Time is Up!

Horace Heffner
Original posting September 10, 1995

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As the New Year approaches, it is good to reflect on things past and present, and herein are some reflections regarding cold fusion. Today, December 13, 2005, we are a little over a year away from the 18th anniversary of Fleischmann and Pons' announcement of cold fusion in a bottle, and over 10 years past the time the original posting under this title was made. I predicted in 2001 (see below) the solution to our energy problems would be in sight by March 2007. There is not much time left for that prediction to be fulfilled. It is still much in doubt as to whether the goal will be reached by then. Things look less optimistic for that now than in 2001, though the expectations for eventual success are higher with each passing year. Continual progress has been made, as can be seen at http://lenr-canr.org/index.html. In, fact, LENR-CANR.org is in itself major progress toward a cold fusion infrastructure,. It is provided by Jed Rothwell, Edmund Storms, various financial supporters, and numerous professionals who contribute articles without fee. There is still much that can be done to establish infrastructure for cold fusion and energy research, some of which is noted on page 7 below. Hopefully serious amateurs can be attracted to the field, as well as young professionals, but a stigma that lingers over the field may not lift until success is clearly in sight and big money rushes in, closing off opportunity. It appears substantial infrastructure for amatuers and individual or retired researchers is not likely to develop before success is at hand. Still, though time is long up for Carl Sagan's prediction, time is far from up for the field of cold fusion.

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Horace Heffner March 25, 2001

Please excuse me, but occasionally I wax philosophical. Now that March 23, 2001 has passed, the twelfth anniversary of the Pons and Fleischmann's announcement, it seems appropriate to recall some early reflections regarding that event. Due to being very busy of late, I have been behind in my vortex reading, so have only now read some of the latest dialog regarding Pons and Fleischmann's contributions to society, which I think will some day will be fully recognized. This post is a bit wordy, especially for me, but I think the assertions are still clearly true and that some of the vison of this post has been fulfilled. It is very satisfying to see the continual

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progress of research, including the recent progress of Edmund Storms who posts here on vortex. Vortex itself I think is a significant tool for progress. The post below was originally made to sci.physics.fusion, which deteriorated to a less than useful forum for me. We are lucky to have vortex, and we are making progress toward solving the energy problem, however slow, fringe, and criticised it may be. I am very happy to play what ever small part in this great play in which I have been or may be cast. I am optimistic that by the eighteenth anniversary the solution to the energy problem will be clearly in sight, if not in the process of implimentation.

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Time is up! According to Gene Mallove's book, "Fire from Ice", in October, 1989, Carl Sagan said: "I guarantee that five years from now, this will be a dead issue. It will either be, there is such a thing or there isn't such a thing. We will not be sitting on some middle ground wondering. The stakes are too high. Either way, the definitive *disproof* of Fleischmann and Pons or the definitive *proof*. The rewards are so great that scientists - competitive, querulous lot - will decide one way or another."

He was referring to H + Pd on a table top, at room temperature, the Pons and Fleischmann experiment. We are now approaching the sixth anniversary of his prediction. Was he right? Was positive proof or disproof, without contention, in existence by five years? Now? It seems like there remains too much debate to say yes to a definitive proof.

The issue of fusion in a bottle seems small compared to the greater issues of whether the behemoths of conventional fusion will ultimately be practical, and, if so, when. If funding is not slashed severely, perhaps definitive answers for conventional fusion will be available in five years.

Even more important, though, is finding a definitive answer to the question "is there a small cheap way to generate energy". A definitive answer to that question involves searching an almost infinite solution space. The process can therefore not end definitively until the answer is yes. The search of this solution space does not preclude a simultaneous search of the conventional fusion solution space. There is

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no reason for researchers in differing areas to destructively compete for much needed research funds by calling each other frauds, crooks, etc. Everybody will be the worse off for it, especially our children.

The cliche that scientists make lousy politicians is being well proven in this news group (sci.physics.fusion). The key to success in the political arena is unity, and there seems to be anything but unity here. As we approach the six year anniversary of Carl Sagan's prediction, we have an opportunity to renew the vigor and cooperation of six years ago. We can work together to broaden the search for energy.

We have taken so much from our children. Confirmation of global warming was announced on TV today, September 10, 1995. Here in Alaska, evidence has been plainly visible of warming for some time, through the sudden bizarre behavior of glaciers that have been stable for 10,000 years. If the warming continues indefinitely the earth will end up like Venus. We have created a financial and environmental debt so massive that the only way we have to pay it off may be through investment in energy research.

