The grandfather clock relies upon the force of gravity to control the timekeeping rhythm of its pendulum, in inverse proportion to the square root of *"g."* When transported westward *against* the rotation of the earth, the centrifugal force of the earth's rotation is diminished and the effect of its gravity field is strengthened, if ever so slightly. Hence the clock speeds up in an increased gravitational field. And of course, if flown eastward *with* the earth's rotation, its centrifugal force is strengthened and the clock slows down a little. Time-dilation? Of course not. We explain the outcome of the experiment by analyzing the "innards" of the clock. But the influence of gravity applies to more than just pendulum clocks. For example in the famous Häfele-Keating experiment,[25] the atomic clock transported eastward lost 59 ns, but the atomic clock transported westward *gained* 273 ns, compared to the stationary laboratory standard. All physical devices used for time keeping are subject to error when accelerated, decelerated, or constrained to move linearly through a variation in gravitational potential.

So if we can't rely upon experiment, let's do the next best thing and look to theory for an answer. What does electrodynamics have to say? Oleg Jefimenko (of Generalizations of Coulomb and Biot-Savart laws[26] fame) has answered this question most eloquently. Using conventional electrodynamic theory,[27] he has analyzed the interactions of charged particles. The simple arrangement shown in Figure 1 can be used as an oscillatory particle-clock. In many cases, these "particle-clocks" behave precisely as predicted by SRT, apparently a brilliant and stunning confirmation of the theory. But when this same particle clock is oriented 90 degrees to its direction of motion, it behaves differently. This is not predicted by SRT (apparently a shocking and stunning defeat?). In the case of Jefimenko's particle-clocks, they slow down for purely conventional reasons, having nothing to do with time-dilation. Some clocks slow in accordance with the Lorentz transformation and *some do not*.

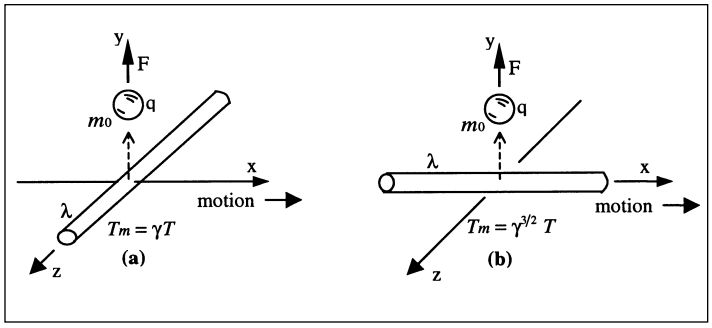

Figure 1. A Jefimenko charged-particle clock. Charged particle "q" is constrained to bounce up and down vertically, along the *y*-axis (repelled upward by a like-polarity line-charge "λ"). This forms a simple oscillatory clock with period *T* at rest, and period *Tm* in motion. When the charges are moved along the *x*-axis as shown, the clock shown in (a) obeys the Einstein time-dilation rule,

$$Tm = \gamma T \text{ , where } \gamma = 1\Big/\sqrt{1-(v/c)^2}\ .$$

When oriented 90 degrees as shown in (b) and moved along the *x*-axis, the same clock does *not* obey this rule. Here $Tm = \gamma^{3/2}\, T$.

But SRT adopts the Lorentz transformation exactly, without any wiggle room. In the final analysis, the concept and definition of *time* is metaphysical. It ought not to be subjected to the whims of anyone's pet theory.

Alternative Theories

Einstein's SRT tampers with space and time in order to *force* the speed of light to be constant *with respect to all observers*. And it pays the price. The theory is reminiscent of a balloon animal. If squeezed at one end, it expands at the other, yielding an overall conservation of paradox. At least five alternatives to SRT have been proposed:

1. The speed of light is constant with respect to an unentrained (or *partially* entrained) aether. (This is the equivalent of an absolute reference frame and violates the *principle* of relativity.)
2. The speed of light is constant with respect to the emitting source. This is the Ritzian "ballistic" theory where the speed of light is (v+c), like projectiles fired from a moving tank (and the antithesis of the Second Postulate).
3. The speed of light is constant with respect to a *fully* entrained ether.
4. The speed of light is constant with respect to the dominant local or *gravitational* field, as proposed more recently by Beckmann.[15]
5. The speed of light is constant with respect to the absorber (the detector), as proposed more recently by Phipps.[8]

Long ago, the great optical experimentalist, Albert A. Michelson, disproved the first two of these five theories. Note that the M-M experiment was compatible with *all* of these theories with the exception of number 1. In a separate experiment, Michelson showed conclusively that number 2 was untenable.[28] So contrary to popular myth, Michelson believed that he had actually *confirmed* the existence of the aether, via theory number 3.

Beckmann has noted that number 3 and number 4 are nearly equivalent theories, if you replace the outdated term "aether" with his more radical idea of "gravity." Beckmann's theory (number 4) squares with all of the experimental evidence, because in every case, the observer has always been tied to the Earth-bound frame of reference. (The double-star evidence does *not* refute his theory.) The light emitted (from binary stars revolving about a common center of mass) would indeed travel with two different velocities initially, and this would cause spectral anomalies that are not observed. But the light rays would merge to a common velocity as the gravitational fields from the two stars merged into one dominant field.[15, p.37] And the dominant field would change yet again upon encountering an observer in the Earth's gravitational field. Beckmann's theory and SRT predict the same answers to first-order in (v/c), and a decisive experiment would have to be performed at second-order, $(v/c)^2$.

In his outstanding book,[8] Phipps argues forcefully and confidently for his absorber theory (number 5) by starting with some of the original ideas of Heinrich Hertz. Hertz proposed a minor modification to the Maxwell Equations in order to make them invariant to the Galilean transformation. The modification involved a simple and straightforward change from partial derivatives ($\partial/\partial t$) to time derivatives (d/dt) in Faraday's and Ampère's Laws. This had the excellent effect of adding a velocity parameter to the Maxwell Equations (a very pragmatic modification for dealing with moving inertial frames, wouldn't you say?). But unfortunately, Hertz assigned the *wrong* definition to his new velocity term, one having to do with the fashionable aether wind of the nineteenth century. As a result, his "Hertzian Theory" was shot down by experiment. To make matters worse, his untimely death in 1894 from blood poisoning, at the age of thirty-six, made it impossible for him to catch his mistake and reassign proper meaning to the new parameter.

Phipps has continued this work by assigning the velocity of the detector to this Hertzian parameter. Phipps goes on to propose an experiment which can decide between SRT and his theory. Furthermore, his experiment can determine the victor at *first-order* in (v/c). This sheds some light on why experiment has not yet determined a winner among the various theories mentioned. With the available technology, experiments at first-order are extremely difficult to do, and experiments at second-order are impossible.

In view of the problems with SRT and the availability of alternatives, is it any wonder that there were (and are) several noted authorities who would have nothing to do with SRT? Some of the famous ones included Dingle, Essen, Ives, Mach, Russell, and Rutherford. *(My apologies to anyone I may have missed (or included) in the* Who's Who Directory of Heretical Physics.) Michelson was even rumored to have said, *"I have created a monster."* Wheeler and Feynman also toyed with the idea of an acausal absorber theory to replace SRT. And late in life, Einstein was said to have had second thoughts.

It is interesting to note that the speed of light is approximately the same as the escape velocity from an idealized electron in a Bohr orbit.[29] Perhaps it will turn out that theories number 2, 4, and 5 are *all* correct within their respective gravitational spheres of influence.

Faster than Light?

There is a new crop of experimental evidence favoring superluminal velocities. Radio waves have been reported to travel faster than "c," at least initially, in the laboratory.[30] However, this result has been challenged as experimental error.[31] Velocities of up to "10c" transverse to line of site have been reported by astronomers. More evidence has surfaced that the speed of light is not "c" in deep space, based on data from the Pioneer 10 and 11 satellites.[32]

Recently, physicists at Princeton made an announcement indicating that they have broken the light barrier.[33] One researcher was quoted as saying: "Our experiment does show that the generally held misconception that 'nothing can travel faster than the speed of light' is wrong." However, their results have been challenged as experimental error due to anomalous dispersion. (Recall that non-TEM transmission modes in waveguide or in optical fibers are subject to dispersion, even when the medium is *lossless*. This spreads out an energy pulse, giving the illusion of increased speed at the leading edge.)

Another heresy to modern physics is the dissident assumption that gravity must act *instantaneously* ($\geq 10^8 c$) in order to explain the motions of the heavens.[34] If gravity propagated (slowly) at "c," the effects of aberration would cause the Earth's orbital energy to increase, and spiral slowly outward. NASA is said to use an instantaneous velocity for the speed of gravity in its calculations.

"Recreational Mathematics" and Its Impact to Physics

I have witnessed dissident protests in the classroom (see *IE* No. 33, pp. 8-9). No doubt the politics of Einstein's Relativity have contributed to the malaise within the physics communi-

ty. In a recent editorial,[35] a noted Emeritus professor of physics discussed the situation with regard to students leaving academia for more challenging work in industry. He goes on to say: "We are experiencing a serious brain drain in physics. . .those who are leaving are in many cases the cream of the crop. I hear this again and again from many different mentors. I find many of those hired for permanent jobs in physics [academia] to be among the least creative. . .my impression is that [industry] is doing a better job in pinpointing creativity. . .[Our profession] believes that there are no more scientific revolutions possible. . .We must not let *The End of Science* become a self-fulfilling prophecy. The best way to prevent the end of science is to provide opportunity in abundance for the most creative and original of our young people. This is not happening. But it needs to."

