INFINITE ENERGY

The Magazine of New Energy Science and Technology

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THE SANDS OF TIME

What lies ahead for cold fusion science?

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Infinite Energy
The Magazine of New Energy Science and Technology

Science Progression: Much Time Remains for Cold Fusion/LENR Science to Make a Major Impact

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LETTERS TO THE EDITOR

Propagation Velocity of Gravity

In *IE* #76 (pp. 28-45) I presented experimental research in condensed matter physics that argued for modifications in mainstream concepts. In that paper, and other earlier works, I postulated that what science and technology have in the past attributed to "noise" and Poisson statistics (such as the variations in the measurement of any property, even a seemingly simple length of a laboratory bench) may be due fundamentally to the interactions of the neutrino flux, and associated transients therein, with the sample and the measuring system. All measurements are unarguably affected by transients in gravity, albeit these transients are small. In Section II of the *IE* #76 paper, I gave measurements on the motion of a projectile showing that that motion is *not* purely parabolic, nor a parabolic section of an ellipse, but approximates parabolic or elliptical motion straying or varying from that motion in a sinusoidal type manner such as in a Fourier representation. I indicated that I doubted that the observed variation (conducted by laser scattering) was due to turbulent air flow caused by the spheroid projectile (activated by a spring-gun).

A link to the hypothesis/postulate mentioned above and the associated data may be found in recent work on the weak force. A cornerstone of studying the decay of radioactive isotopes has been that that decay admits an *exponential* function, and *only* an exponential function—no exceptions. I have argued—from my work in gravity (together with theoretical physicist Dr. W. Stanley) and from my work in the weak force (coupled with that of S. Schnol')—that there could be a hithertofore unobserved differing decay superimposed on the well-established exponential decay behavior of radioactivity. The work described in *Nature* (Vol. 453, June 12, 2008) by Professor P.M. Walker of Surrey University in analyzing the original seminal extraordinary work of Litvinov *et al*. in a paper in *Physics Letters B* (664, pp. 162-168, 2008) shows a *sinusoidal* pattern of electron capture decays for two heavy radionuclides, Pr-140 and Pm-142, in which the peak, average, and trough levels are decreasing linearly with time. The period of the sinusoidal oscillation is ~7 sec. Litvinov and co-workers postulate that the periodic oscillations in the decay data are due to the oscillation of neutrinos between two different mass states—the electron neutrino and the muon neutrino. (The neutrinos are produced in the beta decays.) Professor Walker points out that a simple calculation shows that for values of the difference in neutrino masses in the middle of the presumed range, an oscillation period of the order of 10 sec is predicted. Litvinov's explanation is in consonance with some recent theoretical work/suggestions by H. Lipkin (preprint at http://arxiv.org/abs/0801.1465, 2008). If Litvinov and Lipkin are correct, the contribution to the fundamental understanding of nature and of the weak force, is spectacular, and the 7-sec periodic oscillation in the electron capture decay shown in Figure 1 of the *Nature* paper provides more evidence for neutrino oscillations, or what is called changes in flavor. This bears import on my own collision-induced gravity work, suggesting that in endeavoring to explain gravitational dips during total solar eclipses (such as the recent August 1 eclipse) using push-gravity modeling/theory/formalism, the change in flavor of the neutrino *must* be considered, and this is expected to be associated with a change in collision cross-section. Weight measurement data which I took in southeastern Vermont during the week preceding the August 1 eclipse, during the eclipse, and the week after the eclipse (three to four data points at consistent times every day) showed a significant dip in the measurements taken on August 1 at 7:00 a.m. EDT and at 11:00 a.m. EDT, even corrected for tides in Portsmouth (New Hampshire) harbor. At the time of those data points, the eclipse was total at a location on the China-Mongolia border, and New England USA was thus in the penumbra of the event. [The longest totality (140 sec) for the eclipse was at the location where it terminated in Sibirsk, Siberia, however, I was unsuccessful, even with the assistance of a nearby university-colleague who had been a graduate of the doctoral physics program at the University of Novosibirsk, to motivate a gravimeter study of the eclipse at the university or at a nearby technical institute. It seems here again that there is a resistance to making any measurements that could be contradictory to the work of General Relativity, however, I am hopeful that gravitational data which was planned to be taken during the eclipse in Romania will be forthcoming.]

G.C. Vezzoli
West Windsor, Vermont

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Fusion vs. Fission

The layman is led to believe that the process of nuclear fusion is very similar to that of fission, in that both forms of energy production are the result of a chain reaction—even though they are opposite forms of energy production (fission vs. fusion). Upon closer inspection, we find this is simply not true.

It is observed that with the passage of small increments of time, that in the case of fission an excess of neutrons is produced, each capable of inducing further fission reactions. With each fission, *two* neutrons are produced, each apparently capable of inducing further fission reactions.

On the other hand, with the fusion reaction of, say, deuterium, each reaction offers two distinct entities (products), neither of which demonstrates a propensity to induce further fusion action. This would imply, then, that with any ignition impulse of energy, the number of subsequent reactions quickly drops to zero (a "damping" effect). So, any viable fusion process would imply that the fuel be under a continual state of ignition, and that the fuel is "burned" rather than exploded.

I would like to briefly describe an experiment done many years ago. In this experiment, a sample of deuterium gas was exposed to X-rays of ever-ascending energies over a large range of values. At one juncture, threshold D-D reactions were produced in the gaseous deuterium, but not until the X-ray energy value reached 37,500 eV/photon.

I thought at the time that I could do very much better by running an electric discharge (at approximately 37,500 volts) through that very same sample of gas. It is my conjecture that at an approximately 37,500 volt level of energy, a special form of resonance exists which results in a D-D fusion. It is also my belief that at this voltage level, a narrow band of frequencies exists that is capable of, and responsible for, the fusion of deuterium (D-D). This band of frequencies is probably no wider than 3 to 4% of an octave.

It seems reasonable to assume also that this narrow absorption band could easily have been missed in the past by irradiating the deuterium with random values of various voltages, say, for example, in electron beam experiments.

For some considerable time now, I have wanted to test the foregoing hypothesis, but I lack the resources. Coincidentally, the marketplace column of your wonderful magazine has in the past offered a DC transformer that has a range of 0-60,000 VDC and a 2% bandwidth, ideal for the confirmation of the foregoing.

An extremely simple experiment could be designed utilizing a reaction chamber, a source of deuterium gas and a variable DC transformer capable of reaching values in the neighborhood of 35-40 kV. There is much more to be said, but that is beyond the intent and scope of this letter. I would welcome communication from researchers in the field, or possibly a young student looking for a project. I believe this experiment, if properly written up, could easily qualify for a grant from the DOE.

Donald A. Henry
Sacramento, California

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Debate and Discussion is Healthy for Science

In his letter to the editor in *IE* #82 Glen Perry assesses my theories of the fundamentals of nature and finds points on which he does not agree, albeit he says some nice things about my efforts which are appreciated. In my letter to the editor published in the same issue, I tried to understand his previously published diffraction theory of gravity because it was presented with what I regarded to be a confusing reliance upon my views. This required that I separate the two different contributions. So I described my difficulty in understanding his effort and concluded: "understanding nature is not simple, and it requires many active minds working independently. . .It follows that Perry's effort is welcome even if I have trouble understanding it."

Reviewing Perry's letter, his independence is apparent—and this is the way it should be. I write to illicit responses. My purpose is to learn. From that perspective it does not matter whether the response agrees or disagrees, as long as reasons pro and con are provided. My complaint is there have been too few responses. Perry recognizes the problem when he notes that my ideas are "certainly worthy of discussion" and when he later recognizes that he "has not seen that discussion."

I am pleased to study Perry's comments even though I shall not burden this journal with my evaluation. Readers can reach their own conclusions. But in some instances his disagreement is based upon a confused picture of my position. Occasionally I know of some available evidence which should be kept in mind. So a few clarifying comments are thought to be appropriate.

Perry suggests that particles in my theory are based on energy in motion which slows to cause gravity. This is correct. He refers to energy "rotating," but I believe the energy merely moves and turns as it moves. Energy in each of the paired vortex rings at the center of the particle moves in a helical path and this helical path traverses a circle to form a ring. As the energy moves helically in one direction to form a ring, the ring rotates in the opposite direction by reaction. This is as close as one comes to rotation in my theory, but it is a rotation of the ring, not the energy moving within that ring.

Perry describes the slowing process as "friction or energy emission." In my theory the rate of slowing is too tiny to measure, so ordinary friction is not involved. Of greater significance, the particle does not lose energy, so no energy is emitted by a particle at rest. The result of this minuscule slowing is the absorption of a tiny amount of energy by every particle and the cumulative effect of many particles doing the same thing provides a reduced density and pressure in the energy around any mass. This reduced energy pressure provides a pressure gradient to the higher pressure in remote space, and this gradient causes the acceleration toward low pressure which is gravity. I do not comprehend the concept of an "illusion of gravity."

Perry sees little evidence that "energy moving through space loses energy to its surroundings." That thought is the "tired light" concept which is the language used by physics to sneer at E. Hubble, who proposed it when the failure to find supporting observations compelled him to disagree with the Big Bang theory even though that theory was founded on his redshift observations.

I am convinced energy loss and recessional motion are both involved in the loss of energy as photons move through the endless reaches of space. There are various observations which suggest this, but the simplest is the fact that the largest redshifts (if construed as limited to recessional motion) demand recessional motion through space faster than the speed of light. Physics tries to justify this by suggesting space is expanding to force (and thus explain away) account for a portion of the recessional motion. Since that suggestion is inconsistent with orbital motion in the galaxy and in the solar system, these very large redshifts intrinsically suggest a portion of the observed redshift is the result of the loss of energy as the photons move through space. Atomic clocks provide convincing evidence of energy absorption to produce a low energy density around a gravitating object.

Perry notes that energy in motion to constitute a particle in my theory is ejected as it passes through the paired rings at its center and then moves through a curved path to return to the paired rings which ejected it. He asks "what causes it to curve in its travel." This is a valid question if limited to the energy initially released by the photon which formed the particle, and I cannot provide an answer. Curved motion appears to me to be an innate aspect of energy moving while it is free to turn. In a particle or photon the energy is confined within that entity and the entity moves in a straight line unless some force is used to change its path. It appears that the greater the concentration of freely moving energy the tighter the curvature. I try to delve as deeply as possible into fundamental problems, but the mechanism behind some of them, like this one, are beyond my grasp.

Through the years others have unsuccessfully attempted to comprehend nature by assuming the characteristics of the substance of energy are similar to the characteristics of known fluids, like air or water. But those fluids are constituted by mutually repellent particles. There is nothing to suggest that energy (as a substance) will possess these same characteristics because there is nothing to suggest it is constituted by mutually repellent particles (even if it is not a continuous medium as I envision it). I proceed on the basis that whatever the characteristics of energy are, we must accept them and try to comprehend the nature provided by something having those characteristics. At this point in the development of cause and effect thinking one cannot eliminate all "givens."

Perry describes my theory of electrostatic behavior in which the electron releases to the surrounding energy a laterally stretched modification of energy as something like a vacuum effect. The low pressure of a vacuum is not of special character, as is the modification of energy provided by the electron, and a vacuum acts the same way regardless of what it encounters.

Assuming two electrons encountered a low pressure region between them, the normal action of a low pressure region between two objects is to force them together. Instead, two electrons move away from one another. Perry's description of my concept of electrostatic behavior makes it incomprehensible. In my theory the motion of a charged particle encountering an electron's laterally stretched low density energy does not involve low pressure as such. Instead, the closer portion of the responding charged particle reacts more strongly than the remote portion of that charged particle. Accordingly, a neutral particle ignores an electron's emission while another charged particle moves because of the way its two portions react differently. As a result, an electron reacts to move away from another electron, and a proton reacts to move toward the electron. An ordinary vacuum can only function in one way regardless of the character of the particle upon which it acts, so Perry's concept of a vacuum is not relevant.

Perry asks many questions about the formation of particles by the decay of photons, but (as he acknowledges) his presentation on that and other matters is set forth in an environment filled with his own views. I am reluctant to criticize others attempting the difficult task of trying to comprehend the nature around us. I am convinced that Perry and I would both prefer to have as many good minds as possible enter this arena and participate. So I join him in suggesting to all, come on in with an open mind.

Arnold G. Gulko
Wheaton, Maryland

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Detection of Gravity Waves

I read with interest the article "Possibility for the Detection of Gravitational Waves by the Electrogravitic Property of a Dielectric Material" by Takaaki Musha (*IE* #82). Musha's experiment employs a piezoelectric material. In the article he stated that T.T Brown concluded that dielectric materials underwent changes influenced by the position and orientation of the Earth with respect to the Sun and Moon and the universe, resulting in alignment [see Dadd, R. 1997. "T.T. Brown's Rock Electricity," *Electric Spacecraft Journal*, 20, 21-25]. There is a place in Oregon where a similar alignment occurs, only the alignment occurs immediately and results in a visual pattern. One can visually witness alignment occurring.

Figure 1 shows the results of alignment after the sands have dried. Drying out causes the domains to tip over. This effect in these sands is immediate and visual due to the forming of a liquid slurry of fine quartz, rust particles suspended in salt water. Similar to Musha's experiment, quartz is piezoelectric. A slight agitated motion allows the particles to float in the slurry and self-align, leaving a visual pattern.

Figure 1. Results shown after sands have dried. The dark lines are paramagnetic particles that were aligned. Drying has caused the domains to fall over.

Figure 2. The dark areas are paramagnetic particles trying to stand on end (the domains are aligned vertically). Much like iron filings stand on end when a magnetic is held below. A 100 year old rusting shipwreck is shown in the background.

The event occurs in diurnal cycles, as well as moon 28 day and earth annual orbital cycles.

These particles are free to orient and align completely unrestricted due to the slurry which suspends them temporarily, until they drop into position—similar to a compass needle floating in water, only free to rotate in three dimensions. Floating in this lightly agitated solution, the particles are in pseudo-weightless suspension. Instead of the hundreds of years required for alignment in rocks, the alignment occurs immediately. Figure 2 shows the alignment immediately after it occurs when the sand is still wet. The particle's vertical standing on end (much like iron filings when placed on a magnet) never occurs from wind or wave motions. When taking into account the orbital relationships of the appearance of the phenomenon, something else must be in effect here. This is a huge area with literally tons of paramagnetic particles. Other unusual phenomena occur during these events as well.

I will send by mail free of charge the book and video of this phenomenon to anyone who is interested (email me at toddgrigsbybox@comcast.net).

Todd W. Grigsby
Portland, Oregon

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— In Memoriam —

It is with sadness that we note the passing of our colleague, astronomer Dr. Thomas Van Flandern, who succumbed to colon cancer on January 9, 2009. Tom published numerous articles in *Infinite Energy*.

Tom was born in Cleveland, Ohio in 1940. He received a Ph.D. in astronomy from Yale in 1969, specializing in celestial mechanics. His early work is well regarded within his field, but he was more broadly (and controversially) known for his later scientific contributions. Tom worked at the U.S. Naval Observatory for 21 years and became Chief of the Celestial Mechanics Branch of the Nautical Almanac Office. After retiring from civil service, Tom served as a Research Associate at the University of Maryland Physics Department and as a Global Positioning System (GPS) consultant to the Army Research Laboratory.

Tom is the author of *Dark Matter, Missing Planets and New Comets*, and co-author of other works. He founded Meta Research (www.metaresearch.org) in 1991 in response to the broad problem of getting research support for promising but unpopular alternative ideas in astronomy. Meta Research publishes a quarterly journal, *Meta Research Bulletin*.

Tom is survived by his wife Barbara and their four children.

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(Received January - February 2009)

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William Zebuhr, Chairman
New Energy Foundation Board of Directors

BREAKING THROUGH EDITORIAL

Some Thoughts About Cold Fusion, 20 Years Later: "Schussbooming," Falling into Life and Some Other History

Scott Chubb

It is difficult for me to separate my own human experiences from the ideas and science that have taken place since the announcement of Martin Fleischmann and Stanley Pons on March 23, 1989—in which they suggested they might have created a form of nuclear fusion, through ordinary chemistry. On the one hand, the claims were so audacious and so outrageous that after six weeks, most scientists who tried to repeat the experiments stopped working on the problem and "ran for cover." At the time, I was "fearless," in the sense that I had little responsibility in my life, although this was about to change dramatically. But in spite of the dramatic change, I persisted and stayed involved.

Shortly more than two weeks after March 23, my uncle Talbot Chubb and I literally rushed to get to the office of *Nature* Magazine, fearing we were about to be "scooped" about an idea that we were sure was right. At the time, Talbot said to me, "We certainly will know in six months if what Pons and Fleischmann are claiming makes sense." Adding to the confusion was evidence involving different, lower level effects by Steven Jones and his co-workers at Brigham Young University. The rest, as they say, is history.

There are a number of books about the history of the cold fusion controversy. Even here, confusion still exists. The important books are based on the words and actions of the important, competent people. These include: *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, by my dear friend and the founder of *Infinite Energy*, the late Dr. Eugene Mallove; the later books *Excess Heat: Why Cold Fusion Prevailed*, by Charles Beaudette, and *The Rebirth of Cold Fusion: Real Science, Real Hope, Real Energy*, by Steven B. Krivit and Nadine Winocur. These books provide a very accurate history of the relevant facts.

As opposed to providing a detailed description of the history that is chronicled in these books, I would like to make a number of personal observations, based on my own experiences. Some of these may seem rather personal to share, but I think they are important because they convey a message about perseverance, having a positive attitude and having hope about the future. Gene Mallove captured the essence of this message through his forthright courageous generosity, in basic terms, but also in his book and by taking the kind of firm stand that was necessary to create *Infinite Energy*. My words mirror this attitude.

Carrying on with a particular project, in spite of what appears to be a bleak situation, really can make a difference. Gene stood for this idea in basic terms and in his actions. A second person who also felt this way, whom I really wish had been a dear friend, was the late Guiliano Preparata. Guiliano had an indelible spirit and good will. His underlying ideas were physically sound and important. His colleague, Emilio Del Guidice, has carried on, in the finest sense of doing good science, by re-expressing Guiliano's ideas. Julian Schwinger forthrightly also worked in this field, based on this tradition, as has Brian Josephson more recently. Schwinger, Preparata, Del Guidice and Josephson are all theorists. Arthur Schawlow, an experimentalist and Nobel laureate, also spoke forthrightly about the reality of the associated effects. It is sad to say that these voices have not been heard by the wider scientific community. A reason that I think this has occurred is because of oversimplification and confusion. Hopefully with time, Preparata, Del Guidice, Schwinger and Schawlow will be remembered as they should be remembered with regard to the controversy: as idealists and visionary scientists who took firm stands in seeking the truth and expressing it. Their being so forthright should and must be remembered and acknowledged.

My good colleagues and friends, Peter Hagelstein and Yeong Kim, have also continued in this vein. They have forthrightly presented ideas with perseverance, based on sound physical principles. Others, and there are so many that it is impossible to name all of them, have also been persistent. Mitchell Swartz, David Nagel, Akito Takahashi, K.P. Sinha, Talbot Chubb, Hideo Kozima, John Fisher, Xing Zhong Li, Fanzil Gareev, Yuri Bazhutov and Robert Bass have all persevered. From an experimental perspective, of course, Martin Fleischmann and Stanley Pons, and my colleagues Stanislaw Szpak, Pamela Mosier-Boss, Frank Gordon, Lawrence Forsley, Melvin Miles and Ashraf Imam also deserve significant credit for what they have done, as do Michael McKubre, Irving Dardik, John Bockris, Edmund Storms, Richard Oriani, Yoshiaki Arata and Yuechang Zhang. These are just a few names that should be cited. Steven Jones also belongs in this list, simply because of his heroic stance associated with dealing with explaining a very different effect that potentially might be relevant. Others who have contributed significant experimental results include Jean-

Paul Biberian, Francesco Scaramuzzi, George Miley, Jacques Dufour, John Dash, Francis Tanzella, Alexander Karabut, Irina Savvatimova, Andrei Lipson, Alexander Roussetski, Vittorio Violante, Francesco Celani, Antonella De Ninno, Thomas Passell, D. Russ George, Thomas Benson, Thomas Claytor, Mahadeva Srinivasan, Jirohita Kasagi, Yasuhiro Iwamura, Tadahiko Mizuno, Roger Stringham, Dennis Letts, and Dennis Cravens. The names I am including are the ones that immediately come to my mind. Certainly other people have been involved who have made significant (potentially even greater) contributions to the field.

In thinking about the tortuous and difficult task of reviewing all of the remarkable events that have taken place since the day of the initial announcement on March 23, 1989 (an overview which, I feel, has been adequately done by others already), it occurred to me instead to provide a personal account from those early days which typifies the excitement and adventure of those times.

A remarkable event was taking place for me right at the time of the initial announcement. My wife, Anne Pond, was pregnant, with our first child, Scott Chubb, Jr. I was so excited about the Fleischmann-Pons effect that when visiting the obstetrician's office two days before my son's birth (my wife was a week overdue), I could not take my eyes away from a picture on the cover of *Time*, which showed Pons and Fleischmann and the caption "Fusion or Illusion?". Then two days later, within minutes of the time my son was born, I was actually telling one of the orderlies about the remarkable Pons-Fleischmann discovery. I am sure that my wife was not thrilled with my distraction, but I hope that my family can appreciate that my excitement and awe over science also translates into the personal realm. It was easy to be distracted with such a remarkable science breakthrough in the forefront of the news, and the Pons-Fleischmann story adds an important historical backdrop to the birth of my son. They are actually both very important (one personal, one professional) times from my life.

In thinking about the road that those of us in the cold fusion field have followed, I often think about my father and the important lessons that he taught me about life (and science).

One story I would like to relate about my relationship with my dad becomes a sort of metaphor for taking chances and facing failure (and learning how to move on from it). When I was five, my father put me on skis during a family outing in southwestern New Hampshire, at the Mt. Sunapee ski area. Predictably, my first experience was a disaster—in terms of my ability to ski and my general attitude about the experience. But, my father constantly encouraged me to get up after I fell, and I learned an important lesson from that. Two years later, my family went on a second skiing vacation, this time to a smaller, less formal place called, "Dutch Hill." Because "Dutch Hill" was smaller and had a "friendlier" crowd, I began to enjoy skiing in a new way. As we started to walk out to the slope, I saw a teenager speeding down a trail called "Dutchman's Holiday." He was literally "falling," in a sense. His skis were straight together as he "schussboomed" down the hill. To "schuss" a ski slope means that you go straight down the slope. A skier is not afraid of falling even though as he goes down the slope, he goes faster and faster. As I saw this person "Schussbooming Dutchman's Holiday," I felt a sense of beauty and awe. The fun that he felt was all at once infectious and inspiring.

There are interesting parallels between the excitement that "Schussbooming" creates and the excitement that occurs in cold fusion. Skis barely have contact with the snow, but their effect is real, and the cold fusion effect is almost imperceptible but quite real. Like the quiet sound of skis going faster and faster in the snow, the weak signals that came from the initial cold fusion experiments are now becoming more and more repeatable and understandable. Their intensity is also increasing.

The sense of "risk" in performing experiments that superficially appear to be so impossibly simple and so outrageous, in the context of thermonuclear fusion, inspires a sense of beauty and awe that is similar to the beauty and awe that "Schussbooming" provokes in me. The people who have been involved in cold fusion are not afraid, in a figurative sense, of "falling down" (as when a skier falls down) in failing to meet their expectations in their attempts to accomplish something that for many scientists appears to be impossible.

My father inspired me to do things that are fun, based on finding appropriate "trails" (in a figurative sense) that are not too difficult to follow but are also challenging. The idea of being able to fall and not being worried about failing is an important theme that my Dad passed on to the people who knew him. The idea of finding the right "trail" and "falling down" it, and having fun in the process is an important lesson that should be remembered that has resulted from the cold fusion controversy.

Sixteen years after my "Dutchman's Holiday" skiing experience, I spent a few weeks in Aspen, Colorado as a "ski bum" (working for a few hours to earn enough to cover a lift ticket). While there, I was able to reflect and I took time to simply think about a particular problem for many hours. I realized how to generalize a very complicated, esoteric mathematical problem; this resulted in my becoming more intimately involved with my thesis advisor, Professor David Fox. Fox was an extraordinarily idealistic physicist. The problem he introduced me to—associated with the possibility of what is referred to as a Bose Einstein Condensation, involving esoteric forms of "particles," called "excitons"—was so far-removed from "practical" areas of research that what I accomplished in this project had no obvious bearing on my "getting a job" (and, for that matter, for many years, on anything related to my professional career). Even more bizarre is that, although what I accomplished in this work was really quite extraordinary (mathematically), even the experts in the field (the two-dimensional Onsager-Ising Lattice) were not interested in what I had done.

As a consequence, with negligible support for the work, Fox advised me to find a different advisor. By chance, I contacted Martin Blume, who was an adjunct professor at Stony Brook. Blume was involved with the development of the National Synchrotron Light Source at Brookhaven National Laboratory, and he told me that he would not be an appropriate advisor. But he recommended I contact one of his colleagues, Victor Emery, who also was at Brookhaven. Emery suggested that I talk to a relatively new theorist, Brookhaven's James Davenport. Davenport suggested a problem that I realize, with hindsight, was very different from the kinds of mainstream problems that most graduate students were involved with—studying the behavior of electrons on the surfaces of palladium hydride. I share this part of my journey because these somewhat "obscure," off-the-beaten path problems, involving seemingly unrelated areas of research, are responsible for my becoming involved with cold fusion.

An important lesson that I learned from my time studying is the unexpected possibility that something obscure might in the end be important. An important point is that great science involves intensity and persistence and the belief that "failure" is only in the eye of the beholder. As I look back at what happened, I realize my father's guidance about having fun doing science and in life in general has had a lasting effect that has helped me in the cold fusion field.

My father recommended that I apply for a job at the Naval Research Laboratory (NRL). Eventually, I did this and I became an NRL employee, but only after I was employed first as National Research Council Research Associate and then as an outside contractor.

In the fall of 1988, when I was an outside contractor, things changed dramatically when I started to collaborate with my uncle, Talbot Chubb. Dr. Michael Melich suggested the collaboration, based on work on developing re-usable rockets. Beginning shortly after March 23, 1989, I started to talk to Talbot about the possibility that cold fusion might actually not be a colder version of conventional fusion but that it could involve something very different. I suggested the bizarre idea that it might be possible to explain what Fleischmann and Pons had discovered by combining the two "obscure" dissertation problems that I had been exposed to as a graduate student. Specifically, the kind of Bose Einstein Condensate (BEC) associated with particles (similar to excitons) that are bosons that either are created or are fundamentally associated with interactions involving a periodically ordered lattice is very different from the kind of BEC that occurs in free space. The situation involving the lattice does not require that the BEC form at low temperatures. This observation suggested that a very different "problem," involving deuterons (which are also bosons, on length scales that are far from a potential collision) in a periodic lattice, could be relevant.

The idea that inspired our theory was that in particular situations (involving high-loading in palladium deuteride) deuterons might behave just like "excitons" (like the initial problem Fox suggested to me), which means they can behave like electrons in an ordered lattice, in a certain sense by being wave-like, but (as a result of the second problem I had studied) this basic picture actually made great sense. Then, as opposed to being a colder version of conventional fusion, the deuterons could occupy (or, as I later suggested, interact through) "energy band states" involving ions. Although the language that I am suggesting here seems esoteric, this is actually very far from the truth. What I am (and have been) suggesting (for 20 years) is that it actually is quite reasonable that in fully-loaded palladium deuteride, what is known about how deuterium (hydrogen) behaves in a solid can involve effects that are well-known that are extremely different from the kinds of effects that occur in the absence of the solid. An important point, however, is not merely the fact that a solid is involved, it is the fact that a fully-loaded palladium-deuteride solid is involved. The key point is the assumption that in palladium-deuteride, important details associated with the structure and the behavior of the electrons can play a fundamental role in initiating the associated effects that were observed by Fleischmann and Pons.

I knew from my work involving palladium hydride that this kind of picture can make sense. The point is that when hydrogen is loaded, at low concentrations into palladium, it is known that each hydrogen nucleus (a proton) can behave as if it can become wave-like (and can occupy a kind of energy band state, that we have called an "ion band state," that is similar to the kind of wave-like energy band state that an electron can occupy). I had learned this through the second problem associated with my dissertation at Brookhaven, which Jim Davenport had suggested (although in an indirect manner). What was novel, for both Talbot and me, was the idea that deuterium nuclei (deuterons) could also behave this way. The reason that this idea was new and interesting is that since a deuteron has a proton and a neutron, it is a boson (on the length and time scales associated with the ion band states that we suggested could be important), while a hydrogen nucleus (a proton) is a fermion. The novel and interesting aspect of this difference is that in the seemingly esoteric problem involving "excitons," new and potentially "exotic" forms of interaction can occur, through the formation of the "higher temperature" kind of BEC that can take place associated with excitons.

The very idea that such an outrageous possibility could be relevant stirred the imagination. Refinements based on this seemingly different, seemingly "obscure" idea formed the basis of a somewhat remarkable set of conjectures that appear to have been confirmed: 1) "High-loading," associated with a "well-defined limit" (which does not have to be exact) can initiate the conditions that are necessary for the deuterons to become wave-like; 2) Because deuterons involve proton-neutron pairs, at distances far from the location of possible forms of nuclear overlap, the inherent statistics that are associated with Bose Einstein Condensates are necessary, which means that the associated forms of interaction inherently are different than in conventional fusion, most of the time. In particular, there does exist one reaction (deuteron (d)+d→helium-4+gamma ray) where these kinds of effects are important.

Furthermore, for our initial hypothesis to make sense, any helium-4 that could be created (which is the product that would be expected) would have to be created in such a way that the conditions that could be responsible for it being created would not be disrupted. Three predictions immediately resulted from this: 1) High Loading (values of $x \rightarrow 1$ in PdD_x) appeared to be necessary in order for the situation to involve a periodically symmetric environment that would be consistent with the associated wave-like picture; 2) The reaction would have to preserve the kind of Bose exchange symmetry (associated with the deuteron (d)+d→helium-4+gamma ray reaction) that is necessary far from the location of the reaction; and 3) In order for the helium-4 to be wave-like, the gamma ray could (and should) be repressed, but this would be possible only if the helium-4 was "effectively" created, in a wave-like fashion, at locations outside the crystal, associated with PdD_x.

Considerable criticism initially occurred about what we suggested. But because information from a second laboratory associated with the Navy suggested that we might be correct, work began. Because of the need for inter-laboratory cooperation within the Navy, even involving such a potentially controversial area of research, the research continued for many years. And evidence has been accumulating that what we initially suggested might be right. Refinements have occurred since we made our initial conjectures. Regardless of whether or not the initial assumptions are right, the conjectures have inspired new ideas. This is how science progresses.

What happened seems to be the result of a remarkable collaboration that was only possible at a place like NRL, which has an environment of scientists who are open-minded and where much of the work requires scientific expertise in many different fields. I like to think that what happened in my work with Talbot reflects the important lesson my Dad taught me: the process of "falling" (or making mistakes) should be fun. When we have trust in ourselves, regardless of how many times we are "wrong," we must do what a skier does when he falls—we "get up" and we try again to be right.

The present issue of *Infinite Energy* includes three articles that have been written by scientists who have been directly involved with cold fusion efforts at NRL. These include: "Recent Progress in Condensed Matter Nuclear Science," by Talbot Chubb; "Questions and Answers About Lattice-Enabled Nuclear Reactions," by David Nagel, and "Strategies and Agenda for ICCF14," by Michael Melich and David Nagel. The first article, by Talbot Chubb, includes an interesting summary of his perspective of recent progress in Condensed Matter Nuclear Science (CMNS), which emphasizes some of the key excess heat results developed using nanometer scale size materials. It is interesting to note that Chubb was motivated to write this article after he prepared a report that covers the same material for one of the more prominent scientists from NRL, Peter Wilhelm (who is the head of the Naval Center for Space Technology). David Nagel's article summarizes some of the important questions associated with the "cold fusion"/condensed matter nuclear science field—using a new term (Lattice-Enabled Nuclear Reactions) that has the same acronym (LENR) as the name (Low Energy Nuclear Reactions) that has frequently been used to describe CMNS phenomena. The Melich/Nagel article discusses issues associated with organizing international conferences in our field.

Other articles in this issue pertaining to the "cold fusion" field are: "Dual Laser Stimulation of Excess Heat in a Fleischmann-Pons Experiment," by Dennis Letts and Peter Hagelstein; "Paradigm of Cold Fusion: A Perspective on Scientific Philosophy," by Wu-Shou Zhang, and "Cold Fusion Collaborations: Further Selections from the Cold Fusion Oral History Project," by Marianne Macy. Letts and Hagelstein present important results involving the observation of a triggering effect involving dual lasers that are tuned in such a way that particular optical phonon modes (involving 8, 15, and 20 terahertz frequencies) are excited. The associated effect may be consistent with the theoretical model that Hagelstein has suggested. Zhang presents an interesting perspective about "technical differences between cold fusion and hot fusion, and scientific distinctions between LENR and classical nuclear reactions." Macy provides material from some of the oral histories that she has conducted as part of her ongoing effort (through funding provided by the New Energy Foundation and the University of Utah) to document the work of key scientists in the LENR/CMNS field. *Infinite Energy* published a first segment of interview excerpts in #80; this new piece highlights the collaborative work of Dennis Letts, Dennis Cravens and Peter Hagelstein.

Questions and Answers About Lattice-Enabled Nuclear Reactions

David J. Nagel*

Introduction

Asking questions is basic to many human functions. Without questions, the learning process in schools and universities would be vastly more difficult and less effective. FAQs (Frequently Asked Questions) are a standard part of many websites now. The posing of questions is also an activity fundamental to diverse planning activities, ranging from the formulation of programs to the design of cities. And, questions, commonly driven by "mere" curiosity, are the driving force behind science. So, one can ask: what questions are applicable to the field of low energy, or alternatively, lattice-enabled nuclear reactions (LENR)? That is one of the motivations behind this compilation of some questions, which are asked because they seem significant. The answers are largely the opinions of this author.

There are many ways to organize a set of questions about LENR. One way is to use the sequence shown in Figure 1. It shows the steps in the progression of a knowledge-producing scientific field through the development of technical capabilities to engineering design, fabrication and testing. The results of engineering commonly lead to prototypes, products and profits. If the business phase is large and long enough, there can be significant higher-level economic, social and even political impacts, all based on the original research activities and results. These steps can form a temporal sequence. However, it is not uncommon for other scenarios to unfold, for example, a leap from research directly to prototypes. Many start-up companies follow this path to get to market as fast as possible. Some products can have a societal impact even without producing a major economic effect. Worry over the possible deleterious health effects of nano-materials is a current example, where widespread public concerns have gone beyond the business situation.

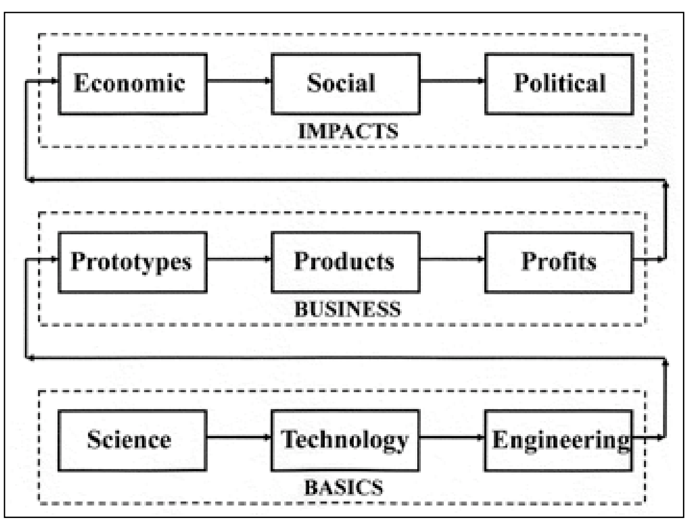

Figure 1. The progression that a concept can take through three levels.

Another way to organize queries about the many aspects of LENR is by the nature of what is being asked, for example, a scientific question or a historical question. Using a chronological order is also sensible. Given that the twentieth anniversary of the Fleischmann-Pons announcement is at hand, it seems worthwhile to ask questions along the lines of where the field has been, where it is now, and how it might develop in the future. That is the approach adopted here, although some of the individual questions will be recognized as scientific and others as business related. We will pose diverse questions, and then offer answers to them. Both what is asked and answered are burdened by the author's limited experience and his perspective from one country, even though the study of LENR is an international activity. It is recognized that questions and answers from others in the field would be different, especially from scientists in other countries. However, it is hoped that this collection of questions and answers will be interesting and even useful. One of the reasons for writing this paper was to attempt to improve knowledge and stimulate actions regarding LENR within the U.S. government. There is some redundancy, so that most questions and their answers can be used separately.

Looking Back

The history of science has cases where the time between experimental demonstration and understanding has varied widely. In some instances, the explanation of a very new phenomenon is given in the first paper with the experimental results on the discovery. The Mossbauer Effect is an example. In the case of superconductivity, over four decades elapsed between the discovery by Kamerlingh Onnes in 1911 and the explanation by Bardeen, Cooper and Schrieffer in 1957. Sometimes, ideas precede evidence. There was another four decade delay between the notion of plate tectonics put forward by Wegener in 1912 and the accumulation of verifying evidence from earthquakes and sea floor spreading in the 1950s. Einstein published the theoretical equations for stimulated emission in 1917, but the maser was not demonstrated experimentally by Townes until 1954. So, the temporal gaps between the experimental demonstration of new effects and their understanding, or the inverse, can span decades, essentially a professional lifetime.

When were LENR discovered?

The year of the initial strong experimental establishment of LENR is contentious. There are claims of the discovery before 1989 of nuclear reactions in lattices at ordinary temperatures. Some people will not accept the initial 1989 and other early reports by Fleischmann and Pons as being adequate evidence. However, others are satisfied with their data. Hence, the production of heat from LENR is sometimes called the Fleischmann-Pons effect (FPE). Whatever one's opinion of the earliest experimental evidence for LENR, there is now a very significant accumulation of evidence that it is possible to initiate nuclear reactions that provide MeV of energy with chemical-scale eV energies. That was held to be possible with only negligible probability, according to the theories available in 1989. Estimations of the probability of LENR being a real phenomenon as a function of time, that is, as a function of accumulating data, have been made. The computed probabilities have steadily increased to almost unity. Now, there are over 100 papers giving experimental evidence of the production of excess heat, that is, heat well beyond what can be explained as chemistry, which must be due to nuclear reactions. In addition, very significant evidence for LENR other than heat has been published.

When were the mechanism(s) of LENR understood?

The question of when LENR was or will come to be understood is even more open than determination of the date at which they became to be known as possible. Many theories, about two dozen distinct concepts, have been advanced during the past two decades to explain the data that must be due to nuclear reactions. At present, none of the theories is widely accepted by those in the field. Some theories have been reduced to equations, which have been the basis for computations. However, there is precious little direct contact between such theoretical and computational results and the many available good measurements. Years, and maybe decades in the future, it might be known if any of the extant theories are correct and possibly useful.

Why were LENR thought to be potentially important?

In 1989, nuclear power was being produced at high levels within fission reactors in many places around the world. For example, at that time about half of the electrical power in France came from nuclear reactors, a fraction that is now near 80%. Fission reactors cost on the order of USD $1B. Large fusion experiments, also costing most of $1B, were in operation in the laboratories of a few countries. After 40 years of research at that time, hot fusion power was (as it still is) nowhere near being useful. LENR were immediately recognized as potentially important nuclear power sources, that is, as much smaller alternatives to large and very expensive central power plants, and the costly distribution of electricity over a power grid. About 10% of electrical power put into the U.S. grid is lost as heat, the equivalent of the output of 40 large power plants. Hence, smaller distributed power sources are attractive. As a measure of the early interest, Figure 2 shows the covers of three major news magazines in the U.S. from the same date, namely May 8, 1989. That was only 46 days after the Fleischmann and Pons announcement at a news conference. Such prompt and widespread attention to a new scientific discovery is rare. It has happened a few times, for example, after the announcement of the discovery of X-rays in 1895. One could buy lead-lined underwear less than a year later! In the case of LENR, the announcement was so astounding that both widespread attention and intense scientific controversy were inevitable. And, mistakes by many people complicated the situation.