I may be an amateur, and my paycheck is my pension, but I can plainly see that the time has come to support energy research of all kinds. It may not be too late to affect the DOE research budget.

Bad science is misleading and therefore less than worthless to the cause of science, the seeking of truth about nature. I would include mathematics in the definition of science because of my belief that the constructs of mathematics have an existence outside of human experience, are part of nature itself, and, as logic, an integral part of science.

Even though maybe not explicitly dichotomized or recognized in most of the debates in the internet news group sci.physics.fusion, there is another, seemingly opposing, but valid, not contradictory, point of view. I would like to call the other point of view the engineering point of view. For the purposes of brevity, I would like to include in the definition of engineering all the physical and psychological human processes involved in the creation of mechanical devices to solve a problem.

The problem spaces of science and engineering, though related, are different. The goals of creating knowledge vs. creating machines are different. Yet the two

processes are so similar, and mutually supportive, the distinction is often not drawn. Engineering is required for science. Every tokamak installation has associated engineering. Engineering is the application of science and quantification to the creation of devices. Engineering has progressed primarily through the advancement and dissemination of scientific knowledge. Engineering and science are the yin and yang of human progress.

The distinction between science and engineering is most relevant because what is "Pathological Science" is often very good engineering. Creating machines is primarily a creative process. There is a large body of literature regarding the creative process. Included in the psychology of creativeness is the technique of brainstorming. This is the conscious act of suspending contradiction, of making flights of fancy, flights of the wildest most unconstrained imagination. Negativity of any kind is not allowed, it poisons the process. Truth is not the goal, a complete unconstrained search of the problem space is the goal. Randomness is an integral part of this creative technique. If multiple people are involved (the best option) then synergy, positiveness, and building on other ideas, no matter how fanciful or intuitive, is the method. The idea is to maintain the critical mass of excitement and focus of attention. The technique involves a separate analytical (critical) stage where some ideas are thrown out, the problem is more narrowly defined, and another brainstorming session scheduled.

There is some supporting evidence for this technique provided by mathematics, namely optimization theory. There is a proof that as the number of variables in an objective function get large, the efficiency of a stochastic search vs. a deterministic search improves. At some problem domain size, if you have no other useful knowledge of exactly what the objective function is, only a black box to evaluate it, the stochastic method is the best approach. This method involves multiple evaluations of the objective function using input variables sampled (pseudo randomly generated) from distributions with assumed variances. Following generating statistics on the calculated function values, input variable variances are increased or decreased based on whether feasible solutions were found and, if so, how much the best candidate solution input values differed from the mean of the feasible candidate solutions input values in terms of variance. The process continues until every input variable variance is within some pre-specified delta. The theory of genetic algorithms extends this notion to the range of logical constructs, and therefore must also apply to physical constructs, because information is embodied in and mirrors the physical world.

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Though there is some applicability of creative techniques to the generation of scientific theory, science is a far more constrained field than engineering. What Edison did was not science, but it was effective. The search for truth is far more constrained than a search for something that works. Thus a position, advocating an orderly approach building on previous knowledge, while true from a science perspective, is less than optimal from an engineering perspective. I think this difference in perspective partly accounts for much of the disunity and misunderstanding going on in sci.physics.fusion.

The problem at hand, working our way out of the scientific and sociopolitical problems of the energy maze, is primarily an engineering problem. We need practical solutions whether the truth and knowledge comes with them or not. However, present established scientific principals are not sufficient to solve this engineering problem in a timely way. A combined science and engineering approach is required.

To tackle a difficult problem, you must believe you can, and be highly enough motivated to see it through. It is too bad that many of the names in the early heady days of sci.physics.fusion are gone. The excitement, the cooperation, the synergy is gone. It is really inspiring to read through the old posts and to see (now contentious) people working in a positive constructive way. For many, especially scientists, the motivation is gone. Most of the scientists are gone. What remains of the usually lesser educated engineering mentality is considered by the scientists to be "lunatic fringe". History tells us that most any problem tackled with sufficient resources can be overcome. It is merely defining the problem and being motivated enough to bring the needed resources to bear on the problem that counts. All that is lacking is a basis for hope, some unifying principles, a useful method of cooperating, some organization and some resources. Carl Sagan was only wrong is his estimation of how resolute scientists (and engineers) would be in reaching a definitive answer, how long we would see it as important. He foresaw the lack of cooperation, but he didn't see how limited in imagination we might be in exploring the degrees of freedom, and therefore how quickly we would lose interest.