The mavericks have often made the point that, once cut loose from the constraints of experiment, modern physics has drifted into the realm of abstract, untestable mathematics. G.H. Hardy would have been very pleased. Quoting Beckmann: "Mathematics is perfectly free and unfettered by experimental observation to define its axioms from which it deduces their consequences; physics, if it is to understand the real world, must build on the two primitive and undefinable pillars [space and time]. It must not tamper with them in order to accommodate higher concepts. It must not redefine the unthinkable; more particularly, it must not make the primitive pillars observer-dependent."

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BREAKING THROUGH EDITORIAL

Aether Science and Technology

by Eugene F. Mallove, Sc.D.

Free energy devices, a.k.a. "perpetual motion machines," have long been scorned as myths by the scientific establishment. Any device that appears to have no *visible* or *readily identifiable* fuel or energy source is regarded by physics as impossible. Since cold fusion had no confirmed nuclear by-products when it was first announced in 1989, the scientific establishment prematurely threw it into the "free energy" bin and dismissed it. It has remained in that category for the past dozen years, despite overwhelming evidence for nuclear by-products associated with cold fusion excess heat, published by many competent researchers.

Long before cold fusion, for at least a century, many inventors had claimed to have created "over-unity" or free energy devices, which purportedly operated on reformulated electromagnetic principles. We have discussed many of these in the pages of this magazine. To have any chance of working, such machines logically could not violate a generic energy conservation principle; they would have to extract energy from some hypothetical invisible plenum, such as the "aether" or "ZPE"—zero-point energy.

Some of these claimed devices may actually have worked or *would* work as advertised if convincingly tested. Whatever the facts of such development and testing, it is undeniable that no such free-energy device has entered the scientific or commercial arena, even as a widely available demonstration motor/generator or proof-of-concept unit. Since people are visually and tactily-responsive ("seeing is believing"), this absence of accessible evidence for free energy machines understandably has made even some open-minded devotees of new energy highly skeptical about whether they are possible. This may be about to change.

In my most recent editorial (*IE* #38), which was devoted to reconsidering Einstein's work, a very important project that is continued in this issue, I mentioned newly emerging evidence for laboratory-tested devices that tap into an "energetic aether." These, of course, are in flagrant violation of allegedly rock-solid modern physical theory, including Relativity. As our last issue went to press, the website of Dr. Paulo Correa and Alexandra Correa <www.aetherometry.com> had just appeared; it was not possible to elaborate about what I and others had learned of such devices at the Correa laboratory.

Now it is possible to be more specific. Since not all readers will have instant web access, and because of the importance of these observations, I am glad to be able to publish a report on my witnessing of such apparent devices, as well as the views of Mr. Uri Soudak, former Chief Technology Officer of Israel Aircraft Industries. In no sense do these letters provide the "seeing/testing is believing" evidence that is required to convince fellow new energy colleagues. But I can think of no realistic scenario involving these careful, hard working scientists that would make the Correa work other than a landmark scientific and technological development. Still, as my letter clearly states, the aether motor technology will have to be replicated by others, or distributed as demonstration devices, for it to be widely accepted. It may be extremely frustrating to readers—and to *me*—that these motors are not currently widely available. However, I am satisfied that the Correas are proceeding along an acceptable program of scientific disclosure and business development, which has already been initiated by the scientific experiments elaborated on their website. Now for the testimonial letters:

—Mallove's Letter to the Correas, June 14, 2001—

Dear Dr. Correa and Alexandra,

Thank you for asking me to write a brief review of my observations after my visits to your laboratory in the Toronto, Canada area in August 2000 and in March 2001. Initially, the observations at your laboratory were covered by a Non-Disclosure Agreement (NDA), but now that you have requested this testimonial letter, you have my permission to post it and use it as you please. [Editor's Note: Posted as of late July 2001 at <www.aetherometry.com>.] I wish to convey, with as great precision as I can in this short space, my observations and conclusions about your work with what might well be called "aether science and technology."

I am trying to be as circumspect as I can about this most remarkable new direction for science, which you have evidently advanced considerably. That takes some doing even for one who is experienced with the astonishing scientific findings in the low-energy nuclear reactions (LENR) field, because what I observed at your laboratory is so very dissonant with what I had come to understand about the alleged certainties of modern physics. Frankly, I was shaken and stunned by the observations and measurements in your laboratory when I was there. I will never forget those experiences. These are my views and only my views, for no one else from *Infinite Energy* was with me and can attest to my observations or has any basis for questioning or substantiating them, apart from their trust in my abilities and integrity.

First, let me mention to newcomers that your technical work has appeared before in our magazine, *Infinite Energy,* beginning in 1996 in connection with your patented Pulsed Abnormal Glow Discharge (PAGD™) electric power generator technology and experiments (Issue Nos. 7, 8, 9, 17, and 23). That excess energy technology was validated to my satisfaction at high power level, using multiple measuring techniques during the on-site visits—employing conventional electric meters, a digital storage oscilloscope, and a computer data acquisition system. On my last visit, when your PAGD™ inverter technology had improved considerably from my first visit, I observed an input DC power to the PAGD™ reactor of 50 watts, with an output motor power (mechanical shaft power of approximately 500 watts). I commented to you that this could easily be

made self-sustaining with a DC generator on the output shaft of the motor, and you agreed with that general conclusion. My understanding is that several other respected Ph.D. scientists have similarly been present in recent times at your laboratory to witness the PAGD™ experiments and even more remarkable ones connected with your already self-sustaining Aether Motor devices, which I will discuss below.

Issue #37 (May/June 2001) of *Infinite Energy* contains your most recent paper with us—one of the most important papers *Infinite Energy* has ever published, "The Reproducible Thermal Anomaly of the Reich-Einstein Experiment Under Limit Conditions." Anyone who wishes to gain an insight into the quality of your work should read this. But that article, I must emphasize, is but the merest "tip of the iceberg" of your much wider discoveries and technical contributions, which you have reviewed with me on both visits and in other conversations. As my editorial in Issue #37 ("A Bombshell in Science") notes, you intended to be publishing much, much more of your experimental and theoretical work on the internet. You have kept your promise and have done so. You have my congratulations and gratitude for this landmark publication. This will make possible widespread validations of your scientific work. I must emphasize to all readers of this letter that reproduction by others is the only way in which your experimental and theoretical work will ultimately be accepted. I know that you seek such reproduction by other careful investigators, because such remarkable reports from unfamiliar scientific territory cannot be accepted at face value by others as true, even though I am truthfully relating them.

I had reviewed some of your written material already on my visits with you and it is spectacular, as those who will download from your new web site will discover. As we well know, there are severe obstructions to publishing frontier scientific work today and this is why you have chosen to publish on the internet for modest down-loading fees. In recent times we have serendipitously discovered that there are actual lists of *forbidden topics*, which formally and informally exist at two major scientific publications, *Science* and *Nature,* and we are all familiar with how excellent work in the LENR field has been banned from those publications and ridiculed in flimsy journalistic accounts. I very much regret that your experimental and theoretical work could not have been reviewed and then published in the various mainstream scientific publications, where it should, by right, be placed. That is a loss for the world and for those publications, but such is the nature of the "peer review" system that has grown to be such a rigid filter against ideas that change reigning scientific paradigms. Nonetheless, I do expect that the publication of your series of extensive articles on the internet will have a revolutionary effect, particularly once your experimental work begins to be validated by others. I think that this will be extremely beneficial to the entire so-called *new energy* field, which is much in need of comprehensive theories with evident predictive value, as your work surely appears to have— based on the many experiments that you showed me, not all of which are related here.

The subject now concerns experiments and conclusions that go far beyond your previously published and patented PAGD™ work. The bottom line of all your work is the complete validation, it seems to me, of the existence of an energetic *aether* (or *ether*, as some may prefer), which you have learned to tap technologically in various ways to make self-sustaining motors. There is simply no other way of explaining what I observed. Others may try to invoke theories of "ZPE" (which apparently does not enter the picture in either an experimental or theoretical sense at all) or will claim that you may be engaging in fraud. That will be their problem, not yours. I firmly believe that you have honestly confronted nature and have no interest in engaging in flimflam—especially since there are far simpler ways to gain financial advantage than by performing elaborate experiments (which, when published, can be falsified or criticized by others) and interlinked theories. If anything, you have held back this information about your technology longer than I would have preferred.

Your findings and accomplishments, above all, open up a new energy source, but it is also obviously profound, new physics. This has come about because of your vigorous pursuit of the truth about the work begun by Dr. Wilhelm Reich in the 1930s and pursued by him and colleagues into the 1950s. I regret to say that prior to your informing me of your intellectual investigation along the general lines of what Reich had begun, I had little knowledge of the work of Reich, and had actually absorbed the insidious and nasty media-generated opinion that it was perhaps some kind of "New Age" smoke and mirrors. How wrong I was!