What early mistakes were made by those in and outside of the field?

Study of the research performed by Fleischmann and Pons, and attempts to replicate it, got off to a terrible start. It was almost as if the field had birth defects. Reporting their results at a press conference was the first mistake. The radiation data reported by Fleischmann and Pons was not defensible. Some technical errors were made in other premature announcements, only to be retracted. Tensions developed between parts of the physics and chemistry communities. Some scientists sought to protect their funding by criticizing LENR reports. Clamorous media attention in the early months of the field did not help. Many scientists dismissed the reported results on cold fusion because they were dramatically at odds with hot fusion theory. In fact, the use of the term "cold fusion" did not help the field. The new experiments were deemed to be simple, which is true if one compared them to large plasma fusion devices, notably Tokamaks. However, they are inescapably interdisciplinary, and often require a team of specialists. The 1989 review of the field by the Energy Research Advisory Board was done quite quickly, before it was adequately recognized that the experiments in the field are actually very challenging. It was also concluded at a time when many researchers were not disclosing their results in order to protect their intellectual property. The rush to judgment by some researchers and major laboratories discredited the field and those who worked in it before the end of 1989. The scientific community, and the public with them, hastily moved down the wrong branch of the diagram of possibilities shown in Figure 3. This was a major mistake that has haunted the field and hampered its development. Governments and investors ordinarily provide money to scientists, who return information and get more funding. For LENR, that cycle has been and remains dysfunctional.

Figure 2. Some prominent news coverage of the field in 1989.

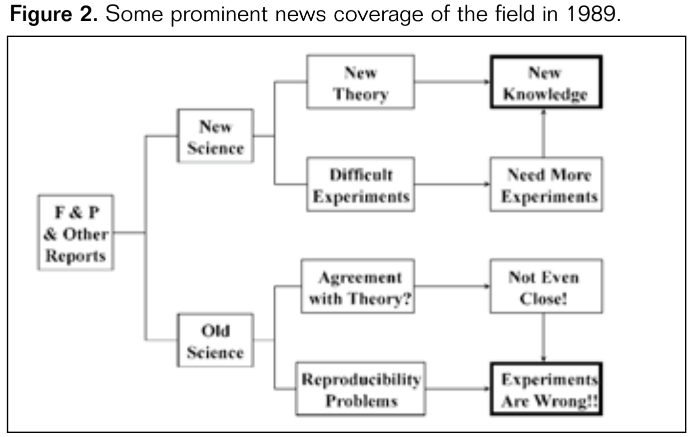

Figure 3. Possible outcomes for the announcement of "cold fusion."

What has been done since the field started?

Many activities have broadened the field from its original electrochemical and heat measurement experiments. This is true for both the way in which hydrogen isotopes (protium and deuterium) were loaded into solid lattices and the types of measurements that have been used to detect anomalous effects due to such loading. The four classes of loading and the four types of measurements are shown in Figure 4. There were new approaches to both loading and measurements within each class shown in that figure. Many variants of electrochemical loading from liquids were demonstrated. Light water, as well as heavy water, electrolytes have been used. A wide variety of materials has been employed in electrochemical and other experiments, for example, nickel and a variety of alloys. Molten salt electrolytes, and co-deposited palladium and deuterium, were other variations on electrochemical techniques. Gas, plasma and beam loading were employed. Diverse types of calorimeters have been used for excess heat measurements. And, a variety of detectors for energetic particles have been used, notably passive track detectors made of the plastic CR-39. In general terms, the variety and precision of measurement tools has improved greatly in the past two decades. In addition, major efforts have been devoted to theories for the FPE. Many essentially independent theoretical ideas to explain various observations in the field have been advanced. Equations have been written out for most of these theories. The number of theories for which the equations have been used to produce numerical data for comparison with past experiments, or prediction of the outcomes of future experiments, is very small. Much of the experimental and theoretical work on LENR since 1989 has respected its intrinsically interdisciplinary character.

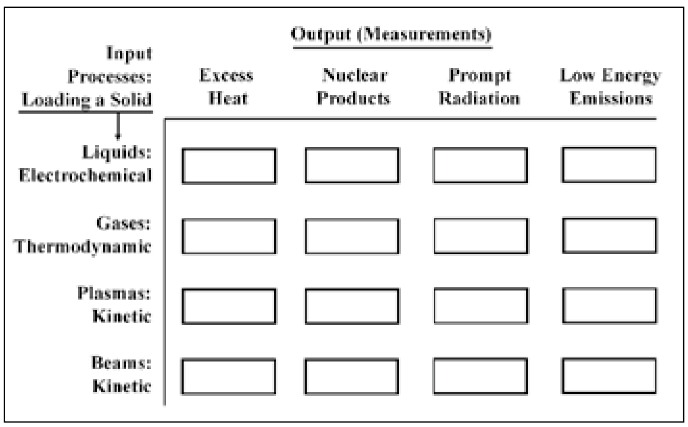

Figure 4. On the left are the four classes for loading protons or deuterons into a solid lattice. They involve (1) use of a liquid containing normal or heavy water in electrochemical experiments, (2) use of hydrogen or deuterium gas in the presence of the material to be loaded, usually at elevated pressures and temperatures, (3) immersing the materials to be loaded into a plasma of hydrogen or deuterium ions, and (4) kinetic loading using energetic beams of protons or deuterons. The four types of measurements are arrayed on the top. They include (1) excess power and energy measurements, (2) assays for the results of nuclear reactions by comparison of the composition of materials before and after experiments, (3) measurements of the relatively few prompt energetic photons, neurons or ions, and (4) measurements of low energy processes, notably sound or infrared emissions.

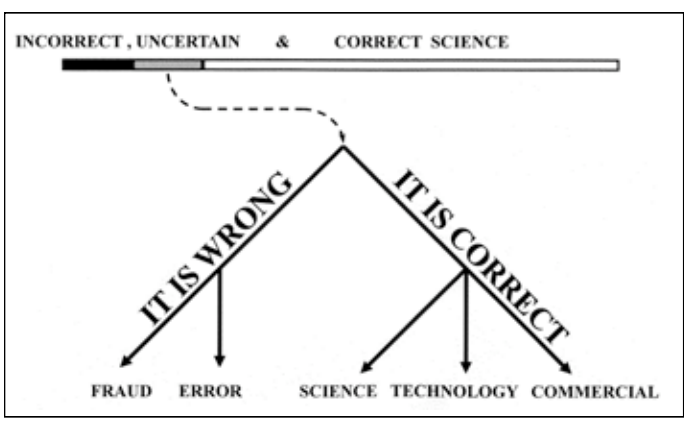

Figure 5. Initially possible outcomes of the discovery of the FPE. It is now known that the reported effect is not due to lies or mistakes. Hence, it is no longer uncertain whether or not the LENR is a legitimate field of scientific inquiry.

What has been found since the field started?

It is now clear from experimental data that it is possible to trigger nuclear reactions, which yield MeV-scale energies, with low energies, on the scale of eV, essentially chemical energies. The "low" in LENR refers to the input or initiation energies, and not to the output energies. Of course, the ratio of one million is not the energy gain, since many eV events can be needed to lead to one MeV event. Observed power gains until now are generally less (usually much less) than a factor of ten. The early accusations that all reports of excess heat were wrong, the result of either lies or errors, have been refuted successfully by data from many experiments. Figure 5 shows the initially possible outcomes. The branch "It is Wrong" was cut off several years ago. There are now over 100 published reports of the production of excess energy. There are many papers that report the appearance of new elements in experiments, but the data on such "transmutations" is not as robust as that for heat production. Data on the appearance of small numbers of energetic particles is also enticing. There are a few reports on low energy phenomena from FPE experiments.

Reproducibility of experiments, even within one laboratory and especially between laboratories, has been a chronic problem in the field. It has improved significantly in the two decades for research, but is still inadequate. There has also been major progress on the control of FPE experiments, but much improvement is needed here also. Some systematic trends for successful experiments have been discovered. There is a significant body of empirical evidence that the production of excess heat requires heavy loading of hydrogen isotopes into solids, and a flux of hydrogen isotopes through the surface of the lattice involved in the experiment. There are correlations between the production of heat and the generation of helium. Significant evidence points to LENR occurring on the surface of solid materials. Similarly, some evidence indicates that the nuclear reactions occur in very small regions, maybe with dimensions on the nano-meter scale. Some researchers think that the lack of understanding from experiments might be due to the fact that modern nano-science research tools, such as the Atomic Force Microscope, are only now being brought to bear for the study of LENR.

Current Status

Why is the field even more potentially important now?

Both the short term and long perspectives on the need for clean energy are problematic. The 1973 OPEC-caused crisis led to long lines at the filling stations that had gasoline in the U.S. In 2008, the dramatic run up in oil prices was another reminder of the vulnerability of the global economy to the costs of energy. In the U.S., recent government support of bio-fuels has had unintended negative effects on the price of some cereal grains, which has also affected other countries. With the relentlessly growing world population, the need for new and improved energy sources will not abate. If LENR can be reproduced, controlled, optimized and commercialized into distributed and clean sources of heat without greenhouse gas emissions, and possibly electricity, the effect might be in the same class with the impact of cell phones on global communications.

Why is the field still controversial?

There are clear reasons why many people are unable or unwilling to accept LENR as a legitimate field of science with significant promise for applications. The most fundamental is simple ignorance. Most people still view the field as it was portrayed in the media in late 1989 and the early 1990s, an example of science run amok. They have not looked at the voluminous data, which are now available and show that it is possible to initiate nuclear reactions with chemical energies. In general, they have not even read up-to-date reports on the field, for example, the summaries that are always written after one of the International Conferences on Cold Fusion. There have been 14 of these meetings on a three-continent rotation, North America, Europe and Asia. The last one in Washington, D.C. during 2008 was attended by 180 people from 15 countries. Some few scientists are keeping up with experimental progress on LENR, but cannot accept what is being reported by workers in the field. Such disagreements in science are not unusual, especially a new field, where they are actually normal. Disagreements certainly do not translate into the field being entirely wrong, a large collection of lies and mistakes.

What can be done to fix the problem of LENR being ignored or disdained?

The key to the field of LENR being accepted, advanced and exploited is education. This must be done by many means, ranging from mass media to courses in schools. The type and targets of such education and the level of detail must vary widely. For example, government officials in the U.S. Congress, including some Senators, Congressmen and, especially, their staffers, should know the status of the field, its prospects and what should be done to attempt to resolve the issues and realize the promise of the field. Senior people in an agency responsible for science, energy or the environment must know these factors, more scientific details and more about prospective applications. In the U.S., such officials work in the National Science Foundation, the Department of Energy, the Defense Advanced Research Projects Agency, the Office of Naval Research, the Army Research Office, the Air Force Office of Scientific Research and the Environmental Protection Agency, among other organizations. The broader scientific community should have available reviews of the field and the detailed technical information about what has been done and found in the past two decades. Scientists in disciplines relevant to LENR should have the most detailed technical information available to them as journal publications, both to satisfy any curiosity and to permit them to apply their expertise to advancement of the field. Mainstream journal editors must learn that LENR are empirically proven. Then, they might consider results from the field for normal publication, as they do in other areas, many of which have far less practical potential. Good popular articles and books on the field should be available to the general public. Teaching materials will become necessary when the field is more widely studied, probably first on the university level. If a new industry based on LENR does develop, then an even wider variety of written, video and other information must be available, for example, for the training of technicians who will work in the industry. For all of these education needs, the web will be invaluable.

What does it mean to reproduce an experiment?

Achievement of very good reproducibility will speed acceptance of the reality of LENR. Reproducibility is commonly and sensibly taken to mean that, if a particular experiment is redone, the results will be repeatable, that is, close to what was originally measured. Reproducing an experiment means that the apparatus, materials and protocols (procedures) are either the same, or sufficiently similar in the key features that determine the outcome of the experiment. Reproducing an experiment is easier if it is done in the same laboratory as the initial experiment. However, the reproducibility that is most meaningful is that between laboratories, with different scientists and organizations. There are two major reasons why experiments are difficult to reproduce, in general, and especially in a new field where the key variables are not known adequately. The first is the difficulty in matching the input factors listed above. The second is the natural tendency of scientists to vary these factors, either because they think they have a better idea, or because they do not have similar equipment and materials. In the case of LENR, it is possible that low levels of impurities within the solid materials in an experiment are basic to the outcome. This could be the case because the impurities create conditions needed for LENR, or because they catalyze the heat-producing reactions, or even because they participate in the reactions. It is very expensive to measure accurately the quantities of low-level impurities in the materials that go into and come out of an experiment. So, few experiments in the field have done a defendable job of assaying impurities that might influence or even determine the outcome of experiments. In short, both the input and output sides of an experiment are challenging to reproduce in many cases, including LENR. But, reproducibility remains a critically important goal.

What is the current experimental situation regarding equipment, calibrations and other key factors?

By now, several hundred credentialed investigators have performed thousands of experiments on LENR. Most of the experiments have involved adequate equipment, despite the shortage of financial support for the field. Reasonable protocols, including calibrations and controls, were commonly employed. Results have been obtained with very good signal-to-noise ratios. What was done and what was found have been given in open meetings, at which the investigators presented what they thought to be important and then responded to questions. Their papers have appeared in numerous conference proceedings, journals and other reports. Many of the papers in the field are available on the web at www.lenr-canr.org. This does not mean that all reports in the field are sound, of course. An increasing number of people, who have examined available experimental reports, now feel that the FPE is confidently established by many experiments, and that it must be due to nuclear reactions. However, details on the types and rates of such reactions remain to be determined specifically and quantitatively.

What is now the experimental data base for heat production, both in general and regarding reproducibility and controllability?
There is a large body of evidence from LENR experiments that it is possible to convert the binding energy in nuclei into free energy (heat). Despite the many variations in experiments, it is no longer possible to attribute all the reports of excess heat to fraud or mistakes. Nevertheless, consistent reproducibility of the output (as well as the input) of LENR experiments is still elusive. There are instances, within and between laboratories, in which a high degree of reproducibility has been achieved, where excess heat is seen in over half of the experiments. But, this is inadequate for two reasons. First, it properly bothers people who examine the field. Reproducibility is widely and properly viewed as the hallmark of experimental science. Second, lacking reproducibility, the controllability that is needed for applications of LENR is woefully insufficient. There are very few experiments in which the degree of control permits the production of excess heat to be turned on, increased or decreased and stopped at will. Practical devices will only follow the achievement of such control. If automobiles were as uncontrolled as are LENR experiments now, they would be useless for transportation.

How many papers are there reporting anomalous heat production?
Cravens and Letts reviewed 167 papers, which sought to or did measure excess heat, and presented their analyses at the 14th International Conference on Cold Fusion in 2008. There are over 100 papers that have reported heat in excess of the input energy from LENR experiments. For most, the excess power has exceeded the input power by less than 50% of the input. In several cases, however, the difference between output and input energy, the "excess heat," has been far beyond what can be attributed to chemistry, sometimes over 100 and even more than 1000 times chemical energies. In some of these experiments, the amount of excess power (energy per unit time) was 100 times or more larger than the smallest excess power that could be measured with instrumentation employed. The volumetric power densities in a few LENR experiments greatly exceed those of the fuel rods in fission reactors. Much experimental work and many results for heat production are very high in quality now, not for all papers, but for diverse reports from different investigators in about ten countries. Strong and relentless criticism of the field has forced its experimenters to be very thorough in the design, testing, calibration and use of their calorimetric and other equipment, and in the conduct of experiments.

Have the products of fusion reactions been measured in LENR experiments?
In ordinary hot fusion, two reacting deuterons (D) produce either a proton and tritium atom with kinetic energies of about 4 MeV, or a neutron and a helium-3 atom with about 3 MeV. These two sets of reaction products occur with equal probability. About once in 10 million reactions, a helium-4 atom and a gamma ray with 24 MeV of energy result from D-D fusion. In cold fusion experiments, the outcome of experiments is very different compared to hot fusion experiments. Tritium has been produced in easily measured quantities in many experiments, but still at levels more than one million times below those for hot fusion. Very few neutrons are produced. Helium-3 appeared in some LENR experiments, but at low levels and with a very different ratio to helium-4 than normal. Helium-4 has been detected in many LENR experiments, both below atmospheric levels and, notably, even above the atmospheric level of 5.22 ppm in some experiments. Heat was measured in many of the experiments that showed an increase in helium. Excess heat correlated with the level of helium production in several experiments. However, energetic gamma rays are not detected when helium is generated, as with ordinary D-D fusion. Much more data is needed in this part of the field. However, it is already clear that the outcome of light atom nuclear reactions in LENR experiments is very different from that of the hot fusion of two deuterons, especially regarding production of helium-4. Hence, other nuclear reactions, which would be consistent with data from LENR experiments, are of great interest.

What about reports of transmutations involving heavier elements? What do they mean? Are they reliable?
If nuclear reactions occur, the nuclei before the reaction are different from those after the reaction. This is analogous to chemical reactions, where the reactants (say, wood and oxygen) are very different than the products (water, carbon dioxide and ash). In the case of nuclei, the change from input nuclei to output nuclei is termed transmutation. If transmutations in LENR experiments are proved, one has direct evidence of nuclear reactions, and does not have to infer it from the level of excess heat production. Hence, it is entirely reasonable to look for the appearance of new nuclei after a LENR experiment, especially if excess heat was measured in the experiment. To do so, the experimenter has to measure accurately the levels of a variety of elements of interest both before and after the experiment. This is challenging even if only solid materials are involved in an experiment. However, in LENR experiments, liquids are involved in electrochemical experiments, gases in gas phase experiments, plasmas in glow discharge and other experiments, and beams in kinetic experiments. The composition of the phases other than the solid lattice, and the exchange of elements between those other phases and the solids, are both fundamental to confident determination of whether or not transmutations occurred during an experiment. There have been reports of transmutations from about two dozen laboratories. As already noted, tritium and helium have appeared in numerous LENR experiments in significant amounts. Only some of the reports of transmutations contain information on isotopic abundances, which is very important. Very few of the experiments have involved the detailed and defensible measurements of the total amounts of elements of interest in all phases in the experiment. One of the primary concerns about reports of transmutations is that the elements reported as products of postulated reactions were initially present, but at concentrations too low to detect with the instrumentation employed. Then, during

the experiments, those elements were concentrated in some regions where they were above the minimum detection limits of the instrumentation. This problem is not applicable to all LENR experiments that have been reported to cause transmutations. However, data on transmutations, in general, is not as robust as the data from excess heat experiments. This certainly does not mean that all the reports of transmutations are in error. A very great deal of relatively expensive experimental work is needed in this part of the field.

What about reports of energetic photon or particle emissions? Are the levels dangerous?

Particles with nuclear energies do not arise from chemical experiments. Hence, the confident detection of such particles would also be a "smoking gun" for the occurrence of nuclear reactions in LENR experiments. Much effort has gone into experimental searches for energetic particles from LENR experiments. There have been many reports of energetic photons or particles being emitted during, and in some cases, after such experiments. There are few reports of weak X-ray and gamma ray emissions. Many papers have reported neutrons, energetic protons and fast alpha particles. The levels of all these radiations are so low that they are neither easy to measure nor dangerous. It is challenging to measure even MeV level energetic ions in LENR experiments because of their short ranges. Neutrons have relatively long ranges compared to charged particles. There have been several attempts to measure neutrons, some with very good equipment and protocols, even in underground laboratories almost free of cosmic radiation. The body of data shows that neutrons have been detected in several experiments, often in bursts. There were attempts to correlate such observations with other events, but generally without success. In many experiments, small eruptive craters have been observed on solid surfaces after LENR experiments. They indicate very local and sudden release of significant energy, maybe as heat or fast particles.

What about reports of low energy phenomena, such as infrared or sound emission?

There have been a few reports of the emission of infrared radiation (IR) from LENR experiments. IR images can exhibit hot spots, and images of operating electrochemical cells show such hot spots. A simple calculation based on temperatures inferred from the IR images indicated that MeV-level, that is, nuclear energy releases are required to explain the data. One experiment measured the emission of sound from an operating LENR electrochemical cell. Here again, fast events consistent with nuclear-level energy releases were observed. Additional measurements of low energy emissions from LENR experiments are needed.

What is the state of understanding LENR, that is, theory?

Theory has only two tasks, to explain the results of past experiments and to predict the outcome of future experiments, especially those that might be designed to critically test a theory. Theoreticians offering explanations of the mechanisms active in LENR can first be asked what they are trying to explain, for example, heat production or transmutations or both. Then, there are the questions of their concepts, and whether or not they have been reduced to equations. If so, it is natural to ask if computations based on the equations have been made. Then, one can ask if the results of the calculations have been compared with available data. Or, can the results be used to design a critical experiment to see if the theory provides numbers in adequate agreement with the experiment. In general, LENR theories fall far short of these goals. There have been about two dozen different mechanisms offered to explain the existence of LENR or some aspect of results in the field. Understanding of many of the theories requires advanced (PhD level) training in physics. Some of the published theories are patently wrong. Almost all of them are incomplete in the sense that they have not been used to compute and explain the results of past experiments. There are a very few detailed comparisons of calculations with the results of data. But, most LENR theories give only qualitative trends and not quantitative rates of nuclear reactions. After twenty years of theoretical work, this lack of quantitative comparisons of theory and experiment is almost an embarrassment to many workers in the field. There have been some attempts to design critical experiments based on particular theories, but these have not proved useful yet. That is, they have neither ruled out any theory, nor provided evidence to support its correctness. In short, developing a theory for LENR is a tough problem, and the mechanisms behind LENR remain contentious and poorly understood now. This is frustrating to workers in the field, and to outsiders who look at the experimental data in the field. However, it is not unprecedented in science. About four decades elapsed between the discovery and explanation of superconductivity, for example. Understanding LENR theoretically and quantitatively is one of the best problems now available in physics. It requires major capabilities in solid-state, nuclear and elementary particle physics. That is, like doing LENR experiments, developing a theory for LENR requires a broad set of skills.

Since there is so much available experimental data on the results of LENR (heat, nuclear ash, energetic particles and other effects) and many theoretical ideas, why is the field still not deemed to be worthy of support?

In the U.S., positional and institutional responsibilities for funding general, energy and environmental sciences have proven to be insufficient to generate the attention that LENR needs. Leaders and managers in the many relevant agencies are busy with other topics. And, many of them seem to be afraid of the poor reputation that still haunts LENR because of the early incorrect, but damning assertions that reports of LENR were all wrong. Few, if any, responsible individuals seem to realize that some of the erroneous reports that damaged the field in its first two years have actually been shown to be wrong themselves. Interest in LENR is not widely viewed as career enhancing. Clearly, incentives are not in place to cause some agencies, leaders and managers to learn the status of the field and to keep abreast of its progress. It has been said that there must be some among the hundreds of managers in relevant fields who pay attention to LENR. Such is the case, but it has not led to a general acceptance of the field as a challenging science with significant promise for important applications, especially the generation of "green" energy.

What is the U.S. government doing about the field?

The only sustained, but also uneven government interest, has been in various offices within the Department of Defense. In the early 1990s, the Office of Naval Research funded LENR experiments at three U.S. Navy laboratories. More recently, the Defense Advanced Research Project Agency supported the replication of experiments originally done in Israel and Japan. The Department of Energy (DOE) has had a few individuals in its headquarters who have paid occasional interest to the field, besides the two formal reviews of the field in 1989 and 2004. There has been significant work at a few of the DOE national laboratories. But, there have been no actions from the limited and sporadic interest in the Office of the President or Vice President of the U.S. in the past. Visits to the offices of six Senators a few years ago yielded no action. Such meetings with staffers have been more a result of personal contacts than institutional responsibility. In general, the offices responsible for science, energy and the environment in the U.S. government have not given either deep or sustained consideration to the results of research on LENR, or to the potential of distributed nuclear power sources that such reactions offer. The total funding of LENR by the U.S. government since 1989 is probably in the range of a few tens of $M. That amount, on the order of $1-2M per year for the past two decades, was far below what was needed, and is still needed, to advance the understanding and possible utility of LENR.

What are companies in the U.S. and elsewhere doing in this field now?
There was early attention to LENR by a few large companies. Now, there remains some such interest, but it is not public information. A few start-up companies have been formed in the arena. In the U.S., six of them still exist, and three have already failed. Some of the companies are very limited. Most have only a few researchers. These companies are generally doing experimental research, but some of them are primarily pursuing theoretical programs. There was one start-up for work on LENR in Canada. The only other existing LENR company known to this author is in Israel. It is the largest such company, and is producing some of the most important results in the field. It is not likely that any of the current start-up companies will have the basis for an Initial Public Offering of stock in the next decade. The development and sale of energy sources based on LENR are now uncertain and distant in time, probably about ten years before the first potential products reach the market.

What is the situation regarding investments in the field?
Investments in LENR by venture capitalists are virtually non-existent. The policy of the U.S. Patent and Trademark Office (PTO) that prohibits even considering patent applications in the field makes it difficult to protect intellectual property in the U.S. This discourages funding by venture capitalists. The start-up companies noted above are generally funded by "angel" investors, very rich individuals who believe in the promise of LENR and want to make a contribution to the development of a clean source of energy, besides making money. The total private investment in LENR globally for the two decades of the field is probably in the neighborhood of a few tens of $M.

How much money has been spent on LENR research?
This is not, and will not be known in detail. It is probably in the range of $50-100M. While that amount sounds like a lot, it was spread over twenty years and many laboratories in several countries. And, it is only one of the two true costs of research on LENR. The other cost is the uncompensated time of hundreds of scientists and engineers. If there were on average 100 unpaid person years spent on the field each year for 20 years, roughly 4 million person hours would have been spent, since there are about 2000 work hours per year. This amount of time by scientists with graduate degrees and experienced engineers is conservatively estimated to be a figure in the neighborhood of the out-of-pocket expenses just cited above.

Looking Ahead

What is needed experimentally, especially regarding materials?
There are three classes of opportunities for improving LENR experiments. The first is to use better "input" equipment for the conduct of experiments. Of the three classes, this is the least urgent. Good equipment has generally been used to both set up and power LENR experiments. But, there is always the possibility of using better designed and controlled equipment to make an experiment run. The second class of improvements is the use of better diagnostics for measuring the "output" of experiments. This is a compelling requirement. There are many types of measurements that could and should be brought to bear on LENR experiments. The more (especially time dependent!) measurements that are made during an experimental run, the more data there will be available to correlate with the performance of the experiment, especially its net (output minus input) power and excess energy. *In situ* Raman spectroscopy is but one example of capabilities offered by commercial instrumentation, which could give new perspectives on operating LENR experiments. Synchrotron x-radiation can penetrate an operating electrochemical cell or a gas loading cell or a plasma chamber. X-ray diffraction and fluorescence data can provide detailed information on the varying atomic structure and composition of the solids in a LENR experiment. The third class of needed improvements involves the materials, chemicals or gases that go into an experiment. If impurities do play some significant role in determining the outcome of a LENR experiment, low level (parts per million and lower) analyses for impurities before and after an experimental run might be invaluable for improving the reproducibility of LENR experiments. The tools of nano-science should be brought to bear on LENR. Atomic Force Microscopes can provide atomic-level surface structural information before and after, and maybe even during experiments. The use of AFMs is particularly compelling because phenomena in nano-scale regions of a solid material might determine the ability to trigger LENR. In short, a program leading to replication of important LENR experiments is needed. Significant funding for LENR experiments will open many opportunities, both technically and in terms of bringing new people with important skills into the field.

What has to be done to achieve adequate reproducibility?
As already noted, the replication of experiments involves employment of equipment, materials and protocols that are adequately similar to the initial experiments to produce similar results. Hence, achievement of better reproducibility requires careful attention to everything that goes into and is done during an experiment. At the moment, not all of the key factors are known, let alone adequately controlled. Hence, it is difficult to replicate experiments, even within one laboratory and, especially, between laboratories, regardless if they seem to be similar in their construction and

operation. Despite this fundamental challenge, the problem of imperfect reproducibility can be systematically confronted. This requires two things, a serious and adequately funded attempt to have similar equipment and procedures, and the use of very well characterized materials. Both the composition and structure of materials have to be known in detail before and after experiments. It is also important to run many experiments in order to obtain statistics on the time-dependent results of the experiments. Remember the many materials tried and numerous tests made by Edison before a reliable filament for light bulbs was discovered. Voluminous data, in itself, can reveal much about the operation of a particular set of equipment, materials and protocols. And, it permits determination of improvements in the experimental outcomes that follow from systematic variations. So-called matrix experiments, in which many experiments are run simultaneously, with one or two parameters being varied between the different experimental setups, can be very valuable. However, they are impractical for complex experiments that require expensive equipment. Parametric variation experiments with such equipment must be done sequentially. At this time, operation of many small and relatively inexpensive setups with simple but adequate diagnostics should be very useful for empirically improving reproducibility of LENR experiments, even before full understanding is achieved. When such understanding is available and employed, it will significantly improve experimental reproducibility.

What about controllability of LENR? When might it be achieved?

Controllability means that excess power or other outputs from a LENR experiment can be turned on, up, down and off at will. This is fundamentally necessary for practical systems, whether it be an automobile or a home heating unit. The situation for controllability of LENR is somewhat similar to that for reproducibility. Neither is satisfactory now, but there has been progress on both. There are many reported cases, when some change has been made in an operating LENR experiment, such as increasing the input current, and an associated change in excess power has been observed. However, this is far from the control that is needed for practical applications of LENR. The experiments with the greatest control now involve the irradiation with laser light of the surface of an operating cathode in an electrochemical cell. That often increases the level of excess power. However, even this control parameter is currently inadequate. Now, it is not possible to say when the full control of LENR experiments, which is necessary for applications, might be achieved. It is possible that some significant changes in generally unexplored experimental parameters might be needed, such as the addition or removal of compounds in an operating LENR cell. If that turns out to be the case, the situation would be reminiscent of the neutron-absorbing rods used to control the output of fission reactors.

What is needed theoretically and computationally?

The first thing would be to do a hard-headed assessment of the currently available theories. The outputs of such an assessment would be two fold. First, it would be useful to document the case for setting aside some of the theories that continue to be touted by their originators. This would serve to reduce the "noise" in the field. Second, it would also be most helpful to identify quite precisely what has to be done to advance the theories that are not trashed. If such theories and their associated equations could be developed further, then the "signal" in the field would improve. The two goals should be (a) to employ the surviving theories to compute numbers for comparison with the results of past experiments and (b) to use the results of calculations to design experiments that will provide critical tests of their bases. There are several more-or-less well-established experimental factors that challenge any LENR theories. They include the need for high loading of deuterons (D) into palladium (Pd) or its alloys, that is, the ratio of D to Pd atoms must be near unity. Strong fluxes of D through the surfaces of solids in electrochemical experiments appear to be beneficial to the production of excess power. The question of where LENR occur, either on the surface or in the bulk of materials, is critical and there is data for both possibilities. Theories should address the location(s) where LENR occur, as well as their rates. The fact that the power and some other outputs of LENR experiments vary widely with time is also important, and has not been adequately addressed theoretically. Beyond theories aimed directly at understanding the mechanisms behind the remarkable results from LENR experiments, modern computational tools for the design of materials and simulation of their kinetics at the atomic level should prove useful. Molecular dynamics codes provide an example of computational tools that might be useful for understanding LENR, especially if these were expanded to include nuclear effects as well as the normal atomic and molecular kinetics.

What should be done by scientific journals and magazines?

There are thousands of scientific journals. Probably a few dozen of them publish papers that are directly or peripherally relevant to LENR. Most of those journals are quite specialized in what they consider for publication, for example, papers on specific types of materials. But, many of them have broad interests. Some of those are among the most respected journals for scientific publication, for example, *Nature* and *Science*. At present, many of the lesser-known journals will publish papers on LENR experiments. However, the more widely-read and prestigious journals still refuse to publish papers from the field. The same remains true for some important scientific magazines, with *Scientific American* being a prime example. Hence, both the broad scientific community and the general public are denied the opportunity to keep up with the scientific progress of LENR, as well as its possible opportunities. It is now very desirable for important scientific journals and magazines to rethink their policy on publication of articles on LENR.

What should be done by scientific societies?

Intellectual societies, for example, the American Physical Society (APS) and the American Chemical Society (ACS), play multiple roles. One is to represent the interests of their field to their governments. Another is to provide and enhance scientific communication by both their publications and conferences. Currently, the only scientific society to actively assert the value of research on LENR is the one founded within the field, the International Society for Condensed Matter Nuclear Science (www.iscmns.org). That relatively new and small Society has had a useful but limited impact to date. Both the APS and the ACS are now willing to have sessions on LENR during some of their annual meetings. Their utility varies significantly. However, this is decent progress relative to the situation even five years ago. It is important that intellectual societies, in both science and engineering, recognize the existence, challenges and promises of LENR in their announcements, publications and conferences. Their doing so will widen the number of people aware of the field and possibly attracted by either its mysteries or its possibilities.

What is the role of universities and schools in training people for the field?

Both the establishment and pursuit of new fields of science influence curricula in universities and even lower schools. This is due to a combination of reasons, including their excitement for teachers and students, and the need for knowledgeable workers in any fields that follow from the science. For example, many universities have new departments, institutes or centers on energy and global climate change. If LENR are controlled and engineered into products, workers to manufacture, sell and maintain such products will be needed. The desired capabilities will range from the creative and sophisticated design skills of specialized engineers, to knowledgeable sales personnel, to the abilities of technicians, who will keep LENR-based energy sources operating. There will be a growing need for training workers for the entire range of functions needed to fully exploit the possibilities of LENR energy sources. However, now many university professors and other instructors think that teaching a controversial subject, like LENR, is unnecessary and unwise, and might even damage the careers of students.

What should be done by agencies of the U.S. Executive Branch?

The U.S. power industry spends less than 0.25% of its annual turnover on research and development. This is a factor of ten less than the average R&D investment by major industries in the country, and, interestingly, 2.5% is also the fractional investment by the U.S. automobile industry. It is clear that significant government support is needed to provide the level of funding required to advance the field of LENR, both in terms of science and applications. The top organization responsible for science and technology policy in the U.S. is the Office of Science and Technology Policy in the White House. The head of that Office reports directly to the President. The National Science Foundation is an independent government agency with an annual budget of $6B (20% of federal support for basic science in the country). There are several cabinet-level agencies in the U.S. government with responsibilities for the funding of scientific, energy and environmental research. They include, but are not limited to the National Science Foundation, the Department of Energy, the Defense Advanced Research Projects Agency, the Office of Naval Research, the Army Research Office, the Air Force Office of Scientific Research and the Environmental Protection Agency. These agencies have mission-related responsibilities for paying attention to and funding research on LENR. There is no need to set up new offices for LENR within such agencies. However, new programs to meet the challenges of understanding and exploiting LENR would be appropriate. A strategy for such programs has been published in this magazine: "Program Strategy for Low-Energy Nuclear Reactions" [#69, September/October 2006, www.infinite-energy.com/iemagazine/issue69/programstudy.html]. An initial investment of $10M annually in pre-competitive research on LENR by the U.S. government is recommended. That amount would logically be increased in the following years, depending on the results obtained from the supported program in its first years. $10M is less than one part in one thousand of the total annual U.S. government research budget, and much less than that when compared to the funds devoted to development and testing of systems, especially military systems.

What should be done by the U.S. Congress?

Several standing committees in the Senate and the House of Representatives are responsible for oversight of science, energy and the environment. For example, the Senate Committee on Commerce, Science, and Transportation has a subcommittee on Science, Technology and Innovation. The Committee on Science and Technology of the U.S. House of Representatives has subcommittees on Technology and Innovation, Energy and the Environment, and Research and Science Education. Like many agencies in the Executive Branch, these groups in Congress have institutional responsibilities for knowing about significant research areas and possibly-important technologies. The staff personnel, who work for the committees, should be giving the status, progress and promise of LENR the same kind of ongoing attention as program managers in the Executive Branch of the U.S. Government. Given the lack of attention to date, it would be entirely appropriate for some of the Congressional subcommittees to hold multiple hearings in the near future on LENR. One potential focus of an initial hearing could be the state and promise of the field. A subsequent hearing might be on the possibility of the U.S. developing a new nuclear industry, which would sell products globally. If, as many people believe, LENR power sources will be commercially important, the U.S. might turn out to be either a net exporter or importer of the potentially significant products in a new industry. Now, the country imports about $700B worth of energy annually. It would be very useful if even one respected U.S. Senator or Congressman took continual interest in LENR, and demanded at least periodic public attention to progress in the field.

What should the U.S. PTO do in the near future?

Patents and Trademarks are fundamental to commerce. The first U.S. patent was issued in 1790, only two years after the Constitution was ratified. The U.S. PTO is now part of the Department of Commerce. The Office is not a scientific authority, and depends on the views of the scientific community. To date, in contrast to patent granting agencies in some other countries, the U.S. PTO has refused to consider patent applications on "cold fusion." Their policy was and remains to treat such applications as they do applications on perpetual motion machines, that is, by immediate rejection without other consideration. They are said to lack "enablement" and "credible utility." It happens that some U.S. patents are granted on LENR and related materials, apparently because the PTO does not recognize them as such. The policy of automatically rejecting patent applications on LENR severely disadvantages start-ups in the U.S. Venture capitalists are generally unwilling to invest in a company, if it cannot protect its intellectual property (IP). And, the PTO policy even deters interest by established companies, who also have to protect their IP, which cost them money to develop or otherwise acquire. The U.S. PTO should realize the fact that it is significantly restricting U.S. companies, both absolutely and in relation to potential competition abroad. It is recommended that the Department of Commerce or the U.S. PTO conduct a review of LENR, which would show it to be a legitimate field of scientific inquiry with several possibly significant applications. If the Office changes its policy after such a review, the only downside risk is the granting of some weak or worthless patents. However, that risk is common, and it does not deter the granting every year of over 150,000 U.S. patents on very diverse topics, many of which prove to be useless.

What are potential roles for both start-ups and existing companies?
The fundamental function of companies is to make profits. New products and services offered by companies can fall into two classes. They can replace existing products, or garner at least some of their market share, by being better, cheaper, longer lived, more attractive or a few other factors, including fashionable. Or, new products can open up markets that did not previously exist. The ability to generate a new market leads to great profits, with Microsoft and Google being prime examples. Many established and profitable companies have a very good position for commercial development and sales of new energy sources based on LENR. The several start-up companies now working on LENR have the advantage of a dedicated focus and the generation of significant early intellectual property. However, they are usually short of money. It is hoped that more major companies will actively monitor and then participate in the advancement of even the science, and especially the technology and engineering of LENR energy sources. This will position them to grow their business in this arena, either on their own or by acquisition of one or more of the start-up companies. A lot of money might be made from LENR in coming decades.

What has to be done to turn the current LENR science into a technology?
Science produces knowledge, which can be used to build capabilities, which are called technologies. Ideally, a rather complete understanding of the physics of LENR experiments would be available to use in making operating prototypes of energy sources. However, sometimes even imperfect knowledge of fundamental mechanisms is sufficient to enable creative inventors to produce something that works. This might turn out to be the case for early energy sources that exploit LENR. However, the surest path to any operating LENR technology is research to provide a useful knowledge base. This is one strong argument for support of LENR research, in addition to being able to confront a sweet scientific problem.

What are the engineering challenges for making LENR products?
Engineering of prototypes and products involves a few familiar phases, namely design, fabrication and testing, which is done first in house by the developers and then elsewhere by early users. These phases will probably apply to any LENR products that are developed. Nowadays, the design phase for almost all new products involves modeling and simulations using complex software based on physical, chemical, electrical, mechanical, thermal and other principles. It is likely that such codes will also be applicable to the design of products based on LENR. Most manufacturing and product testing requires the use of specialized machinery. This, also, will probably apply to both production and assessment of LENR energy sources. It now seems that many of the manufacturing tools that might be needed for the production of products embodying LENR are either available or similar to what is required for making a wide variety of other products. But, a challenge specific to LENR is the engineering scale-up from the current low excess powers, generally on the order of watts, to higher and more broadly useful powers. This requirement comes on top of the needs for reproducibility and controllability already discussed. There are four choices for working fluids, as indicated in Figure 4, namely liquids, gases, plasmas or beams. So, which of these has the best chance to appear in any initial LENR products? The complexity of liquid electrochemical systems tilts against their early commercialization. The energy requirement for vacuum pumping within plasma and beam systems does not favor them. Hence, it now seems likely that, if LENR sources are commercialized, the early versions will be combinations of gases and solids.