There is no particular reason to direct our efforts and our resources to the study of one system, the Pd + D system. This is far too limited a scope for either science or engineering. It should not be excluded from investigation or engineering though, as the Kasagi experiment demonstrates. Who knows what surprise, what wrinkle,

lurks around the corner? Pons and Fleischmann's legacy is a problem domain rich in degrees of freedom. It is really not even limited to issues of fusion.

This research is not like a lottery. This is not a zero sum game. Either everybody loses or everybody wins, even the players not yet born. If there is one winner, we are all winners. This of course refers to solving the big problems, not to whether or not somebody or group makes a a lot of money, which is a totally minuscule issue by comparison.

It is also not true that a succession of seemingly disconnected efforts is misguided. People actively experimenting, however lunatic, have some degree of knowledge of science and technology. The number and quality of those experimenting on their own may soon be greatly enlarged by the demise of the academic fusion community in the USA. This potential increase in the number of independent researchers is the only bright spot in the immediate future! The intuition and diversity in the behavior of people is our strength. Especially if we have a good feedback mechanism. The points generated by a random optimization scheme looks at first disjointed and crazy from the perspective of someone who knows the answer. However, there is an unseen order in the feedback mechanism that quickly homes in on the solution when the local range is found. Darwin had a pretty good handle on this.

Tinkering is not a good approach to constructing nuclear reactors. This is like saying tinkering is a good way to redesign a hand. Nature's optimization and localization of domain has become too advanced when we talk about hands. Of course nature continues the grand experiment. Hands are diverse, the selection process continues, but major changes of problem sub domain are unlikely to be immediately successful. The major tinkering occurred in the days of the Curies, the domain of possibilities has been narrowed since. This is not to say random testing of new or untried materials for reactors should not continue, or the search for new reactors.

This also gets into the subject of the cost of function generation. If you know something about a function, i.e. analogous to having proven science, you can optimize with few calculations. If the domain is large, and computation is costly, e.g. tokamak construction, you can only afford a few function evaluations. Every possible use of science needs to be incorporated. Also, optimizing sub-regimes independently is a useful tool. This way lots of sub domain points con be evaluated cheaply, but at the cost of increased locality.

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Over deliberation, over constraint of sub domain, lack of free exploration, has it's price as well. In the unbounded universe, there are many maximal points, therefore many domains to explore. When a local point is optimized, it is time to spread the net, increase the variance, go where no-one has gone before. This is serendipity, because the surprise, the new sub domain with it's maximal point, is just beyond that ugly minimum.

These two concepts, testing points cheaply, and spreading the net to look for serendipities, give support to cold fusion experimenting via the engineering approach. The test points are comparatively cheap, and the problem domain is larger.

It is true that we can not just give money to anyone who says he is doing research on energy! However, there is so much undone in the way of building an infrastructure for technology exchange. This might include:

Additional internet resources, like complete online periodical articles.
Access to patent information related to energy
How-to information, like how to build neutron detectors, power supplies, etc.
Online supplier catalogs (chemical, metal, electronic, glassware, etc.)
Surplus equipment lists
Mentor programs
Equipment discounts and loaner pools
Venture capital access programs
Small grant program
Computer based tutorials
Consulting network
Course funding or "traveling" courses for CF device construction, etc.
Amateur and professional group meeting facilities (by town or region)
Methods provided for chartering local research groups
Device (claim) testing and verification (free)
Funding for stage two: follow on research to claims showing value and verified

Much of this is now provided by sci.physics.fusion on a very limited and voluntary basis. These capabilities can be expanded with funding. Many private researchers are or will be unemployed or retired. Every little bit of help possible is needed to

make real and to instill the feeling of the possibility for success.

Most important, though is not to regress, not to lose the level of effort we already have. Infighting will surely accomplish this. Cooperation and like mindedness, unity, is the way to save the day. We must keep up the level of interest, the critical mass. A failed result in one area should only be motivation to try another approach. No need for true believers, only true workers with faith in the future.

The engineering approach to cold fusion has only just broken the tip of the iceberg. Only now are higher voltage ranges being explored. There is a nearly infinite range of materials to explore, new input variables like x-rays, electrode currents, magnetism. Already, there are various confirmed anomalies that need explaining. It's too early to build a water heater, just like it's too early to build a new brain. Whether or not Pons and Fleischmann ever did another experiment advancing cold fusion, their legacy is created. The grand experiment will go on. Too many rocks have been uncovered, too many discrepancies with current theory discovered. The Promethean fire is stolen from the gods as surely as if it were brought in a box.