Let me say that my editorial in *Infinite Energy* #37 should give readers the gist of how important I think your paper in that issue is for physics and how historically important was the episode that involved Albert Einstein, Wilhelm Reich, and Einstein's assistant Leopold Infeld in the 1940s. As you know, if it is referred to at all in general biographies of Einstein, the Reich interaction and experiment is dismissed as of no consequence. And, as my editorial points out, Dr. Reich was marginalized and mocked by *Time* magazine in 1999 on the same page with Drs. Fleischmann and Pons. Apart from the misgivings many might have due to circulating misinformation about Reich and his former focus on matters of sexuality and politics, I wish to inform them that I am absolutely certain that the thermal anomaly of the Reich-Einstein experiment is real and has no trivial explanation. I have observed it myself independently under careful conditions here at our New Energy Research Laboratory (NERL) and will be publishing my results at a future time. (Others should know that the thermal anomaly is very easy to observe with *calibrated* mercury thermometers of the proper range and resolution—0.05°C highly recommended—but there are some pitfalls too, so they should read your paper carefully and the much greater body of experimental information that is on the new web site. This puts the thermal anomaly in a broader physics context.) I am also now quite certain that the other physics anomalies observed and published by Reich are real—the electroscopic observations as well as the observations of effects on energy-saturated vacuum tubes, a serendipitous discovery of his with Geiger-Muller tubes. I have not personally measured these latter, but I note that you have done so extensively. It evidently is the basis upon which your Aether Motors work, otherwise I cannot imagine how you could have pursued those motors to the point that you have reached. You have most certainly gone beyond what Dr. Reich claimed to have achieved in these motor effects stemming from energy accumulation in ORACs (orgone accumulators).

On August 27, 2000 at your laboratory, we completed lengthy discussions and activities which included: an overview tour of your most impressive labs, a review of significant introductory aetherometric papers for your then forthcoming web publications, exercising of the PAGD apparatus, demonstration of various heuristic electronic experiments connected with

externally powered electromagnetic coils, and demonstration of an apparently clear, significant (70% reduction level) antigravity effect on an approximately 45 milligram piece of gold foil. I was then given the first demonstration of your first stage Aether Motor. You asserted that it had no battery or other active energy producing elements within its small, approximately 0.2 cubic foot, electronics box, which was then closed. Its only evident power source were two adjacent, approximately one-cubic foot each, metal Faraday cages, each covered by removable ORAC-type covers of about two-inch thickness (with no bottoms). I opened the doors to the Faraday Cages to see that they were empty of power sources. I have no doubt that you would allow me to open the delicate motor electronics box to examine it fully if I were to visit your laboratory today. However, I do not represent to anyone that I have examined its innards. (Your honesty in this matter is accepted by me, pending proof otherwise. Additionally, I have discussed the contents of the electronics box with another Ph.D scientist, who recently visited your lab, and who saw, upon the electronics box being opened for his inspection, only electronic circuitry, no batteries.)

The ORAC covers were removed to show me that nothing was electrically connected to the metal boxes. No matter, because each of these ORACs were connected to the Aether Motor by only a single insulated copper wire, with a metal contact. There was no evident ground wire or metal object of any kind to complete the circuit to the ORACs! Yet the motor started upon being connected to the ORACs. Its short output shaft could be mechanically stopped by my hand and it had the tug of a motor I would estimate to be in the several watt range. It would restart instantly upon being released. On that year 2000 visit, the motor moved from 50 RPM to the several hundred RPM range, varying with time and conditions, but on my second visit, you had arranged a second Aether Motor set up that operated in the several thousand RPM range, as shown by a tachometer. The tug of its shaft seemed to put it in the few tens of watts range in mechanical output. I would have wished to stay longer to make exacting mechanical measurements of the output power, but the overwhelming experience of observing interaction with the motor was quite enough for that visit! I hope to return to your facility to make such detailed measurements with you. I was most astonished and fascinated to observe effects with your Aether Motor that seem incontrovertibly connected with the *biophysical* energy processes characterized by Reich. Holding my hand to one of the wire leads to the Aether Motor would make it increase its speed! Holding another person's hand, with mine still attached to the wire lead, would make the motor run even faster! These are the most astonishing observations I have ever made. I was standing on a concrete floor with rubber-sole shoes. I can think of no other explanation (barring fraud, which I rule out) other than some sort of "biological transduction" of energy into the motor. Moreover, the motor circuit included an external transparent glass evacuated discharge tube with two aluminum plates. While an Aether Motor was operating, bright discharge sparks were occurring in the glass chamber between the plates. It is a completely alien concept to accepted physics, but apparently true, that ordinary mass-bound charges, electrons, were apparently being brought into existence from the plenum of the energetic aether.

On each of the visits, the motors appeared to run indefinitely, and you asserted that you had run them for periods of up to eight hours, but that there was no fundamental limit to their being powered indefinitely by the new energy source—the energetic aether. There was no apparent diminution of motive power while I was in the room for a period of approximately one hour.

We continued each visit with further discussions of the performance characteristics of the new Aether Motor technology and its possible extension into demonstration devices, which I hope will eventually be forthcoming. (I am happy that you have now completed the patent application process for these Aether Motors.) We also discussed other validation approaches to further your efforts and proposals. I must say that of all the laboratories I have visited in my entire life in science and engineering, yours has been by far the most impressive and worthy of significant funding. I am deeply appreciative that you gave me the opportunity to learn about your experiments and theories at a level that few if any outsiders previously have had. You have done absolutely brilliant work that deserves the most rigorous verification and ultimate acceptance by the scientific/technological community. Whether your aetherometric theories of motor operation are accepted is another question, but I have little doubt that the motor technology itself will be validated in due course one way or the other, providing you are forthcoming with details of construction.

[*Editor's Note*: The Correas' website designates under "Experimental Aetherometry, Volume 3," seven extensive technical modules that will relate the rediscovery of the Orgone motor. Since these modules are expected to be like the eight high-information content modules on aetherometry already released, it will be possible for other parties to build aether motors to confirm (or reject) the Correa claims. I understand that these aether motor modules have already been prepared, but they have not yet been released due to patent application considerations.]

Let me end this testimonial with an assessment of the greater significance of the discovery and proof of an omnipresent, biophysically active energetic aether is comparable to the magnitude of the Copernican upheaval, and opposition to it will be, as expected, no less intense. Let me state the implications and conclusions into ones of which I am personally very certain:

• There is an energetic aether that can be tapped to create electrical power and heat.

• The energetic aether has definite biophysical properties with possibly a strong bearing on living systems.

• The Second Law of Thermodynamics has limited validity, and it is clear from the historical record how such a disastrous restriction was postulated. The thermal anomaly of Reich is the final nail in the Second Law's coffin. The Second Law is not absolute and must be revised or extended.

• There is space and time but no space-time. That is, Einstein's theories of relativity are fundamentally wrong (despite their efficacy in rote formulaic application in certain areas) and must be replaced by one or more developed or developing theories.

• Most important for technology as well as science: *Mass free* charges apparently exist as part of the energetic aether and are the basis for many of the critical observations made by Reich and others since the 1940s, including the motor-force observations that Reich made and published and his apocryphal but undoubtedly real (and witnessed) self-running electric motor.

You have gone beyond his work to make robust systems.

• Gravity can be controlled by electromagnetic means.

• The mechanistic description of the world as "nothing but" atoms and subatomic particles flitting about in a formless vacuum, through which only electromagnetic radiation flows, is completely wrong. The complex aether is the most fundamental plenum of existence.

It has been a long time since March 23, 1989 when I became involved with the cold fusion controversy, and later began to reassess what other anomalous claims in science—particularly those associated with energy—might be real. We have seen many, many strange things, about many of which to this day we cannot be certain. Other claims that were initially surprising—such as heavy element transmutation—have now gained acceptance, at least within the cold fusion/LENR ranks. It seems that matter can disintegrate and change in drastic ways with minimal external perturbation. It is possible, but barely so in my view, that cold fusion and LENR will turn out to have nothing to do with an energetic aether and may be completely explainable by "conventional" physics. That may be true within certain limited regimes, but not I think, in larger scope. In my view, the heavy element transmutation aspects are particularly amenable to explanation under the influence of mass-free charges in an energetic aether. We shall see.

What you have shown quite clearly is a class of new discoveries, processes, and theories, which recapitulate discoveries that were marginalized earlier in the twentieth century. The matter of the "Reich-Einstein Affair" is particularly appalling, but those familiar with the dynamics of the "cold fusion" controversy will not be surprised. These emerging discoveries now underway will lead, I believe, on a straight path to the development of free energy devices and propulsion systems of unlimited capacity. I believe that a common historical pattern will be repeated: many simultaneous discoveries of effects connected with this energy will occur. Technological devices are the only way in which the scientific establishment will be forced to change its very bad ways and gross misconceptions about physics, chemistry, and biology. The fossil fuel age will begin to come to a grinding halt and the age of free energy and unlimited powers for humanity will begin. If we are lucky, the world of science, as we have known it, will soon begin to undergo a radical, wrenching change. It will not be easy, but it is now inevitable. *—(End of Letter)*

—Uri Soudak's Letter to the Correas, June 22, 2001—

(Reprinted with the permission of Uri Soudak and the Correas.)