Can LENR be optimized to reduce costs and increase outputs?
Virtually all products involve some optimization to reduce manufacturing or maintenance costs or improve some aspect of system performance. If the expected understanding, reproducibility, controllability and the possible commercialization of LENR energy sources are achieved, then it is almost certain that optimization will follow. The century-long history of the automobile provides a good example. Cars first became more reliable, then more comfortable and, finally, more efficient. So, now it is much too early to consider optimization of LENR in detail. However, two general comments can be made. First, bringing very new technologies to market generally takes considerable time, not just a few years, even if the science behind them is known. Hence, sophisticated engineering of many (any?) LENR power sources is not likely to be done in the coming few years. Second, LENR sources might not be very simple systems. That is, they may require significant ancillary control and other equipment, such as pumps and power supplies. Think of a home furnace or a fuel cell, which involve much more than the core unit where combustion or recombination occurs. It is likely that much of the equipment required for long-term operation of any LENR power sources will be adapted and optimized from other complex, but commonly-used energy sources.

What are the prospects for long term (reliable) energy producing units based on LENR?
This is another relatively inscrutable question. Favorable answers to issues, such as reproducibility and controllability, have to be correctly concatenated for LENR based power units to make it to market. Among all the "ilities" (http://en.wikipedia.org/wiki/Ilities), reliability is both necessary and essentially impossible to estimate at this time. The duration over which any energy source works properly depends fundamentally on two factors, fuel and the maintenance of required conditions for use of the fuel to produce power. Loss of one or the other of these required conditions usually terminates energy production. We tend to think of running out of fuel as the more likely problem. However, if

it is necessary to maintain exquisite control over the conditions, especially lack of contamination, for large surface areas in LENR sources, then fuel might not be the most worrisome and demanding limit to their reliability. This is an enticing engineering problem. Its solution will become clear only as more is known of the basic mechanisms for LENR to occur and some early engineered units are made, operated and diagnosed.

What is the likely time scale for development of the early LENR products?

Given the results from some of the more important experiments, it seems quite possible that prototype products might exist in five to ten years. They will almost certainly be proprietary, and not widely known or available. If such prototypes exist, and they prove to be viable energy sources, with some (several!) significant prospective applications, then the early products might be on the market by 2020. And, if the products find market acceptance for any reason, low cost, performance or any other, then the period from 2020 to 2030 could see significant growth in the annual sales of LENR-based sources. These projections are certainly speculative. But, the early history of the scientific field of LENR, and historic timelines for bringing really new hardware technologies to market, both make it unwise to expect faster product development and market penetration or creation. Of course, sale and widespread acceptance of LENR sources could also turn out to be very important, but on a much slower time scale than envisioned above, taking maybe several, rather than two decades. However, the growing global need for energy, and the attractions of small and widely deployed clean nuclear power sources, especially in developing countries, may tend to move the field along more quickly than several decades. That is an exciting possibility.

What might be the power levels of commercial LENR systems?

Energy sources vary widely in the powers they deliver. The small battery that keeps time in a cell phone yields less than one milliwatt, while a large power plant can generate much more than gigawatts. The utility of an energy source is determined largely by the power it can deliver, either steadily or variably, or for some repetitive duty cycle. Hence, it is natural to wonder about the powers that might be available from LENR sources of energy. There are some soft limits on both the low and high end of the possible range. On the low end, the likely complexity of LENR sources, while not great, will probably keep them from being very small, say, under a milliwatt. On the high end, the facts that current LENR sources do not now produce either high powers or high temperatures tends to weigh against early products giving very high powers. It seems reasonable to expect the largest early LENR sources to have power levels below 1 to 10 kW, that is, possibly adequate for home heating and for powering small vehicles. This perspective on larger LENR sources is not solidly defensible. Consider transistors. The first ones were large single devices. Now, commercial chips with half a billion transistors are moving into production. It is conceivable that, after a few decades of development, even megawatt LENR sources consisting of large numbers of individual modules will be possible. Now, multi-megawatt fuel cell installations containing hundreds of identical sub-units are being planned. There is no obvious reason to bet against this also happening for LENR sources.

Will it be possible to produce electricity from the excess heat due to LENR?

Currently, LENR produce heat, the so-called excess heat. In general, temperatures in operating LENR electrochemical cells are below the one-atmosphere boiling point of water at 100°C. However, in some cases, such cells have achieved higher temperatures and boiled dry. In one infamous case, the Pd electrode melted, indicating that a temperature exceeding 1554°C, the melting point of Pd, was attained. In one gas phase experiment in Japan, a vessel with nanometer scale Pd particles and high pressure deuterium gas was preheated to about 140°C. It then reached a temperature near 200°C, reportedly from energy released by LENR. If high temperatures can be maintained in LENR power sources, then it is possible to use ordinary rotating electrical generators. There is a question of the most appropriate size for such generators, but they would work, in principle. An alternative means of generating electricity from heat is to use solid semiconductor converters. They offer the possibility of working with lower-temperature LENR sources, and do not have moving parts. The older of these is thermoelectric materials, which have long been commercialized. If such materials span two regions at different temperatures, the flow of heat through them produces a voltage (and vice versa, that is, they can also be used as heat pumps). There is an immense motivation for the development of efficient thermoelectric materials, namely refrigeration. The availability of good thermoelectric materials would make possible solid-state home and other refrigerators without compressors for working fluids. The newer solid-state technology for converting heat to electricity is micro-gap thermo photovoltaics (MTPV). This technology is a relatively recent development. It could turn out that both commercial LENR sources, and either thermoelectric or MTPV materials, will be developed and mated for production of electricity from LENR power. Electricity consumption in the U.S. averages about 1 TW, so significant generation of electrical power from LENR sources would require many units, if each could produce 10 kW of electricity.

Are there other potential uses of LENR besides production of energy?

Conceivably, yes. Considerable existing experimental data indicates that it is possible to transmute one element into another, not only with light elements as input, but across the periodic table. If the nuclear ash from LENR can be produced in needed amounts, and it has significant applications, then such reactions might be used to produce a less abundant and valuable element from another available and cheaper element. Some countries, the U.S. included, stockpile key elements, which are needed in the manufacture of important devices and systems. If it were possible to use LENR to make desirable but scarce elements, the need for stockpiling critical materials might be reduced. Then, industrial countries would not be at the mercy of the vagaries of geology, or international markets, politics and tensions, to obtain needed materials for any reason, commercial or military. In order for this possible use of LENR to come to pass, most of the same factors like reproducibility and controllability, which are needed for energy production, would have to be tamed. Even if production of elements in significant amounts by LENR were possible, the costs of the processing would certainly be an important consideration. Of course, the energy market is a larger and more compelling force for the development and commercialization of LENR compared to materials production. It should be noted in passing that some people have hoped that LENR could be used for the remediation of nuclear waste, that is, the rendering harmless of very long lived isotopes produced from fission reactors. That application seems unlikely. Even if it were possible, it would be very expensive, especially compared to the current, apparently safe and cost-effective methods of storing nuclear waste in secure above-ground casks.

What about radiation safety during operation of LENR sources?
Current fission reactors and hoped-for fusion reactors both produce large fluxes of energetic particles that are dangerous to human and environmental health. So, it is natural to wonder about the emissions that might come from LENR energy sources during their operations. The experimental data now available shows that it is very difficult to measure energetic photons, neutrons or ions from LENR experiments. That is, they appear to be entirely safe from the prompt radiation viewpoint. However, it could turn out that LENR sources optimized for energy production will be significant radiation sources, and require some shielding plus stringent operational safety features, such as interlocks. Further, scale-up of LENR sources to higher power levels than current experiments, which are rarely above 10 watts, could conceivably produce radiation safety concerns. However, it is also possible that LENR energy sources will be intrinsically safe, maybe even safer than electrical systems that can short to ground and gas systems that sometimes explode.

What about radioactive waste from operation of LENR sources?
Fission reactors have the tremendous disadvantage of producing a great deal of radioactive waste, which must be stored for thousands of years to reach safe levels by radioactive decay. The fast neutrons within contemplated fusion reactors will also induce troublesome levels and amounts of radioactivity. Hence, the worn out structures of a Tokomak or other hot fusion reactor would be important and problematic radioactive waste. As with prompt radiation, measurements indicate that the small LENR experiments already conducted have not produced significant radioactive waste. Placing solids from LENR experiments on photographic films and other detectors has shown evidence for some residual emissions. The radiations do appear to be low in both intensity and energy. However, they have yet to be adequately characterized. Again, optimization and scale-up of LENR sources might lead to some difficulties in this arena. But, problems with radioactive waste from LENR energy sources are not assured. Past measurements of harmless helium produced in LENR experiments bode well for the possibility of LENR sources not being a source of radioactive waste. Of course, even without radioactive waste, LENR sources will eventually wear out and produce scrap, much as do automobiles. Like any waste, dysfunctional LENR power sources will have to be handled in an environmentally responsible and economically sensible fashion.

Could LENR provide the basis for weapons?
Historically, many new energy sources have been used for warfare. It would be nice if LENR sources were an exception. However, there have been a few LENR experiments that clearly produce high energy density events. The best known is the experiment by Fleischmann and Pons that was reported in the *Journal of Electroanalytical Chemistry* in 1989 (Volume 261, pp. 301-308). They had a cell running with a cathode consisting of a cube of Pd 10 mm on a side. Their article carried a notice "Warning! Ignition?" It went on to say, "We have to report here that under the conditions of the last experiment, even using D_2O alone, a substantial portion of the cathode fused (melting point 1554°C), part of it vaporized, and the cell and contents and a part of the fume cupboard housing the experiment were destroyed." This event raises the concern that LENR could be employed either to augment existing weapons or even develop new kinds of weapons. The possibility of fast energy releases from LENR experiments not only raises the weaponization question. It shows that it is fundamentally necessary to insure that any commercial sources based on LENR are very safe. Fortunately, small modern sensors, plus diagnostic and information systems, make monitoring of the operation of complex systems much easier and cheaper. This translates into the ability to detect potential problems before they become serious, with benefits similar to the early detection of cancers. It may turn out that LENR energy sources will not be explosive.

What are now the most attractive applications of LENR sources?
If LENR sources produce only heat, even if they do not involve high temperatures, they might still be useful for production of clean water. Polluted rivers supply drinking water to many people around the globe, who develop dire health problems. The ability to produce clean water for people in developing countries, and also to desalinate water in them and even developed countries, could have great impacts. If LENR products can also produce electricity, then they might find use for powering millions of homes, either as the primary or backup sources of energy. They could also be employed in offices, factories and other buildings, and for military installations and operations. As is the case for most energy sources, unexpected applications would be found for commercial LENR sources.

Can LENR be the basis for a new nuclear industry?
Here, again, there is a series of issues that have to be favorably resolved for LENR to be the basis of a significant new industry. Not only must LENR be commercialized and the products prove to be reliable, they must also be cost competitive in one or more old or new major markets. It is clear that LENR sources need not capture a large fraction of the market for energy sources for them to be the basis of a significant new industry. Even a few percent of the global energy market is a very large amount of money. Hence, achievement of a niche status by LENR sources could make them economically important. If they are indeed, clean and green, as current research indicates they might be, then their market could be significantly larger and their impact correspondingly greater. Nothing other than adequately funded research and development of prototypes needs to be done by the U.S. or other governments in the next decade or so to speed development of a new nuclear power industry based on LENR. If LENR source commercialization proceeds increasingly over the following one or two decades,

then it is likely that an industrial association, industry magazines, many websites and LENR-centric business conferences will emerge and grow with the new industry.

What about the overall economic aspects of LENR?

The economic importance of LENR energy sources will follow from their market success, which is entirely unclear now. But, given the size and increasing importance of the world energy industries, it is possible that LENR-based energy sources will have significant economic impact even within the lifetime of current school children, that is, in roughly half a century. It should be remembered that sales and service of commercial LENR sources will not be their only economic impacts. The favorable effects of small and distributed nuclear power sources on reducing the cost of emplacing, maintaining and managing the power grid in the U.S., plus ameliorating expensive power losses during transmission, will also be important. Brown- and black-outs due to power interruption caused by problems in centralized power plants or the grid are very expensive. This is both because of their interruption of factory production and, increasingly, due to their deleterious effects on digital computer and communications systems across the economy.

Are there any certain or potential societal or political implications?

Consider the current importance of micro-credit in many poor countries. Its impact greatly exceeds the amount of money involved. Hence, it is not unreasonable to contemplate the favorable impacts of small and possibly long-term distributed nuclear power sources on developing countries. Should the U.S., or other developed countries, commercialize LENR power sources, providing them to developing countries at reduced prices could be incredibly beneficial. The situation might be analogous to giving some countries medications at reduced prices, which is done now to the benefit of all involved. If LENR-based energy sources do become really important commercially, even in one country, they will have political implications. Should their importance be global, as it is likely to be the case if they are commercially important anywhere, then there will be global societal and political concerns.

Conclusion

It is now clear from two decades of experimental results that the study of LENR is an exciting new science. The current and potential issues in the field are certainly numerous and challenging. LENR also offers the possible basis for small, clean, distributed sources of nuclear energy. However, it may be a decade or more before practical LENR energy sources are realized.

The field is now in something of a "Catch-22." Ordinary publication of results in good journals is needed. That might start to happen routinely, if more scientists simply studied already available data. But, there is little motivation for scientists, or editors or program managers, who depend on scientific opinion, to become conversant with available data. The field needs attention to get attention.

The questions posed above are only a sub-set of the questions that can be asked about LENR. In particular, there are many more scientific questions, such as the possible effects due to the use of amorphous alloys as cathodes in electrochemical cells. And, as noted earlier, the questions considered here are only some of those of interest to one person in one country. But, the author has given numerous presentations on the field and been asked many questions. Hence, some of the questions qualify as "frequently asked."

The entire set of questions posed and considered above is not the result of any survey of colleagues in the field, although they might be of significant interest in the LENR community. It is hoped that other scientists in the field will expand this limited set of questions and provide their opinions as answers. That activity might help focus the very restricted resources that are now available in the field.

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About the Author

Dr. David J. Nagel is a Research Professor in the School of Engineering and Applied Science of The George Washington University. He has been involved in LENR as a scientist, reviewer or manager since the March 23, 1989 press conference by Fleischmann and Pons. He attended all of the International Conferences on Cold Fusion, and chaired the 14th such conference in August 2008 in Washington, D.C.

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Cold Fusion Collaborations:

Further Selections from the Cold Fusion Oral History Project

Marianne Macy

The New Energy Foundation Cold Fusion Oral History Project at the University of Utah continues through 2009 with ongoing interviews and processing of transcripts. Although the interviews will not be made fully public until the project is finished and all subjects have given permission for release, *Infinite Energy* will continue to publish excerpts through the coming year (a first segment was published in #80); some of the material herein has been edited for publication purposes with the participation of the interviewees. The oral history interviews are especially relevant now, at the time of the twentieth anniversary of the Fleischmann-Pons announcement. The entire collection of processed interview transcripts will be made available through the University of Utah's online distribution and at the Marriott Library of the University of Utah.

The dedication of the researchers is striking throughout the interviews. Despite difficulties in funding, materials and other technical issues, as well as the challenges posed by understanding the multiple questions and problems presented by the experiment, researchers persevere. Often what helps a researcher get through these tough patches is close collaboration with other researchers. These collaborations often help move the field forward another step.

In this issue, we want to highlight one such collaboration. Herein are interwoven segments (from their separate interviews) of a collaboration between two experimenters, Dennis Letts and Dennis Cravens, who discovered and are working on refining an effect with laser stimulation, the significance of which will be determined over time and ongoing current work. The use of theory and how that contributes also unfolds here with material from Peter Hagelstein's interview.

DENNIS LETTS ENTERS THE FIELD

Dennis Letts is a Texan with a background in mathematics and a master's degree in business science. He was involved in the oil business from 1970 to 1987, when changes in world oil prices in 1986 led Letts to another extractive industry, gold mining. He worked in South America, Honduras, and then the Amazon jungle in Bolivia, where he heard the Fleischmann-Pons announcement over a radio in the rainforest. He returned to the U.S. and poured over related papers in the chemical library of the University of Texas at Austin, where through 1992 he read the work of Martin Fleischmann, Stanley Pons, John Bockris, Mike McKubre, and other scientists publishing on cold fusion. In 1992, he was ready to do his own experimentation. Letts describes his first foray into the field and the start of his collaboration with Dennis Cravens as follows:

> I submitted a paper in Hal Fox's *Fusion Facts*, which was published. It was about the idea of spin alignment and stimulation with radio frequency waves. That interested Dennis, and he called me up one evening and we hit it off on the phone rather well. He came down to see me in Austin. He was doing a lot of that then, driving around seeing all the scientists. So I had wanted to get him to do the experiment. Of course, he refused. He says, "We need people to do this. You need to learn to do this yourself. The field needs experimental work done, so get busy. I'm not going to do it for you." So I went out and somehow put together the equipment that I needed to do this testing.
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> I started on July 10, 1992. That was my first day of experimenting. My first experiment involved the application of radio waves to a test tube, a 1-inch test tube with palladium and heavy water. A typical cold fusion cell. Lithium deuteroxide (30 milliliters), a piece of palladium wrapped in a little coil of platinum wire and a little Teflon lid. It took a couple of months. Cravens had to coach me a little bit over the phone and fax. The RF [radio frequency] experiment was based on known physics—nuclear magnetic resonance. I envisioned the deuterium nucleus spinning inside the lattice in a magnetic field. You know how a football is thrown in a spiral—it rotates very nicely and smoothly? Well, a deuterium nucleus does something similar—it wobbles like I would throw it, kind of loose, it doesn't spin smoothly, it kind of wobbles. Occasionally the deuteron picks up an electron and the orbital electron. . .creates a magnetic field at the nucleus. This is very well-known in classical mechanics and classical physics. So if you have a nucleus shaped like this and it's wobbly and it's sitting in a magnetic field created by the orbital electron, you can calculate how strong that is. It's about 12.5 Tesla, 125,000 gauss, and that's huge. A magnet can take a pair of pliers out of your hand at about, say, 2,000 gauss. This is 125,000 gauss so it's quite strong. The wobble rate of the deuteron in that magnetic field is 82 megahertz. So I thought, "Well, what would happen if you coupled energy with that wobble rate and caused it to flip over?" What is known is that the deuteron flips over and why that should produce heat isn't known, it was just a guess based on observing some experiments. So

> what I found out was when I applied the 82 megahertz RF frequency to the cell at a very low power of 30 milliwatts, the cell got very hot. I wrapped a coil around the outside of the little test tube, and then applied the radio waves to that copper coil and the energy then got coupled to the cathode surface. . .Occasionally I was able to see very large temperature gains, sometimes as large as 20 degrees but they were rare.
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> The RF thermal response happened almost instantly. As soon as you get around 82 megahertz, 81.9 and in that range, right around that point I would see very large increases in cell temperature in correspondence with the application of the RF. The trouble is, it didn't travel well. I couldn't get it to work in other people's labs and it was hard to even do it in mine, but when it happened it was spectacular.

The support of John O'M. Bockris, one of the top electrochemists in the world, encouraged Dennis Letts to continue.

> I even took the experiment down to John Bockris' lab in late 1992. It demonstrated down there as well. Bockris saw it. I flew it up to Salt Lake to Eneco. I demonstrated it to John Bockris and all the Eneco people. Yan Kucherov was there and Bockris was kind of jumping up and down and so he invited me to write a paper with him for Maui, ICCF4. Bockris introduced the RF effect to the community. I was pretty much an unknown back then and nobody really, rightfully so, would pay much attention to some "wacko." Bockris was always willing to try new things and still is at age 85.

COLLABORATION: LETTS, CRAVENS AND A WELL-KNOWN THEORIST

The oral history interviews detail the collaborative relationships that develop over the years. Early on, researchers met at conferences once annually but relied mainly on phone and fax communications. With the ease of email came a simpler means of collaborating with colleagues which has surely benefitted the community. As indicated above, one early collaboration which still exists is between Dennis Letts and Dennis Cravens—often known in the community as D1 and D2, since they share the same first name.

Dennis Cravens has an undergraduate degree in physics with a minor in chemistry, biology and math, a graduate degree in molecular biophysics, and postdoc work on acoustical properties of energy transfer of uranium atoms. Cravens explains, "My postgraduate involved how uranium atoms would interact with each other so you know how excited they could stay at different pressures so they could do laser separation, which was somewhat classified at the time and some still is; you don't want the bad guys to separate laser isotopes too easily." Cravens was recognized by Martin Fleischmann at ICCF4 as being the author of the best paper of all submitted that year. Cravens notes:

> It was on practical methods in cold fusion and the things that make it work and load cold. At that time, it wasn't well known. You'd have to look through a lot of papers to find it. I had a reflux calorimeter, so I was actually making cells that would boil water and then doing calorimetry by condensing the water in the steam area so I could keep it boiling for days on end. Pons and Fleischmann had boiling cells but all the water would boil off and then they're out of business. Mine kept on going and going, so he [Fleischmann] liked that.

An interest in interdisciplinary scientific problems led Cravens into many areas, including cold fusion. In the 1990s, he wrote in *Infinite Energy* his belief that the greatest impact of cold fusion would be in water purification. He maintains that "lighting a light bulb's one thing, but the biggest use I think would be to get a cup of cold water in the hands of people in Third World countries."

Peter Hagelstein, associate professor of electrical engineering at MIT, is a theorist who has done years of work relating to the interdisciplinary sciences involved in what historically was known as cold fusion. He was an electrical engineering and computer science undergraduate at MIT. He got a graduate degree at MIT in electrical engineering. The electrical engineering department at MIT at that time was a home for applied physics research. Hagelstein worked at Lawrence Livermore National Laboratory on X-ray lasers, which he also pursued later at MIT, among other subjects.

Hagelstein's oral history details more of his journey. But his work with lasers would play into what Letts and Cravens stumbled into—stumbled being an operative term encompassing years of hard work, as the discovery of what came to be known as the Letts-Cravens Effect came as the result of an experiment. Letts tells how that happened:

> In September 2000, one day, because platinum costs were so high, I had purchased a gold wire instead of a platinum wire to wrap around as my anode. . .I probably would have gotten by with it and maybe not have found or seen the laser effect except I had a high concentration of lithium ions in the electrolyte. The lithium ions reacted with the gold and caused it to dissolve, fairly slowly but it dissolved nonetheless, and in a few hours I came out to look into the cell and the cathode had horribly ugly, green, nasty, dark stuff all over it. It was the gold. It had dissolved from the anode and part of it had gotten into the liquid and then transferred over to the cathode and made a big, nasty, green, dark mess on the cathode. So I knew that wouldn't work for RF purposes, at least I didn't think it would. So I quickly put the RF away and said, "What am I gonna do now? I've got time and money invested in a cell; what can I do?" This is literally how it happened. I opened up the top drawer of my toolbox and there was a laser pointer that I had purchased from somewhere and I said, "What the heck." It had a battery in it, so I put it in a vise and put it on my benchtop where the cell was sitting. At the time I was not doing any sophisticated calorimetry. I just wanted to see a large effect. Well, I clamped it down and I put this 1 milliwatt laser pointer. . .on the cathode, turned my back for 10 or 15 minutes. I paid attention then to my display monitor on the computer and the cell temperature had gone up 2 degrees in just a few minutes. I knew right away that was bizarre, that shouldn't be happening. And I knew we were on to something pretty cool and I just didn't know what, so I had my usual reaction. I picked up the phone and called Cravens and

said, "Dennis, what the hell's this? What can this be?" And he was pretty well stymied too. He said, "I don't know. Does it scale with more power?" and I said, "Well, I only have one." He said, "Well, go to Radio Shack." So I literally hung up the phone and zoomed over to Radio Shack and got a second laser pointer and pointed it on the cathode and it went up by 4 degrees. The instant I put the second laser on, it just took off. It was shining on this dark, ugly mess that I knew was gold but I didn't know all the details.

Even though the gold was dissolved, that was my very best laser stimulation. I subsequently bought more powerful lasers that do about 25-30 milliwatts instead of 1 but they don't work any better. . .As far as the magnitude of the effect, my very first little foray was by far the strongest effect that I have seen given the strength of the laser. As it turned out, the two lasers that I selected, from what I know now, happened to be resonant with some of the surface states we now think exist there. I couldn't have picked two better laser frequencies or wavelengths to work on this application. I'd like to claim brilliance, but it was just luck, brilliant luck.

I called Cravens up and reported what happened to the cell. It was making a lot of apparent excess heat with very, very feeble laser stimulation and we decided to collaborate on developing the effect and write some papers. We made a little box to put the cell in to isolate it from the ambient temperatures. We do it in a more sophisticated way now, but it was a simple little box and it needed a window in it, so I put in a polycarbonate square piece through which I could shine the laser. An hour earlier, when I shined the laser on the cathode out on the open bench, it worked beautifully. Two hours later after I got the Styrofoam box built, put the plastic window in and shined the laser through it, it didn't work. The cell was dead. So I picked up the phone and whined pitifully to Cravens and he said, "You know, I've heard or read somewhere that plastic can change the polarization angle of the laser without you even knowing it. Put a piece of square glass in there and it may come back." I did that, and shined the laser in and—Bang!—it came right back on. So the polarization effect was very important. Cravens discovered that by telephone without even looking at the experiment, which I thought was pretty clever. We reported all of that in 2003. But this was the fall of 2000. Even though we started with two laser beams, it's a nuisance to have two things out there shining, so I just kept on working with one for a long time and then I bought more expensive lasers that are more powerful. Now I have tunable lasers and better equipment. We continued to work with a single laser at that time. By 2002 we had done this dozens of times and we continued to turn it off and on and try to understand why it's working and we tried different wavelengths. We found that other wavelengths would work and some wouldn't. So there seemed to be some selectivity to it.

In 2002 Peter Hagelstein found out about it through a mutual friend named Bill Harrington out on the West Coast. So in the spring of 2002 everybody showed up, Peter Hagelstein, Mike McKubre [an electrochemist from SRI], Matt Trevithick [a former student of Hagelstein's] and Cravens. I had a house full of nerds in my small lab. Our purpose was to see if this was remotely real. And they didn't get a very good show at that time, but they got one later. And Peter planted the idea of using two lasers to form a beat frequency. This was 2002 and we did a very bad thing. We ignored Peter for five years. We tried it and it didn't work right away. We were getting results from a single laser, so why do we need Peter if we're getting results? You only need Peter when you're in trouble. So we got in trouble later and we had to repent. Peter saw the effect to some degree in a small way and then Peter and Mike and everybody went home. We call this NerdFest 1!

I started the next cell, #565. And it worked very well. I mean, you get one of these every five years or so. You'd put the laser on and it would go up 2, 3, 4 degrees with a single laser and the magnetic field would trigger it. So I called Mike McKubre up and said to him, "Sorry you guys, the new one, the one that was loading when you were here, is really producing heat and doing well" and I said "it's even sensitive to magnetic fields," which Cravens and I had worked on in the past. McKubre said, "Dennis, that can't be. This is crazy. That makes no sense at all." And I literally said, "Michael, have cell, will travel. I will bring all of this to your lab in California next week if you want" and he said, "Bring it out." My wife Kathy and I loaded her Expedition. . . We arrived on a Sunday, downloaded all the stuff, took it up to Mike's lab, got it all running Monday morning ready to show all its features, the laser stimulation and the magnetic field effect and all that. It did nothing. Monday morning it wouldn't run at all. We're all groaning. I said, "Let's not give up." So we worked on it Monday and Tuesday. This was in April 2002. Worked all day Monday, all day Tuesday, not a drop, zero, dead. And this cell had done miraculous things before. So I brought my kit along to do surgery. I took the cell apart at four or so that afternoon, cleaned the cell, did all kinds of nice little maintenance, put it back in and loaded it low current for an hour or two while Mike and I talked and brought cell power back up to 8 watts or whatever it was supposed to be and we left it running overnight.

So Wednesday morning rolls around and all the guys show up, Matt Trevithick and Fran Tanzella [a senior chemist at SRI], the whole gang. We're all standing around and I say, "Okay, let's give it a whirl." The first thing I tried was moving the very tiny magnets which don't do anything (they were small, thin magnets). Keep in mind that nothing worked—magnets, lasers, nothing—the last two days, so the guys had seen the null effect. They knew that normally nothing happened when I did it. So I rotated those magnets 90 degrees so the magnetic field was going straight into the cathode, perpendicular to the cathode, rather than going across the face of the magnet, which was the position that usually triggered the effect. And sure enough it went up 400 milliwatts, as I recall; as soon as I rotated the magnetic field the cell temperature started going up. Everybody was excited.

Rome wasn't built in a day and neither are scientific advances. Dennis Letts talks about the issues of whether the effect could be controlled, and what in fact the actual value of it would be, as well as how Peter Hagelstein came to be more involved:

> At that time, and even to some degree now, it was very urgent that we have some way to demonstrate that we could have some control over this effect. Most of the time it just happened when it felt like it. So being able to treat it like a light switch kind of was useful and it still is now. It's just not as useful now perhaps as it was back in 2000 and 2002 or 2003. But the effect is so without support theoretically and from a mainstream physics standpoint that most people believe it's a measurement error, so it's a very handy thing to be able to switch it on without doing hardly anything to the cell. You have the cell running with a little postage stamp cathode in it and you shine a feeble laser beam on it and it makes the cell temperature go up 2 or 4 degrees, that's unheard of. I mean, that shouldn't be able to happen and yet it does very clearly.
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> From 2000 to 2003 our success rate was 87% when we followed protocol. From 2003 to 2007, I had 0% success. Fifty-nine cells were produced. I made 59 cells that didn't work.
>
> Failure is depressing. It's just awful. Our colleagues in Italy were producing the laser effect with pretty good results. Several other people around the world were doing it during that period of time, and Cravens and I got no results. We were the only guys on the planet who couldn't do it. We lost our mojo is what I like to say. At that point in time, after having failed, fast forward over to 2003 to 2007, March of 2007 I called Cravens up. I said, "You know, I'm tired of failing." I said, "Why don't we do something out of sheer desperation? Why don't we pay attention to Peter? Peter said that we should create beat frequencies and we haven't really done that. We haven't really worked at that since 2002. We sort of tried but didn't." And he said, "Okay, that's reasonable, what do you want to do?" I had both lasers. I had been loaning my second laser out for the last few years prior to that, so I had some excuse not to try it but the main reason is that I was unfocused and just too lazy and I wasn't desperate enough. I said, "Okay, we'll try the simple dual laser at about 8 terahertz." Peter had given us this tip. He said, 'If you put the two laser beams together and let them beat together they will occasionally, in fact they will quite often, beat in subtraction mode, which means the frequency of this laser minus the frequency of this second laser will be something on the order of 10^{13} cycles per second." And that's called terahertz and there are at least two and perhaps three known frequencies for palladium, palladium deuteride. The two that Peter predicted in 2002 were, as I recall, 8 terahertz and 15 to 16 terahertz in that range, and we never could get them to work, mainly because we didn't really try, to be honest. And so we really were committed in March of 2007 to make it work, so we were really focused on it (no pun intended). We put the two laser beams on in such a way where they would subtract and create about 8 terahertz, which was about where Peter said would work well. I mean, just in minutes, up goes the cell temperature, so I'm jumping up and down. I just saw it. It was kind of a quick, small effect. You see, I've done this experiment about 10,000 times now, nearly that I guess. That was Cell #662, the one that finally worked in March of 2007 and we typically do ten, fifteen, twenty runs in each cell so it's easy to see when a cell behaves so differently. So after you see all those that don't work, when you see something a little strange you catch on quick. Wow, there's something there that's different. It's easy to see. So we saw it at 8 terahertz and we said, "Oh man, that's cool," and we tuned around it a little bit and it got better, got worse as you got away from it. So we went on up to 15 and with the same experiment, same cell and everything we just changed one of the lasers out, tuned to 15 terahertz and did the same thing right where Peter said it would be. By this time we'd found a chart in a book that showed that palladium hydride had a third frequency that Peter hadn't mentioned but we just thought "what the heck, we're on a roll, we'll try it." It was 20 terahertz and this one is sort of a problem for Peter because I don't think it quite fits his phonon theory.
>
> I don't think palladium deuteride has a beat frequency around 20 terahertz but palladium hydride has an edge beat frequency around there, so Peter said, "Maybe you've got some hydrogen mixed in there just by contamination and that's what's making that work." So we don't know; that's a current experimental issue and we don't know. The point is that back in March 2007 we tried 20 terahertz and cell temperatures increased sharply. So we had three frequencies that we triggered and these matched Peter's frequency theory pretty well to the letter, at least two of them did. And so the three of us wrote a paper for the *ACS Sourcebook* last year on phonon stimulation type effects. And then we all went our separate ways for a little while and shut down and rested for awhile and then began again just a few months ago at the beginning of 2008 and repeated the experiments of 2007 and got identical results. And, in fact, since then we've done 48 or 50 runs that we used to contribute to our paper for ICCF14, or I should say ICCMNS14. We filled in all the blanks and you can clearly see around those particular frequencies and no others you see a very high thermal response. So you have what you might call an optical phonon spectrum of one of these cells and it happens at exactly where Peter predicted, 8 and 15 terahertz and a third frequency that we don't quite know about yet at 20 terahertz. And they seem to be about 5 or 6 terahertz wide, which is a very narrow band where the effect works.

Letts clarifies the "narrow band" as follows:

> It's highly repeatable down to a very narrow band and the significance of the width of the band is like tuning an FM radio; if you get right on in the middle of the

> band, you get your maximum thermal response; if you get a little too far to the left or too far to the right, thermal response is less. Instead of making 250 milliwatts it will make 60. But if you're right in the center it really gets hot. So what happened, we put all of these experiments together since March 2007 to March 2008, saw that we had almost perfect repeatability between the two years. We applied the same frequency to a different cathode and got almost identical results both in tuning required and the thermal response.

Now that the issue of repeatability has been tackled, do Letts and Cravens imagine that their effect will become an experimental staple? Letts responds:

> I don't think so. I think it will be more useful to the theoretical community. At least that's my thinking. There's a couple of reasons why I don't think it will be real popular. It requires two lasers and an optical spectrometer, which a lot of people don't have. It's expensive—$7,000 to $8,000 to buy that equipment. Plus there are so many other exciting ways to do stuff now—the Superwave, the Arata-Zhang method. But of course those are not cheap either. Our main purpose in pursuing the dual laser method was to give information back to Peter for him to be able to make some progress on his theory, and Professor K.P. Sinha and Andrew Meulenberg are doing some similar work and we've sent them courtesy copies of all of our work. So our laser work gives theorists data for their ideas. We want to work very closely with theorists to try to make the theory and experiment fit together better so we can progress in our understanding. We don't necessarily want to create the ideal, perfect demonstration experiment, although it's great if that works out too, but our main thrust is to give information to theorists so that they can improve our understanding of the heat effect.

What is going on with the Letts-Cravens Effect? Dennis Cravens discusses his analysis:

> Peter Hagelstein is involved with the question of how does the energy couple to the lattice. Because you've got millions of electron volts coming out and the lattice and the heat and all, after a couple of eVs, you would rip apart your atoms. So you've got lots of energy, millions eV's of stuff coupling to lots of little things, and how can it do that? What is the coupling constant and how could it happen? So phonons, the movement of things within the lattice, is one way to do that. And there are known phonon resonances. There are optical phonons, acoustical phonons and lattices. When does this occur in the reaction? Well, anything heat is movement of the lattice molecules. As you heat up something, they're moving, so there are phonons involved. Anytime there's heat, there's movement of the lattice structure. That is related to phonon transmission. There's optical and acoustical models and they're moving in or out and sideways. . .It's known that there are optical phonon resonances at 8, 15 and 20 terahertz. Terahertz would be a better range to do things in, but it's really hard to do because it's not microwave. You can't get there electrically, you can't get there optically, so you've gotta do something else. So one way to do it, we have found, is putting two frequencies, two different laser beams of different frequencies so that they get a beat interference. It is like playing two bad notes on a guitar. You get the beats and by tuning the lasers, you can get to the terahertz. But it's very, very weak. . .As far as I know, we are the only ones that do that because most people want much more power. In order to do that, we have to have the lasers at the right angles. Laser beams are polarized. The electric fields go up or down or across. So you have to have two of the electric fields going up at the same direction, at the same location, you have some impurity or something there that responds to both frequencies so they can add them, just like a soundboard on the guitar. And then you couple to the lattice so there's a specific angle that both can interact on the cathode. The polarization levels and the E field has to be so that the cross product of the two laser beams has a component perpendicular to it because there's something called a Poynting vector, which is the direction of energy flow and that sort of thing. So we aim them both at about a 45 degree angle of the cathode's surface but from different directions so the cross product of the E field is non-zero and it has a Poynting vector perpendicular to the surface.

Peter Hagelstein was asked whether the lasers that Letts and Cravens shine on the surface are actually mixing to form the terahertz level frequencies that he was calculating:

> They've got a measurement of each of the laser wavelengths. They have measured the wavelengths of each of the individual lasers and the arithmetic calculation of the difference in frequency between the two lasers is the straightforward thing to do. It's actually hard to measure the difference frequency directly, but the measurement of each of the laser frequencies they've done not to great precision but they have a decent characterization of that. However, the more interesting question is: how can you get a response at the beat frequency? That's the "$64,000" kind of question.

How does Hagelstein know that there's a non-linear mixing going on? He responds:

> Again, the experimental result itself sort of stands independent of associated explanation or reasoning. What's the basis of the question is as follows: people do experiments in which laser beams at different frequencies are sent through a nonlinear crystal and you can generate some difference frequencies easily enough of the beams coming out. Of course, these experiments don't usually involve laser beams incident on a metal surface. People do experiments where surfaces will be pulsed with intense laser beams. At MIT, one of my colleagues does work in that area. He has very intense lasers. He'll hit a surface and there'll be a response at optical phonon frequencies. Of course the intensity that's used in those experiments are on the order of ten orders of magnitudes higher than the intensity that's used by Letts and Cravens. As a result, you would actually not expect a mixing of frequencies

of the electromagnetic light. Unless you've got a very interesting effect, that I guess was discovered first by Fleischmann, something called surface-enhanced Raman. The current understanding of surface-enhanced Raman is that you have microstructures near the surface which will cause a concentration of the electric field to give you an affected field that is orders of magnitude higher than the actual field that you bring to the surface. At the moment, I don't actually believe that a surface-enhanced Raman effect occurs in this experiment. It might and it's a possibility and perhaps if it was studied further we might be able to clarify it. I actually think that the nonlinearity is coming about from things deeper into the surface of the new physical process that we are studying.

What could that new physical process be? Hagelstein considers this question and expounds on the role of optical phonons:

The new physical process is that which gives rise to the excess heat in the first place. I think that's where the nonlinearity is most likely coming about. Again, it's really hard to see how you're going to get a mixing near the surface that's going to give rise to new electromagnetic frequencies through anything but a very, very powerful version of surface-enhanced Raman. And at the moment, again, I don't think it's been settled one way or another and surface-enhanced Raman is, in fact, still on the table as a possibility. Because the intensities are so low, I consider it to be unlikely. But I cannot rule it out today.

Over a great many years, there's been a lot of discussion between myself and my colleagues as to how you investigate the role of optical phonons. For example, at MIT I did experiments where I tried to see the Raman shoulder associated with the optical phonon absorption or emission of helium-neon on the surface of palladium and other metals. In association with the DARPA contract, we worked with Keith Nelson at MIT, who actually did experiments using much fancier equipment than anything I had available, to stimulate the surface and look for the Raman side bands as a way to try to study creation, generation and emission, Raman emission associated with these optical phonon modes. Those experiments were unsuccessful. We talked about using free electron lasers that can provide a source of terahertz radiation. In fact, we contacted some laboratories that hosted very intense, powerful free electron lasers, and discovered that they didn't want to work with us because cold fusion is "tainted." In association with trying to put together proposals, we identified five different labs that hosted terahertz free electron lasers and initially the inquiries were met by a slammed door.