The launching of this website is a celebration for me. I have known the Correas for many years now and am well acquainted with their work. My first encounter with them was while I was deputy for Israel Aircraft Industries' Executive Vice President and CTO. We were at that time searching for new technologies and were in the process of converting a heavily military industry into a more commercial one. The field of Energy seemed to us a good investment and one of the world's imminent needs. I received a detailed proposal from the Correas, presenting their mature invention of the PAGD/XS-NRG device, which was detailed both in patents and in their literature. Having been exposed to hundreds of inventions and proposals as a part of my daily work, I was surprised at the depth and detail of a device that, according to current physical science, could not possibly be working!

Several months later, my superior retired and I became the Chief Technology Officer of IAI at their headquarters in Ben-Gurion Airport. However, IAI was then entering a difficult financial situation and further investigation into the Correa invention was postponed but not abandoned. As soon as I could, I requested a demonstration and traveled to Toronto to attend it, which turned out to be an exhilarating experience. I told the Correas at that time that I would propose their project for investment by IAI.

Two factors were against us however: first, the high risk that was involved in a phenomenon that was not yet backed by a solid theory, and secondly, the fact that IAI was not completely out of its own financial problems—its priorities were set elsewhere. Nevertheless, I thought that a small investment could be made to greatly reduce the risk by a thorough checking of the device at IAI premises. In 1997 however, I decided to leave IAI for several reasons, one of them being the CEO's decision to abandon this route.

Moving to new Executive jobs in North America, I have kept my contact with the Correas, both because I admired their continuous and amazing work, and because of my growing admiration for their talent and wisdom. I see myself as very fortunate indeed in having been able to closely follow the revelations of the new Theory of Synchronicity and the stream of unbelievable experiments and devices that followed. I was part of their joy when the universe unfolded in a pure and simple way to them which permitted the solving of many of the inconsistencies and paradoxes in existing physics. Finally I could understand mass and massless energy in all its forms. A year ago I witnessed experiments to tap into the unlimited energy surrounding us and into a simple formation of gravity fields. No one on earth has achieved this before!

This is why the launching of this web site is a celebration. It is opening a new era for mankind. An era without energy limits, an era without any transportation limits, an era devoid of need for destruction because there is no limit to prosperity. Paulo and Alexandra Correa, thank you! *—(End of Letter)*

— Where to Go From Here —

The scientific experiments leading to the aether motors and the build-up of a theoretical framework under the rubric "aetherometry" are now beginning to be detailed on the

A simple, commercially available leaf electroscope.

Correa web site. Whether outside parties will be sufficiently motivated to begin verification efforts remains to be seen. There is a paradox: Early release of detailed descriptions of the aether motor technology could have a suppressing effect on systematic efforts to confirm the scientific measurements of aether properties by means of electroscopes and thermometers. But *widespread* convincing proof of aether motor function could as well spur retrospective examination of those fundamental measurements. The Correas have not chosen the latter course, and that is their prerogative. For now, they are exploring with select people other ways to further their research and its commercial potential.

Open-minded scientists concerned with new physics should temporarily put their theoretical prejudices aside and examine the large body of disclosed aetherometric evidence. The Correas first discuss what they term the "gravitokinetoregenerative phenomenon," a property that turns on its head the conventional "static electricity" assumptions about what keeps the delicate gold leaves of a conventional electroscope in deflection. Their concise abstract:

> Basic experiments demonstrate that, for any set deflection angle of the electroscope leaf from the vertical under atmospheric conditions, the work performed against gravity by a "charge gas" trapped in a conductor is neither predictable from current electrostatic or gravitational theory, nor equivalent to the electric energy calculated or measured oscilloscopically as being required to charge the said electroscope to the set and calibrated deflection. Furthermore, the results suggest that, quite independently from the mechanism of charge cancellation by recombination with ions of opposite polarity, electroscopic leakage rates depend upon the rate of regeneration of the kinetic energy of the trapped charges performing both electric and antigravitational work, as sourced upon hidden variable(s) in the local medium. We found therefore that, in order for the electric work of repulsion performed by charge against charge to be conserved, the work performed by charge against local gravity must be constantly supplied by regeneration of the kinetic energy of the trapped charges from the surrounding medium.

Ergo, every leaf-electroscope since time-immemorial has been a "perpetual motion machine" in disguise, powered by some aetheric environmental factor! They then proceed to examine long-time records of spontaneous electroscope discharge rates to find correlations with environmental factors. In these they attempt to find local and non-local hidden variables, both electric and nonelectric, which affect discharge rates. In one of many provocative conclusions, they propose that a hidden variable of solar origin tends toward the *arrest* (stopping) of discharge in atmospheric electroscopes. They summarize, "Only this nonlocal variable therefore could account for the power of the local medium to regenerate the kinetic energy which charge spends in performing work against gravity when trapped in a conductor subject, in turn, to electrostatic repulsion. Essentially, the kinetoregenerative power of the local medium is in turn replenished by this component of solar radiation."

Of course their objective from then on is to identify the theoretical mechanisms of aether function that can do this. By their fourth web-posted monograph, "Electroscopic Demonstration of Reverse Potentials of Energy Flow Able to Draw Kinetic and Electric Charges," they are able to show by involved but conceptually simple demonstration how utterly wrong our understanding of simple electroscopes has been, if their assessment is correct. Their short abstract says it all:

> Methodological objections are raised to the conventional understanding of the charged states of the electroscope, and a new classification of charging methods is proposed. The existing hiatuses in conventional electrostatic theory of the electroscope stem from complete ignorance of the electroscopic action of observable reverse potentials, first proposed by Dr. Wilhelm Reich over sixty years ago, which establish centripetal radiative fields capable of drawing both nonelectric kinetic energy and the electric energy of charge trapped in conductors. From an experimental examination alone of the electroscopic interactions of the human body, the authors conclude, as Reich did, that there is an energy specific to living systems and to the ground, which is neither electric nor electromagnetic.

This revelation of an entirely new world of physical phenomena, by means of extremely simple experiments, is reminiscent of Oersted's 1820 experiment in which the deflection of a suspended compass needle near a current-carrying wire revealed the presence of an unsuspected surrounding magnetic field.

Today's physics establishment imagines that only giant particle accelerators, "gravity wave" detectors, and gargantuan neutrino capture tanks can move the frontiers of physics outward. It would never take the time to visit a high school physics lab, obtain a suitable electroscope, and attempt to verify (or reject) the Correas' claims. Do not forget that these same establishment folks in 1989 thought that they could debunk cold fusion by quick theoretical studies and rushed, poorly performed experiments. These physicists live in a dreamworld of the arrogance of power.

In their fifth monograph the Correas address the many possible objections to unconventional explanations of the *thermal* anomalies associated with orgone accumulators (Oracs). In addition to the *indoor* Reich-Einstein thermal anomaly experiment, which they presented in digest form in Issue #37, the Correas present much more extensive data from *outdoor* experiments. They claim to show that the thermal anomalies cannot be accounted for by the blackbody spectrum of radiation from either the Sun or from the Oracs themselves.

By monograph six, the Correas are able to spell out what they believe to be the outlines of the governing physics in both the thermal and electroscopic experiments. Their abstract, in part:

> . . .we present evidence for the fact that the energy concentrated inside ORACs and responsible for the anomalous deceleration and arrest of electroscopes placed within them, irrespective of charge polarity, is neither thermal nor electric. The proposed methodology allows us for the first time to determine the comprehensive values of the energy and power of ORAC devices (in Reich's idiom, to measure the actual orgone energy values, and their variation, within these devices), and as well to establish that the electroscopic kinetoregenerative phenomenon is not a thermal one. We close the presentation by suggesting that the Aether energy effect responsible for the thermal and electroscopic anomalies

observed within the ORAC is neither electric, nor electromagnetic, nor gravitational per se, but antigravitational. In full agreement with our Aetherometric Theory of Synchronicity (AToS), we conclude that, by a heretofore unknown process, charges trapped in a conductor undergoing electrostatic repulsion—or, for that matter, in a dielectric undergoing electrostatic repulsion, as can be easily observed with electroscopic leaves made of dielectric materials—and subject to a local gravitational potential, are able to tap local Aether energy and to convert some of its nonelectric and nonelectromagnetic energy into their kinetic energy. This kinetic energy is associated with charge but distinct from it, and charge spends it precisely to counteract the continuous action of the local gravitational energy. This counteraction is maximal at electroscopic discharge arrest. The kinetoregenerative phenomenon demonstrates therefore that there exists another form of energy which is neither electric, nor electromagnetic, nor gravitational. Yet, this energy appears to be responsible for an array of electric, thermal and gravitational anomalies.

So there you have it, if the Correas are correct, a radically new conception of energy that pervades our terrestrial and cosmic environment—biophysically active and able to be observed by the most basic of physical measurements. This is obviously a tall order to try to accept after a lifetime of thinking about physics in very different terms. (It is not easy for *me* though I have personally observed motors and energy collectors that apparently embody these principles!) In essence, the Correas are suggesting that most of the physical universe has been in hiding and that it can be revealed through their aether measurement methodologies. Though this may seem very "Copernican" in its pretensions, this is not all that much more than mainstream physicists claim when they speak of cosmic "dark matter," "dark energy," "quintessence," or the like comprising the vast bulk of the universe. The main difference is that the Correas provide concrete, falsifiable, table-top experiments to bolster their claims. In the tradition of Einstein's famous "gedanken" experiments that so set back physics, Theory-of-Everything speculators today in mainstream physics pose ever more esoteric mathematical sand castles *(e.g.* string theory), almost none of which can be checked with experiments.

It will fall to engineers and scientists of good will to examine this most profound proposal for a new scientific order, to explore it to its core, and to change the world with it if they find that it works. To quote the Correas from their web-posted essay, "Usages of Science: Use and Abuse of Physics": ". . .we tend to think about science as merely intellectual capacity to comprehend the world. But comprehension itself is worthless—for actual understanding only comes from transforming the world, from acting upon what is comprehended, from experimenting, from altering our perception."

At *Infinite Energy* and New Energy Research Laboratory we will do our best to explore and illuminate for our readers and colleagues this most challenging and promising field, the rebirth of aether science and technology. As I said in ending my cold fusion history in *Fire from Ice* (1991): ". . .heed the eternal challenge of science not to follow where the worn path may lead, but [to] go instead where there is no path, and leave a trail."

Ninth International Conference on Cold Fusion (ICCF9) Meets in Beijing, China

Eugene F. Mallove

The first International Conference on Cold Fusion of the twenty-first century (ICCF9) was held at Tsinghua University in Beijing, China from May 19 through May 25, 2002. Conferees gathered at the International Convention Center in the new, luxurious Uniscenter Hotel. New experiments with irrefutable evidence for nuclear-scale excess heat and nuclear products of low-energy nuclear reactions (LENR) made this a good step forward. Nevertheless, the lack of clear-cut evidence of progress toward near-term commercialization of the real but elusive excess heat phenomenon was disappointing. Still, it must be said that significant efforts to commercialize this new energy source are occurring worldwide, a fact not evident in the public discussions.

This ICCF9 report, prepared soon after returning from China, is only a brief overview of the conference. More reports may be published in future *IE* issues. Full technical papers will be in the conference Proceedings, available for purchase from the ICCF9 website (http://iccf9.global.tsinghua.edu.cn).

ICCF9 was the first ICCF meeting to be held in China. The last one, ICCF8, was held May 2000 in Lerici, Italy (see report in *IE* #32). Two other ICCFs have been held in Asia: ICCF3 (1992) and ICCF6 (1996), both in Japan. Following the traditional Europe-Asia-North America rotation for ICCFs, ICCF10 will be held in the U.S., quite possibly in the Cambridge/Boston area, but certainly on the eastern seaboard. The chairman for ICCF10 is cold fusion theorist Prof. Peter Hagelstein of MIT's Department of Electrical Engineering and Computer Science—hence the pull toward the Boston area. ICCF10 will occur either in September or October 2003, because it was agreed among members of the international organizing committee that a two-year separation between ICCFs is too long.

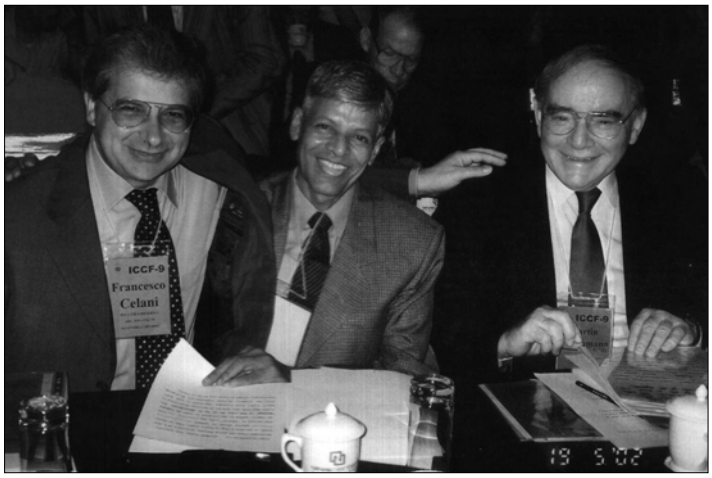
ICCF9 was sponsored by: China's Fundamental Research Division of the Ministry of Science and Technology; the Physics Division II of the Natural Science Foundation of China; the Chinese Nuclear Physics Society; and the Department of Physics at Tsinghua University. It is gratifying to observe the open-mindedness of these Chinese science organizations. Would that ICCF10 could be sponsored by the U.S. DOE, NSF, the American Nuclear Society, and the MIT Physics Department. But please don't hold your breath for that!

According to the ICCF9 Organizing Chairman, Professor of Physics Xing Zhong Li of Tsinghua University, the conference had 124 attendees, with 17 "accompanying persons." The ICCF9 conference book contains 104 abstracts. The bulk of these papers were presented in poster sessions on three different days, with two to three minute oral summaries being given to the full assembly of participants. Presentations that were deemed to merit longer lectures to the whole group received 30 to 50 minute time allotments. Some 77 of the attendees were from abroad, but the rest were all from China, which appears to have an active interest in cold fusion, dispersed among a variety of physics departments and organizations. Known attendees and paper submissions (or abstracts) came from Australia, Belarus, China, France, Georgia, Germany, Greece, India, Indonesia, Israel, Italy, Japan, Romania, Russia, Spain, U.K., Ukraine, and the U.S.

Jed Rothwell provided this initial impression of ICCF9, which seems appropriate: "These conferences are more difficult to describe than they were a few years ago because experiments are much more sophisticated. Results are no longer binary: heat or no heat. Bare bones, basic repeatability is good in most experiments. Results are usually multifaceted: heat plus charged particles plus transmutations. When results are less than satisfactory, it is because they vary over a wide range and do not correlate well with one another. Expectations and standards are rising. A few years ago researchers were pleased to see something happening in most runs. Now they want to see the same thing happen to within an order of magnitude."

ICCF9 was similar to the other ICCFs since about ICCF6 in Toya, Japan—a mixed bag of very, very good material and experiments (*e.g.* the Mitsubishi Heavy Industries report on repeatable transmutations—see below), and lots of modest improvements, hints of progress here and there, as well as some very marginal experiments. My overall feeling about the cold fusion field is one of general sadness and pessimism, tempered with glimmers of hope. Certainly, the reality of these phenomena keep being re-emphasized with a widening circle of experiments, but the field is generally unheralded and/or disrespected worldwide. This was the first ICCF at which the infamous anti-cold fusioneer, Dr. Douglas Morrison of CERN, was not present to assault cold fusion researchers with ludicrous questions. Because of his passing last year, there will be no absurd critiques circulated to the outside world. This year, Robert Park of the APS was simply silent about ICCF9—his cold fusion "informer" was no more.

Conference organizer Dr. Xing Zhong Li and Tsinghua University Vice-President Yang Jiaqing.



Francesco Celani, Mahadeva Srinivasan, and Martin Fleischmann.

Almost no one in the cold fusion field seems to have a clue about what has to be done to energize the field—pardon the pun. And there is even peculiar, inexplicable concealment of certain advancements by some researchers. One example: Shortly before ICCF9, reports of a new cold fusion method developed by a respected U.S. researcher were circulating. It involves shining a low-power laser onto a coated cathode in an electrochemical cold fusion cell in calorimetric balance (no excess power). Remarkable, rapid increase in output power of the cell occurs when the red laser light hits an appropriate "active" spot on the cathode, or so it is said. Moreover, the effect is very repeatable. Yet for reasons that are not clear nor seemingly justified, no report of this work was provided at ICCF9. With that episode as background, the end-of-conference talk about "cooperation" and more rapid sharing of information seemed like so much hogwash.

The only speaker who talked openly and directly about an intent to commercialize was Dr. Les Case. His work, four years beyond ICCF7 in Vancouver at which he announced his gas-phase "catalytic fusion," has not yet emerged from the shadows. He spoke at ICCF9 in expansive terms, and indeed he has launched a real new and important area in cold fusion (possibly the *most* important direction), but it is still unclear how far from his goal he remains.

We worked very, very hard here at New Energy Research Lab (NERL) in Bow, New Hampshire to help Dr. Case use his big (100-liter internal volume) dewar cell to verify excess heat and try to achieve self-sustainment with his new proprietary formula—a patent-applied-for catalyst very unlike earlier ones that were successfully tested by him and others. Sorry to say, just after ICCF9 we came to this conclusion about our initial Case work after performing a second week-long series of runs with his catalyst: Almost no excess heat, possibly at most a few watts out of approximately 100 watts input. This is obviously far below self-sustaining, and it is possible that some unconfirmed defect was present in these tests. For his part, Dr. Case remains confident that various "know how" items have not yet been properly integrated into these experiments, because his own laboratory work with a smaller device evidences great performance, he says. Nothing would make us happier than to see our New Hampshire catalytic fusion colleague succeed brilliantly by creating the self-sustaining reactor he believes to be just around the corner. Other work on catalytic fusion, of a still confidential nature, is proceeding elsewhere in the U.S., using Seebeck envelope calorimetry of much smaller samples.