Of course, interaction of intense terahertz beams with metal surfaces by itself would be a very interesting and probably very rich area of condensed matter physics, but the fact that under discussion was palladium and deuterium was enough to make things not go forward. But I think that using a terahertz source to stimulate the surface is a good idea. I guess that we had decided early on, although to this day I don't believe I've seen an absorption curve of water in the terahertz region to make this decision on. I suspect that water's not very transparent in the terahertz region. So in principle if you're going to use terahertz radiation to stimulate the surface, either you have to minimize the amount of electrolyte between the surface and your source or else get a vacuum version of the experiment going on, or work with an intense source so that you can just eat the absorption loss in the water. Nevertheless, I think that using a direct terahertz source on the surface of a Fleischmann-Pons type experiment is an incredibly important thing to study, especially doubly important now in light of the measurements of Letts' lab.

Hagelstein was asked what kind of experiment he suggests to test the phenomenon directly:

One approach is to use terahertz-free electron lasers. Another approach is to use solid-state terahertz lasers. There are terahertz cascade lasers that are diode type lasers. This technology is potentially available in the lab these days and for the price of a collaboration with somebody working on such lasers, you could actually get sources that could be worked with more simply and more efficiently. The big labs that host the terahertz-free electron lasers wouldn't have to have any concern about their reputations being besmirched.

I think diode lasers are available in the laboratory. I think they may not yet be available commercially. There's Lincoln Laboratories and there's the lab at MIT, the person is Qing Hu, who works on terahertz lasers. There's other universities that work on terahertz lasers as well. So they definitely exist. And with respect to whether they're commercially available or not, a few years ago we researched this and couldn't find any suppliers, but that's a few years ago. I don't know what the situation these days is, but it's worth looking into again and seeing if anybody's marketing them. They might be.

If you're going to get into this, you want to be able to scan terahertz radiation. You want to go from like 3 terahertz to 30 terahertz or so and you need to be careful about polarization. You're trying to couple to compressional modes, so if you just come in normal to the surface, unless your surface is very non-uniform you're not going to come up with compressional modes very well.

It is understood that with the wrong polarization, energy is not transferred. Peter Hagelstein explained that what happens is even more interesting:

It's extremely interesting. According to my ideas and models these days, the coupling for one step of the process that's involved in making excess heat is specific to compressional polarizations. It's actually pretty easy to couple to transverse modes, which are not longitudinal and hence not compressional. In an early laser stimulation experiment I was involved in, we weren't aware of this. I mean, this was before the more recent models that clarified things theoretically and before realizations were published by ENEA/Frascati that studied the polarization dependence of the laser coming in. What they found is that if they came in at a shallow angle and had the electric field polarization going into the metal deuteride, that was the polarization that could give rise to excess heat, whereas if they came in with the other polarization transfers to the surface that that polarization didn't give rise to any excess heat effect. Now we went back and hassled Letts and Cravens about it and they said that's exactly what they saw in their initial experiments but they didn't recognize it and determine which polarizations were important. They saw that polarization was important, but it was Violante's group that actually figured out which polarization made a difference and they were studying the different plasmon modes that the light did couple to in trying to figure out how to best couple to. So that the answer is that you can couple to either polarization inside the metal deuteride perfectly well, it's just that one of the polarizations is alive and one of the polarizations is dead. One works, one doesn't. That's really important, as far as I am concerned. It's extremely important. It tells you something about mechanism, namely, I am really key on experimental results which feed into theory that tell you something about how things work and this polarization dependence is a very important piece of the puzzle which experiment has relatively recently clarified.

There's probably twenty different experiments, in my view, that shed light on physical mechanism. Namely, at the last conference I got my arm twisted to give a talk on input of theory to experiment. Theoretical ideas are pretty much useless until they make a connection with an experimental result in a laboratory. And at the moment I believe that there's on the order of twenty different experimental results which tell you something about theory or could be used to support a theoretical statement. The laser stimulation by itself counts as one and those beat laser experiments count as another.

The continuation of the oral histories provides details of the experimentation, time and work involved in these aspects of condensed matter nuclear science. Dennis Cravens states that when it comes to getting work done in this field, "All kinds of stupid little things can go wrong and we've tried them all. There are times where you're out of business for a year or two and then you get stuff. You have to get a long range view and appreciation for it. Tenacity is the biggest ingredient."

Dual Laser Stimulation of Excess Heat in a Fleischmann-Pons Experiment

Dennis G. Letts* and Peter L. Hagelstein**

This paper was presented at ICCF14 (14th International Conference on Condensed Matter Nuclear Science), held in Washington, D.C. in August 2008. A shortened version of the paper will be published in the forthcoming Proceedings.

Progress made since 2007 in the triggering of excess power by terahertz stimulation of deuterated palladium is reported. The stimulation was provided by tuning dual lasers to one of three specific beat frequencies corresponding to the known optical phonon frequencies of deuterated palladium (8, 15, 20 THz).

Results imply that optical phonons may be involved in the Fleischmann-Pons effect, giving preliminary support to Hagelstein's phonon theory. The importance of laser beam polarization is also demonstrated, confirming earlier work reported by Violante et al. and by Letts and Cravens.

1. Introduction

As is well known by now, Fleischmann, Pons and Hawkins first proposed the idea that nuclear reactions might be induced in deuterated palladium on March 23, 1989.[1] The idea was controversial from its inception and sought scientific legitimacy from experimental results, not from established nuclear theory.

The experiments of Fleischmann and Pons showed electrode power densities and figures of merit normally associated with nuclear reactions, but few indications of the required nuclear ash. Their claim for nuclear reactions induced in the solid state at room temperature rested mainly on the fact that known chemistry could not yield the observed power and energy densities. Their 1989 nuclear claim would take several years to confirm experimentally.

In 1993 Miles and his collaborators reported the presence of ^{4}He in roughly the amount expected from D-D fusion and was commensurate with the excess power observed.[2] ^{4}He and excess power were also reported by Apicella *et al.* in 2004 at ICCF11[3] and by Violante *et al.* in 2005 at ICCF12.[4] There is now enough diverse experimental evidence to suggest that nuclear reactions really are possible in the solid state at room temperature.

1.1. Hagelstein's Phonon Theory

From the beginning, Hagelstein pondered the role of phonons in the solid state fusion of deuterium. His early work proposed a coherent phonon-photon mechanism, as described in Reference 5 and presented at the ASME meeting in December 1989.[5] Hagelstein proposed that the large nuclear energy release might be radiated into the lattice phonon field "one phonon at a time." By 2003, Hagelstein modified the details of his phonon theory to favor a phonon exchange process, as reported in his ICCF10 conference paper,[6] but the role of phonon coupling remained an important part of his overall theory. In his Conjecture 4, page 847 of Reference 6, he states that, "Anomalies in metal deuterides are stimulated by strong phonon excitation." In 2007 Hagelstein refined his phonon theory further in "Progress Toward a Theory for Excess Heat in Metal Deuterides"[7]; in this paper, Hagelstein proposed once again that phonons are involved in the transfer of 24 MeV quanta from the D+D → ^{4}He reaction to the lattice in the form of heat. The transfer is conjectured to be accomplished in a series of smaller quanta mediated by phonons. Hagelstein suggests that the optical phonon modes are the most likely candidates—especially the modes with low group velocity typically located at the edge of an optical phonon mode. In palladium deuteride, the band edges occur near 8 and 15-16 THz.[8,14-15]

2. Experiment Meets Theory

The work discussed in this paper began in March 2007 as a series of experiments conducted by Letts and Cravens in collaboration with Hagelstein. Our goal was to see if an experimental connection could be made with the phonon aspects of Hagelstein's theory. Hagelstein's theory is complex but our experimental approach was simple—we used dual lasers in beat mode to create beat frequencies near the three known optical phonon modes for palladium deuteride. We had data for palladium hydride but guessed that the deuteride phonon modes would be similar. Figure 1 was our guide for all of the initial experiments and is still in use as more data is accumulated in continuing experiments. Before experiments were run, Hagelstein predicted that the edges of the optical phonon band would be the best candidates for stimulation to produce excess power. This region is where low group-velocity compressional phonon modes exist. This is consistent with our observations near 8 and 15 THz; but the response near 20 THz requires an alternate explanation. Perhaps the simplest conjecture for this higher frequency response is proton contamination, which might be expected to produce a zero-group velocity band edge near 20 THz. Such a conjecture may be confirmed in future experiments in which the proton contamination is better controlled, and from phonon band calculations done for palladium deuteride contaminated with hydrogen.

2.1. Instrumentation and Calorimetry

This work covers 19 tests from 3 cells; two time periods are involved—March 2007 to May 2007 and then April 2008 to the present. The goal of this work is the creation of a beat frequency versus excess power graph to determine if stimulation of the three optical phonon modes of palladium deuteride leads to the observation of excess power. If phonon modes are involved with the production of anomalous heat, then such a graph might show elevated excess power production clustered about the three phonon mode frequencies.

All experiments were conducted in Austin, Texas. The experimental setup is shown in Figure 2. Isoperabolic

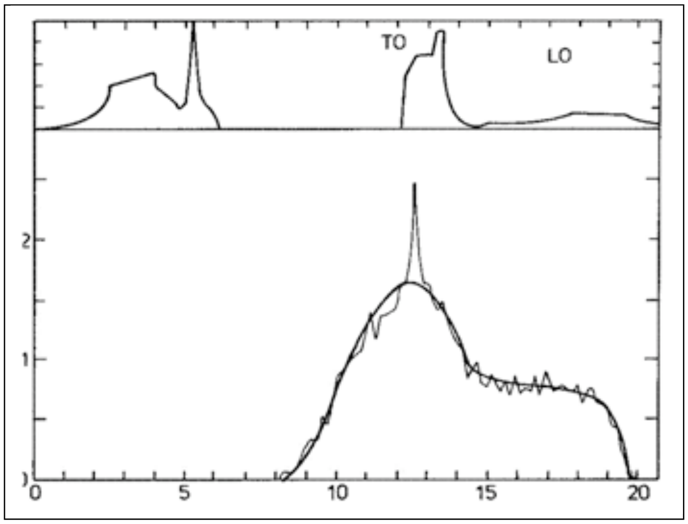

Figure 1. The PdH density of states from Reference 8, where the horizontal axis is frequency in THz.

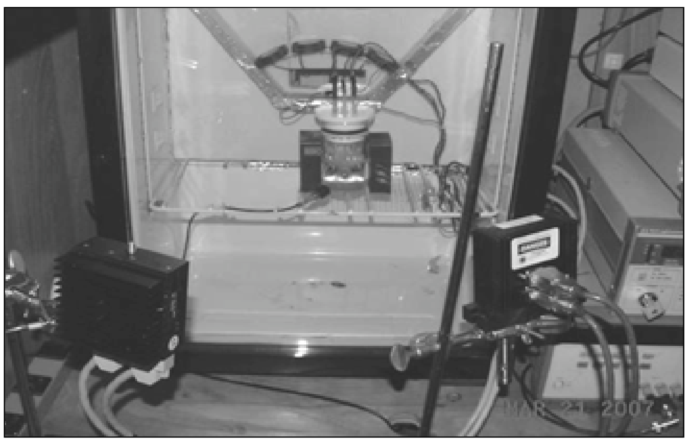

Figure 1a. Cell in temperature controlled enclosure stimulated by dual lasers. Note optical spectrum analyzer detector in front of cell to check laser calibration during the experiment.

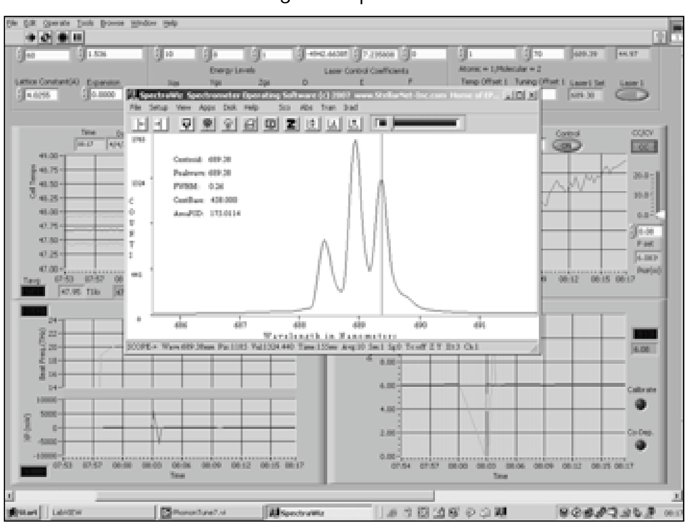

Figure 1b. Measurement of the laser spectrum as the experiment proceeds. Laser wavelength measurements have a precision of about 0.25 nm.

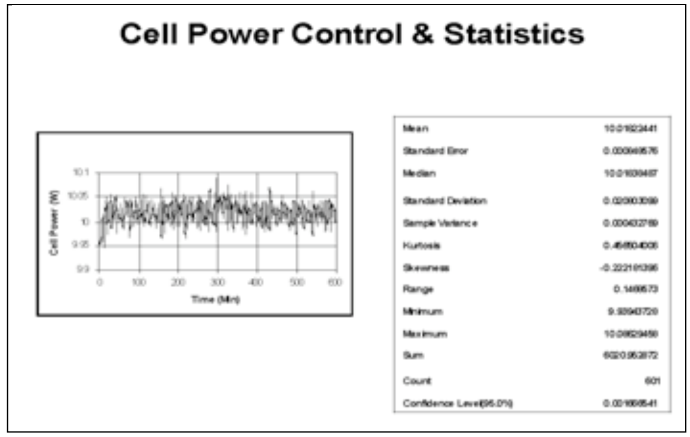

Figure 1c. Cell power is held constant with a variation of ±0.02 watts. Current is held constant and voltage is varied to maintain the power set point.

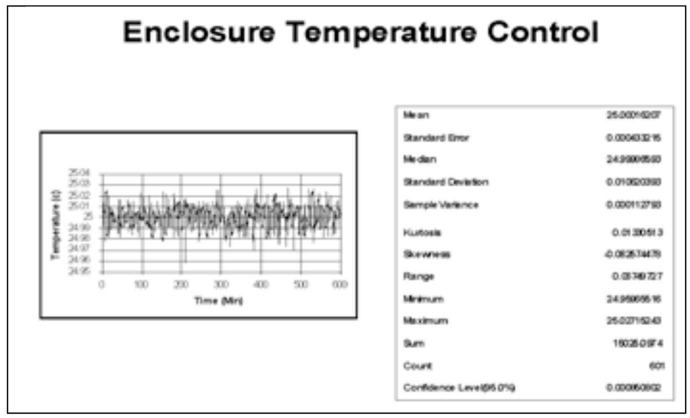

Figure 1d. The cell's ambient temperature variation is about ±0.02°C.

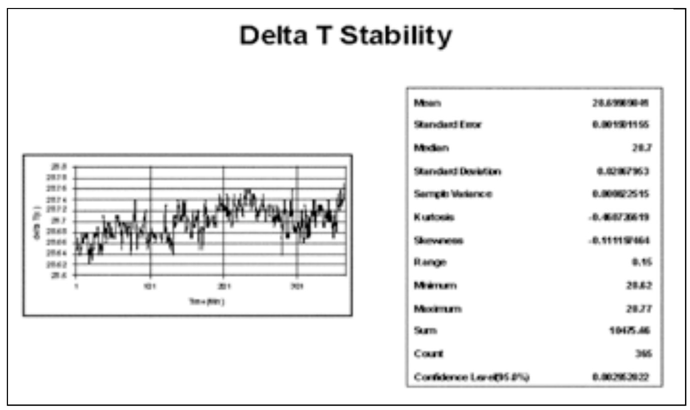

Figure 1e. Tight cell power and ambient temperature control results in a stable temperature difference between the cell and its environment.

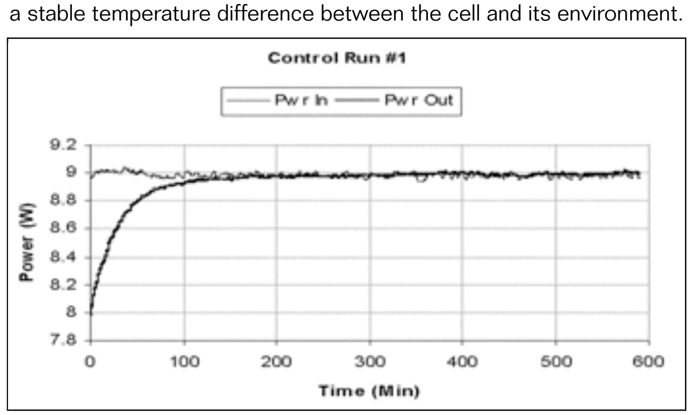

Figure 1f. The isoperabolic calorimeter demonstrates good long-term stability when the cell is not producing excess power. Most experiments run for 10 hours or less, so the calorimeter is stable over the time scale of the experiments.

calorimetry was used on all tests. Lab temperature is fairly stable at 26 ± 1°C. The temperature enclosure (the black box shown in Figure 2) is controlled by Labview and held at 25 ± 0.03°C. Cell power is provided by a digital HP E3632A power supply. Cell power is held constant by Labview to within 10 mW. Cell power is typically 7 - 10 watts with current up to 1.25 to 1.5 amps. Current and cell power are held constant.

Figure 2. A typical equipment setup for this work; data is collected by a Labview-controlled Agilent 34970A and all data is displayed and stored on a PC.

Figure 3. Typical cell configuration is shown. Magnets as shown provide a 700 gauss field across the cathode face. In the foreground is the optical spectrometer detector used in tuning the lasers within 0.25 nm. Air is vigorously stirred within the temperature-controlled enclosure.

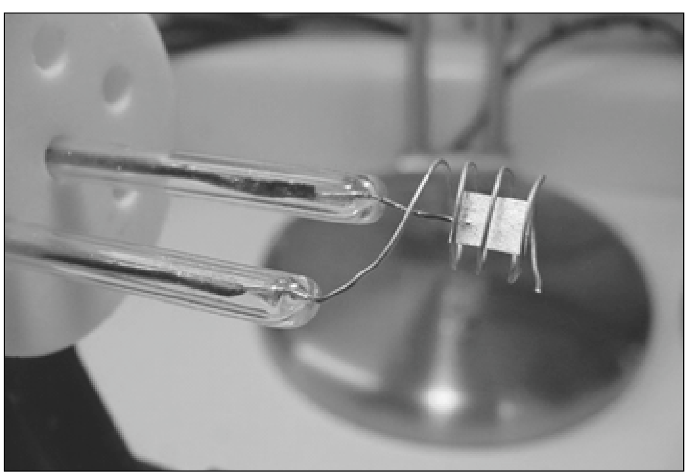

Figure 4. A typical cathode and anode assembly; cathode is typically a Pd plate 8 mm x 10 mm x 0.2 mm with a Pt anode coil. Pt wires are sealed inside soft glass 5 mm tubes. Cell lid is Teflon.

See Figures 1a-1f for experimental details.

Lasers are controlled by an ILX 3722B and an Optima laser controller. The laser controllers typically keep the lasers within .25 nm of tuning set point. Data is displayed on a PC and all data is recorded to a spreadsheet. The lasers are kept in tune by checking with an optical spectrometer (Figure 1b).

2.2. Experimental

A typical cell is shown in Figure 3. Two thermistors record cell temperature. One probe is placed slightly above the cathode and one probe is placed slightly below the cathode. The average of the two probes is compared to enclosure temperature in computing excess power. Cell is not mechanically stirred. 100 grams of 0.5M LIOD is used as electrolyte. Platinum coated alumina pellets are used as recombiners, providing a closed electrochemical cell.

The work reported in this paper began on March 20, 2007 with experiment 662N (see Table 1); the cathode stock was palladium made from a billet provided by Scott Little of Austin, Texas. The palladium was taken from a large palladium target from Texas Nuclear. The target was unused and of unknown purity but thought to be at least 0.995. The cathode was prepared following a 17 step protocol described previously.[9] The cathode was cold-rolled using a 90 degree rota-

Table 1. 49 data points from 19 experiments taken from three cells tested in 2007 and 2008. Cells were tested at ~50-60°C.

No.	Experiment	Date	Freq. (THz)	XP (mW)
1	662n	3/25/2007	3.12	0
2	662n	3/25/2007	3.81	0
3	662n	3/25/2007	4.50	0
4	662n	3/25/2007	5.18	0
5	662n	3/25/2007	5.86	0
6	662n	3/25/2007	6.33	15
7	662n	3/25/2007	6.80	25
8	662i	3/21/2007	8.00	80
9	662g	3/20/2007	8.30	125
10	662k	3/22/2007	8.40	130
11	662i(2)	3/21/2007	8.48	80
12	662i(2)	3/21/2007	8.61	90
13	662i(2)	3/21/2007	9.06	60
14	662i(2)	3/21/2007	9.65	40
15	662i(2)	3/21/2007	10.95	30
16	662i(2)	3/21/2007	12.31	40
17	662i	3/21/2007	13.62	0
18	662i(2)	3/21/2007	13.68	20
19	662i	3/21/2007	14.23	70
20	662i(2)	3/21/2007	14.36	20
21	662f1	4/13/2007	14.50	140
22	662j1	4/15/2007	14.70	70
23	662w	3/31/2007	14.70	70
24	662y	4/2/2007	14.70	120
25	662a1	4/4/2007	14.70	80
26	662j1	4/15/2007	14.70	150
27	662s1	4/23/2007	14.70	66
28	669a1	5/8/2008	14.75	350
29	662i(2)	3/21/2007	14.88	80
30	662f1	4/13/2007	15.20	160
31	662a1	4/4/2007	15.30	200
32	662f1	4/13/2007	15.82	140
33	662b2	5/5/2007	15.90	50
34	662w	3/31/2007	16.02	40
35	662f1	4/13/2007	16.45	80
36	662f1	4/13/2007	17.09	0
37	662i2	3/21/2007	18.23	0
38	662c2	5/5/2007	18.40	100
39	662w	3/31/2007	18.56	70
40	662t1	4/25/2007	18.80	50
41	662i2	3/21/2007	18.87	0
42	670a	6/6/2008	19.28	100
43	662o	3/25/2007	19.40	200
44	662i2	3/21/2007	19.50	30
45	669u	4/30/2008	20.00	250
46	662x1	4/29/2007	20.50	250
47	662o2	5/17/2007	20.70	300
48	662i2	3/21/2007	21.40	130
49	669a1	5/8/2008	22.11	0

tion with each pass. This is thought to minimize stress build-up in any one direction. The cathode was 5 x 12 x 0.2 mm and was loaded at 75 mA/cm^2 for 120 hours. Electrolyte was 100 g of 0.5M LIOD. Excess power was not produced until gold was plated onto the cathode, magnets were in place around cell at ~700 gauss and dual lasers were applied with a beat frequency of 8.26 THz. Cell resistance also declined sharply as the Fleischmann-Pons heat effect appeared. Cell power dipped slightly just before cell temperature increased, as was reported in the Guruswamy experiment[10] and the SRI experiment[11] in 1990. See Figure 5.

The dual lasers were tuned to 664.59 nm and 677.74 nm for a beat frequency of 8.75 THz and held for 114 minutes. There was no thermal response. Lasers were then tuned to 664.61 nm and 677.01 nm for a beat frequency of 8.26 THz. Cell temperature increased immediately and cell resistance declined. The cell's thermal response to beat frequency is shown in Figure 6.

2.2.1. The Null Experiment

In the first five days of this campaign, excess power continued to be observed around 8 THz. By March 25, 2007 we began to wonder if we could produce a null result by scanning the beat frequency over the range 3 to 7 THz, stopping just short of the 8 THz optical phonon frequency for $PdD_{0.9}$. The lasers were tuned to 3.12 THz and scanned to 6.8 THz in steps of 0.7 THz with a hold time of one hour at each step. Hagelstein's phonon theory suggested that only optical phonons are involved in producing the heat producing Fleischmann-Pons effect—so it was important to *not* see an exothermic response when the beat frequency was below 8 THz. This was observed in experiment 662N, shown in Figure 7.

2.2.2. Other Phonon Modes

By March 25, 2007 it appeared that the lowest $PdD_{0.9}$ phonon mode had been fairly well explored around 8 THz. We then wondered if either of the two other known palladium deuteride phonon modes would be thermally responsive. Experiment 662O was started with lasers tuned to 632.8 nm and 659.83 nm to produce a beat frequency of 19.4 THz. Cell temperature began to increase within 20 minutes of turning on the dual lasers. Cell temperature increased 0.8 degrees over 200 minutes to produce an excess power signal of 200 mW. The lasers were left on for about 10 hours and excess power continued to be observed. After about 10 hours the lasers were turned off and excess power declined to ~0 W. See Figure 8.

Figure 8 shows that a beat frequency stimulation of 19.40 THz triggers an exothermic reaction in palladium deuteride. This provides a comforting connection with some established physics in that the literature[8] shows the upper edge of the highest frequency optical phonon mode in PdH to be

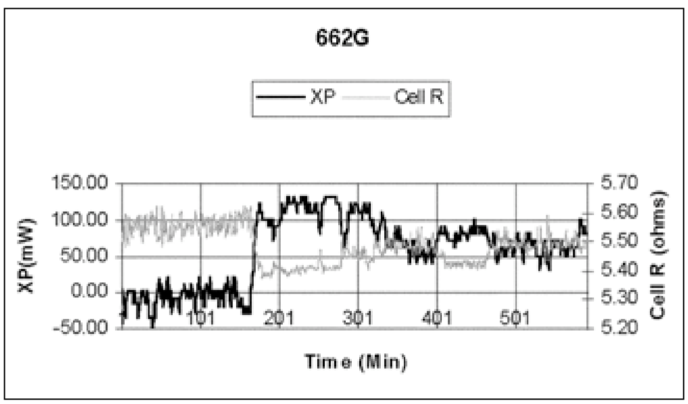

Figure 5. The apparent small endothermic response that precedes the exothermic response has been observed in other experiments since 1990. Cell resistance also declined quickly as the cell triggered.

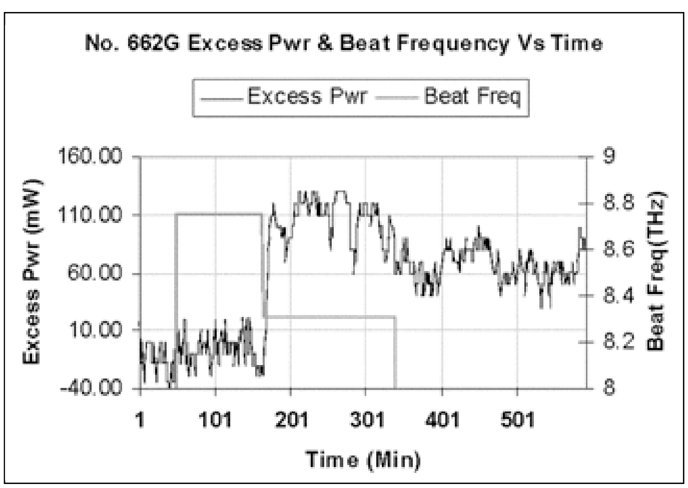

Figure 6. Plot shows the lack of a thermal response at 8.75 THz followed by an immediate thermal response at 8.26 THz. This highly selective frequency sensitivity was observed throughout this experimental campaign.

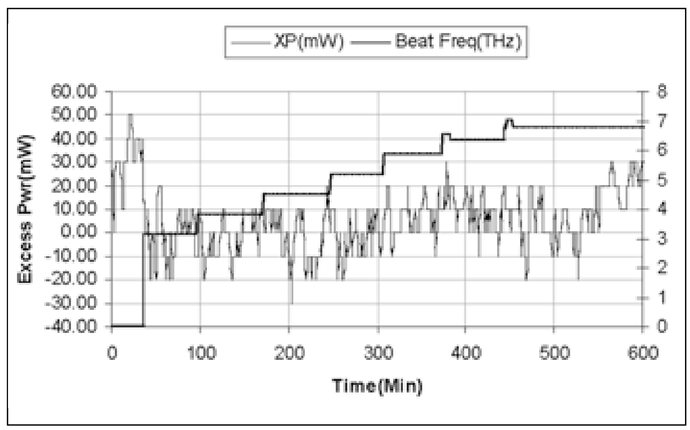

Figure 7. This shows that no excess power is produced when the beat frequency was scanned from 3 - 7 THz. This graph also shows that the calorimeter is stable over 10 hours with a power variation ~10 mW. Laser power of ~40 mW is not seen by the calorimeter when the beat frequency is off resonance.

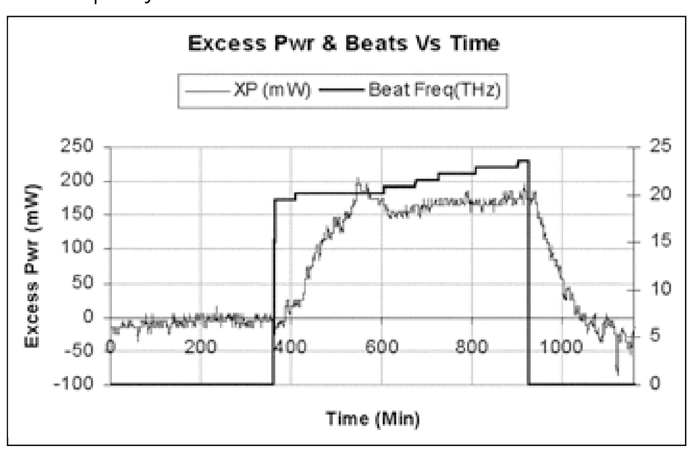

Figure 8. This graph demonstrates that the highest frequency palladium deuteride phonon mode at ~20 THz is also response to beat frequency stimulation.

near 20 THz. See Figure 1.

The middle optical phonon frequency near 15 THz was tested from April 2 to April 23, 2007 in a series of nine experiments under various conditions of cell temperature and current densities. All nine experiments produced excess power, ranging from 66 mW to 200 mW. Figure 9 shows the thermal response of the cell to stimulation near 15 THz.

As Figure 9 shows, the excess power signal strengthened as the beat frequency approached 15 THz. However, the excess power signal saturated after 5 hours and returned to zero, even with the dual lasers on.

2.2.3. Laser Polarization Angle

In 2003 Letts and Cravens reported that single laser beam polarization seemed to be an important factor in stimulating deuterated palladium cathodes.[9] When the laser beam polarization was at a 90 degree angle with respect to external magnetic field lines, excess power production was maximized. When the polarization angle was at a zero angle with respect to the external magnetic field lines, excess power production stopped.

In 2004, Apicella and his collaborators advanced our understanding of the importance of laser polarization with their paper, "Some Recent Results at ENEA."[3] In this paper, P polarization was shown to be enabling for the production of excess power, while the S polarization was not. The P polarization effect as reported by Apicella was with respect to the cathode surface only, not an external magnet field as reported by Letts and Cravens.[9] Apicella and his collaborators proposed that the P polarization was effective in triggering excess power due to its ability to create charge separation on the cathode surface. P polarization means that some or all components of the electric field of the laser beam are perpendicular to the cathode plane.

2.2.4. Optical Phonon Mode Bandwidth

After a few scans of the beat frequencies across the three optical phonon mode edges, it became apparent there was a very narrow band that would trigger excess power. It quickly became a goal to delineate the bandwidth of the sensitive regions. One example of the left edge of the 20 THz sensitive region is seen in experiment 662t1, shown in Figure 10.

The strategy was to begin scanning the beat frequency below the suspected left edge of the 20 THz optical phonon mode. In Figure 10, the scan started at 18.31 THz and advanced in steps of 0.16 THz. When the scan reached 18.79 THz, an exothermic cathode response was observed as cell temperature increased sharply. The edges of the optical

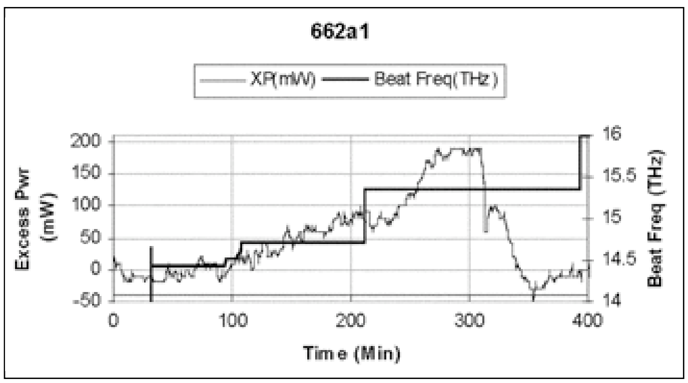

Figure 9. The cell responds thermally to the middle optical phonon frequency near 15 THz. Excess power increases as the beat frequency approaches 15 THz.

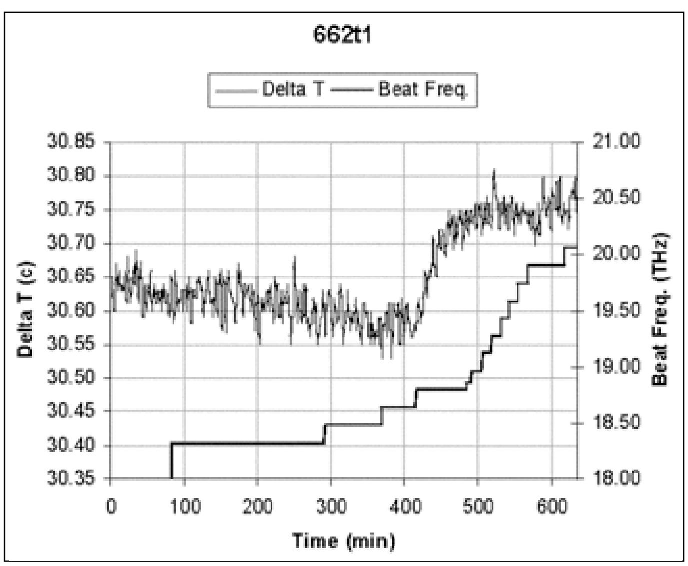

Figure 10. The possible left edge of the optical phonon mode that triggers an exothermic reaction in palladium deuteride.

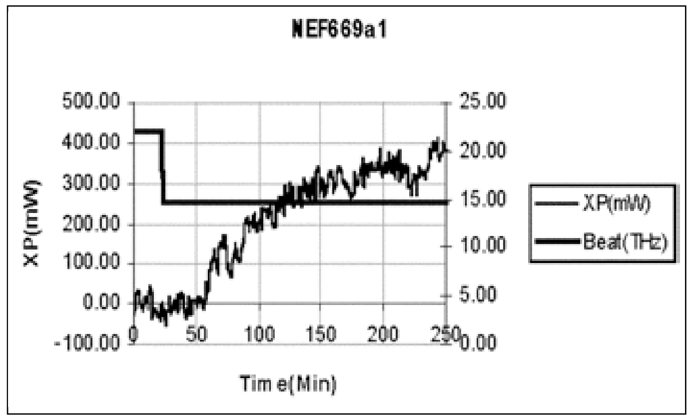

Figure 10a. A beat frequency of 22.11 does not trigger excess power, suggesting that the right edge of the highest frequency optical phonon mode is slightly to the right of 22 THz.

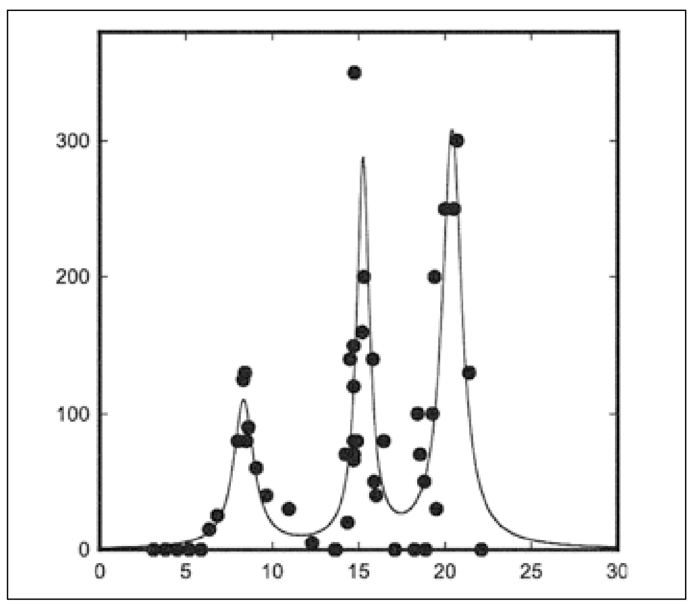

Figure 10b. Three specific triggering beat frequencies. Vertical scale is XP in mW. Also shown is a curve from a least squares fit to a sum of three Lorentzians $\Sigma_j\, c_j/[(f-f_j)^2+y_j^2]$, where the center frequences f_j and broadening parameters y_j are indicated.

phonon bands may vary with loading, cell temperature or other factors not yet known. This particular experiment showed the left edge to be at 18.79 THz. Two experiments tested 21.40 THz and 21.70 THz and both tests triggered excess power, suggesting that the band edge is above 22 THz. Figure 10a shows that the right edge of the 20 THz mode appears to be near 22.11 THz.

Using a similar strategy for the other two optical phonon modes lead to a series of graphs that identified the edges of all three modes for deuterated palladium. Figure 10b shows a composite graph of all three modes. The mode near 8 THz is centered at 8.3 THz and has a mode width from 7 - 9 THz at its half maximum. The mode near 15 THz is centered at 15.3 THz and has a mode width from 14 - 16 THz. The mode near 20 THz is centered at 20.5 THz and has a mode width from 18 - 21 THz.

Data from the left side of each optical phonon mode was put into Excel and an excess power versus frequency correlation was performed. The results are shown in Figure 10c.

Various researchers have reported that excess power production is often larger when cell operating temperature is elevated. Hagelstein proposed that this is due to the energy required for the formation of molecular D_2 near Pd vacancies. Our results are qualitatively consistent with this, as increasing cell operating temperature from 62°C to 73°C increased excess power production from 225 mW to 900 mW, a 400% increase. This increase of excess power with temperature is a larger effect than has been reported by Storms (the activation energy in Storms experiment was meV, while the activation energy that would correspond to

Correlation of Excess Power and Frequency

Frequency Range	Correlation
3-8 THz	.85
13-15 THz	.82
17-21 THz	.83

Note: to get meaningful statistics, only data from the left edge of the mode to its peak was correlated.

Figure 10c. There is a high degree of correlation between excess power and beat frequency.

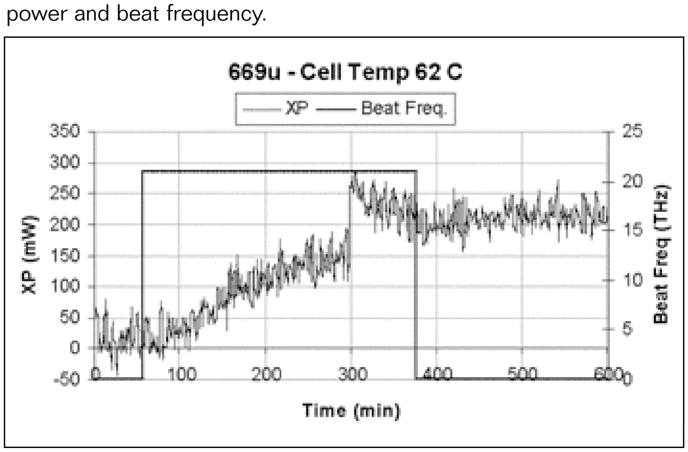

Figure 11. A 21 THz beat frequency triggers excess power of 225 mW at a cell temperature of 62°C.

the present result is 1.5 eV). Results from experiments 669u and 669v are shown in Figures 11 and 12.

3. Improved Reproducibility with Dual Laser Stimulation

The use of dual lasers to produce beat frequency stimulation near 8, 15 or 20 THz has resulted in high reproducibility (~90%); we don't know if this is a permanent benefit, but for the last year cathodes made from several palladium sources have produced excess power when stimulated at one of the mode frequencies reported in this paper. An example of this reproducibility is shown in Figure 13.