Conference organizer Prof. X.Z. Li's group at Tsinghua University reported excess heat (at 2 watts/cc level) in a gas-loaded system involving palladium wire in a deuterium atmosphere. Gas-phase excess heat work of any kind bears a clear relation to what Case is doing in catalytic fusion, so this paper was of special interest. It speaks of a "pumping effect" of deuterium into palladium, a phenomenon which will certainly merit scrutiny by others.

Some very good news that may help commercialization efforts in the thin-film area: A group at Japan's Yokahama National University led by Drs. Ota and Fujii tried ordinary water electrolytic cells with thin-film-coated metal beads and tiny cylinders, similar to Dr. James Patterson's thin-metal film-coated plastic beads, which were so successful in the mid-1990s. (There are hints that cold fusion work at CETI may be coming to life again. Stay tuned!) The Yokahama group succeed in getting excess heat from about 25% of its cells. The excess heat was not very high—about 50% excess at maximum. Dr. Michael McKubre of SRI International had not been convinced about the calorimetry of Patterson cells before ICCF9, but in his summary toward the end of the conference he said that he was impressed with the Yokahama work.

There were numerous papers confirming various kinds of nuclear products—in Russia, China, and Japan. Prof. John Dash and Dr. John Warner at Portland State University reported excess heat results in the 10-25% excess range using titanium cathodes in heavy water cells. They also found that trace amounts of gold had formed during some runs, as detected by neutron activation analysis—a presumptive transmutation.

Dr. Iwamura *et al.* at Mitsubishi Heavy Industries Advanced Technologies Center had the most spectacular work, which will be reported in the *Japanese Journal of Applied Physics* later this summer. They used a very expensive vacuum chamber with *in-situ* XPS (X-ray photoelectron spectrometry) detection to observe the transmutation of an atomic species; cesium and strontium were used separately. The species is plated onto a palladium and CaO-layered sandwich of material through which deuterium gas passes as it is drawn through layers by vacuum on the other side. The upper surface (facing the D_2 gas) is 400 Ångstrom-thick pure Pd, followed by a 1,000 Å multilayer sandwich of CaO and Pd. Then the bottom layer, facing the vacuum, is 0.1 mm thick Pd. The upper D_2 gas-facing Pd layer has deposited on it the cesium (or strontium). Iwamura *et al.* obtained a *time-history* of the transmutation phenomenon: Cesium (Cs) transmutes to praseodymium (Pr), *i.e.* Cs-133 goes to Pr-141. As the Cs declined, the Pr increased correspondingly.

The group hypothesizes that there is a gain by the initial species

of two alpha particles (two He-4) or a Be-8 nucleus! The time-history of the growth of the new species matched the decline of the old species. Contamination has been completely ruled out by exhaustive testing. In the case of strontium, the reaction is: Sr-88 goes to Mo-96. As detected by SIMS analysis, the molybdenum isotope produced is Mo-96, highly anomalous with no possibility of being naturally-occurring Mo. Overall, the Mitsubishi work is as close to being a confirmation of what might be called "modern alchemy" as can be imagined.

Italy, well-represented at ICCF9, has an official cold fusion program which operates at several centers. Also, it is known that the Pirelli Corporation at Milano has a working group in cold fusion. The group of Dr. Antonella DeNinno *et al*. at ENEA has apparently demonstrated massive excess heat in current-fed exploding wires laid down on a substrate in a D_2 gas atmosphere (this work was initially presented at ICCF8). Cold fusion pioneer Dr. Martin Fleischmann has concluded (told in private discussions) that this group has demonstrated *megawatts per cubic centimeter* of power, although the researchers claimed "only" 3-4 *kilowatts per cubic centimeter*. In private remarks, Fleischmann continues to be convinced that military authorities are now in on all of this and looking toward the use of cold fusion processes in weapons.

Of great interest concerning the Italian program is that physics Nobel laureate Carlo Rubbia has recently been quoted in the Italian press to this effect: he believes that cold fusion is real and important. Rubbia apparently was so eager to hear a firsthand report from ICCF9 that he called several Italian scientists home from the conference on the day before it ended. A delicious emerging irony in this: a nemesis of cold fusion from its early days, science journalist Gary Taubes, had written a book, *Nobel Dreams*—a scathing personal attack on Rubbia's high-energy physics work—this, long before cold fusion was announced. Rubbia may yet get his revenge on Taubes! (A further aside: U.K. physics Nobel laureate Brian Josephson continues to follow reports from the cold fusion field with great interest, and is dismayed that mainstream scientific publications are not paying attention to this work.)

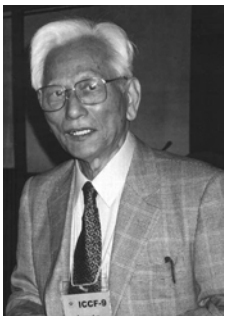
We also learned at ICCF9 that a small cold fusion group from Virginia has managed to secure a contract from the U.S. Army for a cold fusion experiment. It is also known that DARPA (Defense Advanced Research Projects Agency) in the U.S. has provided limited funding to a few high-profile cold fusion projects in academia (at MIT, of all places!) and industry, but whether such funding continues anywhere is not known. Dr. Edward Teller's associate, Dr. Lowell Wood of the Lawrence Livermore National Laboratory (a nuclear weapons research facility), attended both ICCF7 and ICCF8. At the latter conference, Dr. Wood seemed impressed with the quality of papers and appeared convinced of the reality of the phenomenon.

A four person group from Israel attended ICCF9. It was good to see interest from a country that perhaps more than any other might benefit from the advent of the peaceful use of cold fusion energy.

Roger Stringham of sonofusion fame (First Gate Energies, Inc.) has moved his laboratory to Hawaii from California. He gave a talk at ICCF9, basically a review of his ultrasonic implantation of deuterium into metals. He reiterated his findings of helium-4, helium-3, and tritium in some of his earlier experiments.

Professor Yoshiaki Arata and Dr. Y.C. Zhang presented research in the same general area in which Stringham works, which is reported in two recent papers. Here are the abstracts:

"Intense Sono-implantation of Atoms from Gases into Metals," *Applied Physics Letters*, 1 April 2002, Vol. 80, No. 13, Yoshiaki Arata and Yue-Chang Zhang, Cooperation Research Center for Science and Technology, Osaka University, 11-1 Mihogaoka, Ibaraki, Osaka 567-0047, Japan.


Yoshiaki Arata

Abstract: It was found that various gaseous atoms can be easily implanted into metal powders under ultrasonic cavitation inside a vessel with water (H_2O, D_2O, or a mixture thereof). Inert gases (^{3}He, ^{4}He, Ne, and Ar) and others (N_2, air, H_2, and D_2) were strongly sono-implanted into metals such as Ti, Fe, Ni, Cu, Zr, Pd, Ag, Ta, Pt, and Au, which were originally set in the vessel as foils, and were broken into ultrafine metal powders during intense ultrasonic processing. A large amount of implanted atoms was verified to exist in these powders from mass spectroscopic analyzes.

"Nuclear Fusion Reacted Inside Metals by Intense Sono-implantation Effect," *Proceedings of the Japan Academy*, Vol. 78, Ser. B, No. 3 (2002), Y. Arata, Y-C. Zhang.

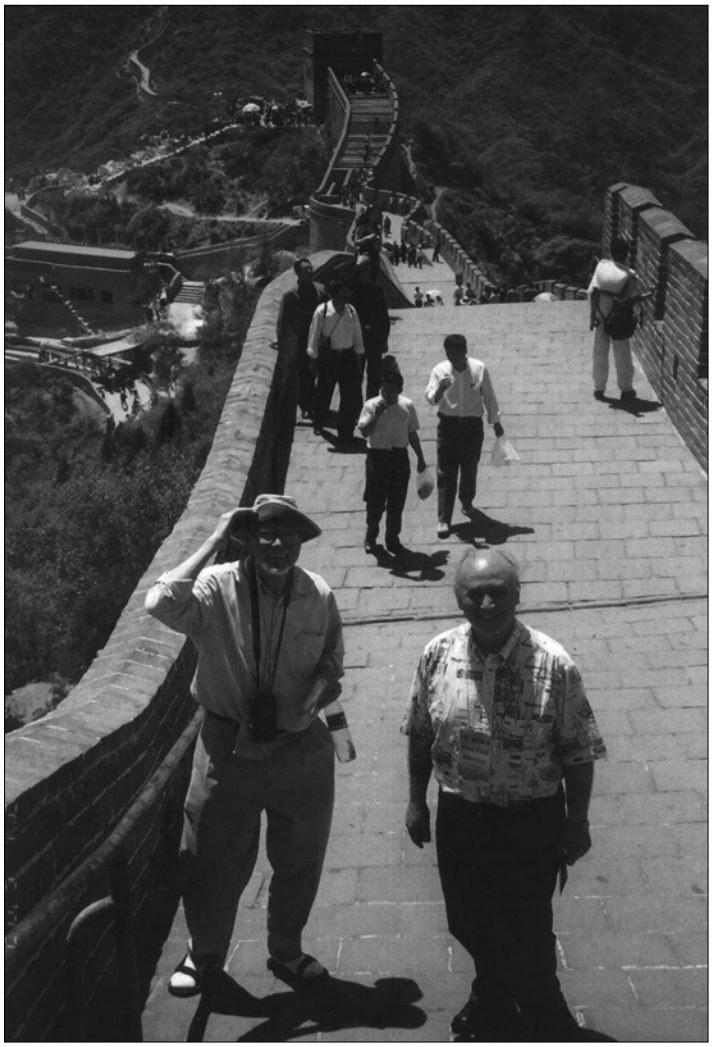
Partial Abstract: "Using intense ultrasonic cavitation effect, metals kept in heavy water were changed to nanometer-sized fine powder and simultaneously condensed a large amount of deuterium for 1 ~ 2 days. Mass analyzes of gases released from the revenant metal powders revealed existence of ^{3}He and ^{4}He. . .excess energy was recognized in only D_2O working liquid. . ."