4. Discussion

Can these experiments connect with any known physics? On the face of it, the observation of a thermal response at the specific beat frequencies 8, 15 and 20 THz [shown in Figure 10(b)] combined with the observed dependence on polarization implicates compressional optical phonon modes in PdD as participating in the physical process responsible for excess heat production in the Fleischmann-Pons excess heat effect. The lower two specific beat frequencies observed (8 and 15 THz) are consistent with the interpretation of optical phonon modes with low group velocity in PdD. The higher frequency response (20 THz) represents a conundrum for a PdD explanation, since it lies above the LO band edge. One possible resolution of this is to assume that

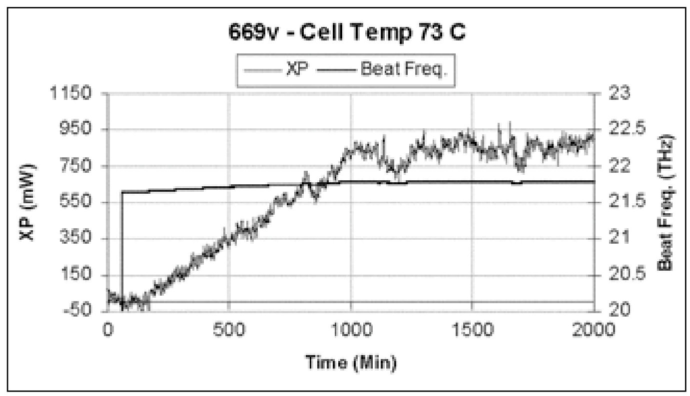

Figure 12. The cell triggers at about the same beat frequency as 669u but the thermal response at 73°C is four times greater than at 62°C.

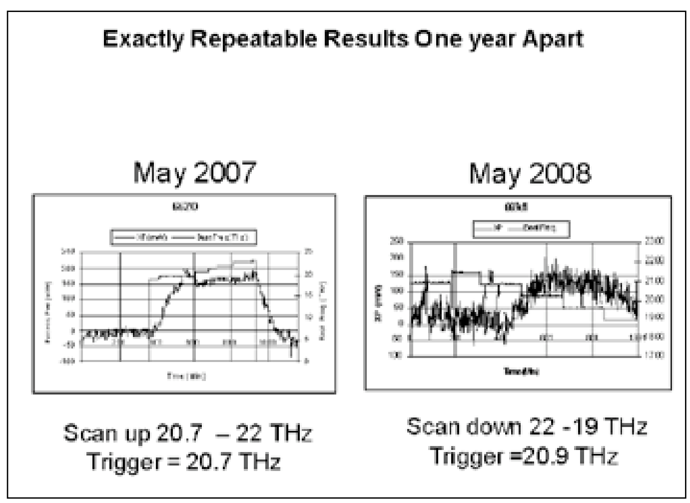

Figure 13. Two experiments were conducted one year apart. When stimulated with beat frequencies near the 20 THz optical phonon mode for palladium deuteride, both cells responded nearly identically. Excess power produced in both cells was ~175 mW.

H is present as a significant impurity, and that 20 THz is due to an LO band edge associated with hydrogen in mixed PdDxHy. Whether this is the case or not can be determined in future experiments where the hydrogen content of the heavy water is better controlled. Additionally, our results motivate theoretical studies with mixed hydrogen and deuterium loading to verify under what conditions, if any, a splitting of the LO band near the band edge occurs.

Although gold does not form a hydride, it should be noted that the gold overlayer has not been ruled out as the site for the exothermic reactions observed during this campaign. It has been conjectured that the reaction site is in the palladium deuteride, but this has not been proven. This will be addressed in experiments to be conducted. The D/Pd ratio was not measured during these experiments. The cathode was loaded for 120 hours at a low current density and then loaded for an additional 24 hours at 1 amp. This loading protocol usually produces a cathode that is responsive to dual laser stimulation.

While the present work is suggestive, it is not complete—work is underway now to collect more data points between 3 and 25 THz in the coming year. It is hoped that this work will give theorists some useful data and motivate our experimentalist colleagues to collaborate more closely with theorists to advance our understanding of Condensed Matter Nuclear Science.

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About the Authors

Dennis Letts has been working on electrochemical heat systems since 1990. He has built over 600 cells to study deuterium in palladium systems. He has pioneered the use of radio frequencies and lasers to trigger exothermic events in D-Pd electrochemical cells.

Dr. Peter Hagelstein's recent efforts have included the invention of semiconductor technology that could allow efficient, affordable production of electricity from a variety of energy sources, as well as continuing investigations of low-energy nuclear reactions. Peter chaired the Tenth International Conference on Cold Fusion (ICCF10) in 2003.

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Paradigm of Cold Fusion:
A Perspective on Scientific Philosophy

Wu-Shou Zhang*

Abstract

Technical differences between cold fusion and hot fusion, and scientific distinctions between low energy nuclear reactions (LENR) and classical nuclear reactions, are presented. It is pointed out that LENR is realized through interactions of multi-scale coupling; it is characterized by nonlinearity, non-equilibrium and complexity. The techniques of cold fusion are small-scale, distributed and flexible. All of these characteristics are consistent with trends of contemporary science and technology, whereas hot fusion departs from these tendencies.

It is well known that the truth of cold fusion has been discredited or ignored in the science world since its infancy until now. Obviously, the poor reproducibility of cold fusion experiments is mostly to blame for this widespread, long-lasting denial. However, there is another important reason: the fact that the arbitrary refusal or neglect by most of the skeptics has prevented them from realizing and reconsidering new findings and progress made by the cold fusioners. While they remained on the sidelines, a new paradigm of science and technology has developed along with the evolution of cold fusion research. This has monumental significance regarding the explanation and understanding of cold fusion. It was their clinging and depending on the old paradigm, their ignorance and refusal to acknowledge the new paradigm that trapped cold fusion R&D in so many scientific and political difficulties. The history of cold fusion includes the failure of the old paradigm, and the development and success of the new one. The author deeply feels an obligation to write this article to present to the scientific community this paradigm, with the fervent wish that cold fusion will be re-evaluated with a more objective attitude rather than shunned away without the due responsibility, even in a time when the experimental facts are not yet clear and theoretical explanations vary.

Differences between cold fusion and hot fusion are summarized in Table I. The most important difference is the temperature. The working temperature of cold fusion is from ambient (the characteristic energy is 0.03 eV) to 200°C (0.05 eV). Even in gas-discharge or beam-target systems, the particle energy is only a few keV. These temperatures or energies are 6 orders less than the hot fusion values, which is on the order of 100 MK (10 keV) for D-T fusion, the easiest nuclear fusion.

There is an analogy between nuclear fusion and chemical reactions. Hot fusion resembles an inorganic chemical reaction. Increasing temperature and pressure to extremely high values are traditional procedures to accelerate reaction rates. Cold fusion is similar to a biochemical reaction, the reaction rate of which is modified through choice of catalyst (enzyme), control of the pH, and other subtle conditions. This similarity is also embodied in the spectrum of products. Hot fusion products are simple, *e.g.* the ashes of D-T fusion are helium and neutron, in addition to tritium and proton for D-D fusion. The products of cold fusion are more complicated—they range from helium (the product of D-D fusion),[1] silver (the product of a reaction involving a palladium cathode),[2] to praseodymium and molybdenum (products of multi-body reaction involving surface additives, *i.e.* Cs + 4D → Pr, Sr + 4D → Mo),[3] and many sorts of nucleons with Miley-type spectra[4] even for the Pd-D system, the original and simplest one in this field. Product spectra depend on reactants, temperature, impurities and other conditions not clear yet today. The way these elements affect the reaction rate is something like the way yeasts affect the flavor of wine during brewing, *i.e.* different yeasts give different tastes. Another merit of cold fusion is that the nuclear products are stable nucleons only with trace amount of tritium (in ppb order) and neutron (in ppt order); this is in contrast to the radioactive nucleons, charged particles, neutrons or γ rays in hot fusion or nuclear fission. The green and safety nature of cold fusion is similar to that of biochemistry too. And these also are the trend of R&D of modern chemical engineering, materials and renewable energy.

Another remarkable feature of cold fusion is on engineering and organizing. Hot fusion requires the involvement of many scientists and technicians from various backgrounds cooperating together for one purpose, as done in the Manhattan Project seven decades ago or the Apollo Project four decades ago. A hot fusion reactor is always a huge device with complicated design. Its operation depends on orders from the central control system. Its electrical supply, radiation protection and daily maintenance must be carefully arranged and strictly executed. It is no surprise that this sort of system has inherent fragility similar to that of a fission plant (*e.g.*, the Chernobyl reactor in Ukraine) or a big dam (*e.g.*, Three Gorges Dam in China). Geological and meteorological disasters, management defects and wars all may result in the sudden interruption or breakdown of the regional electricity supply, or even horrible disasters such as happened before. In a word, big plants make for big trouble.

For cold fusion, all of these issues are no longer problems. Its small scale makes the operation, management and commercialization very easy by comparison. The cooperation in R&D of cold fusion depends only on the free will of researchers and manufacturers. Application of cold fusion will be similar to that of solar and wind energies, *i.e.* every family, community or office will be the consumer and manufacturer at the same time; and the electrical system will be the network structure, in which trouble in any unit does not affect the stability of the overall system. In light of this, it is reasonable to conclude that hot fusion is the technology of the industrial era and cold fusion will be the technology of the web era.

The technical, administrative and commercial characteristics of these two types of fusion will determine the fate of their futures: hot fusion vs. cold fusion in the energy market will be similar to the story of the Iridium Satellite Constellation vs. the Global System for Mobile communications (GSM) in the wireless communication market, which happened about one decade ago.

Because hot fusion falls away from the evolving direction of modern science, technology, society and economy, the author is sure that this Cold War's sci-tech plot has no future. Its end is only a matter of time and price, *i.e.* human beings (specifically governments of some large countries) will not pay much more money on this dead technology. This technology will not be the first loser. Similar examples were the Analog High-Definition Television Project[5] and the Robot Project[6] in Japan. The phenomenological reasons for these two lessons were the mistakes of industrial decision-making; the deep reason was the limitation of sci-tech paradigm at that time.

The terms low energy nuclear reaction (LENR) and condensed matter nuclear science (CMNS) are gradually being accepted to replace cold fusion in describing the related phenomena in a broader sense. As a scientific field, LENR differs from classical nuclear reactions in many ways, as listed in Table I and especially in Table II. First of all, beam interactions with target are the classic method to study nuclear physics and particle physics since Rutherford established the planetary model of the atom by electron backscattering. As the research in these fields evolve, beam energies become higher and higher, accelerators grow bigger and bigger, seemingly with no end in sight. A sidenote of this mode is the term high energy physics, which has the same meaning as particle physics. A sharp contrast hence occurs, *i.e.* only the maximum accelerator on earth can be used to study the physical processes in the minimum scale in the universe. The explanatory fact is that some accelerators and their accessories are bigger than the extent of small cities, and building a new accelerator is a tremendous undertaking both economically and politically. However, the discovery of LENR interrupted this big-small mode. It needs only a tabletop device rather than town-size buildings to achieve the high energy process. The truth that a mini-size device produces something much larger than expected is unprecedented. For example, pyroelectric fusion can achieve D-D fusion with a temperature difference of only 40°C,[7] and laser wakefield can accelerate electrons to GeV (10^9 eV) on a scale of centimeters.[8] LENR is the leading method in this new tide of discoveries, although many scientists, especially physicists, do not understand that now. This wave is not only revolutionary but also as progressive as that which happened in the information industry, *e.g.* electronic storage and publication will end the ever growing need for library buildings. LENR will start a new era: an amateur can carry out particle experiments in his/her garage; national laboratories or research universities will not be the kings in high energy physics. Physics will begin far away from politics, thus ending their close relationship which was formed in World War II. This process also resembles the development of computers: the personal computer has replaced the workstation and mainframe computer as the mainstream; individuals and families rather than enterprises and governments have become the main customers.

Because nuclear reactions take place in scales below fm (10^{-15} m), while angstrom (10^{-10} m) and above are the scale of chemical reactions, nuclear reactions release/absorb energies from 1 to 10^2

Table 1. Technical comparisons between hot fusion and cold fusion.

	hot fusion	cold fusion
reaction condition	high temperature (10^8 K)	low temperature (300 to 500K)
stability requirement	avoid instability	requiring non-equilibrium
effects of complexity	pursue simplicity, avoid complexity	use complexity
products	radioactive nucleons	stable nucleons
sorts of product	few	abundant
form of released energy	radiations	heat
key discipline	high temperature plasma physics	inter-disciplinary, including chemistry, condensed matter physics, nuclear physics, etc.
number of technical routes	one or two (TOKMAK, ICF)	many
social traits of technology	artificial multi-tech integration, purposive design	self-organization, spontaneous evolution
management style	dictatorial & rigid	distributed & flexible
scale	big	small
safety	low	high
commercial investment	high cost & high risk, long period	low cost & low risk, short period

Table 2. Scientific comparisons between a classical nuclear reaction and a low energy nuclear reaction.

	classical nuclear reaction	low energy nuclear reaction
experimental equipment	accelerator	tabletop device
correlation between nuclear physics and chemistry	no	yes
chemistry analogy	inorganic chemistry	biochemistry
particle number involved	two-body reaction	multi-body reaction
mechanism	simple	complex
theoretical field	electromagnetism, quantum mechanics	quantum mechanics, corporative interaction in condensed matter
coherency	incoherent	coherent

MeV, while the chemical energy is around 1 eV, the irrelevance of chemical environments is the essential feature of nuclear reaction. In contrast to classical nuclear reactions, LENR depends not only strongly on temperature and reactant concentrations etc., as in chemistry but also on sample pretreatments as in metallurgy as discussed above. It means that the multi-scale coupling (from 10^{-10} m to 10^{-15} m) occurs in LENR. This coherent feature is same as that in nano-science and biological engineering, and LENR is far ahead in this direction.

Although nuclear reactions are quantum phenomenon, the key discipline of hot fusion is plasma physics, which is confined in classical mechanics (Newton-Maxwell paradigm). The simplicity, stability and certainty are both the basis and the purpose; complexity and instability should be avoided as far as possible. For example, at least 50 destructive instabilities occur in Magnetic Confinement Fusion (TOKAMAK)[9]; the Raleigh-Taylor instability is also fatal in Inertial Confinement Fusion (ICF). In contrast, non-equilibrium and instability are the necessary condition for LENR. The LENR mechanism is also related to the cooperative and complicated interactions in condensed matter, although exact processes are unknown today. These non-linear, non-equilibrium and complexity characteristics are the tendencies of modern science, *e.g.* chaos, dissipative structure, complex fluid (soft matter) and neuroscience, etc.

Two politics-science decisions can be cited as evidence of the present viewpoint in some sense. One is the cancellation of the Superconducting Super Collider project (SSC, $11 billion USD cost, $2 billion USD spent) in 1993.[10] Another is hot fusion. The largest thermonuclear reactor, International Thermonuclear Experimental Reactor (ITER), planned as early as in 1985, is now being constructed by China, Europe, India, Japan, Korea, Russia and the USA. It will cost 11 billion Euro. A meaningful decision is that the U.S. Congress passed a bill rejecting the payment of $149 million USD due for the project in December 2007.[11] Although most reviewers consider these two decisions as only finance issues, the author considers them to be signals of failure of the old paradigm in modern society.

One scholar once commented that the cancellation of SSC could be described as the extinction of a science dinosaur. Without doubt, the ITER is the technology dinosaur in the world now. The annihilation of dinosaurs on earth was unavoidable. Biological evolution was faster after that. For similar reasons, the dinosaurs of science and technology will encounter economic and political difficulties sooner or later, but human beings will not lose their ways to explore and utilize the nature. The problem with low energy nuclear reactions is whether human beings are willing to suppress their prejudices and learn what has been discovered during the past 20 years.

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About the Author

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RECENT PROGRESS IN CONDENSED MATTER NUCLEAR SCIENCE

Talbot Chubb

This report is an update on the physics/chemistry discipline known as Condensed Matter Nuclear Science (CMNS), also known as Low Energy Nuclear Reactions (LENR). The paper was prepared for Peter Wilhelm, Head of the Naval Center for Space Technology. Much has happened in the last seven years. CMNS started with the cold fusion research of Fleischmann and Pons, first reported in 1989. At that time the physics/chemistry community considered that it was impossible for a chemistry process to stimulate a nuclear reaction. Twenty years of laboratory research has shown otherwise. Related work on coherence physics, such as is concerned with Bose condensates in optical lattices, has widened the horizon as to what can be achieved. The latest results show that at least one CMNS sub-discipline has become a potential competitor for overcoming the energy shortage facing the civilized world.

Recent History

There was an important CMNS meeting held in Washington, D.C. in August. Dave Nagel, Mike Melich, and Ashraf Imam played key roles in arranging the international ICCF14 meeting. Substantial progress was reported in several CMNS subdisciplines. Especially noteworthy was progress in the area of cold fusion called "metal catalyzed fusion," also called "nanoPd fusion." It is an area to which I, Glen Schmidt, and Ashraf Imam made a contribution some years ago in a study that discovered that the nanoPd crystallites called Pd-black lose their nanometer character when chemically reduced crystallites make direct contact with Pd metal or have bare surface crystallite-crystallite contact.

Metal catalyzed fusion is the research area pioneered by Drs. Arata and Zhang (A-Z). Important progress was made in A-Z studies carried out in 2002, 2005, and 2007/8. The first new big step occurred in 2002. This work showed that a new catalyst developed at Tohoku University in Sendai, Japan was superior to the Pd-black catalyst that had been used by A-Z previously. The 2005 study replaced over-voltage electrolysis with gas loading of the catalyst at elevated temperature. The new catalyst was found to be seven times more effective than Pd-black in a comparison gas loading study. Production of fusion heat using gas loading instead of electrolysis was demonstrated at 191°C. About 25% of the heat required to maintain a 191°C temperature in the catalyst bed was due to nuclear reaction and 75% was due to an electrical heater. See Figure 1. The 2007/8 study showed that a small measurable rate of fusion heat generation could be produced by gas loading of the new catalyst at room temperature. In this latest study 7 grams of the new catalyst was pressurized with D_2 gas. Heat from the catalyst kept a thermally insulated reactor slightly above room temperature long after heat from a brief period of exothermic chemical reaction had been dissipated. Continuing nuclear heat generation maintained the positive temperature difference throughout the remainder of the run, which lasted for hundreds of hours. Since there was no input power, this run can be considered "proof" of autonomous heat. With D_2 gas loading, the steady temperature difference between catalyst bed and room was 1.7°C; in contrast with H_2 gas loading it was 0°C. If future work increases this temperature difference to 5 or 10°C and the demonstration is documented to show that heat generation persists for two months, it would be difficult for a rational critic to deny the reality of cold fusion.

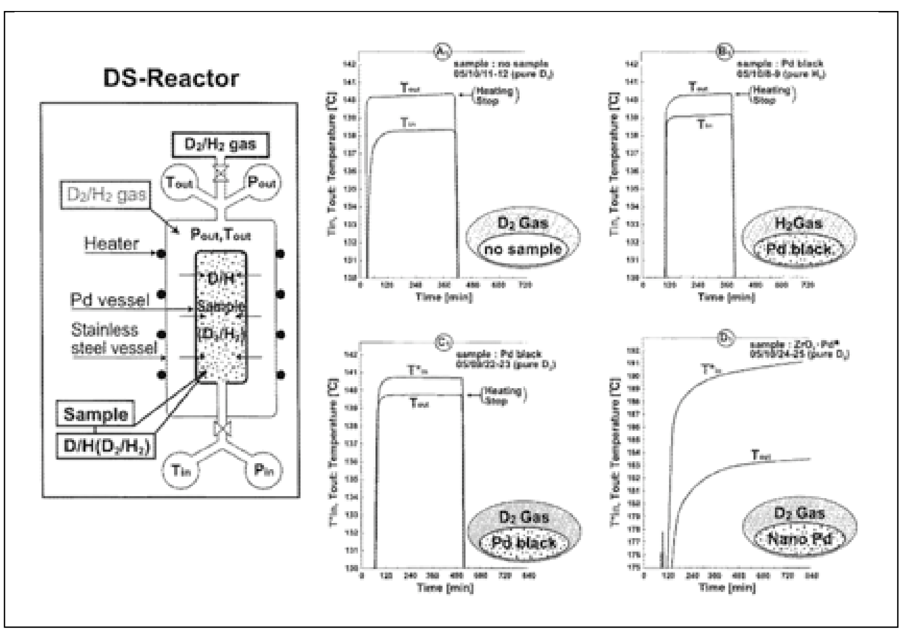

Figure 1. Gas loading tests from Y. Arata and Y-C. Zhang, *Proc. ICCF12*, pp. 48-49 (2006). The DS-cathode is called the inner–vessel of a double cylinder reactor. Thermal insulation covers reactor/heater. Four tests were carried out using a reactor pre-heated to 141°C: 1) D_2 flows into volume between outer and inner vessels, no catalyst; 2) H_2 flows into volume between outer and inner vessels, uses Pd-black: $T_{out} > T_{in}$; 3) D_2 flows into volume between outer and inner vessels, uses Pd-black: $T_{in} > T_{out}$; 4) D_2 flows into volume between outer and inner vessels, uses ZrO_2,nanoPd catalyst: $T_{in} > T_{out}$ and T_{in} rose from 141°C to 191°C.

Can Chemistry Promote Nuclear Reactions?

A central belief dominating twentieth century science has been a conviction that nuclear science is distinct and separate from chemistry and solid state physics, *i.e.* there is no means by which the electromagnetism quantum physics of molecules and condensed matter can affect the strong force physics of nuclei. This remains the view of mainstream science and its professional journals.

Experiments have shown that there is no single special technique that must be followed in order for nuclear reactions to occur. Instead, a multiplicity of techniques involving condensed matter and operating in different chemistry/physics regimes can be used to cause nuclear reactions to occur. The more successful of the CMNS studies discussed at ICCF14 can be grouped into three areas based on the differing technologies employed: cold fusion, metal catalyzed fusion, and detection of MeV alphas. I have assigned names to these groupings: the Fleischmann-Pons Effect (cold fusion); the Arata-Zhang Effect (metal catalyzed fusion); and Detection of MeV Alphas. Properties characterizing these groupings include:

Fleischmann-Pons Effect (cold fusion)

Uses over-voltage electrolysis of D_2O electrolyte to deposit D atoms on bulk Pd metal to produce D/Pd ratios at above chemical equilibrium value. Bulk Pd means rods, plates, wires, cylinders, etc. Process is operated so as to produce heat outflow rates greater than electrical power input. Process viewed as depending on three-dimensional lattice symmetry.

Arata-Zhang Effect (metal catalyzed fusion)

Uses nanoPd catalyst in the form of Pd-black or as the active metal in a heterogeneous catalyst to absorb D_2 gas and thereby produce a long-term continuing heat generator with power rate that is greater than auxiliary heater power or reaction stimulation power. Uses near-reversible equilibrium chemistry. A reactor that generates fusion heat with no electrical input heater power and no process stimulation power is called an autonomous fusion reactor. Process viewed as depending on two-dimensional lattice symmetry.

Detection of MeV Alphas

Oriani uses over-voltage electrolysis of D_2O or H_2O electrolyte to plate D and/or H atoms onto Pd or Ni cathodes, depositing ^7Li + H or ^6Li + D atoms onto his cathode. The SPAWAR team (Szpak, Mosier-Boss and Gordon), originators of the co-deposition approach to prepare the cathode, co-deposit Pd + D onto a metal substrate. In my opinion, both processes depend on two-dimensional lattice symmetry and the decay of a metastable Bloch-geometry ^8Be nuclear state into MeV alphas and that not all of the MeV alphas detected are a byproduct of dd-fusion. It is my opinion that most of the detected alphas are due to fission of excited ^8Be in the 6 MeV energy level, shown in Figure 2.

Probability that Chemistry Has Resulted in Nuclear Reactions

It is interesting to calculate the odds that none of the CMNS studies have resulted in a chemically-induced nuclear reaction. Odds are based on the views of the most experienced laboratory researchers in each of the three sub-disciplines.

We assume that the most experienced and successful worker in each of these three areas is correct in thinking that the probability that his/her work has produced a nuclear reaction is at least 10:1. The calculated probability that *none* of the three sub-disciplines has resulted in a nuclear reaction is less than one in a thousand, *i.e.* the odds favoring CMNS reality is at least 1,000:1. One concludes that it is essentially certain that a chemistry induced change can allow deuterons to participate in a nuclear reaction.

An example of strong experimenter belief is given in Oriani's ICCF14 Abstract, where he states that his latest "experimental technique. . .has shown the generation of nuclear particles in each of twenty-five consecutive experiments. . ." Oriani is a Princeton Ph.D. who gave chemistry lectures at NRL long before cold fusion was conceived.

Impact on Renewable Energy and Space Technologies

Production of heat at 200°C seems assured. Initial use for home and other off-grid heating seems assured. Electricity production at ~25% efficiency seems assured. Catalyst chemist Les Case says that electricity costs will depend on the price of Pd and the amount of Pd needed per kW capacity. He says that at present day Pd prices, the catalyst he has used will produce electricity at present day costs (cents/kWh). If electricity production in central power plants adopts metal catalyzed fusion, the risk of nuclear pro-

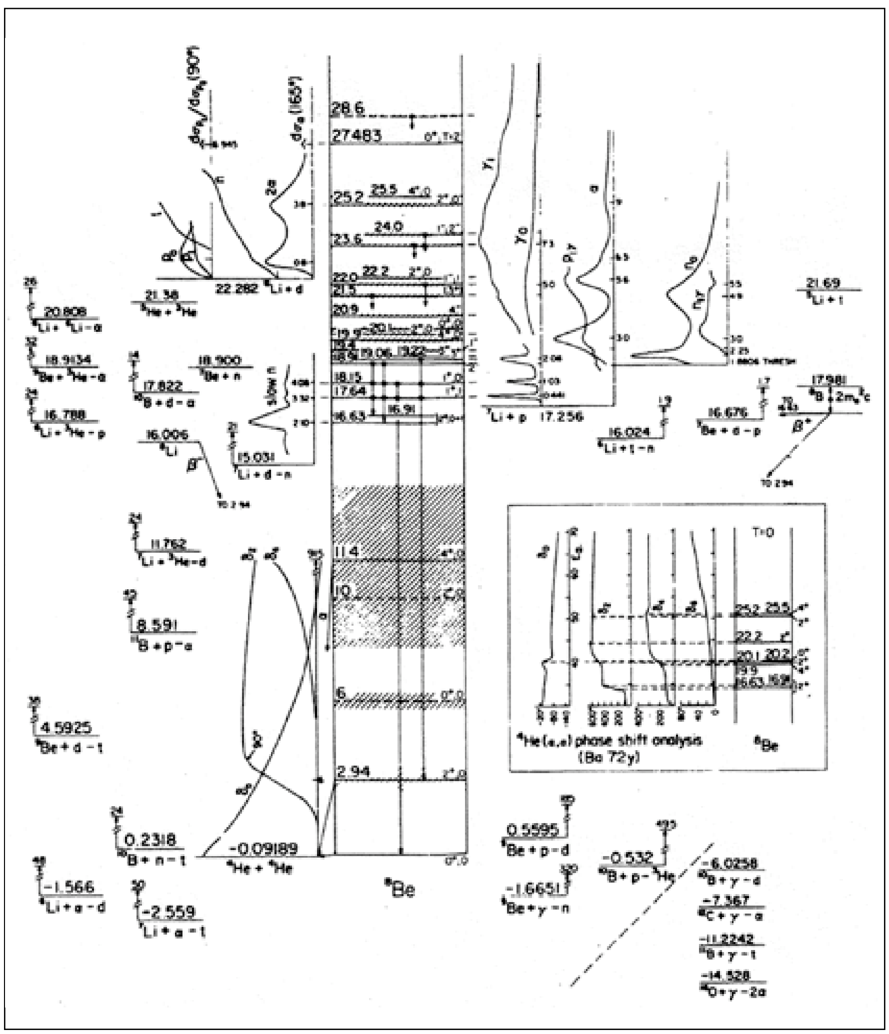

Figure 2. Energy level diagram for ^8Be, from K. Heyde, *Basic Ideas and Concepts in Nuclear Physics* (Institute of Physics Publishing, Bristol UK, 1994, p 54). The 6 MeV level is predissociated, so energy of paired alphas is not precisely specified.

liferation will be greatly reduced. Space technology will be substantially simplified. Solar cells will be replaced with catalyzed fusion heat-cycle power generators. Much in-orbit and interplanetary propulsion will be produced at high specific impulse by expelling fusion-heated H_2 gas.

Factors Limiting Recent Metal Catalyzed Fusion Progress

Cold fusion has made almost no progress in gaining attention from the world science community. Nuclear physicists typically accelerate charged particles onto target atoms and study cross sections and scattering angles. The scatterings occur at a single center-of-mass. Most nuclear physicists cannot imagine deuterons undergoing a catalytic nuclear fusion reaction.

In the metal catalyzed fusion world, catalysts enable deuterons to assume a delocalized, coherently partitioned lattice structure similar to that assumed by electrons in a metal. This "multi-center" geometry is a totally unexplored nuclear physics geometry. There is no preferred center-of-mass. Conduction electrons in metals have no preferred center-of-mass. They are called quasiparticles. Deuterons with no preferred center-of-mass can undergo symmetry-preserving reactions that are totally unlike the bang-bang reactions of classical nuclear impact physics.

Plasma fusion physicist Thomas J. Dolan is the only nuclear physicist/engineer known to me who is seriously considering the possible role of Bloch (no preferred center) deuteron geometry in CMNS processes. He served as a U.S. technical representative at the International Atomic Energy Agency (IAEA) headquartered in Vienna, Austria. He is now a Professor at the University of Illinois and has published a textbook on plasma fusion. He wrote me a supportive letter regarding my paper, "Many-Centers Nuclei."

Available Publications

There is a huge amount of evidence showing that chemistry can cause nuclear reactions to occur. Much of this work does not directly lead to radiationless metal catalyzed dd fusion. However, this body of work shows that there are a variety of chemical processes that can induce nuclear reactions. As mentioned, an important set of studies are the co-deposition experiments at SPAWAR and at the University of Minnesota (Prof. Oriani), which produce numbers of MeV alpha particles.

Available material includes my published book *Cold Fusion: Clean Energy for the Future* and my website www.cfescience.com. It contains downloadable files, including the theory paper "Overcoming the Coulomb Barrier in Cold Fusion."

Strategies and Agenda for ICCF14

Michael E. Melich and David J. Nagel
Co-Chairmen for ICCF14

Our experiences before and during ICCF14 may be of use to the organizers and attendees of future ICCF conferences.

The ICCF series of conferences has been on a three-continent rotation. Hence, after Sochi, Russia, it was appropriate to hold ICCF14 in North America. We volunteered to organize it in Washington, D.C., a few minute's walk from the national capitol and close to the regional Metro system with two hopes in mind. One was to attract staff from the nearby offices of Senators and Congressmen. The other was to make it easy for program managers from U.S. government funding agencies and technical organizations to attend, especially those with responsibilities for science, energy and the environment. We also hoped that having the conference in the heart of the U.S. capitol would attract mainstream press coverage. None of these possibilities materialized at the desired level. However, the CBS TV show "60 Minutes" did videotape part of the conference. The limited attendance by government and press was due in part to the conference being scheduled in August, when many people are on vacation, and during a presidential election year. That had to be done because hotel room rates in August are almost half of what they are during other times of the year.

Regarding the agenda, conference organizers can be either reactive or proactive in their approach to obtaining papers for presentation. In the reactive mode, they form the agenda from the papers that have been offered in response to a Call for Papers. However, for most conferences, the organizers also invite presentations from important workers in the field that will be of broad interest to attendees. These invitations are honorific and they insure that the best work is highlighted. Our approach to ICCF14 included, but went beyond, simple arrangement of submitted papers into logical sessions and some invitations to particular scientists.

We felt that there were some topics within the field that needed up-to-date technical reviews for presentation at the conference. Hence, we commissioned a few reviews from key workers, in addition to inviting several luminaries in the field to give papers. The commissioned reviews were on:

1. The evidence for excess heat, the Fleischmann-Pons Effect (FPE);
2. Calorimeter design and performance for measurement of the excess power and energy in the FPE experiments;
3. Experiments using gas loading to produce excess heat;
4. Scattering of deuterons on deuterons within a metallic environment to assess the "screening" at energies below the coulomb barrier.

It is hoped that the commissioned papers will form the basis for several papers to be published in a mainstream review journal, such as the *Reviews of Modern Physics*.

The architecture of the agenda was chosen to meet certain objectives. One was to provide during the first two days of the conference a broad overview of the field, with very important work on heat and materials being on the opening day. This was done to insure that people who could attend only one or two days of the conference would be able to get a sense of the breadth and quality of what has been done and found in the field. We learned that about 25% of the attendees were only able to attend the first two days and many indicated the structure of the agenda encouraged them to attend and participate. As one long-time attendee of the ICCFs, who is widely experienced in advanced scientific topics, commented: "This conference had much 'juice,' that is, information not available in the published literature or in standard format conferences or streaming video conferences." The introductory two days justified his staying for the entire week.

A second goal recognized that several people in the field, who have made major contributions to its development, are well past retirement age. It was felt that the chances to publicly honor such pioneers would be few. Learning of their latest work was also important. Hence, we scheduled two sessions on the second day, one to honor Professor Yoshiaki Arata from Osaka University in Japan and the second to honor Dr. Stanislaw Szpak from the SPAWAR Systems Center in San Diego. The session for Professor Arata began with Dr. Talbot Chubb's overview of Arata's work on cold fusion, and ended with a presentation by Professor Arata on his most recent and very provocative results. The session for Dr. Szpak, who could not attend, consisted of an overview of the work he and his colleagues have done and published since the inception of the field. It was presented by a few of his colleagues led by Dr. Frank Gordon. We hope that future ICCFs will also include sessions recognizing key pioneers in the field.

The second day also included sessions on very important topics—gas loading, particle measurements and challenges facing the field. In the evening, the annual public session of the International Society for Condensed Matter Nuclear Science (ISCMNS) was held. It was organized and chaired by William Collis, the founder and Chief Executive of the Society.

There are four classes of measurements done on FPE experiments—heat, nuclear ash, energetic particles and low energy phenomena. The measurements of nuclear reaction products has tended to fall into two main classes, namely the detection of light products, such as tritium and helium, and the measurement of elements of moderate or heavy mass across the periodic table. The second type of research goes under the banner of transmutations, and is of widespread interest and major importance in the field. Hence, the opening session on the third day was on transmutations. However, there was not enough time in that session to cover all the work in the subfield. Therefore, Professor George Miley from the University of Illinois organized a workshop on transmutations during the

Friday afternoon immediately after the conference. Approximately 50 scientists attended, a measure of the interest in transmutations.

Most of the third day was designed to serve workers in the field, both technically and for recreation. There was a session during which leading workers from several countries presented histories of research on the FPE in their countries during the almost two decades since the inception of the field. Presentations were made on work in China, France, India, Italy, Japan and Russia. This session was a major step forward in a separate project to produce and publish country histories for activities and results in the field. Some already exist in either English or the language of the country. Translations to English are in progress now (early 2009), with the goal of publishing a matched set of books, one for each country, later this year. The history of U.S. work has been covered for the earlier years in various books and the UK history is generally limited to the efforts at Harwell, covered in the history of Harwell. U.S. and UK histories are being expanded, using recently available archives. For example, it has been discovered that several laboratories of a major U.S. corporation conducted FPE experiments. One laboratory did not even bother to look for excess heat, focusing exclusively on the conventional expectation that ionizing radiation and particles would be most easily detected. These results were published in a major U.S. physics journal, saying "nothing was observed." A sister laboratory did seek the heat signature, found it was present, but erratically. They realized that a major materials research effort would be required and consequently stopped their research, as energy was not a major product of this corporation. There was never a "corporate" position on the FPE experiments, yet certain prominent scientists from the corporation have for nearly 20 years consistently defended their public position that "there is no heat effect of interest in the deuterated palladium system."

The afternoon of the third day was devoted to the traditional conference outing. Most of the attendees participated in a visit to the Udvar-Hazy Center of the Smithsonian Air and Space Museum about an hour's drive from the conference hotel. The conference banquet was held after the tour on the third day. In addition to the meal and musical entertainment, the evening included the presentation of the Preparata Medal to Dr. Irving Dardik. William Collis prepared the medal on behalf of the ISCMNS and Dr. Michael McKubre described the work that led to selection of Dr. Dardik. Dr. Dardik's acceptance speech was a thoughtful and inspiring reminder about the rewards of curiosity and scientific investigation.

The fourth day of the conference included two sessions on theory, the topic in the field that had the most papers at the conference. There were also sessions on accelerated beam experimental results, on optical experiments and another partial session on materials. That day also included the last of three poster sessions, the other two being on the first and second days.

The conference concluded with a half day of presentations. Several significant experimental papers, which did not fit well in the earlier sessions, were given in the first session. That was followed by the concluding session. It included a conference summary, followed by two panel discussions. The first was on Experimental Design and the second on Realizing the Promise.

The feedback from attendees indicated that ICCF14 was a successful scientific conference. The 97 papers scheduled for oral or poster presentation included some very important new results. The information given at the conference and to be published in the ICCF14 proceedings adds significantly to the large and increasingly-compelling evidence for the ability to trigger nuclear reactions giving millions of electron volts of energy with chemical energies on the scale of electron volts. This new and exciting scientific field is sufficient in itself. However, the possibility of clean and safe distributed nuclear power sources based on the FPE makes interest in the understanding, controlling and optimizing of lattice induced nuclear reactions even more urgent.

Problems Related to Obtaining Foreign Visas

—IE Staff

Anyone who has ever applied for a visa knows that you must deal with not only your own country's rules, but more importantly those of the nations you intend to visit. Some may not be aware, but travel to the U.S. has been greatly affected since September 11, 2001; the U.S. tightened its borders after 9/11. This included reducing the number of visas granted to foreign travelers (or, in most cases, making the application process more difficult or longer). The organizers of ICCF14 learned a hard lesson about foreign travel, a lesson that they hope will aide future organizers in their planning process—that rules can change unexpectedly which will adversely affect your careful planning.

U.S. Embassies have had ongoing changes in their protocols over the past eight years. Last year was no exception. Unfortunately for the organizers and attendees of ICCF14, some administrative changes occurred with the Russian visa process after the ICCF14 instructions on visas were posted. These changes kept some Russian researchers from being able to attend.

Obtaining visas to attend scientific meetings in the U.S. has always required extensive clearance processes for scientists from various countries. The organizing committee of each conference provides foreign attendees material submission dates for visa applications, based on the advice of the U.S. Consulate in each country. For ICCF14, Russian scientists were (in February 2008 at the time of conference planning) required to submit their application package and complete their personal interview within a particular time window, since the visa they received would have to be used within a set period of time (90 days) after issuance and was a single entry visa. ICCF14 organizers were informed that the security clearance process was taking about four or five weeks. Thus for a conference in August, applicants were instructed to complete their interviews no earlier than May 1, 2008 and no later than June 1, 2008 (at that time, the issued visa would be good for 90 days only, so being approved before May 1 would not allow travel to the conference in early August). In the late spring, after most scientists had begun their application process, rules were changed by the U.S. government. Multiple entry visas (without a 90-day time limit) were being made available and the clearance prosecution time became longer, as much as six to eight weeks.

Because the visa application process has multiple steps (paperwork, interviews, fees), many Russian applicants were unable to complete the process before the end of June 2008 (though most of them had applied within the timeframe suggested by the ICCF14 committee). Consequently, most of them did not receive a visa to attend ICCF14.

It is unfortunate that the conference was unable to benefit from the direct participation of the Russian researchers who have been a vital part of the CMNS community. The papers submitted by the Russian scientists, however, are being edited and reviewed for inclusion in the proceedings. This middle-of-the-process visa application change at the U.S. Embassy in Russia highlights another hurdle that conference organizers should anticipate in the future.

— The Possibility of Plutonium Reduction —

Edward Esko*

The issue of what to do with the nuclear waste left over from nuclear reactors and nuclear weapons programs remains a vexing problem in countries around the world. So far, no satisfactory solutions have emerged. Nuclear waste is clearly an impediment to a clean natural environment and poses a threat to world peace.

Environmental, health and public interest groups including Greenpeace, Physicians for Social Responsibility, Friends of the Earth and the Natural Resources Defense Council, are united in opposition to the reprocessing of nuclear waste—a practice in which radioactive plutonium and uranium are separated from used or "spent" nuclear fuel from nuclear power reactors. The reprocessed waste is then reused as fuel.

In a May 2008 fact sheet entitled "Reprocessing: Dangerous, Dirty and Expensive—Why Extracting Plutonium from Nuclear Reactor Spent Fuel is a Bad Idea," the Union of Concerned Scientists stated that reprocessing would increase the risk of nuclear terrorism:

> Less than 20 pounds of plutonium is needed to make a nuclear weapon. Commercial-scale reprocessing facilities handle so much of this material that it has proven impossible to keep track of it accurately. A U.S. reprocessing program would add to the worldwide stockpile of separated and vulnerable plutonium that sits in storage today, which totaled roughly 250 metric tons as of the end of 2005—enough for some 40,000 nuclear weapons. Reprocessing the U.S. spent fuel generated to date would increase this by more than 500 metric tons.

In a December 9, 2008 letter to President-elect Obama, a coalition of 150 environmental and health groups pointed out that reprocessing would increase environmental contamination and threaten public health, cost hundreds of billions of dollars of taxpayer funds and not solve the nuclear waste problem—not even in France:

> Although France reprocesses all its spent nuclear fuel, it is faced with the same difficulties the United States has in siting a permanent geologic repository. The proposed permanent repository site in Bure, France faces overwhelming public opposition, similar to Yucca Mountain in Nevada. In addition, reprocessing has polluted the environment, including the ocean as far away as the Arctic Circle, and has created a stockpile of more than 80 metric tons of separated plutonium.

The coalition of public interest groups went on to urge the Obama Administration to focus on securing nuclear waste at reactor sites. However well informed, this measure doesn't solve the problem of what to do with existing plutonium reserves. As we move forward, our goal should be not simply to bury radioactive waste in reactor sites, but to explore the possibility of reducing or even eliminating existing stocks of plutonium and other radioactive elements.

The Nature of Plutonium

In *A Guide to the Elements* (Second Edition, Oxford University Press) Albert Stwertka describes plutonium (Pu):

> Plutonium [atomic number 94] is the most important of the transuranium elements, all of which follow uranium [atomic number 92] in the periodic table and all of which are artificially made.

In *Nature's Building Blocks: An A-Z Guide to the Elements* (Oxford University Press) John Emsley describes the destructive power of this manmade element which has no role in nature or in the human body:

> By the spring of 1945 several kilograms had been amassed, and the first atomic explosion, using plutonium, took place at Alamogordo, in the desert of New Mexico, on July 16. It detonated 6 kilograms of plutonium and was triggered by using small conventional explosives to force several pieces of plutonium together to give the critical mass necessary for a runaway chain reaction.
>
> The second plutonium explosion was in the form of a bomb, code-named "Fat Man" which was dropped on the Japanese city of Nagasaki on 9 August 1945. The explosive capacity was equivalent to several thousand tons of TNT, killing about 70,000 citizens and wounding 100,000.
>
> The even more destructive hydrogen bombs are themselves triggered by a plutonium bomb whose explosion generates temperatures high enough to cause hydrogen atoms, in the form of the heavier isotopes, deuterium or tritium, to fuse together. Because of the fission-generated heat needed to generate the fusion reactions, these are classified as thermonuclear weapons. As hydrogen atoms fuse, they emit vastly more energy than an atomic bomb and give explosions equivalent to millions of tons of TNT.

The ^{94}Pu minus ^{11}Na Pathway

In Quantum Conversion Theory, in which elements can be induced to change into each other, with relatively low inputs of energy, it may be possible to produce low energy nuclear fusion reactions, which in turn trigger simultaneous low energy nuclear fission reactions. It may be possible to prompt large, heavy atoms, such as lead, uranium or even plutonium, to shed protons and neutrons and convert into lighter elements, peacefully, naturally and without destructive consequences. Quantum Rabbit low energy transmutation studies, conducted since 2005 and reported in *Infinite Energy*, suggest such a possibility. Holistic educator Michio

Kushi suggested the possibility of low energy nuclear fusion/fission reactions as early as the 1960s.

The Quantum Conversion approach to the problem of plutonium reduction is simple, direct, and balanced. Atoms of sodium (^{11}Na) are subtracted from atoms of plutonium (^{94}Pu) to form atoms of bismuth (^{83}Bi). This low energy fission reaction is triggered by a low energy fusion reaction in which sodium (^{11}Na) reacts with sulfur (^{16}S), (with oxygen as a fill gas under vacuum), to form cobalt (^{27}Co). Quantum Rabbit research protocols suggest the reaction could be achieved in a sealed tube at approximately 3.5 Torr with as little as a 4 amp discharge between anode and cathode.

The formula is as follows:

PLUTONIUM MINUS SODIUM (+SULFUR)
INTO BISMUTH (+COBALT +NEUTRONS)

$$\begin{array}{ccccc} & {}^{16}\mathrm{S} & & {}^{27}\mathrm{Co} & \\ & + & & + & \\ {}^{94}\mathrm{Pu} & -{}^{11}\mathrm{Na} & = & {}^{83}\mathrm{Bi} & + \text{Neutrons} \end{array}$$

In addition to the reduction of plutonium, and the production of bismuth and cobalt, this reaction would most likely produce the discharge or release of neutrons. Plutonium has 19 known isotopes while bismuth has one natural isotope, Bi_{209}. The principal isotopes of plutonium, the way they may react to form bismuth, and the possible shedding of neutrons, is presented below:

$Pu_{238} - Na_{23} = Bi + 6n^*$
$Pu_{239} - Na_{23} = Bi + 7n$
$Pu_{240} - Na_{23} = Bi + 8n$
$Pu_{241} - Na_{23} = Bi + 9n$
$Pu_{242} - Na_{23} = Bi + 10n$

*n equals the number of neutrons discharged during the reaction

Quantum Rabbit is not equipped to handle plutonium or conduct the above-suggested research. However, we would be happy to serve as technical or theoretical advisors to established laboratories such as Oak Ridge, Los Alamos, Bhabha Atomic Research Center in India, and others around the world.

As stated above, the goal of this research is to secure a clean natural environment for future generations and reduce potential nuclear threats to human health, peace, and well being.

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About the Author

Edward Esko is the founder and president of Quantum Rabbit LLC. He is co-author, with Michio Kushi, of *The Philosopher's Stone: A Guide to Transmutation and the New Science* (One Peaceful World Press, 1994). He has previously published in *Infinite Energy*.

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Electric Currents, Magnetic Fields, Magnetic Pulses and Electromagnetic Propulsion

John R. Warfield*

Abstract —
A single circular loop conductor [ring] with its current induces a magnetic field, not only surrounding the ring but also within the substance of the ring. Subsequently, that portion of the magnetic field, which is located within the body of the ring, interacts with its own current to produce Lorentz forces. Electromagnetic propulsive forces are produced from this process. However, these forces are either blocked by the intact structure of the ring, or they are symmetrically oriented in opposing directions. As such, these later forces counteract each other. Essentially, all the forces are balanced; consequently there is no propulsion of the ring. However, if a directed magnetic pulse [magnetic flux compression producer—EMP] distorts the magnetic field relative to one side of the plane of the ring, then for the duration of this pulse there will be within the ring some Lorentz forces that are neither blocked by its physical structure nor annulled by opposing symmetrical forces. Accordingly, these forces are unbalanced. As a result, there will be electromagnetic propulsion of the ring along its axis.

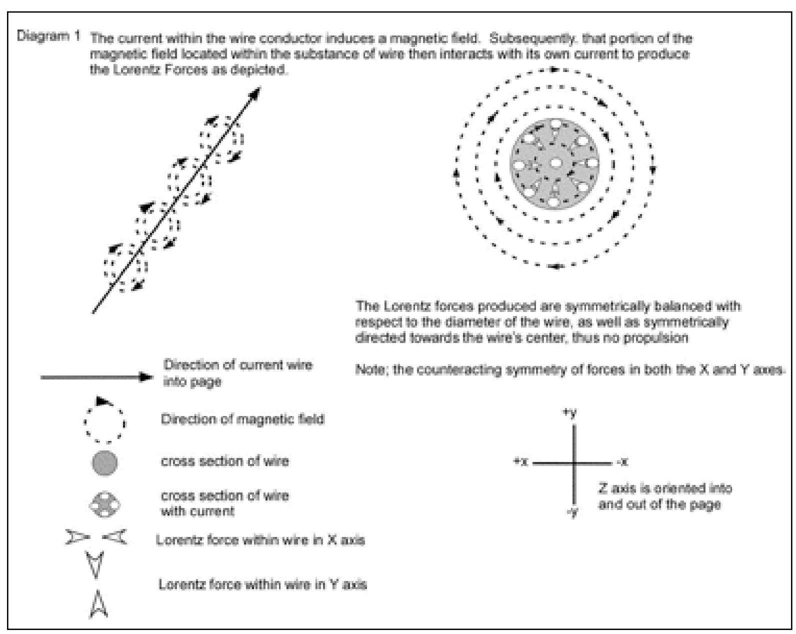
Introduction —
The intention of this article is to posit a theory of electromagnetic propulsion based upon an electric current, a magnetic field, as well as directed magnetic pulses [EMP]. It is fundamentally a very simple concept based upon these three assumptions:

❑ A current within a wire conductor induces a magnetic field not only surrounding the wire, but within the substance of the wire as well.
❑ Subsequently that portion of the magnetic field which is located within the wire interacts with its own current, again within the wire to produce Lorentz forces, once more within the wire
❑ By means of magnetic flux compression technology, one can project a powerful magnetic pulse in a specific direction, analogous to a gun [EMP].

Subsequently, these three assumptions will be used to assemble a hypothetical electromagnetic propulsion device. Due to the complex three-dimensional nature of this concept, it is considerably easier to explain this model if one uses diagrams. For that reason six diagrams will be presented. Each diagram will present a concept that will lead to the next diagram, until finally the concept of electromagnetic propulsion is explained. The six diagrams are listed as below.

1. A single straight wire conductor with a current.
2. Two straight wire conductors with their currents flowing in the same direction.
3. Two straight wire conductors with their currents flowing in opposite directions.
4. A single circular wire conductor [ring] with a current.
5. Two circular wire conductors [rings] with both of their currents flowing in the same direction.
6. A single circular [loop] conductor [ring] with a current, along with its induced magnetic field—the latter of which is distorted on one side, relative to the plane of the ring, by a directed magnetic pulse. As a result there is electromagnetic propulsion.

Diagram 1 —
Diagram 1 illustrates a single straight wire conductor, with its current flowing into the page. The wire with its current induces a circular magnetic field, not only surrounding the wire, but also within its own substance. Subsequently, that portion of the magnetic field which is located within the body of the wire interacts with its own current, to produce the Lorentz forces as depicted in Diagram 1. Notice both the density of the magnetic flux, as well as the direction of the Lorentz forces are symmetric with respect to the wire's diameter. Furthermore, the Lorentz forces are oriented symmetrically in a circle towards its center. Electromagnetic propul-



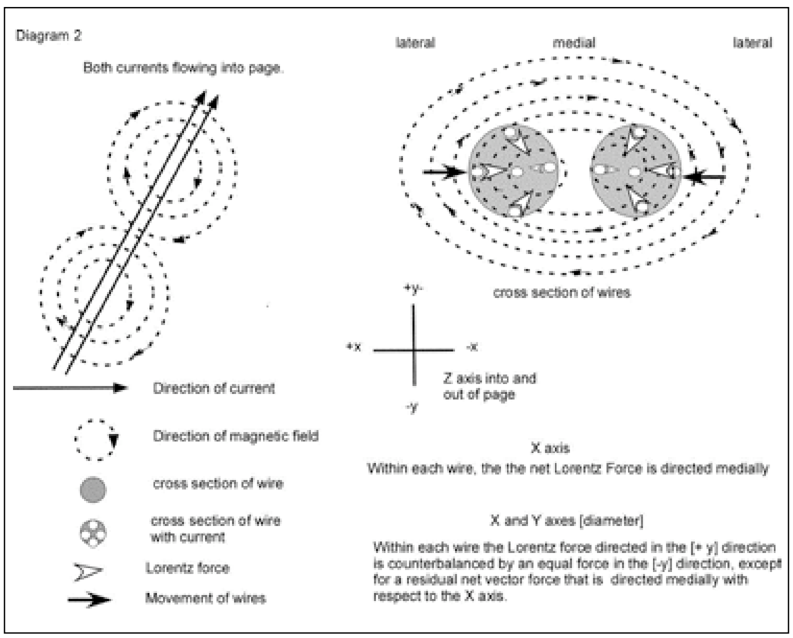
sive forces are produced by this process; nevertheless, due to the above symmetry these forces are balanced. As a consequence there is no motion. On the other hand, if these forces were somehow asymmetrical rather than symmetrical, there would be propulsion. Nonetheless, this is not the case.

In this and subsequent diagrams, the overall Lorentz forces will be divided into separate vector forces within the X [+x,-x] and Y [+y,-y] axes, and additionally in the Z axis, with respect to the later illustrations. For example, in this diagram the Lorentz forces counteract each other in both the X and Y axes. However, in reality all the Lorentz forces are oriented symmetrically in a pattern of a circle towards the wire's center. As such, they again neutralize each other. In both instances, there is no propulsion as these forces are balanced. In essence, the two scenarios are analogous to each other. I have chosen this method of explanation so that one can easily envision the concepts. Otherwise, the diagrams and description will be too complex to comprehend.

Diagram 2 —

Diagram 2 illustrates two straight wire conductors with both currents flowing into the page. Each separate wire with its current induces it own magnetic field, not merely surrounding its own wire, but also within the substance of its own wire. As depicted in Diagram 2, the two magnetic fields interact to create one overall modified field. Subsequently, that portion of this modified field which is located within the body of each wire interacts with the current in that same wire to produce the Lorentz forces as illustrated. Notice with respect to each wire, the density of the magnetic flux in the X axis [+x versus –x directions] is asymmetrical, furthermore greater laterally compared to medially. Therefore, the Lorentz force that is directed medially is greater compared to the force which is directed laterally. Observe as well, relative to each wire, that the density of the magnetic flux takes the form of a mirror image symmetrical pattern, in the X and Y axes [diameter], relative to the X axis. Therefore, the resulting Lorentz forces neutralize one another, except for a residual vector directed medially.

Consequently, with respect to each wire, the direction of the net Lorentz force is medial and, as a result, the wires propel towards each other. This process is actually electromagnetic propulsion, nevertheless impractical, given that once the wires are in contact, all motion ceases. Make note that outside the substance of the wires in the region of the interacting magnetic fields, there is no force. Forces are located only within the body of the wires, where the one modified magnetic field interacts with each of the two currents.



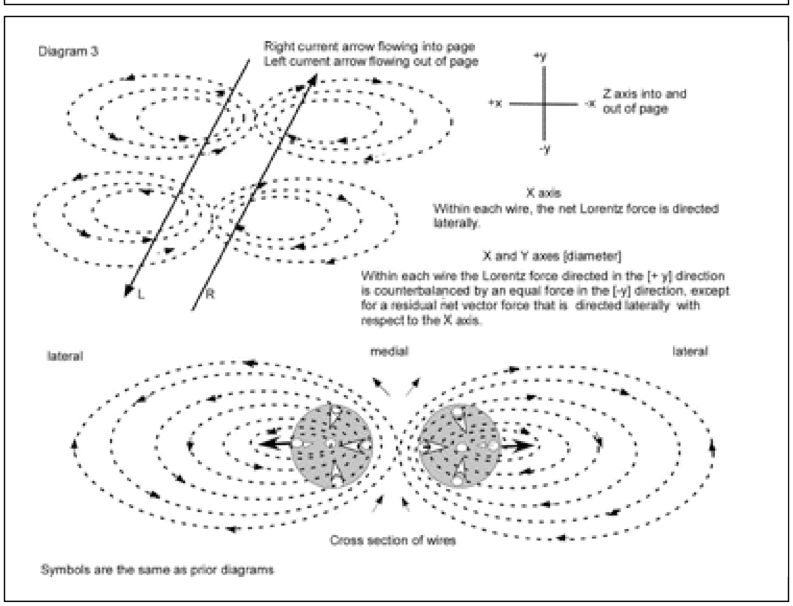


Diagram 3 —

Diagram 3 illustrates two straight wire conductors with opposing currents. The left current is flowing out of the page, whereas the right current is flowing into the page. Each wire with its current induces its own magnetic field, not only surrounding itself, but also within its own substance as well. The two magnetic fields interact and form two separate modified fields, as depicted in Diagram 3. Subsequently, that portion of each modified field which is located within the body of its own wire interacts with its own current to produce the Lorentz forces as illustrated. Notice relative to each wire that the density of the magnetic flux in the X axis [+x and –x directions] is asymmetrical, moreover greater medially compared to laterally. Therefore, the Lorentz force that is directed laterally is greater compared to the force which is directed medially. Observe as well, relative to each wire, that the density of the magnetic flux takes the form of a mirror image symmetrical pattern, in the X and Y axes [diameter] relative to the X axis. Therefore, the Lorentz forces neutralize each other, except for a residual vector directed laterally.

Consequently, with respect to each wire, the direction of the net Lorentz force is lateral, as such, the wires propel away from each other. Once again, outside the substance of the wires in the region of the interacting magnetic fields there is no force. Force is located only within the body of each wire, where the current within that wire interacts with its own associated modified magnetic field. This process is electromagnetic propulsion, though impractical, since once the wires travel a given distance from each other, the two magnetic fields will cease to interact. Subsequently each will turn into a single wire, as depicted in diagram 1.

Diagram 4 —

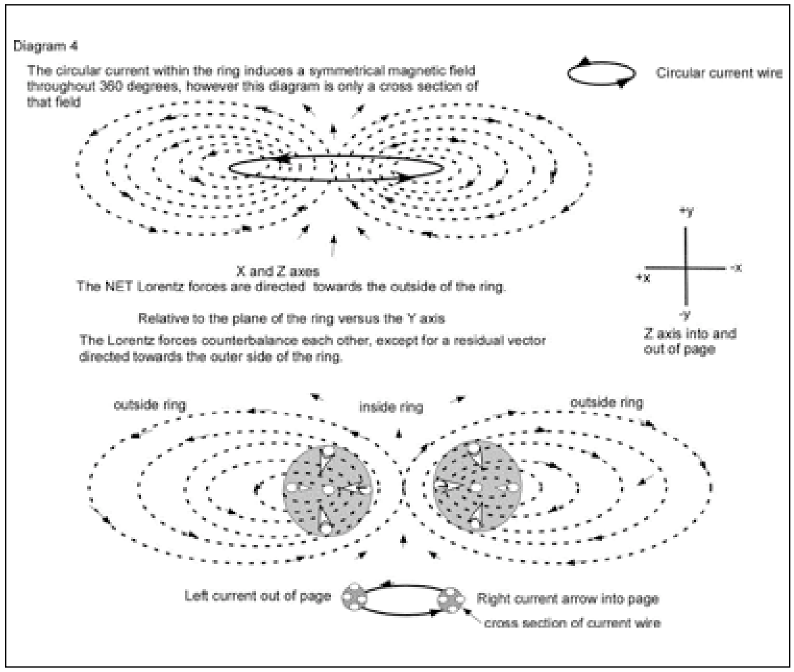
Diagram 4 illustrates a current in a single circular [loop] conductor. In future deliberations, this structure will be defined as the ring. The shape of the magnetic field created by this current is equivalent to the classical magnetic field induced by a loop current, as depicted in Diagram 4. The ring with its current produces a magnetic field not just surrounding itself, but also within its own essence. Subsequently, that portion of the magnetic field which is located within the body of the ring interacts with its own current to produce the Lorentz forces as depicted. Notice relative to the plane of the ring, the density of the magnetic flux in the body of ring is asymmetrical, moreover greater within the inner side of the ring compared to within its outer side. Therefore, throughout 360 degrees, the Lorentz forces that are directed towards the outside of the ring are greater compared to those forces which are directed towards its inside. Observe as well, relative to the plane of the ring versus the Y axis, the density of the magnetic flux within the ring takes the form a mirror image symmetrical pattern. Therefore, throughout 360 degrees, the Lorentz forces neutralize each other, except for a residual vector, directed towards the outside of the ring. Consequently, with respect to the plane of the ring, the overall net Lorentz forces are directed symmetrically and equally outward, throughout its circumference. Nevertheless, the ring is a physically intact structure; accordingly it resists these forces.

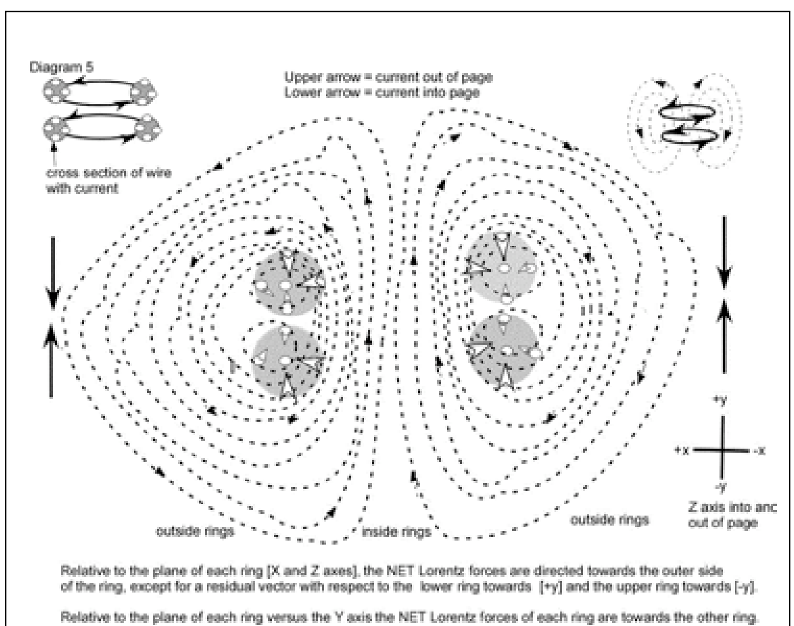
In other words, all the Lorentz forces produced within the ring are either blocked by its solid structure, or else they neutralize one another. As such, there are no unbalanced forces. Consequently, as previously depicted in Diagram 1, there is again no propulsion. Alternatively, if the Lorentz forces were somehow asymmetrical with respect to both sides of the plane of the ring, there would be propulsion. Commit this last concept to memory.

Diagram 5 —

Diagram 5 illustrates two parallel ring conductors, oriented in the same axis, with their currents flowing in an identical direction [solenoid]. Each ring with its current induces a classically shaped loop current magnetic field, not only surrounding its own ring, but within its own substance as well. The two magnetic fields interact to form one overall modified field as depicted. Subsequently, that portion of this overall modified field, which is located within the body of each ring, interacts with the current in that same ring. This produces the Lorentz forces as depicted. With respect to the plane of each ring [X and Z axes], the density of the magnetic flux in that ring is greater within the inner side of the ring compared to within its outer side. Therefore, throughout each circumference, the net Lorentz forces are directed symmetrically towards the outside of the ring—except for a residual vector towards [+y] with respect to the lower ring, and a residual vector towards [-y] with respect to the upper ring. Furthermore, relative to the plane of each ring, versus the Y axis [throughout the ring] there is now an asymmetry of the density of the magnetic flux within each ring. On one hand, with regards to the upper ring, the density of the magnetic flux is greater towards the top of the page [+y] compared to the bottom of the page [-y]. On the other hand, with regards to the lower ring, the density of the magnetic flux is greater towards the bottom of the page [-y] compared to the top of the page [+y]. Accordingly, the upper ring's net Lorentz forces are directed towards the lower ring [-y]. Conversely the lower ring's net forces are directed towards the upper ring [+y].

In other words, all the individual Lorentz forces within each ring either neutralize one another, or else they are blocked by its physical structure, with the exception of those unbalanced forces which propel each ring towards the other ring, with respect to the Y axis. Once again, this is electromagnetic production, nevertheless impractical, given that once the two rings are in contact, then all motion ceases. At





that time, together they will act analogous to a single ring, as illustrated in Diagram 4.

Diagram 6 —

In order to comprehend this last and crucial diagram, one must appreciate the concept of magnetic flux compression producers [see Appendix 1]. This apparatus produces an extremely powerful directed magnetic pulse [EMP], which can be used as a military weapon, analogous to a gun. However, in this situation it produces electromagnetic propulsion. Recall in Diagram 4 that relative to the plane of the ring [X and Z axes], the net Lorentz forces are symmetrically directed, throughout its circumference, towards the outside of the ring, Nevertheless, the ring remains intact. As a consequence, there is no propulsion. Recall as well, relative to the plane of the ring, versus the Y axis [throughout the ring], there is mirror image symmetry of the Lorentz forces. As a result they neutralize one another, with the exception of a residual vector, oriented towards the outside of the ring [blocked]. Thus, again there is no propulsion. In summary, with reference to Diagram 4, there are electromagnetic propulsive forces produced within the ring, nevertheless they are either blocked by its intact structure, or else they counteract one another. Essentially, there are no unbalanced forces and so no propulsion.

Now imagine that a single uniform magnetic pulse from a magnetic flux compression producer is emitted from the center of this ring. Moreover, presume as well this device is attached to the ring. Furthermore, assume the pulse is directed towards one side, relative to the plane of the ring [+y]. Therefore, for an extremely brief period of time, this pulse will distort the shape of the magnetic field on that side. Thus, with respect to the plane of the ring versus the Y axis, the mirror image symmetry previously depicted in Diagram 4 is lost, as now illustrated in Diagram 6. Observe, relative to the plane of the ring, at this instant in time, there are symmetrical net vector forces directed throughout its circumference towards the outside of the ring, which are neutralized by its intact structure. In addition, there are other net vector forces directed throughout its circumference towards the top of the page [+y] that are now not neutralized. Given that these later forces are unbalanced, with respect to the Y axis, then during each pulse there will be electromagnetic propulsion towards the top of the page [+y].

In other words, the directed magnetic pulse mimics the magnetic field that was induced by the upper ring as illustrated in Diagram 5, even though that ring is nonexistent.

Furthermore, if one assumes a very rapid sequence of EMPs, then each EMP will be associated with a pulse of propulsion. Accordingly, there will be continual motion in the +y direction. I have arbitrarily chosen to expand the magnetic field on the side of the directed magnetic pulse. Alternatively, one could contract the magnetic field on that side as well. If so, the propulsion would be towards the bottom of the page [-y]. See Appendix 2.

In this model the force on the producer related to the production of the EMP is in the opposite direction with respect to the emitted EMP. Notice that the direction of the propulsion of the ring related to the unbalanced Lorentz forces can be in the opposite direction as the force on the producer, or on the other hand in the same direction. These individual forces do not necessarily have to counteract one other, nor are they unavoidably equal to each other, conceivably the disparity in magnitude is massive. Furthermore, an explosion within a closed system [box containing the compressor] does not propel.

For simplicity of explanation, I have used the concept of magnetic flux compression producers too freely. In reality, it is an explosive apparatus which self-destructs during the production of one extremely powerful directed magnetic pulse. Alternatively, this hypothetical propulsion device requires a non-explosive magnetic flux compression producer capable of extremely rapid sequential pulses [see Appendix 1].

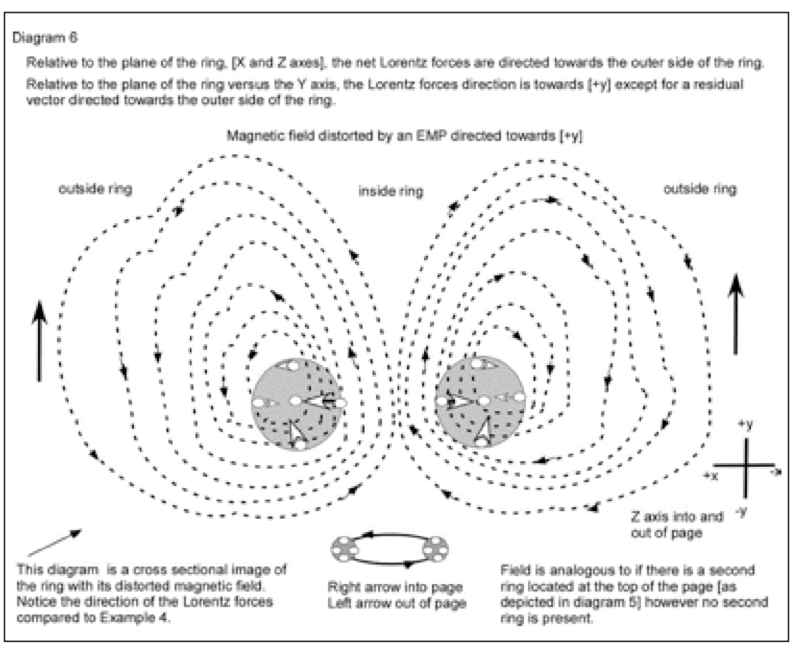
Conclusion —

To reiterate this concept in simpler words, a circular conductor [ring] with its current induces a magnetic field not only surrounding the ring but also within its own essence. Subsequently, that portion of the magnetic field which is located within the body of ring interacts with its own current to produce Lorentz forces. There are electromagnetic propulsive forces produced within the ring by this process.

However, these forces are either blocked by its intact structure, or else the forces are oriented in opposite directions. Moreover they are symmetrical, therefore they neutralize one another. In essence, all of the Lorentz forces within the ring are balanced. As a consequence, there is no propulsion of the ring.

Alternatively, when the magnetic field on one side of the plane of the ring is symmetrically distorted by a directed magnetic pulse [EMP], then within the ring there are still Lorentz forces. Nevertheless, in this scenario some of them are not blocked by its intact structure, nor are they neutralized by other symmetrical opposing forces. As a result, these forces are unbalanced. Consequently, during each pulse; there will be electromagnetic propulsion of the ring along its axis.

Now what form would this hypothetical device assume? Moreover, how would its function appear? First, it would be circular or disk shaped [ring]. Second, it would be associated with a strong mag-



netic field. Third, on one side, relative to the plane of the disk, it would emit very powerful magnetic pulses. Fourth, it would propel along its axis. Fifth, it would emit electromagnetic radiation, either related to the production of the magnetic pulses or due to the rapidly changing magnetic field. Finally, it could travel anywhere within the space of the universe without a propellant.

It should be obvious that the hypothetical device described in this paper possesses many of the characteristics usually associated with Unidentified Flying Objects. Furthermore, if they truly exist and if there is intelligence behind them, then perhaps those who build them do not manipulate gravity as assumed, but rather they are the absolute masters of electromagnetism.

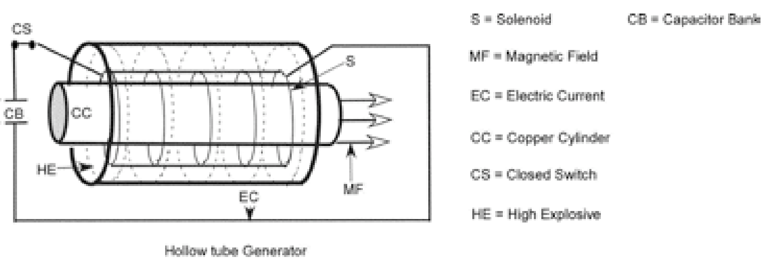
— *Appendix 1* —

Figures 1 and 2 describe the physical structure as well as the function of two types of magnetic flux compression generators. This device is an explosive, self-destructive apparatus good for only one extremely powerful magnet pulse. I have used this model only to demonstrate the underlying physics of the concept. However, I believe there are other analogous or similar devices which are non-explosive as well as non-self-destructive. Even so, they are not readily available for review in the literature, given that they are classified. Nevertheless, in order for the device to function appropriately with respect to this concept, it will have to be non-destructive/non-explosive as well as capable of emitting an extremely rapid sequence of directed pulses.

— *Appendix 2* —

Figure 3 depicts a non-magnetic object—a frog being levitated by a strong magnetic field within the center of a solenoid. The magnetic field induced by the solenoid distorts individual atoms [electrons and protons] within the frog, causing them to become diamagnetic. As a result, each individual atom of the frog repels the induced magnetic field; consequently the frog levitates. Observe that the frog is accelerating away from the surface of the Earth, yet there is no compaction [inertia] as the frog is weightless. In the same way, if

Figure 1



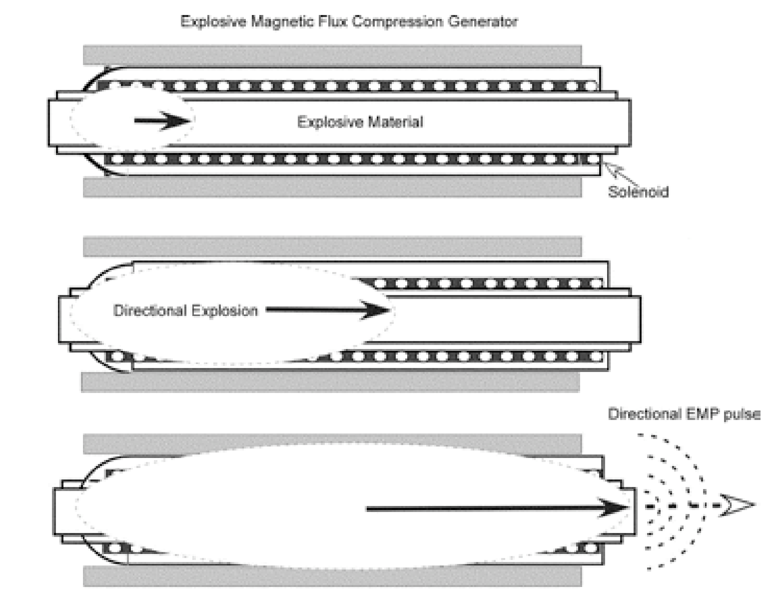
The function of the MK generator is as follows

A longitudinal magnetic field is induced inside a hollow metallic conductor from the discharge of a bank of capacitors into the solenoid which surrounds the cylinder.

The explosive charge which is located around the tube is donated at a time when the current through the solenoid is at a maximum.

The convergent shock wave caused by the implosion causes a rapid contraction of the central cylinder, compressing the magnetic field, and creating a massive increase in the inductive current [hence also a massive increase in the magnetic field] The first experiments were able to obtain magnetic fields of millions of gauss

Figure 2



Magnetic Flux Compression Generators were invented by Sakharov and C.M. Fowler.. A Flux Compression Generator [Magnetocumulative Generator] is a directed Electromagnetic Pulse [DEMP] gun

Figure 3

Figure 4

Los Angeles, California 1991


Figure 5
December 8, 1992 Pueblo, Mexico


Figure 6
Glascow, Scotland 2002

this apparatus was located in outer space and accelerated at 1 G, again there would be no compaction. Thus, by using this magnetic model, an astronaut placed within an imaginary propulsion craft, as illustrated in this paper, will not necessarily experience inertia [compaction]. Consequently, very high accelerations could be achieved without harm to the astronaut.

Notice the resemblance of the shape of the hypothetical propulsion device described in this paper relative to the UFO phenomenon. Even the triangular shaped UFO has circular lights on its undersurface. In addition, the underside of the UFO may be associated with a contracted magnetic field rather that an expanded magnetic field; as hypothesized in this paper, there is propulsion. Furthermore, UFOs are associated with a strong magnetic field, propel along their axis and emit powerful magnetic pulses, as well as electromagnetic radiation. The Figures 4-6 are taken from the website "The Best UFO Pictures Ever Taken," online at www.ufocasebook.com/bestufopictures.html.

About the Author

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The Pyramid Electric Generator

Peter Grandics*

Abstract

We describe here an electric generator capable of harvesting power from Earth's electric field. The generator comprises a geometrically optimized square base pyramid-shaped antenna connected to a set of coils near the pyramid's apex. The coils consist of a coil of high turn number (secondary coil) positioned coaxially within the primary coil; together, these function as a resonant step-up transformer winding, as they are inductively coupled and connected to the pyramidal antenna, which acts as a quasi-capacitive series element coupled to the surrounding space to provide a specific resonant frequency. The primary coil is connected to an AC driver operating in the LF radio band. Activating the primary coil drives the pyramid to a high AC voltage essential for its power attraction function. A power of over 7000VA and an apparent power gain of nearly one hundred-fold have been measured in the secondary coil relative to the input power. We have also established the operating principles for the pyramid electric generator.

Introduction

We have introduced a new theory of space, energy and matter that predicted that electromagnetic interaction propagates through a carrier medium called the space lattice that is made up of pulsating, phi-based spiral vortices (STAR), the excitation of which generates propagating sinusoidal lines of force across the space lattice recorded as electromagnetic waves by our instruments.[1,2] We have also predicted that natural pressure gradients exist within the space lattice that can be used for power generation by inserting a capacitor into such gradient. Furthermore, we proposed that a pyramid-shaped capacitor is an optimally shaped device for tapping the energy gradient of the space lattice that manifests in the form of an electrical potential gradient in Earth's atmosphere. This was demonstrated subsequently.[3-5] We also found that the pyramid-shaped capacitor design should be based on ratios of pi and phi[5] because electrical energy propagates along a phi-based spiral.[6]

We suggested that elemental particles are composed of STAR oscillators that make up the space lattice by electromagnetic compression of its elemental cubes into pulsating conical/vortexual subunits that form the basis for all particles of matter.[1,2] We proposed that atomic and subatomic oscillators could vibrate in a very wide frequency range from sub-acoustic to extremely high EM frequencies.[1,2] This does not mean that material bodies can take or maintain a physical form at any energy level, but this allows the recycling of atoms into the space lattice as part of their natural evolution.[2,6]

Atomic oscillators are constantly in motion, and such motion is electric because all motion and energy is fundamentally electric.[2,6] It is known that atomic bodies constantly exchange electric energy. Such energy flow can be harvested when the spiral physical nature of electric waves is recognized. Russell observed that energy always moves during its generative cycle in spirals towards the higher potential[7]; therefore, we reasoned that an "attractor" of high electric potential must be provided to pull in the random electromagnetic emissions of atomic oscillators. The "attractor" must function as a phi-based antenna/waveguide to focus the phi-based electric emissions of atomic bodies into the apex of a vortex, the same method Nature uses for power multiplication.[6,7] Therefore, a phi-based pyramidal antenna/waveguide must be at a high voltage in order to perform its attractor function.

As all material manifestations are tonal,[8] acoustic vibrations are always generated during the oscillations of atomic bodies, even though they are inaudible most of the time. Sometimes, however, they are detected; one such example is Earth's hum.[9-11] Earth's solid mass and its atmosphere represent a coupled resonant system, and is therefore our expanded target power source. Acoustic waves are also electrical[8] and can be harvested. To achieve this goal, the geometrically optimized pyramidal antenna/waveguide must be connected to an oscillator that operates at a high voltage and is

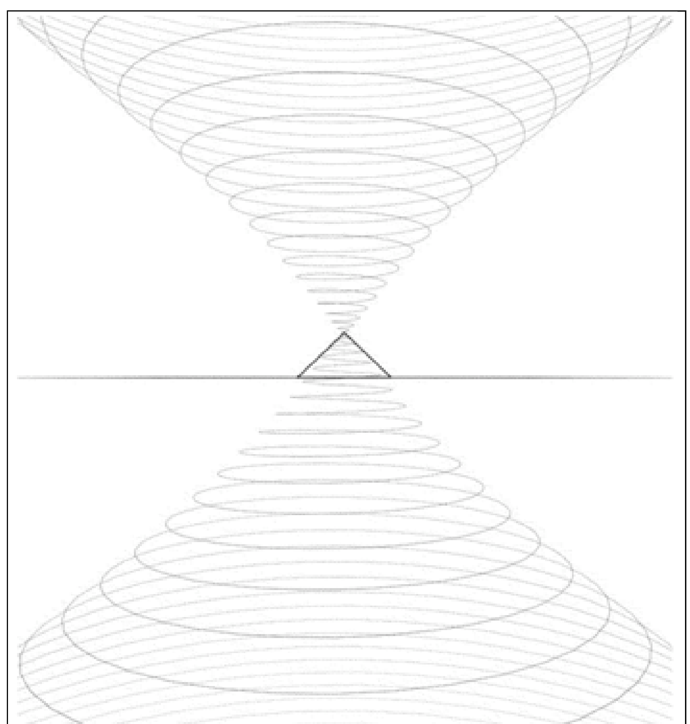

Figure 1. Proposed electric vortices propagating across the pyramidal antenna/waveguide.