The work employs foils of Ti, Pd, Ag, Ta, Pt, and Au from which nanometer-sized powders are created that are deuterium-loaded. It was disappointing that Drs. Arata and Zhang did not acknowledge Stringham's work, which certainly is related to theirs and preceded it. This is but one small indicator of the mind-boggling fragmentation that goes on in a field that is itself under attack from the outside. I have told my cold fusion colleagues for years: "We're in a life-raft together already. Nobody should be poking holes in the life-raft!"

One of the key concepts that has emerged prominently at both ICCF8 and ICCF9 is that of *flux* of hydrogen (deuterium or protium) into and through metals as a beneficial attribute for producing LENR reactions. The term *flux* is to be considered in contrast to the parameter of *loading ratio* (the ratio of hydrogen nuclei to the number of metal lattice nuclei), which was much discussed in past conferences as a necessary condition for excess heat production. This important theoretical concept had been put forth long ago by Dr. Mitchell R. Swartz, of Jet Energy Technology, Inc. of Massachusetts.[1,2] At ICCF9, Dr. Swartz's work on optimal operating point excess heat determination was highlighted when Prof. Hagelstein narrated a video tape that Swartz had prepared for the APS meeting this past spring.

The Wednesday in the middle of the conference was devoted to a sight-seeing and technology-related outing for the conferees. We were transported via two large buses through some horrible traffic jams in Beijing, and outward on free-flowing superhighways toward the Great Wall of China (at Badaling). For a few hours we all walked and climbed the awesome, ancient structure, which stretches some 7,000 kilometers over mountaintops and into valleys across China. It was a relaxing interlude.

The outing also featured a stop at the Beijing Ti-Gold Great Wall Corporation, whose primary business is using ion-implantation to coat decorative and architectural objects (metallic and non-metallic) with a luscious film of gold overlaying titanium. The company also sells ten models of ion-implanting machines and vacuum chambers. The company was one of ICCF9's spon-



Tom Passell and J.P. Biberian at the Great Wall.

sors. President and owner of the company, Prof. Wang Dian Ru, has taken the bold step of financing a collaboration between his company and Tsinghua University on a major cold fusion experiment. (To my knowledge, no such collaboration on a cold fusion experiment exists in the United States.) Students, graduate students, and one of Dr. X.Z. Li's post-doctoral associates work on the project, which bears some relation to the earlier-described Mitsubishi Heavy Industries experiment. A state-of-the-art IR camera that peers through glass ports provides a measure of metal temperature distribution caused by the deuterium that is made to penetrate palladium and other metal substrates in the vacuum chamber.

The concluding day of the conference was largely devoted to reviewing what ICCF9 had accomplished and "where to go from here." The perennial discussion arose concerning the two-humped distribution of numbers of cold fusion researchers plotted on a graph against their ages. The bulk of researchers are, indeed, getting on in years and by retirement, illness, or death will be disappearing—perhaps before the hoped-for victory party at the humorously posited ICCF15. So, how will the much smaller "hump" of younger researchers be able to carry out all the work that must be done? How to get more people involved in the field? Remarkably, not a peep was heard during this multi-hour review about developing commercially available *demonstration units*—or about cold fusion commercialization period! I held back my frustration and did not speak to that point (as I had at ICCF8), for fear of being impolite to the tired group of colleagues.

One way to gain greater acceptance may be to form a peer-reviewed journal for the field, because with Prof. George Miley leaving as editor, *Fusion Science* (formerly *Fusion Technology*) appears no longer to be allowing LENR papers among its hot fusion pages. (Such papers are said by the new editor to be "not of interest" to the readership.) Some bright news: Professor Peter Hagelstein of MIT told us that he has been discussing with a major science journal publication house the possible launch of a "Condensed Matter Nuclear Physics" journal.

Professor A. Takahashi of Osaka University discussed his experience in helping to found the Japan Cold Fusion Society. There was discussion of whether cold fusion communities in other countries should form similar national cold fusion societies, and should there be an "International Cold Fusion Society."

The latter question went unresolved. Prof. Hagelstein and Jed Rothwell are eager to have a more permanent web presence for cold fusion, in the form of permanently posted archival papers and notices. This will undoubtedly be launched before ICCF10. (Editor's Note: This site is now available at http://lenr-canr.org.)

Final Thoughts: China

The struggle and ferment in the cold fusion field was exposed at ICCF9 amid the backdrop of the great business turmoil and industrialization now going on in China. This heretofore sleeping giant is clearly in the process of waking up to new ways. This is evident in huge billboards everywhere touting both Chinese and Western industries in the computer, energy, and biotechnology fields. Though the People's Republic of China may be a communist nation, capitalism is rampant there. New enterprises are popping up everywhere on the streets of Beijing—especially cell phone businesses. Many shops were lined up on a single street, each selling cell phones! In one bookstore I visited, a large number of items were Chinese translations of popular business-culture "how to" books from the West. Large numbers of bicycles and tricycle cargo-carrying vehicles were everywhere, but Beijing has millions of cars and new buses too. The infrastructure is being inexorably built up. It was very charming to observe the *bamboo* construction scaffolding on high-rise buildings in the works. Some ICCF9 attendees likened the atmosphere of China in 2002 to Japan in the early 1970s.

Across from the very Westernized hotel in which the conference was held, an entire city block of single-level masonry buildings was leveled during the five-day conference! This to make way for some new construction in the bustling Tsinghua University area. The leveling was begun by dozens of workers with sledgehammers and pick-axes, followed by only a single backhoe type machine. Most of the debris was carefully recycled and taken away by dozens of tricycle vehicles as well as numerous horse and donkey-drawn flat-bed carriages. Even the old bricks were carefully packed in regular arrays for recycling. Ditto for little pieces of scrap metal and old, worn wire. It was simply amazing. Perhaps the cold fusion field will trigger a similar and long-overdue leveling—a demolition—of weak structures and obsolete paradigms in physics, even though most cold fusioneers seem quite oblivious to this prospect.

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The "Lifter" Phenomenon: Electrogravitics, Antigravity, and More

Eugene F. Mallove

Introduction

All over the world, a strange hobby is gathering force—the making of cheap, lightweight craft of aluminum foil, balsa wood, and thin wire, which "thrust" themselves silently upward in defiance of gravity. The model airplane-type structures, superficially resembling triangular kites, have no wings, propellers, or moving parts. Indoors or out, the craft leap upward against nylon tethers that *hold them back* from zooming ever higher!

The "power" source for such craft? Nothing more than a ground-based high-voltage power supply (30,000 volts and above)—often scavenged by tinkerers from old computer monitors. The internet-linked hobbyists compete to see how much *mass* (actually, *weight*) of payload (now in the several grams range) their "lifters" can carry. They proudly display their new designs and accomplishments in internet images and video clips; consult the several websites referenced to view the videos and the original material.[1]

T.T. Brown, 1905-1985

Even more amazing about this space-age hobby: *There is no clear, generally accepted physics explanation about how these craft work!* In electronics terms, they are *asymmetrical capacitors* (one side or electrode holding more charge than the other) onto which charge has been applied from a DC source. There is an unmistakable force generated that makes these craft experience movement in the direction toward the smaller-sized electrode. The "skeptics," of course, have the usual knee-jerk reaction that this odd phenomenon can be explained very simply. They contend that the craft are simply imparting motion to ionized air molecules and are thus relying on jet thrusting action of propelled mass, *i.e.* it's just "ion wind." But some (not all) experiments, and *all* calculations of the "ion wind" explanation that I have seen, find it quite insufficient (by orders of magnitude, depending on the assumptions) to explain the "antigravity" thrusting. Are these lifters then, in part, "reactionless thrusters," or are they manifesting, in part, genuine antigravity effects?

Know well that some of these lifters have been turned 90-degrees and used in pairs to thrust horizontally about a shaft, creating an interesting motor effect. (What are the energetics and ultimate efficiencies of such motors? These are interesting and important questions, to be sure.) It is not possible to say one way or the other how lifters work their magic—and that is part of the excitement and charm of this area. This is a field ripe for open-ended experimentation and development of products.

Bear in mind that the conditions and parameters of individual experiments are very important—firm conclusions about physics cannot be drawn from a single isolated experiment, no matter how carefully done. What is the voltage level, degree of asymmetry of the capacitor, orientation, steady DC power vs. pulsed DC power, etc.? It may well be, for example, that the truly interesting physics emerges only at higher voltage levels; there is evidence of that in the historical record of "electrogravitics"—Thomas Townsend Brown's work (see below)—from which this field emerged.[2]

Do It Yourself!