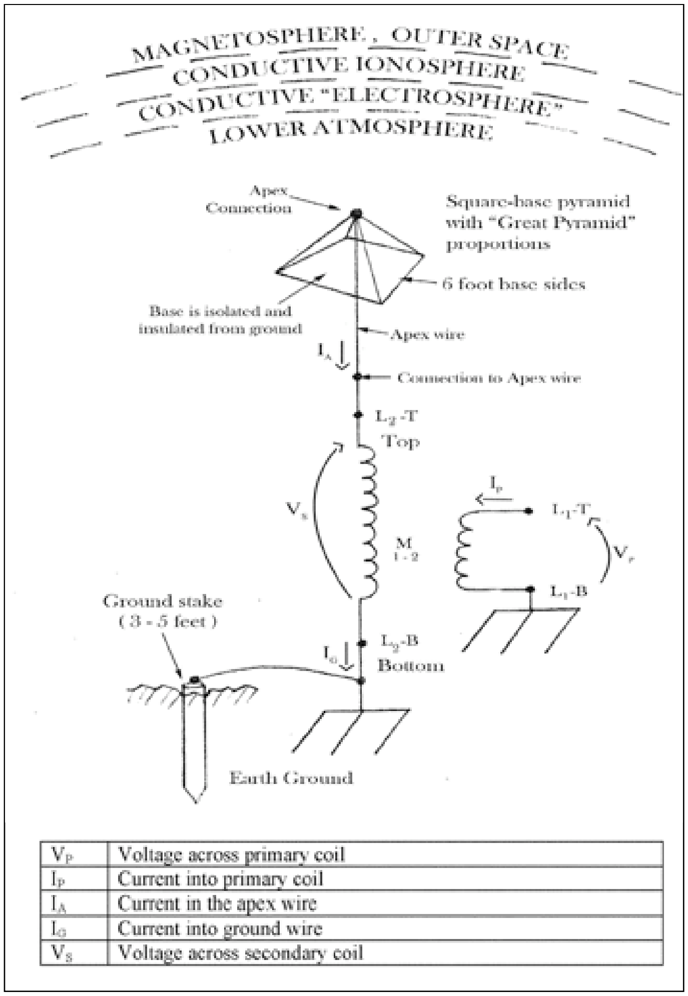

Figure 2. Circuit diagram of the pyramid generator.

tuned to a suitably selected frequency.

The appropriate resonant frequency is dictated by practical considerations. Atomic oscillators can vibrate in a very wide frequency range[1,2,6-8] but short wavelength radiations are readily re-absorbed by atoms and are rapidly attenuated; therefore, long wavelength electromagnetic emissions must be targeted. The operation of the pyramid generator is feasible in the ELF to the LF range. As electric radiation propagates in a vortex,[2,6-8] the emitted electric vortex over the pyramidal antenna (Figure 1) increases its "size," and so it is capable of funneling atmospheric electromagnetic emissions of the same frequency back into the antenna/receiver. Thus, the pyramid electric generator could capture the energy emitted by both telluric and atmospheric atomic oscillators. We have tested these assumptions and demonstrated the basic principles of an electric generator.

Methods and Results

In our previous papers, we reported that a pyramid-shaped capacitor/antenna converts atmospheric electrostatic discharge impulses (ESD) into a periodic high-frequency signal that can be detected in an insulated coil placed in proximity of the capacitor.[3,4] Here, we report on further developments including a new design for the resonant coil system and an expanded range of energy sources targeted. A Tektronix TPS 2024 digital oscilloscope was used for signal acquisition and analysis, and Tektronix A503 current amplifiers were used with the Tektronix A6302 and A6303 probes

Table 1. Physical and electrical parameters of the resonant transformer.

Secondary turns of wire	840 turns
Height	0.85 m
Wire diameter	0.00081 m
Primary turns of wire	5+9/10 turns
Height	0.21 m
Wire diameter	1/4" O.D. copper pipe with 0.030" wall thickness
Primary	
Inductance	98 μH
Resistance	0.118 ohms
Secondary	
Inductance	24.16 mH
Resistance	24.42 ohms
Capacitance	1040 nF
Winding ratio	1:142
Mutual inductance	6.13 mH

for current measurement. High voltage was measured using a Tektronix P6015 probe. An ENI-1140LA amplifier was used to activate the primary coil of the resonant coil system at the selected frequency.

For the experiments, we used a geometrically optimized pyramid-shaped antenna/waveguide.[3-5] The pyramid expressed ratios of pi and phi and was approximately a 1:125 scaled down replica of the Great Pyramid of Giza, 6 feet base length. The pyramid was built of a wooden frame and covered with triangular copper sheets of 0.66mm thickness on its sides that were electrically connected. It was placed on an insulating base and positioned in the general North-South direction.

The secondary coil (coil 2) was wound with a 20 AWG insulated magnet wire with a monofilament nylon spacer (0.06mm diameter) between the turns on a fiberglass cylindrical coil form, and connected to the conducting surface of the pyramid near its apex. Thus, the secondary coil was connected with the pyramidal antenna/waveguide as a quasi-capacitive series element to provide a specific resonant frequency with coupling to the surrounding electromagnetic environment. The other lead of coil 2 was grounded (Figure 2). The purpose of the monofilament spacer was to reduce interwinding capacitance in coil 2 by increasing distance between wire turns of the coil.

A primary coil of larger diameter (coil 1) was wound on or near the lower cylindrical volume of coil 2 with the lead connected to Earth ground, and served as a driver coil inductively coupled with the secondary (coil 2). The secondary was isolated from the primary by layers of silicon rubber and foam. The two coils make a resonant step-up transformer, the parameters of which are shown in Table 1.

In the experiment, the controlled variable was the voltage, which was increased in steps until the maximum voltage in the secondary (permitted by circuit components and test equipment) was achieved for the given resonant frequency. The objective was to "drive the atmosphere" by radiating a high voltage (~20kV p-p), LF (40-120 kHz) signal developed by the high ratio step-up transformer and fed by a powerful signal generator at a resonant frequency either with an L-C resonant secondary coil, or a resonance determined by the pyramid as a quasi-capacitive series element connected to the secondary coil of the transformer and radi-

Table 2. Power measurements in the pyramid-resonant coil system.

Primary							
Frequency (kHz)	83.00						
$V_{P\ RMS}$	3.97	9.86	20.3	26.7	33.8	39	40.8
$I_{P\ RMS}$ (mA)	87.6	300	792	1117	1389	1541	1838
$I_{P\ \text{angle with respect to}}\ V_P$	18°	14°	22°	23°	21°	20°	28°
Total power (VA)	0.35	2.95	16.02	29.70	46.77	59.95	74.75
True power (W)	0.33	2.87	14.85	27.35	43.66	56.34	66.01
Secondary							
$V_{S\ RMS}$	341	1150	3540	4670	6220	7140	7710
$I_{A\ RMS}$ (mA)	62.2	193	438	579	728	834	919
$I_{A\ \text{angle with respect to}}\ V_S$	42°	44°	52°	52°	54°	53°	52°
Total power (VA)	21.2	222.8	1552	2707.8	4530.2	5957.5	7087.3
True power (W)	15.7	160.3	956	1668.1	2664.5	3587.6	4366
Power sec/prim	61.1	75.4	96.9	91.2	96.9	99.4	94.8

ating to the local atmosphere. The experimental data are shown in Table 2. The current readings were also confirmed by using "current sensing" resistors (not shown). An apparent power gain of nearly 100-fold was achieved in the secondary coil relative to input power in the primary coil, and a linear increase of power in the secondary coil with the increase of input power into the primary coil (Figure 3). The power in the secondary coil exponentially increases with the pyramid apex voltage (Figure 4).

A load may be connected to coil 2 to draw power from the system. The load may also be connected via a tertiary coil inductively coupled to the secondary coil. The load may be a resistor, a rectifier or storage capacitor powering a DC load.

A graphic representation of the test system is shown in Figure 5. The pyramidal antenna/waveguide, placed on an insulating base, is coupled to the surrounding electromagnetic environment and serves as an antenna/waveguide for the concentration of atmospheric and telluric electromagnetic oscillations at resonance frequency. As the flow of electric energy is always balanced,[6] two vortices may cross the pyramidal antenna: one from the direction of the ground concentrating into its apex and another one from its apex propagating into the atmosphere (Figure 1). The atmospheric electric vortex possibly acts as an ionic antenna pulling in EM radiations from a large atmospheric domain.

The pyramid's apex is attached to a coil of high turn number (Figures 2 and 4). This secondary coil wound on a non-conductive coil form serves as a step-up transformer, and forms a resonant circuit with the pyramid that acts as a quasi-capacitive series element. The secondary coil is activated by a signal from an RF generator via a primary coil of a few turn numbers inductively coupled to coil 2. The measurement points are also indicated, displaying the positions of voltage and current probes. The data are captured by a digital oscilloscope and recorded on a laptop computer.

Discussion

This study demonstrates a novel approach to harvest Earth's electric energy, including the rational design principles of a "free-energy" device. The masses of Earth and its atmosphere represent a coupled resonant system that is continually electrified by solar radiation. We have found that a pyramidal antenna designed based on the ratios of phi and pi is optimal for the capture of atmospheric electrostatic discharge (ESD) impulses.[3-5] Atmospheric ESD is a product of solar radiation and is a wide bandwidth phenomenon. The pyramid as a wideband, non-resonant antenna is uniquely adapted to harvest the energy of atmospheric ESD.

This study also points out an additional energizing mechanism at work during the operation of the pyramid electric generator. The observation is derived from our theory on energy, matter and space[1,2] as well as that of Russell.[6,7] Both Russell and we observed that spiral motion is a fundamental action of matter and that the vortex is the mechanism of power multiplication in Nature. A vortex concentrates power into its apex where the highest velocity of motion, the highest pressure and the highest electric potential resides.[1,2,6,7] As electricity propagates along a pulsating phi-based spiral,[6,7] a phi-based antenna/waveguide is suitable to focus its energy into the apex of a vortex inside the antenna. A phi-based pyramid is optimal for this purpose.

As atomic bodies can oscillate in a very wide frequency range,[1,2,6-8] a pyramidal antenna/waveguide coupled to an oscillator that is tuned to a suitable frequency can focus the "electric noise" from Earth's atomic oscillators into the apex of the pyramid that is at a high voltage. Russell observed that energy moves towards the higher potential during its generative cycle[7]; therefore, it is essential that the pyramidal antenna be at a high potential. Our data corroborated this assumption.

To select a suitable frequency range, one can begin with the observation that all material manifestations are tonal,[8] and that acoustic frequencies are always generated during the oscillations of atomic bodies even though they remain inaudible most of the time. Acoustic waves are also electric[8]

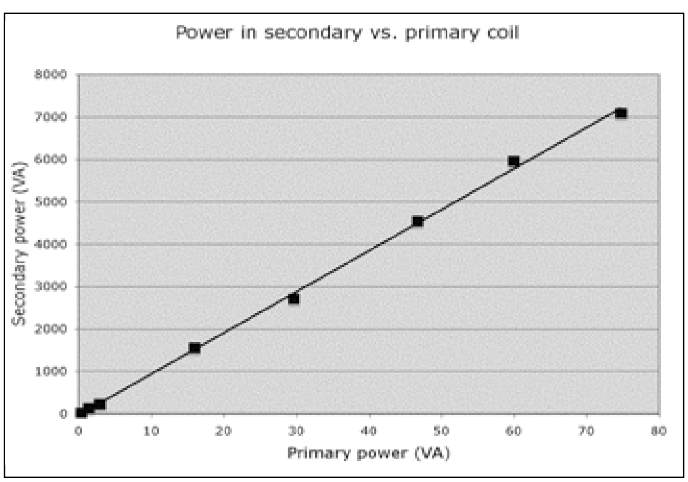

Figure 3. Power in the secondary coil as a function of power in the primary coil.

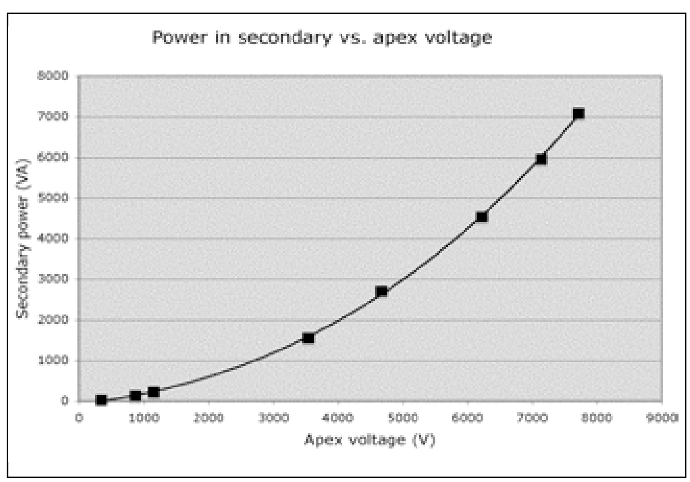

Figure 4. Power in the secondary coil as a function of pyramid's apex voltage.

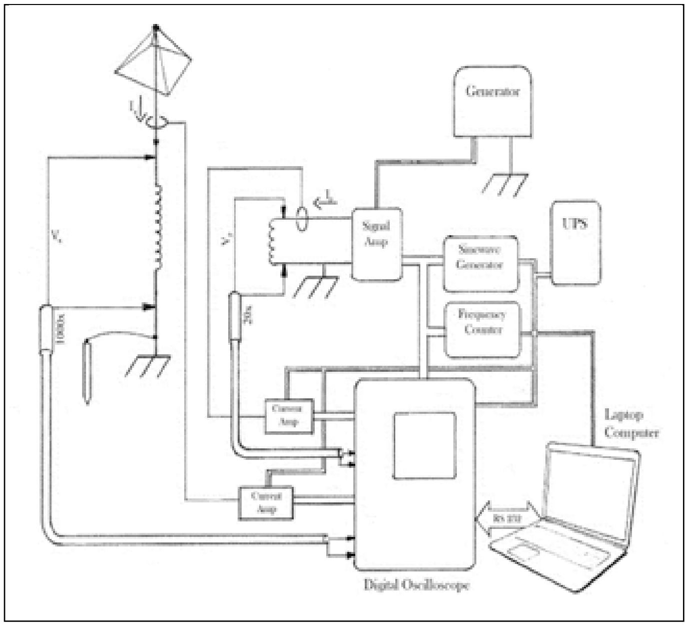

Figure 5. An outline of the test circuit.

and therefore Earth's enormous mass and its atmosphere represent a coupled resonant system that offer a vast source of power.

For practical reasons on our small pyramid, we have chosen a frequency above the audio range but below the radio frequency spectrum; our antenna was made to radiate at the target wavelength. The wavelength of the 50-120kHz range is 6km to 25km, thus allowing energy to be harvested over a large atmospheric domain. As electrical energy propagates in a vortex,[6,7] the emitted electric vortex (ionic antenna) over the pyramidal antenna/waveguide increases its "size" and enables it to funnel the same frequency atmospheric electromagnetic radiations back into the antenna. Thus, the pyramid electric generator captures at the selected frequency the energy emitted by Earth's atomic oscillators.

The presence of an electric (ionic) vortex was demonstrated by radar testing of the space over a 44m tall fiberglass pyramid located near Moscow.[12] The Russians found that the large ionized column of air over the area of the vertical axis of the pyramid had a width of about 500m and reached an altitude of 2km. It is remarkable that this effect was induced by a nonconductive pyramid surface demonstrating a significant degree of atmospheric ionization even under fair weather conditions. Thus, a suitably sized large or an electrically activated small pyramid should open a low impedance path to higher elevations of relatively conductive atmospheric domains.

In conclusion, the total power that can be extracted from Earth's atomic oscillators must be extremely large, likely far exceeding current global electric generation capacity. In our experiment, we obtained over 7000VA power at about 20kV p-p pyramid voltage using a 6 feet base length pyramid. This power is nearly 100 times greater than the power necessary to drive the pyramid to the required operating voltage. By constructing the pyramid generator from dedicated high voltage components and using higher drive voltages, a significant increase in power output is envisioned. The power extraction will also be enhanced by further optimization of the designs of both the pyramid and the coil system.

The power output will also increase by employing larger pyramid structures and coils. As voltage is the primary factor in attracting power into the system, the necessary voltage can be provided by the vertical atmospheric potential on a tall pyramid. Since the atmospheric vertical potential gradient could go as high as 1200V/m near Earth's surface under fair weather conditions,[13] a pyramid height of 80-150m seems sufficient to provide the apex voltage for a self-sustaining power generator. As the pyramid scales up volumetrically, a power generator pyramid of the size range of the GPG could likely have an output in the hundreds to thousands of megawatts range. Groups of several pyramid electric generators could be placed within specific geographical areas, thus combining their energy harvesting capacity.

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Peter Grandics has an MS in chemical engineering and a Ph.D. in biochemical engineering. He has worked in the fields of biomedical research and recently in physics focusing on new energy technologies. He intends to help find answers to our current global energy challenges.

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High Accuracy and Precision Measurements of the Variation of Period of a Simple Pendulum as a Function of Amplitude

K. Garduno, O. Cosereanu, K. Mahokin, E.M. Harley, and G.C. Vezzoli*

Abstract
Using photogate methodology, we conducted very precise measurements of the period of a simple pendulum as a function of amplitude using deflections of 1, 5, 10, 15, 20, 25, 30, and 35 degrees from the vertical. Additionally, we utilized the average of data taken from two stopwatches. Our results show a monotonic slight increase in period of the pendulum as the amplitude of the oscillation increases over the above angular domain.

We interpret our results to show that there is no domain of deflection for which the period remains constant within the accuracy of modern measurement instrumentation, and thus simple harmonic motion (SHM) of a pendulum as attributed to Galileo is an approximation. We also show that the velocity for the first cycle of the pendulum is a positive-sloped linear function of the amplitude of the deflection.

Introduction
Simple harmonic motion (SHM) is a term that has historically referred to an oscillating system for which the period of the motion (time for one cycle) is *independent* of the amplitude of the motion (such as the angle of deflection of a pendulum relative to the vertical, or the motion of a spring). For a pendulum, the period is independent of the mass (m) of the bob, and the simple pendulum is called a "mathematical pendulum" because the suspending string/cable is considered weightless and inextendible, and existing in a *uniform gravitational field*. The term "harmonic" is actually a misnomer because the restoring force is proportional to mg[sin(ø)] whereas the displacement is proportional to the angle, ø. However, for small ø, the sine of ø is approximately equal to ø (in radians), and the motion is approximated as harmonic. The property of SHM ascribed to a pendulum is attributed to Galileo, who apparently was inspired to the conclusion by observing long cord ceiling lamps swinging while suspended from the ceiling of the Cathedral of Pisa. (It is likely that Galileo utilized his own pulse as the timing device to reach this conclusion.) Some physics textbooks,[1,2] however, generally cite that the motion of a pendulum only approximates SHM, whereas others cite that the period does *not* vary and is independent of changes in amplitude.[3] Mathematical analyses generally divide the problem into two aspects: a mathematical treatment that describes the motion for small angles of deflection in which sin(ø) divided by the angle ø in radians approximates unity (usually ø < 15°, even though the approximation is valid for ø=30° as well), and a mathematical treatment for larger angles of deflection. The former gives the value for the period as T ≈ 2pi{SQRT[L/g]}, where L = length of the suspending cable, and g is the acceleration due to gravity at the location where the pendulum characteristics are being measured. The above mathematical expression is often utilized to calculate g; however, this is only valid for low angles of deflection whereby sin(ø) divided by ø equals 1. More recently, experimental work of Aggarwal, Verma and Arun,[4] using a microprocessor which digitally measures the momentary blocking of a laser beam by the pendulum, reports continuous *increase* in period from 2.00477 sec to 2.02093 sec when the angle of deflection is varied from 5 to 30°.

The authors of the papers are graduate and undergraduate students, taking a very advanced course in Math Functions and Problem Solving (essentially, Advanced Analysis), and in doing so, the professor (G.C. Vezzoli) includes intricate "math-labs" in the curriculum in order to generate innovative and novel mathematical functions as well as re-generate established functions. In this particular math-lab, our mission was to re-examine what is traditionally referred to as simple harmonic motion and, using precision methods, to determine whether the period vs. amplitude properties of a simple pendulum indeed yield a flat zero-slope function from scatter plot data, or whether on a high resolution fine scale these data have a bona fide slope. This current paper then resolves this problem.

Apparatus and Procedure
We utilized a brass 200 gram standard laboratory weight for the bob, suspended from a cotton string of length = 45.0 cm, the period of which was measured by a photogate, using its pendulum mode. Additionally, two experimenters utilized stopwatches to measure the time for five cycles of the pendulum so that any human error due to reaction time could be averaged statistically and compared to the pure photogate data. The angle of deflection relative to the exact vertical was measured by each of four experimenters with a protractor oriented with its straight edge parallel to the laboratory bench (which was initially insured to be horizontal by a leveling device) and then averaged. The experiments were conducted in Lebanon, New Hampshire in June 2008, in the evening in a small, remote room on the second floor of a brick and masonry building far removed from any vibrations due to human or automotive traffic. The stand that supported the pendulum was sturdy and clamped to the laboratory bench. Five persons worked in careful teamwork to conduct the experiment and to oversee the individual data accumulation. Careful scrutiny insured that the pendulum moved in a single plane, parallel to the plane of a laboratory wall, without perturbation. Any data that derived from experiments that showed the slightest out-of-plane motion, or agitation, were discarded.

Results and Discussion
The data table and graphed data are shown in Table 1 and Figure 1. The photogate data indicate an average slope of 0.01 sec/degree, or a 21.7% change in period per 0.436 radians, or 0.59 sec per radian. There is no *prima facie* reason to expect a different natural functional behavior of the motion of a pen-

dulum when comparing low angles of deflection with high angles of deflection. Instead, it is the *theoretical* descriptions that differ because at high angles more terms of the expansion of the sine of the angle must be included, and the approximation of $[\sin(ø)]/ø = 1$ is *not* valid. The slight difference between the photogate and stopwatch data are attributed to human reaction time. All of the graphs of Figure 1, however, show a similar increasing directly correlated scatter plot trend, generally with a discontinuous increase in slope at angles of deflection of ~25 degrees or greater. The photogate data for a deflection of 15 degrees yields a value of g= 971 cm/sec^2, which associates with an error of 1% as compared to the standard value of 981 cm/sec^2, whereas the overall averaged tau values yield g=1011 cm/sec^2. The deviant data point for the photogate measurements at 35 degrees in Figure 1 (top and third from top) merits further study and explanation.

Because the experiment was conducted in air, the phenomenon of drag must be considered in the analysis, and since in free-fall drag is proportional to velocity, we then calculated the velocity for one cycle for each angle of deflection. This was done by dividing the total arc length of motion traveled by the pendulum over one cycle by the period of oscillation. The resulting values for velocity in cm/sec vs. angle of deflection in degrees are shown in Figure 3. As expected, the pendulum velocity increased proportionately with the angle of deflection, and the linearity of the dependence is strikingly evident. To our knowledge, this is the first report of linear velocity dependent data for the first cycle of a pendulum as a function of amplitude of deflection.

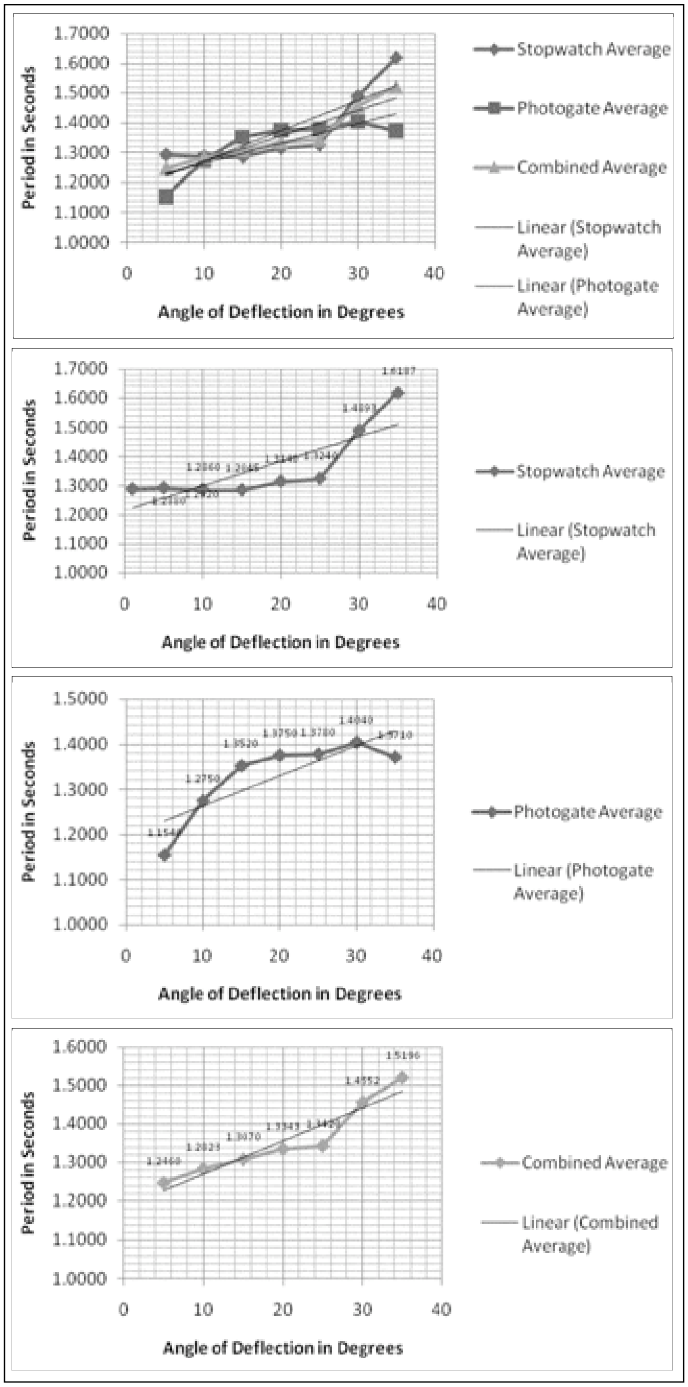

Figure 1 and Table 1. The data table and graphs for the stopwatch, photogate, and combined data showing period of the pendulum in seconds vs. angle of deflection in degrees.

Conclusion

We have proven that the scatter plot data for period versus deflection magnitude of the motion of a simple pendulum from 5 to 30° shows a directly correlated scatter plot with positive slope of ~0.01 sec/degree of deflection, and these data agree well with the work of Arun and co-workers.[4] Our work also is in accord with the work of Reference 4 that indicates that a digital photogate system is necessary for obtaining high-accuracy results. Thus we show that what has been traditionally described as simple harmonic motion, whereby the period of the motion is independent of the amplitude of the motion (zero slope) is an approximation. High accuracy reproducible experimentation proves an increasing functional dependence. We show that the velocity of the pendulum for the first cycle is a linear positive-sloped function of the initial angle of deflection of the pendulum. Finally, we measured the acceleration due to gravity in Lebanon, New Hampshire as 971 cm/sec^2. The observation that the data of Figure 1 show discontinuous slope changes, rather than purely monotonic smooth behavior, is in accord with gravity fundamentally having statistical properties as would be expected from a particle flux, rather than smooth variation of properties as expected from a field-based model.

Significance of the Work

With the acceptance of our paper by the editors of *Infinite Energy*, we were asked to supplement the paper with some commentary on the significance of our work. Since the embracing of the amplitude-independence of the period of a mathematical pendulum relates to the accuracy of the timing

Table 1

Pendulum Experiment

	Trial 1				Trial 2				Average
	Stopwatch 1		Stopwatch 2		Stopwatch 1		Stopwatch 2		Stopwatch
Angle	5 cycles	1 Cycle	5 cycles	1 Cycle	5 cycles	1 Cycle	5 cycles	1 Cycle	1 Cycle
1	6.6300	1.3260	6.1900	1.2380	6.5000	1.3000	6.4400	1.2880	1.2880
5	6.5600	1.3120	6.5300	1.3060	6.5300	1.3060	6.2200	1.2440	1.2920
10	6.3100	1.2620	6.3800	1.2760	6.5900	1.3180	6.4400	1.2880	1.2860
15	6.5300	1.3060	6.3200	1.2640	6.4700	1.2940	6.3700	1.2740	1.2845
20	6.7200	1.3440	6.5000	1.3000	6.5000	1.3000	6.5600	1.3120	1.3140
25	6.7900	1.3580	6.4400	1.2880	6.7200	1.3440	6.5300	1.3060	1.3240
30	6.6000	1.3200	0.0000	0.0000	8.0900	1.6180	7.6500	1.5300	1.4893
35	7.7500	1.5500	8.4700	1.6940	8.0600	1.6120	0.0000	0.0000	1.6187

	Trial 1		Trial 2			
	Photogate		Photogate		Photogate	Total
Angle	1/2 Cycle	1 Cycle	1/2 Cycle	1 Cycle	Average	Average
1	0.0000	0.0000	0.0000	0.0000	0.0000	Out of Plane Oscillati on
5	0.5950	1.1900	0.5590	1.1180	1.1540	1.2460
10	0.6490	1.2980	0.6260	1.2520	1.2750	1.2823
15	0.6690	1.3380	0.6830	1.3660	1.3520	1.3070
20	0.7010	1.4020	0.6740	1.3480	1.3750	1.3343
25	0.6880	1.3760	0.6900	1.3800	1.3780	1.3420
30	0.7090	1.4180	0.6950	1.3900	1.4040	1.4552
35	0.6850	1.3700	0.6860	1.3720	1.3710	1.5196

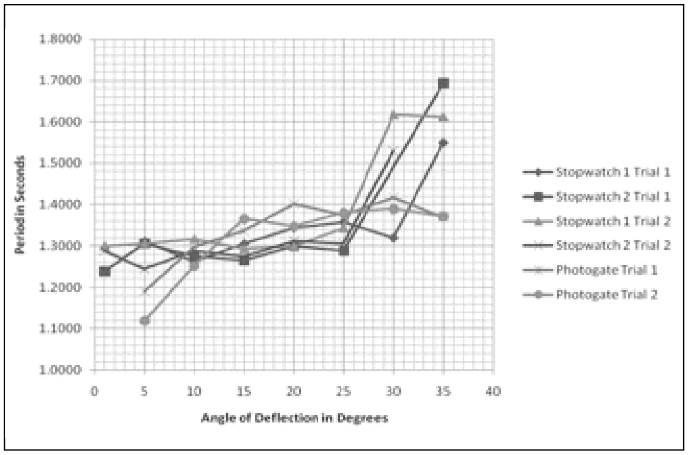

Figure 2. Graph of photogate data and stopwatch data, again using a high-resolution expanded ordinate scale, emphasizing positive slope of directly correlated scatter plot data for period in seconds vs. amplitude in degrees.

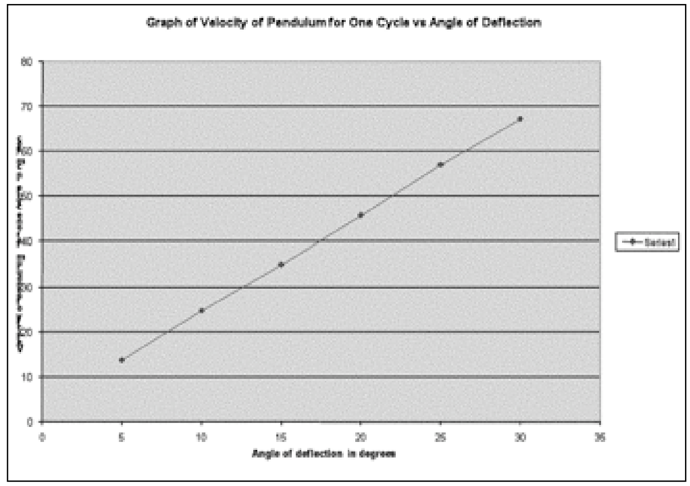

Figure 3. Graph of calculations from data giving velocity of the pendulum for the first cycle in cm/sec vs. initial angle of deflection of the pendulum in degrees.

device, our proving that there is no domain of angular deflection that corresponds to zero slope relationship, reflects significance to timing corrections that must be infused into experimental procedures and interpretations in studies involving the measurement of time. This implies, for example, that an anomaly which causes a variation in amplitude, however minute, will affect the frequency of motion, and even be pertinent to Special Relativity. This realization is also significant in terms of interpreting high-resolution ultra-sensitive gravitation experiments that are interrogated by pendula methods such as anomalies in gravity detected during a total solar eclipse or a planetary line-up whereby the data are not in accord with either Newtonian theory or General Relativity. The fundamental reason and mathematical analysis thereof explaining exactly why there is a positive slope in the period vs. amplitude data for the motion of the pendulum is a valid objective for future work.

We hypothesize that the explanation in some way relates to the time-dependence of gravitational interactions. Since gravity is shown not to be instantaneous, and if gravity indeed is a collision-induced push phenomenon, then there must be time-constant involved in the gravity-property-bearing external-particle's collision with nucleons. [Our initial calculations, based on experimental data on neutrinos (as the candidate external particle) suggests that the interaction time-constant is of the order of 100 usec.] This suggests that gravity is quantized and that the value that is determined for g is an average value, in that g must be continuously changing.

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Katie Garduno grew up in West Fairlee, Vermont, where she now lives with her husband and two children. She graduated from Thetford Academy (VT), where she excelled in mathematics. She is enrolled in the Accounting degree program at Lebanon College where she will be graduating in the spring of 2009. She is employed full time at M2S, Inc., a medical imaging company, as their Senior Financial Analyst.

Pictured from Left to Right: Oana Cosereanu, Elizabeth Harley, Krista Mahokin, Katie Garduno (Photo by Rudi Hanacek)

Oana Cosereanu is an international student from Galati, Romania; she graduated after completing advanced mathematics courses and with a degree in Public Administration. She has been in the U.S. for two years and is enrolled in the Management degree program at Lebanon College. She will be graduating in the spring of 2009. She plans to return to Romania to pursue a career in management and public administration.

Krista Mahokin was raised in Lee, New Hampshire, and is a graduate of Keller High School (TX) and the University of New Hampshire's Hospitality Management program. Throughout school, she enjoyed the challenges of mathematics more so than any other courses. She is currently a flight attendant for Southwest Airlines, and is enrolled in the Radiography program at Lebanon College with a graduation date of 2010. She is considering a career in medical physics. Krista has worked in the mathematics and model-construction of hyperbolic space.

Elizabeth March Harley majored in Art History and Art Conservation at George Washington University. She returned to Vermont to raise a family and operates an antiques business specializing in Shaker and New England vernacular furniture. She is enrolled in the Radiography program at Lebanon College with a graduation date of 2010. Elizabeth notes, "Mathematics has always been an interest and a challenge." She is currently involved in a project with Dr. Vezzoli on the bio-mathematics of the lemniscate function and its relationship to incipient zygote formation.

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Physics Fundamentals

Ali Fawzi*

This is the final installment in a four-part series of articles by the author. The first, "Why the Quantum?," was published in Issue 77, the second, "A Fresh Look at Quantum Mechanics," was published in Issue 82 and the third, "Towards Unification of Natural Interactions," was published in Issue 83.

— Abstract —

Are natural phenomena a continuous or discrete landscape? Is quantum mechanics an island by itself? Is general relativity another island, and all we can do is try to impose a bridge connecting the separate discrete islands?

Or maybe the picture is not at all like that. Perhaps nature appears to us to be an assembly of isolated pieces but all these pieces do share a single common root. If that is the case, then one should look for not only bridging quantum and gravitational theories, but joining all physics to their natural root. Conservation laws which are the foundation of all theoretical physics are not exempted. Beside these basic conservation laws, the current-day theoretical physics problems are considered namely, the mass or Higgs question, the fine-structure constant and the radiation (field) problems.

In this article we shall try to relate the fundamental conservation laws along with the above-mentioned issues to a unique root.

Introduction

Conservation of energy, conservation of linear momentum and conservation of angular momentum are regarded as the three fundamental laws of physics. Some physicists trace these laws to the fundamental properties of space-time. They agree that the law of conservation of momentum stems from the homogeneity of space, while the law of conservation of angular momentum comes from the isotropy of space and the law of conservation of energy originates from time homogeneity. Although this proof is partially comforting, it does not quite answer the question: Why does nature conserve these three dynamical variables? Physics needs to reduce these conservation laws to a single basic root. Actually succeeding to find this root shall reduce physics to geometry, which is a very old dream not yet realized.

But this is not the whole picture. Physicists sometimes seem to dare violate those basic conservation laws. For example, the energy conservation law is violated for very short durations as a result of the "slack" furnished by Heisenberg uncertainty principle.

The relation between geometry and basic conservation laws of physics on the classical and the quantum scale shall be discussed.

A second issue which is of a great concern to physicists is how mass was acquired, or the Higgs field issue. The second part of this article investigates how different particles get the masses which we observe. The third part discusses the fine-structure constant and its relation to space-time geometry. The fourth and last part tries to probe the radiation phenomenon, namely electromagnetic and gravitational radiation.

1. Physics and Geometry

1.1. The Beginning

It goes without saying that we would not have reached this far in understanding the "laws of nature" through what we call physics without the concept of inertial frame of reference (IFR). Galilean, Newtonian, Maxwellian and relativistic physics are direct results of the existence of an infinite number of inertial frames of reference. It is ironic though that both Galileo's relativity principle and Newton's dynamics laws implicitly deny the existence of inertial frames. On one hand, their work is based on the existence of inertial frames of reference. On the other hand, there is no reference to any maximum allowed velocity between any two such reference frames. This is manifested by the additive law of Galilean relative velocities and by the "action at a distance" principle implicitly expressed in Newton gravitation law. We can cite a conceptual fault here, for without a ceiling to all velocities to any observer, there would not be any inertial frame of reference. We showed before that without an absolute maximum velocity which relates time coordinates to spacial coordinates, there would be no discreteness at all and thus the inertia concept which defines the inertial frame of reference would not exist. [This was the original problem that forced Max Planck to assume the discreteness of energy in order to avoid the absurd conclusion that matter must give up all its energy to radiation.] A closely related feature of space-time is orthogonality between axes or degrees of freedom. Galilean relativity advocates the equivalence of all natural laws in all inertial frames of reference. However, transformation from one inertial frame of reference to another does not contain any invariant measure. Thus Galilean physics implies universal orthogonality between time and space. When Poincare, Einstein and Minkowski rightfully assigned an absolute universal ratio which relates spacial coordinates to temporal coordinates (though very large), it made it possible to define a definite set of orthogonal time and space axes. Any arbitrary rotation by a specific spacial axis has to be accompanied by a dependent rotation by the time axis (as well as other spacial axes) in order to preserve orthogonality. This dependence or coupling process is the exact process that created space-time as we know it. Note that if this ratio between spacial units and temporal units was infinite ($c = \infty$), then the condition that any time axis is orthogonal to any spacial axis should be satisfied. This condition is nothing but the singular "point" condition (pre-Big Bang condition).

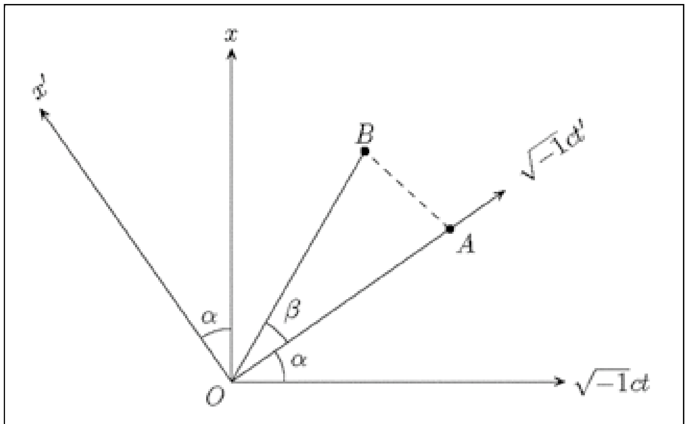

Figure 1. Geometrical presentation of non-additive velocity transformation: Line OA represents fermion (a) and line OB represents fermion (b).

1.2. Fermions and Velocities

Fermions, the only species of particle having an intrinsic directly observable rest mass (inertia), can have an observable velocity v lying between 0 and c ($0 < |v| < c$).