If you are challenged by the prospect of doing your own experiments, or perhaps you just want to participate in this new lifter hobby, by all means go to it. Tim Ventura, a computer systems man who fell into this vocation/avocation, has put up an excellent website for hobbyists and others who are just getting into the field: www.americanantigravity.com

The site has relevant links to technical material of all kinds. Tim Ventura even sells a computer CD with many megabytes of material on it. He was kind enough to let us reprint several of his carefully done "how to" construction segments. So even without web access, newbies can begin to make their own lifters right from this issue of *Infinite Energy*. We also reprint Ventura's gung-ho introductory essay (p. 16), which relates how an Alabama company, Transdimensional Technologies, which made the first of the modern lifter embodiments, led to launching his American Antigravity enterprise. Finally, I have posted some good references at the end of this Introduction. Particularly apt is one published in January 2002 by the staff of our collegial publication, *Electric Spacecraft Journal*.[3] That same journal reports the replication of one of the T.T. Brown experiments by Larry Davenport.[4]

Proceed with caution! This is a fun hobby, but high voltages are involved and we want you all to be safe and alive to see where this lifter phenomenon ultimately leads.

Do not undertake experimentation without fully understanding the hazards involved!

Electrogravitics: The Historical Background

The most important aspect of the lifter phenomenon is recapitulation of the work of American inventor and physicist Thomas Townsend Brown (1905-1985), who grew up in Zanesville, Ohio. From his teen years, Brown was captivated by the relationship of electrical phenomena to gravity. He worked on many inventions that seemed to bear on this connection, and out of which the term "electrogravitics" seems to have emerged. (One of his influential professors in Ohio, Dr. Paul Biefield, allegedly was a classmate of Albert Einstein—an interesting historical connection—and the origin of the term "Biefield-Brown effect.") Tom Valone[5,6] has compiled much useful information about the patents and work of T.T. Brown. He notes, "Unknown to many non-conventional propulsion experts, T. Townsend Brown's electrogravitics work after the

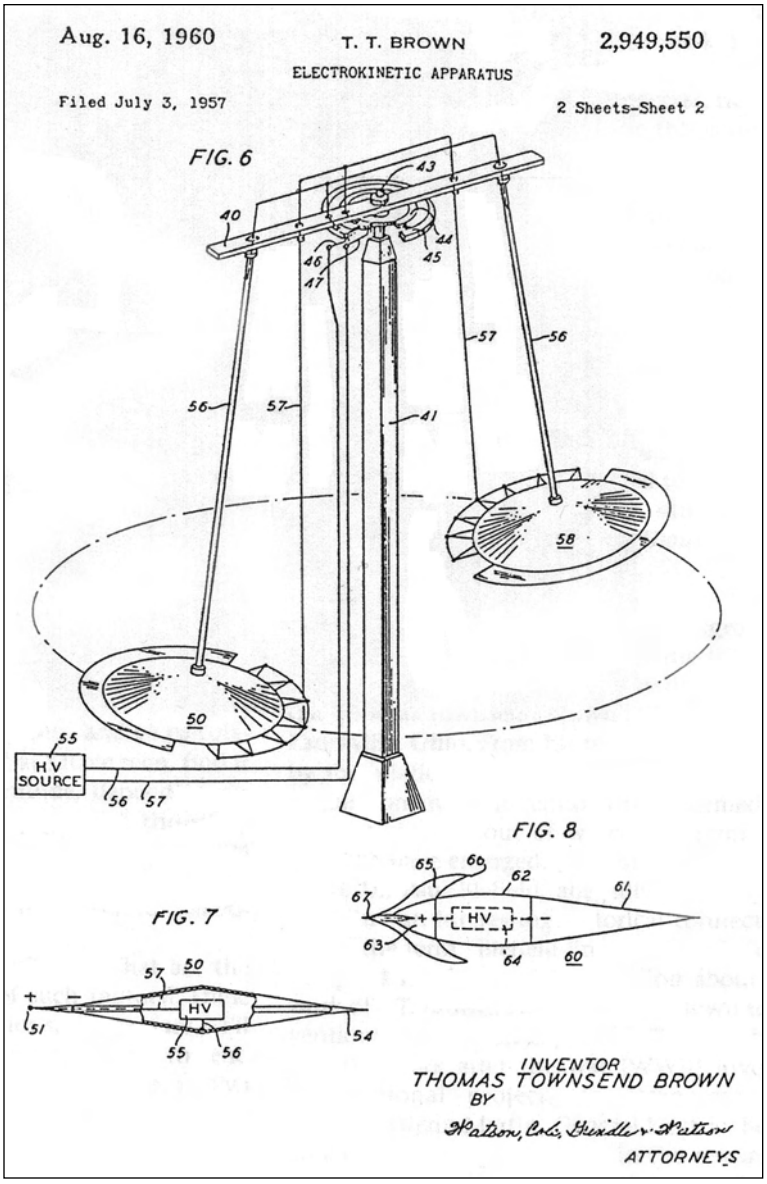
war [WWII] involved a classified multinational project. American companies such as Douglas, Glenn Martin, General Electric, Bell, Convair, Lear, and Sperry-Rand participated in the research effort. Britain, France, Sweden, Canada, and Germany also had concurrent projects from 1954 through 1956."

The NASA Patent—and Denials

Another remarkable development in this lifter saga occurred this year. A NASA employee was awarded a U.S. patent, #6,317,310 on the asymmetrical capacitor thruster and it is assigned to NASA! We have reproduced key introductory portions of this patent (see p. 30) which seems to be grand theft of the intellectual property of T.T. Brown, with no referencing thereof! The abstract glibly states: "The high voltage source applies a high voltage to the conductive elements of sufficient value to create a thrust force on the module inducing movement thereof." There is no discussion whatsoever in this patent of the infamous "ion wind" explanation of the thrust. Given that NASA is in the business of space travel in vacuo, it is implicit that the inventor and agency believes this technology is relevant to spaceflight—either that or it is grossly misleading people. Is NASA somehow trying to slip potentially *reactionless* thrusters (*i.e.* Newton's Third Law-violating technology!) or heretical "antigravity" technology into the public arena? Probably not. As it is often said, "Never attribute to malice what is more readily explained by stupidity." From an internal NASA memo (sent to Ventura in late May 2002):

> An article appeared in the May 11, 2002, issue of Wired.com/news, by Michelle Delio, about the controversial, and as-yet unresolved, "Lifter" effect; also known as "Asymmetrical Capacitors," "Electrogravitics," and the "Biefeld-Brown effect (circa 1955)." This effect claims anomalous thrust from high-voltage capacitors, and therefore, falls within the scope of Breakthrough Propulsion Physics (BPP). Marc Millis [of NASA] was quoted in the article. The version cited in the *Wired* article is from Tim Ventura, a UNIX programmer for AT&T Wireless. This topic is controversial because most of the recent work, work that was not coordinated with the BPP Project, has focused on promoting claims rather than on credibly resolving the unknowns, and has published these claims in inappropriate venues. Such activities have tainted the overall credibility of BPP research, by association. Fortunately, a new effort, involving a reprogrammed Congressional earmark, has been tasked to conduct an independent experimental test of these "Asymmetrical Capacitor" claims. This new effort, managed by MSFC's Gary Johnson, involves a MSFC-managed earmark to the West Virginia Institute for Software Research (ISR). This work is now being coordinated with BPP Project.

Yet other "PR" on this topic from NASA alleges that the ion wind explanation suffices to explain Biefield-Brown! Well, the physics establishment has had almost a century to deal with this question and has simply ignored it, but now NASA, with its patent already in hand, proposes to perform a funded study on it. . .toward what end?



Open-Ended Questions: Other Antigravity and Reactionless Thrusters

The basic questions prompted by the lifter phenomenon are these: 1) How do electrical phenomena relate to gravity? 2) Are there "reactionless" forces that can be generated by electromagnetic devices? The know-it-alls of the physics establishment have preferred to deal with the question of gravity's relationship to "electromagnetism" with its favored grand unified theories. "Reactionless forces" are simply too much for the poor physics community to deal with in virtually any forum—so it doesn't discuss them at all—that would be dangerous to its enterprise, like discussing cold fusion dispassionately, without malice.

Whenever actual devices come along, such as the gravity shielding experiment of Evgeny Podkletnov and others,[8] the Establishment develops paroxysms of denial. It is going absolutely berserk already when the "lifter" phenomenon is brought up. It knows subliminally that it can't explain it satisfactorily, so it thinks it can deal with it with a few jokes from spokesman Robert Park. But unlike cold fusion, the lifter phenomenon is already absolutely repeatable on demand. And, it looks like technological applications are possible. Sooner or

later, folks are going to find out that the Emperor—Fizzix—really has no clothes. For now, enjoy our coverage on this uplifting matter.

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1. A few websites for lifter information:

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http://www.americanantigravity.com

✦JLN Labs, France (J.L. Naudin)
http://jnaudin.free.fr/html/lifters.htm

✦Transdimensional Technologies
http://www.tdimension.com

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✦U.S. #1,974,483, Sept. 25, 1934, "Electrostatic Motor."

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