Consider a single fermion (a) traveling with velocity v with respect to inertial frame of reference K as defined by the (x, $\sqrt{-1}ct$) orthogonal axes shown in Figure 1. This ratio, $\sqrt{-1}c$, between the two orthogonal axes preserves the invariance of the line element $d\tau^2$. The velocity of fermion (a) is given by

$$\frac{v_a}{\sqrt{-1}c} = \tan\alpha$$

Now consider a second fermion (b) passing very close to fermion (a). Fermion (a) shall record the velocity of fermion (b) as v_b'. Again v_b' is given by

$$\frac{v_b'}{\sqrt{-1}c} = \tan\beta$$

Note that the hypotenuse which defines the world line of fermion (a), becomes a time axis t' for K' defined by the orthogonal (x', $\sqrt{-1}ct'$) axes. This can be attributed to the universal coupling between spacial and temporal units. If an observer in reference frame K measures the velocity of fermion (b), he will record it as v_b where v_b is given by

$$\frac{v_b}{\sqrt{-1}c} = \tan(\alpha + \beta)$$

And since

$$\tan(\alpha + \beta) = \frac{\tan\alpha + \tan\beta}{1 - \tan\alpha\tan\beta}$$

Substituting $v_b/\sqrt{-1}c$, $v_a/\sqrt{-1}c$ and $v_b'/\sqrt{-1}c$ for the corresponding tangents, we get

$$v_b = \frac{v_a + v_b'}{1 + v_a v_b'/c^2}$$

which is the exact Lorentz transformation equation.

This derivation would not have been possible without the concept of "point." Flat geometry is based upon the fundamental idea that straight lines are composed of an infinite number of individual points right next to each other, and any straight line is defined by only two points. But since points can only be defined by fermions (you cannot define any point by a photon), the distance between any two neighboring points cannot be zero. It is a finite non-zero distance. Implicitly this means that only one point can occupy an infinitesimally small place in space-time. It is impossible to define two distinct points in an infinitesimally small volume. This is actually what Pauli exclusion principle states. Recall that Pauli principle, which is a direct result of the universality of the speed of light, applies only to fermions (1/2 spin particles). The greater the rest energy of the neighboring fermions, the shorter the distance between them. [An implicit conclusion of Pauli exclusion principle is that a classical point is impossible to attain. You need infinite energy to reach a classical "zero" point. This implies that Euclidian continuous flat space contradicts both the HUP and Pauli exclusion principle.]

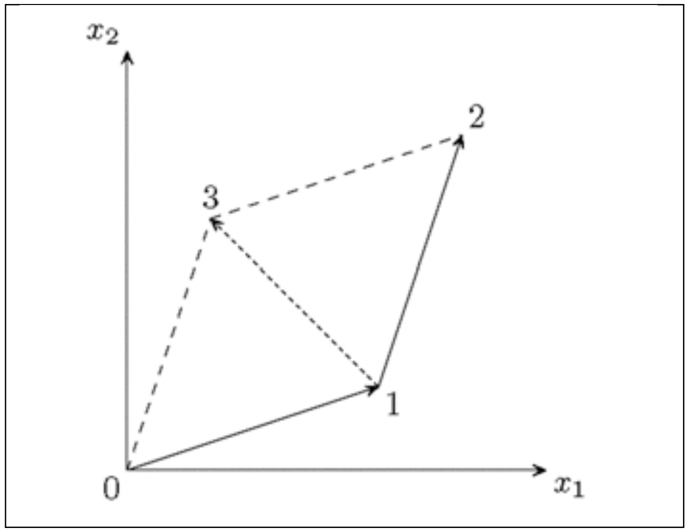

Figure 2. Change of velocity due to outside effect.

1.3. Orthogonality

The rotation of the time axis ($\sqrt{-1}ct$) in Figure 1 to define the inertial frame of reference (x', $\sqrt{-1}ct'$) through an angle α, had to be accompanied by a dependent rotation of the spacial axis x. This was absolutely necessary to preserve orthogonality. But what is orthogonality and why is it absolutely necessary to preserve it in any inertial frame of reference?

Orthogonality means perpendicularity. Actually the root of orthogonality goes back to Euclid. The word contains two Greek words—"ortho" which means straight and is used by Euclid to mean right and the word "gonia" which means angle. So, orthogonal in Euclidean terms means right angle. The concept of orthogonality or "right angle" is fundamental in Euclidean flat geometry. Without orthogonality, there would be no Euclidean geometry. What the concept of orthogonality provides to flat geometry is the notion of independence or null effect (zero). Orthogonality distinguishes between the concepts of dependence and independence. A particle moving in the x direction shall have no impact whatsoever on the y direction. Its displacement and velocity as seen from the y axis perspective shall be zero. When you look at a building from an airplane, there is no way that you can deduce its height. To you, the heights of all buildings are the same (zero). Euclidean geometry is unimaginable without orthogonality or the zero concept. This brings us back to the idea of the "point." A point is a zero entity (zero length, zero area and zero volume). Euclidean vectors, vector algebra would not have been possible without orthogonality. [Euclidian flat space is a continuous one composed of an infinite number of classical "zero" points. Discreteness is manifested in Euclidian space by the point concept. On the other hand, discrete space-time fabric does not contain classical points.]

Discreteness is an implicit manifestation of orthogonality. If we examine orthogonal axes, we see clearly the uniqueness and discreteness of orthogonality. Only the y axis is orthogonal to the x axis. Any slight rotation, no matter how small it is, of any of them breaks the orthogonality condition unless it is accompanied by the exact same rotation of the other axis. The "discrete nature" of orthogonality can be seen too in orthogonal functions. The functions sin nx, sin mx are orthogonal with respect to Lebesgue measure on the interval from 0 to 2π (for $n \neq m$, where n and m are integers).

We shall see that orthogonality is the fundamental reason behind the conservation laws of physics.

1.4. Conservation of Linear Momentum

We learned from the previous section that velocities do not add up algebraically in a flat space-time. In other words, Euclidean geometry when adjusted to preserve the constancy of the speed of light is not linear regarding velocities. But let us now approximate space-time to a Galilean space-time where velocities are additive (though conceptually incorrect). Consider a two-dimensional space defined by the spacial axis (x_1, x_2) as shown in Figure 2. If we accelerate a fermion (a) to a velocity v_1 (where $v_1/c \approx 0$) and eject it from point (0) and take a snapshot after a unit of time, recorded by a stationary clock in the rest inertial frame K, this fermion shall be recorded to be at point (1). The direction of the velocity is given by the direction of line (01). The value of this velocity is the norm of vector 01. Suppose that right at this instant, this fermion is hit by a single photon γ of a specific wavelength. If we take a second snapshot after the elapse of a second unit of time by our stationary clock, the photo shall record the fermion to be at point (2). We can safely infer that the velocity of our test fermion has changed in both direction and magnitude. Its direction is shown by vector 12, and its magnitude is the norm of the velocity vector 12. How can we estimate the outside effect which caused this fermion to change course? The only way this particle can have its velocity changed is by an outside effect "shown" by vector 13 where (0,1,2,3) is a parallelogram. This fermion (a) shall look to an observer at K exactly like another fermion (b) passing by fermion (a) at the same collision instant with a relative velocity (with respect to (a)) equal to vector 13. It is the exact same case discussed in the previous section if the c effect is ignored. Because of this identical observation for fermion (a) after being hit and fermion (b) which has a definite relative velocity with respect to (a), we can conclude that the outside effect is rightfully represented by vector 13. Thus we can say

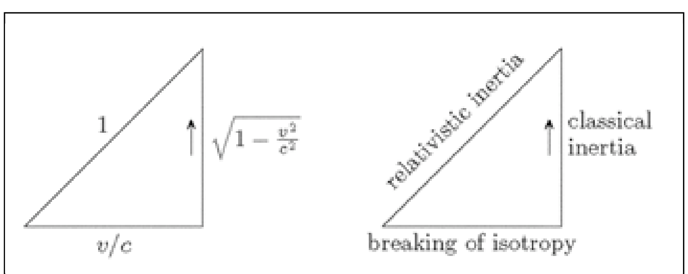

Figure 3. Transformation of classical inertia to relative inertia.

that the vector representing the initial condition 01 plus the vector representing the outside effect 13 shall add up to give the vector representing the final condition 12. Suppose now that we repeat this same experiment except that we double the initial velocity of fermion (a). Clearly everything has to be doubled. The outside effect and the final condition vectors shall be doubled. This can be seen easily by considering geometrical similarities.

Now suppose that the velocity is kept the same as in the first experiment, but that we use two fermions identical to fermion (a). Obviously we shall need two photons identical to photon γ to produce the same outside effect and the final velocity for each identical fermion shall be the exact same one as in the original experiment.

From the above discussion, we can conclude that any outside effect (kick) to a fermion (it has to be massive) is preserved by its vectorial addition to its initial condition. The sum of these two vectors is identical to the vector representing the final condition.

The similar effect of both doubling the velocity or the inertia (mass) suggests the conservation of the product of the two parameters (m and v). Thus, the conservation of the linear momentum is basically due to the concept of adding velocities along with the similarities of flat geometry (vector algebra). Again, these concepts would not have been valid without the idea of the "point" which is a manifestation of Pauli exclusion principle or the universality of the speed of light.[1]

But what about velocities close to c? Clearly the additive law of velocity is not a good approximation in this case. Hence in order to preserve the vectorial adding principle (or apply the rules of flat geometry), the definition of inertia has to depend on velocity. Recall that the sum of the velocities is not their algebraic sum. The transformation from classical inertia to relativistic inertia is performed through the famous right-angle triangle shown in Figure 3.

1.5. Conservation of Angular Momentum

Conservation of angular momentum (or moment of momentum) can be directly deduced from the principle of conservation of momentum as discussed in the last section. Conservation of momentum is basically due to the discreteness of any point (or event) in space-time. This discreteness necessitates the principle of Euclidean vectorial addition. We have discussed this addition principle when spacial coordinates are subject to linear transformation only. This linear transformation is expressed by the rotation of the time axis ($\sqrt{-1}ct$). Every x is transformed to x' where $x' = x - vt$. This discussion revealed the conservation of the dynamical[2] parameter

$$p = mv \Big/ \sqrt{1 - \frac{v^2}{c^2}}$$

in all spacio-temporal coordinates. However, this is not the only possible transformation. We can keep the origin of the spacial coordinates at the same place, while "rotating" for example the yz plane around the x axis. But we have to ask what will cause this rotation to occur. Obviously a point on the y axis (plus another on the z axis) has to be dragged as shown in Figure 4. This point as we mentioned before has to be a fermion, thus having intrinsic rest observable inertia (mass). In order to move it, an amount of motion (or linear momentum) has to be applied externally as we showed in the last section. The greater this linear momentum is, the greater the rotation shall be in a predetermined specified time interval. However, there is another parameter here which affects the ability to induce rotation and that is the "arm" of the rotation. Again, the greater the arm, the greater this ability. Thus this rotation can be represented by a vector which depends on both the linear momentum plus the arm. It is only logical to assume that this vector acts along the x direction with a value that is proportional to the product of the linear momentum and the arm. Recall that the vector of linear momentum which causes the linear transition of the spacial coordinates (through the rotation of the temporal coordinate) is conserved because it does conserve the state of orthogonality after the linear transit of the spacial coordinates. The same argument is valid when we address the vector which "rotates" the spacial coordinates represented by the moment of the momentum vector. Because the rotated axes conserve their orthogonality, the vector which causes this rotation has to be conserved.

1.6. Conservation of Energy

We derived the laws of conservation of linear and angular momentum by showing that dragging the spacial coordinates in a linear manner, or rotating them, does not change the state of orthogonality. That is why the "action" which causes this coordinate transformation is conserved. However, we mentioned nothing about the condition of orthogonality itself. As we all know, the condition of orthogonality, as Euclid stated it, is examined by distance measuring. In Euclidean geometry, distances between points are given by the known equation

$$S^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2$$

where S is the required distance between two points having Euclidean (orthogonal) coordinates (x_1, y_1, z_1) and (x_2, y_2, z_2) respectively. This relation defines a sphere of radius S with its center at (x_1, y_1, z_1). Clearly any point on the surface of this sphere (x_i, y_i, z_i) shall preserve the invariance of S^2.

The increments Δx_i, Δy_i, and Δz_i can take any value that keeps the invariant S^2 unchanged. Hence, we can say that the sum of the squares of the individual degrees of freedom increments is always the same. The fact that orthogonality enables us to obtain this scaler by means of simple addition of any of its three (or more) scalar components is nothing but the energy conservation law, which states that the total energy of a closed system is unchanged. Of course, in the case of velocities comparable to c, the S^2 has to be replaced by the invariant line element $d\tau^2$ given by

$$d\tau^2 = dx_i^2 + dy_i^2 + dx_i^2 - dT_i^2$$

1.7. "In Between" Violations of Conservation Laws

One form of Heisenberg uncertainty principle states that $\Delta E \Delta T$ cannot be zero. Hence we can violate the energy conservation laws via creating unbalanced energy equal to ΔE on condition that this amount has to be balanced out before the elapse of ΔT at most. (It is like a kid stealing money from his mother's purse, but putting it back before she checks the purse.) This "in between" imbalance, or violation of conservation laws, is only virtual. This stolen energy can never be directly observed. This phenomena can be explained by the discrete nature of fermions. If you recall, the sphere which defines the points that preserve an invariant S^2 is defined by large numbers of fermions next to each other. However, the distance between these fermions cannot be zero. It depends on the rest energy of these fermions, as mentioned before. In between neighboring fermions, there is an "uncertain" area where laws are not obeyed. This finite non-zero separation of neighboring fermions (Pauli exclusion principle) is the reason why we see the effect of virtual particles.

1.8. Conservation Laws and the Early Universe

As shown, we experience the fundamental conservation laws of physics as a result of the space-time geometrical orthogonality. This orthogonality gave rise to vector algebra, which is the reason why linear and angular momenta are conserved. Now this orthogonality is a distinct property of flat Euclidean geometry. If space-time fabric is not "flat," we cannot speak about any orthogonality, and consequently conservation. Matter (or discreteness) came very shortly after the Big Bang as a result of the absoluteness of c. However, as energy was condensed in a very small region of space-time, space-time geometry was very far from being flat at this stage of universe age. Consequently, this early space-time fabric did not possess the orthogonality property which distinguishes Euclidean geometry. Hence we have no right to assume the validity of physics conservation laws very early on.

Actually relieving physics from these criteria during the very early stage of the universe solves one of the mysteries confronting physicists today, namely the antimatter question. Remember that antimatter came into the picture as a direct result of the conservation of angular momentum. Thus if angular momentum was not conserved during the short period after the Big Bang, why should fermions be created in matter-antimatter form? To sum it up, we conclude that antimatter was not a necessity during the short period which followed the beginning of the universe.

2. The Mass Question

2.1. The Problem

The current available theory which describes particles and forces is called the Standard Model. This theory has basic flaws. One of these flaws is its "blindness" to various parti-

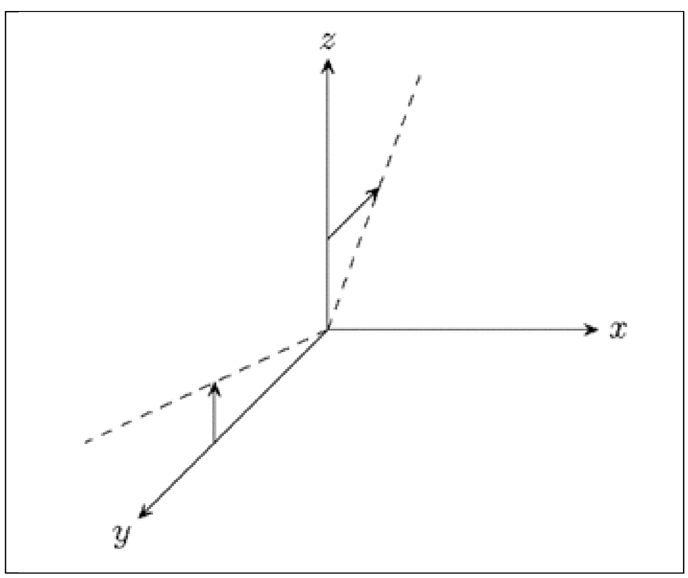

Figure 4. Linear momentum in the yz plane causing angular momentum (or moment of momentum) about the x axis.

cle's mass. In order to use this theory, one has to *a priori* measure the different masses of the fundamental particles and inject these observation results into the Standard Model. So it is only reasonable for anyone using the Standard Model to ask why we have fundamental particles with these different masses? As of now, we don't yet know how different particles got their observed masses and why the ratios of masses are what they are.

2.2. Higgs Proposal

Scottish physicist Peter Higgs tried to answer the mass paradigm. He proposed a background field, since called the Higgs field. This field is a uniform, non-zero scalar field and permeates all empty space at all times. The novelty in Higgs' proposal was that this field is a nonzero field filling empty space. Particles "swimming" in this non-zero field interact with it with different strength. If the particle interacts strongly with the Higgs field, then it is a heavy particle (a proton). If it interacts weakly with it, it is a light particle (electron). Finally, if it does not interact with it at all, it is massless (photons).

2.3. Is It Really Higgs Field?

Recall that a uniform background energy E_b was assumed in order to derive Schwarzschild solutions of general relativity.[1] Actually, assuming the existence of this uniform field is the essence of Mach's principle. The value of the massive object M is directly related to the existence of this field. If this field is zero, then the mass shall not have any physical meaning. The non-zero field value itself is immaterial; what matters is that it is not zero. [It serves only in energy and space-time resolution as per HUP.] We can think of this field as the inertia datum necessary for any matter to be observed (far away resting stars). This non-zero field arises from the fact that the lowest energy states of the field which distinguish the empty space cannot be zero. Space-time cannot be defined in a zero field state (the time yardstick is of infinite length or duration). This non-zero field which defines empty space is a direct result of the discrete nature of the universe and its relation to the measurement process. We cannot measure any observable without defining the datum (or the zero) of this measurement. However, this zero cannot be really a zero. In order to define a point or "singularity," you need a fermion or discrete non-zero energy. [For example, "zero" in any ruler is made of fermions of non-zero mass (or energy). The "zero" energy of a simple harmonic oscillator is $1/2h\nu$ and not a classical zero. In other words, if there is "nothing," then there is "nothing" to be measured from this "nothing." Actually, the "non-zero" nature of any datum is a direct result of the absoluteness and universality of c.] Hence we can say that nature "hates" zero or total emptiness.

This non-zero background field which fills the empty space is the zero (or datum) on our mass scale. Its value is relevant only to the measuring process, but this is not all that this non-zero field does. It has a second function which is as important and that prevents any mass from quite reaching the universal light speed c.

Now let us address the question: How does the mass scale work in nature? Or in other words, why do we see some massive, light, or massless particles? The answer lies in the way this field (background non-zero empty field) interacts with different particles. Suppose that we have a fermion surrounded by this non-zero field. As explained previously,[3] this fermion does have a maximum allowable frequency which is proportional to its mass. In any specific spacial direction, the maximum speed that this fermion acquires during its course is very close to c. As its velocity approaches c in any direction, its inertia in this definite spacial direction shall approach infinity, while its rest inertia shall stay unchanged in all other orthogonal spacial directions. This tremendous increase in the particle inertia in a unique direction shall prompt the background field to decelerate the fermion in this particular direction. If we generalize this directional vibration to all possible spacial directions, we will end up with the fermion rotating around a central point which represents its classical stationary place. The locus of the vibrating fermion is a sphere with a radius equal to $\hbar/mc$. The equivalent classical picture to this fermion vibrating around its classical position is a field pulling the rotating fermion toward the central position. The more the pull is, the closer the orbit to the classical central point. In other words, the background scalar field interacts with different particles with different strengths. A massive particle (*e.g.*, the proton) shall be pulled strongly to its classical central position. A lighter particle (*e.g.*, the electron) shall still be pulled toward the central classical point with a weaker strength, which makes the amplitude of the oscillation greater ($\hbar/m_ec$). A massless particle (*e.g.*, the photon) shall totally ignore this empty field and thus will not have any central point to be pulled to. Therefore a photon cannot be stationary; it has to keep on traveling in a perfect straight line with velocity c in vacuum. In other words, it shall not vibrate. That is why we cannot build a clock using only photons.

This interaction between the fermion and the empty scalar field is what gives mass to the fermion. Without this Higgs field (or maybe better named Mach's field) the notion of mass would not be there.

2.4. To Be or Not To Be

Here is the irony: On one hand the existence of this universe (including ourselves) necessitates the existence of both a non-zero "floor" and a non-infinite "ceiling" (c). The non-zero background, Mach's (Higgs) field, the lowest energy level in a simple harmonic oscillator ($1/2h\nu$) are different sides of the same coin—the "floor." On the other hand, in order for us to observe this universe, we had to resort to unphysical concepts: the zero, and consequently infinity, the null, and consequently the ultimate. Our eyes see light because of the zero concept. Actually the concept of the photon is intimately related to the zero concept. Our computers would have never worked without the zero concept. We deny the existence of the zero while it is absolutely necessary for us to see "non-zero objects" within this universe. This is what made the issue of measurement ambiguous, to say the least.

3. The Fine-structure Constant

3.1. General

As Feynman put it: "It is one of the greatest damn mysteries in physics, a magic number that comes to us with no understanding by man." He was referring to α or the fine-structure constant. This coupling constant α is approximately equal to 1/137 and is given by (in cgs units)

$$\alpha = \frac{e^2}{\hbar c}$$

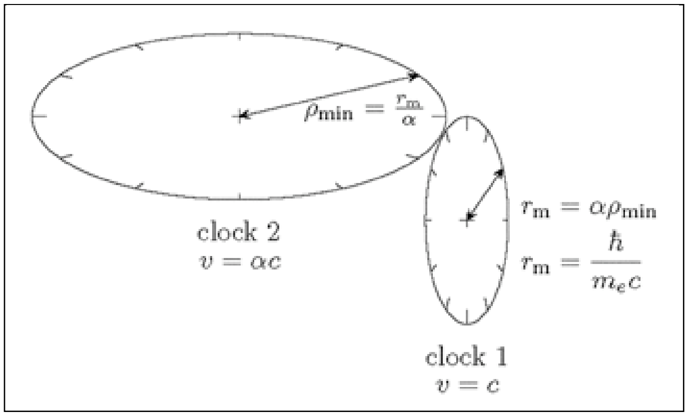

Figure 5. Two clocks coupled together by absoluteness of c.

where e is the electrical charge of the electron, $\hbar$ is Planck-Dirac constant ($\hbar$ = h/2π), and c is the universal speed of light in vacuum.

Strangely, α has several physical meanings, namely:

• It is the ratio of the velocity of the electron in the lowest circular orbit in the Bohr atom model, to the speed of light in vacuum, or

• It is the ratio between two energies:
1. The energy required to bring two electrons (or protons) from infinity to a distance r_0, and
2. The energy of a photon with a characteristic wavelength equal to $2\pi r_0$, or

• For an elementary particle (fermion) with rest mass m, and corresponding amplitude a ($a = \hbar/mc$), α is the ratio between two energies:
1. The energy required to bring two of these elementary particles (*i.e.* two electrons or two protons) from infinity to a distance a, and
2. The mass energy E_r of this elementary particle mass ($E_r = mc^2$), or

• It is the ratio between the amplitude (or radius of vibration) of the electron due to its rest mass ($a = \hbar/m_e c$) and the radius of the first circular orbit of the Bohr hydrogen atom ($a_0 = \hbar^2/e^2 m_e$).

Thus we see that beside the fact that this mysterious coupling constant has various physical meanings, we seem not to be able to relate it to natural constants. In the following section, we will try to reach to the root of this puzzling constant and its relation to basic space-time geometry.

3.2. The Root

We have learned that there are two natural potentials. The first one is global in nature manifested by the inertial mass which has to be measured from a non-zero background field which fills the empty space. The second one is local due to the spin charge; this energy source has to be stored between two spin (electrical) charges. These two potentials are the origin of the two major or classical forces, namely gravitational and electrostatic forces. We can use any of these forces to build a clock. However, there is a problem with building two completely independent clocks with these two energy sources. The problem lies in the fact that each elementary 1/2 spin fermion (spin or electrical charge) has to have an intrinsic or inertial mass. Hence any clock built using elec-

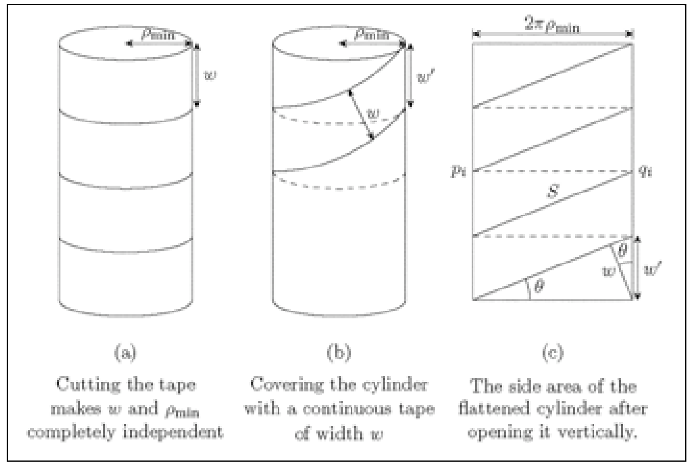

Figure 6. Geometrical representation of the fine-structure constant α.

trostatic potential must include inertial mass or the other source of energy as well. This "mixing" or superposition is very similar to Einstein's experiment where an observer sends a light signal vertically from a light source on the floor and receives it back after the light photon is reflected by a mirror on the ceiling right above the light source. If this experiment is observed by another observer who is going with a relative uniform velocity with respect to the first observer, then, because of the absoluteness of the speed of light, c, to all observers, distance, time, energy and momentum shall be relativistically affected (through factors γ or 1/γ). The exact same concept applies to the superposition of the basic two clocks.

To clarify this idea, consider a classical clock based on electrostatic potential. The clock is composed of an electron attracted to central positive charge due to the spin (electrical) potential. The electrical potential has the value of e^2/ρ (actually it is the potential anomaly from the infinite position), where ρ is the distance between the two charges (or radius of rotation). The minimum period of this clock is given by

$$T_e = \frac{h}{e^2/\rho} = \frac{h}{e^2}\,\rho$$

The corresponding minimum radius of rotation r_e is

$$r_e = \frac{c}{2\pi}\,T_e$$

Substituting for T_e, we get

$$\frac{\rho}{r_e} = \frac{e^2}{\hbar c}$$

Or

$$\frac{\rho}{r_e} = \alpha$$

where α is the fine-structure constant. This ratio, ρ/r_e, is the root of the fine-structure constant, α. Two opposite elementary electrical charges (e^+, e^- or a dipole) cannot cause the orbiting speed of one charge around the central one to reach c at a radius of rotation equal to the dipole's distance. The maximum orbiting speed available for the oscillating charge is only αc, where α is the fine-structure constant.

Thus an electrostatic potential of e^2/ρ shall make the electron oscillate at a radius of $r_e = \rho/\alpha$ with an orbiting velocity of c, or equivalently oscillate at a radius of ρ with an orbital speed of αc. In other words, satisfying Planck's relation, $h = ET$, necessitates scaling the velocity of the orbiting fermion from c at radius r_e to $(e^2/\hbar c)c$ or αc at radius ρ.

This proportionality ($\rho/r_e = \alpha$) says that the maximum orbital speed that the electron can reach because of the electrostatic potential is αc. Thus the electron shall oscillate with its maximum frequency (or minimum period) associated with this specific electrostatic potential. Hence, for a central observer, this electron shall be stationary or non-radiating electron. It will return to its starting position before being missed as a result of time discreteness. Now, the electron which oscillates because of the electrostatic potential does have an intrinsic mass as well, thus it shall oscillate due to the inertial mass (energy) content with a period T_m where

$$T_m = \frac{h}{m_e c^2}$$

where m_e is the electron intrinsic mass.

The corresponding minimum radius of oscillation r_m is given by

$$r_m = \frac{\hbar}{m_e c}$$

Obviously, the minimum angular momentum of this oscillating electron is $\hbar$. In order to figure out the minimum radius of rotation of the electron due to the electrostatic potential, we equate its classical angular momentum to $\hbar$. This quantization shall reveal a minimum radius of rotation $\rho_{\min}$ where

$$(m_e c)(r_m) = \hbar = (m_e \alpha c)(\rho_{\min})$$

Or

$$\rho_{\min} = \frac{1}{\alpha} r_m$$

So we have two apparently independent and superimposed clocks. One runs at radius $r_m = \hbar/m_e c$ with orbital velocity c, and the other runs at radius $\rho_{\min} = r_m/\alpha$ with orbital velocity of αc (see Figure 5).

3.3. The Fine-structure Constant and Space-time Geometry

Let us try to make a geometrical picture of this "compound" or superimposed clock. Consider a perpendicular cylinder of radius $\rho_{\min}$, and a flexible tape having a constant width, w. Let us suppose that we want to cover the curved area of this cylinder totally with this tape without any overlapping. One can easily start cutting this tape into segments of equal length, with the length of each segment equal to the perimeter of the cylinder, and then cover the cylinder with the tape segments applied vertically, one after the other until the whole cylinder is covered. Obviously, the tape width, w, does not come into the picture here. Any arbitrary width will do, *i.e.* it will cover the whole side area of the cylinder without overlapping.

Now suppose there is another condition enforced on this covering process, and that is that the tape has to be continuous, *i.e.* cutting is not permitted. Enforcing this new condition shall break the independence between w and $\rho_{\min}$. For any specific helix wrapped around the cylinder, w has to be related to the vertical pitch of the helix (see Figure 6). If this perpendicular cylinder is flattened out by cutting one of its vertical sides and then unwrapped to form a planer sheet, then the continuity and non-overlapping condition necessitate that the intersection point of the bottom border of the tape with the left cylindrical vertical side line has to have the same elevation as the intersection of the top border of the tape with the right cylindrical vertical side (points p_i and q_i as in Figure 6(c)). [The perpendicularity of this cylinder is a direct manifestation of the orthogonality between charges previously mentioned.[1]] The vertical pitch in this case, $\acute{w}$, is given by

$$\acute{w} = w/\cos\theta$$

Now what does this have to do with the fine-structur constant α? In Figure 6(a), where we see that w can take any value, it is actually the classical picture of an electron orbiting around a positive charge (neglecting the gravitational effects). In this classical picture, the electron can take any orbit. In Figure 6(b) and Figure 6(c), the relation between w and $\rho_{\min}$ is established. For any particular w, the following relation always holds true

$$\frac{w}{2\pi\rho_{\min}} = \sin\theta$$

If we introduce r_m where $w = 2\pi r_m$, then

$$\frac{r_m}{\rho_{\min}} = \sin\theta$$

If S is the tape length for one cycle, then

$$\frac{\text{vertical pitch } \acute{w}}{\text{tape length of one cycle (wavelength)}} = \frac{w/\cos\theta}{S} = \sin\theta$$

This quotient can be defined as one of the following ratios:

- the tape width w divided by the cylinder perimeter;
- or the tape "radius" r_m divided by the cylinder radius $\rho_{\min}$;
- or the vertical pitch $\acute{w}$ divided by the tape length for one cycle S.

This quotient is nothing but the fine-structure constant α. Hence,

$$\alpha \equiv \sin\theta$$

And

$$\cos\theta = \sqrt{1-\sin^2\theta} = \sqrt{1-\alpha^2}$$

Or

$$\acute{w} = w/\cos\theta = w/\sqrt{1-\alpha^2}$$

This geometrical representation has a real physical meaning. First of all, the tape continuity is a manifestation of the absoluteness and universality of the light speed, while the non-overlapping condition is a manifestation of the quantum nature of time. The tape width (or radius) which represents the mass clock perimeter (or radius) is related to the cylinder perimeter (or radius) which represents the electrostatic clock perimeter (or radius) by the ratio α, the fine-structure constant.

The angle θ and the factor $\cos\theta = \sqrt{1-\alpha^2}$ and its implication on the apparent tape width ($\acute{w} = \frac{w}{\sqrt{1-\alpha^2}}$) is the relativistic time dilation and length contraction expression. Thus, we see that the fine-structure constant is there because of local curling of space-time as a result of the existence of at least two 1/2 spin elementary particles. This curling is manifested by the cylinder curvature. The quotient $\acute{w}/S$ or $\sin\theta$ is the ratio between the distance covered along a straight line $\acute{w}$, and the distance along the cylinder's body, S, in order to reach the same elevation. Equivalently, the distance between two spin (electrical) charges can be measured globally on a at space-time, $\acute{w}$, and alternatively a locally curled space-time, S, caused by the existence of two spin charges.

Finally, we can find some equivalence between space-time and continuous elastic medium. The product $\hbar c$ is equivalent to the elastic modulus of elasticity, while the reciprocal of the fine-structure constant $1/\alpha$ is equivalent to the shear modulus of the elastic medium. The square of the quantum electrical charge e^2 is equivalent to a measure of Poisson's ratio.

4. Non-inertial Radiation

In this section we shall address the following questions:

1. Why do electrical charges (and gravitational charges for that matter) radiate energy, when accelerated, in the form of electromagnetic waves (or gravitational waves)? [Any acceleration shall make an electrical charge radiate. In the mass case, the second time derivative of the quadrapole moment must by non-zero.]
2. Why does the charge (*e.g.*, the electron) not act on itself?
3. What is the nature of the radiation energy, and why does it possess no inertia?

4.1. The Puzzling Radiation

In his Nobel lecture, Richard Feynman indicated that, in his youth, he believed profoundly that there is no field. According to him, this idea solved the two sources of difficulties in quantum electrodynamical theories—the electron infinite self energy, and the infinities due to the infinite number of degrees of freedom in the field. However, he had to give up this "idealistic naive" proposition because he could not explain the force of radiation resistance without the existence of the field. We argue here, almost forty years after Feynman's lecture and sixty years after his "naive" proposition, that he was right after all. *There is no field*. The source of the force of radiation resistance, which is necessary for the conservation of energy, is once more the absolute constancy of the speed of light.

In order to clarify this idea, let us consider the following experiment. An astronaut is space walking in the vicinity of a space shuttle about two hundred kilometers from the earth level. This astronaut holds an electron in his hand. Obviously, the astronaut shall not observe any light coming from the electron, as it is completely stationary with respect to him. His space mission commander inside the shuttle shall record the exact same thing, a stand still electron emitting no light at all.

Now, what about the people at the control center on earth? To them, the electron is constantly accelerating, thus they must see it emitting electromagnetic waves (photons) continuously. So actually the existence of this radiation energy is dependent on the status of the observer. This reminds us of the nature of magnetic force ($\boldsymbol{F}_m = q\boldsymbol{v} \times \boldsymbol{B}$) which definitely depends on who measures it. An observer who is at rest with respect to the particle shall experience no magnetic force whatsoever exerted on the electrically charged particle, while another one going with uniform velocity $\boldsymbol{v}$ with respect to the first observer shall observe the very same particle being pushed in a direction perpendicular to both $\boldsymbol{v}$ and $\boldsymbol{B}$. Einstein proposed that this "apparent" force is nothing but a manifestation of the absolute constancy of the speed of light.

We will show in the following section that this form of energy (radiation) is a direct result of the fact that there is an absolute universal ceiling to all velocities to any observer.

4.2. Why Radiation?

We showed before that there are only two natural potentials in nature, namely gravitational and electrostatic potentials.[1] These two potentials are rooted in space-time global curvature and local curling. The gravitational potential is a manifestation of one form of energy and that is the inertial mass. The inertial mass would not exist without an arbitrary non-zero uniform energy filling space-time. The second form of energy which is responsible for electrostatic potential is the "spin" energy. This energy only exists between at least two elementary 1/2 spin fermions. Now we notice that in electrodynamics the lowest order radiation is the dipole radiation (electromagnetic radiation) which depends on acceleration. On the other hand, in general relativity, the lowest order radiation is the quadrupole radiation (gravitational radiation).

Let us first consider the spin (electrical) charge. It was shown before that any two opposite charges at arbitrary distance d can constitute a classical clock. [Two similar charges can constitute a virtual clock using the Coulomb potential of e^2/d between them.] The maximum orbiting speed of an orbiting pole with respect to the other charge is c with a radius of oscillation equal to d/α where α is the fine-structure constant. This orbiting charge is not differentiable from a stationary charge. Suppose that this "stationary" charge is to be shaken using an outside agent, and its velocity in a given spacial direction is given by

$$v = a_0 + a_1 t + a_2 t^2 + \ldots = \sum_{i=0}^{\infty} a_i t^i$$

Then, this shaking process can always be decomposed into simple periodic components using Fourier transformation. Each one of these simple harmonics represents an orthogonal clock with orbiting speed equal to v_i. Now if we consider only one harmonic, then an outside observer shall find the electrical charge oscillating with two speeds, one is c and the other one is v_i. (See Figure 7.) (Generally, there is an infinite number of v_i.) Now, because of the absoluteness of c, the outside observer cannot see the resultant of the two velocities different than c. This relativistic effect will create an effective extra resistance and radiating energy (electromagnetic waves) to compensate it. The opposite case, (*i.e.* absorbing photon) is manifested by the same two orthogonal clocks except that the velocity direction v_i shall be opposite to the orbiting velocity c at the tangent point of the two orthogonal clocks. Hence we actually do not need the field concept to account for the force of radiation resistance as Feynman thought; it is simply a relativistic effect.

But why do electrical charges going with uniform velocity (to some observers) *not radiate*? The answer is simply because of the nature of the Fourier transform of the a_0 term in the previous sequence. The Fourier transform of this con-

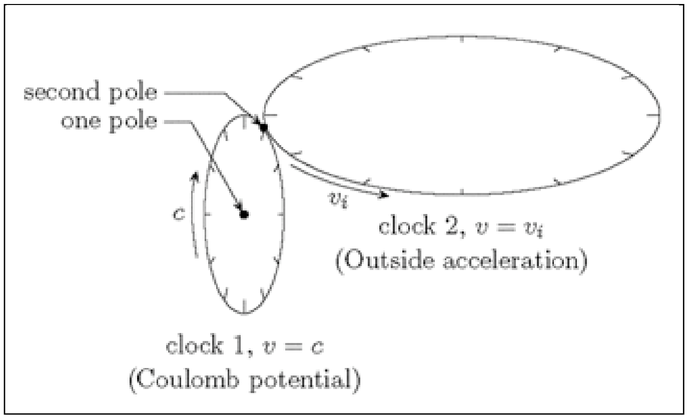

Figure 7. Two orthogonal clocks due to Coulomb potential and outside acceleration.

stant velocity $v = a_0$ is not a periodic function. Thus we cannot form the second clock (with $v = v_i$) if the charge is going with a uniform velocity.

A final and very interesting note here is that we noticed that all the above reasoning is based on a dipole configuration. A "single" electrical charge shall radiate when accelerated only if another electrical charge is there to "detect" the radiation. Hence we conclude that if there is only one electrical charge in the universe, it shall never radiate without the existence of another charge anywhere in the universe. This is very much resembling the Mach's field and the notion of inertial mass, which substantiates the proposition that the concept of charge has to be rooted in space-time geometry.

Now let us move to the gravitational charges or mass. Again, any stationary mass is oscillating with orbiting velocity of c and radius of oscillation of $\hbar/mc$ around a central point which indicates its classical position. If this "stationary" mass is shaken using outside agents, we can treat this case exactly like the previous case of the electrical charge except that the second term in the velocity sequence (a_1t) shall not contribute to the different harmonics (or the second clock). Thus in the mass case, both the first term (a_0) and the second term (a_1t) will not contribute to Fourier transform. The reason is that the mass possesses an inertial property, or in other words the mass can have kinetic energy. Actually Newton's second law is a manifestation of this property. Thus any work (energy) added through a uniform acceleration ($v = at$) shall contribute only to the inertia of the mass through the relativistic equality

$$W(\text{or kinetic energy}) = mc^2(\gamma - 1)$$

Only the gravitational charge (mass) does have this property while the other charge (electric or spin charge) does not. In fact, this is the root of the equivalence principle (inertial mass gravitational mass). Note that there is no equivalence principle when it comes to electrical charge.

5. Conclusion

Definitely, nature is not complex. Gliding high above natural phenomena, we can see what cannot be noticed when immersed in minute details. The first striking feature of this global observation is the unique source of all natural phenomena. The absolute universality of this source is what makes it unique. Maybe that is why Einstein's theory of relativity has as much right to be called theory of absoluteness.

The second feature is the duality in nature. Any one part or half is absolutely necessary for the other to exist. Absoluteness and relativity, mass and Mach's (Higgs) field, electrical charge and its dipole, are clear manifestations of this amazing reality.

Looking back more than one hundred years ago, one cannot help asking the question: Why didn't theoretical physicists think of looking for a unique root to physics? Maybe because they never believed that such a single sole root exists.

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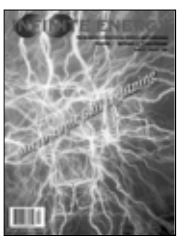

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