Questions & Answers with Thomas Ligon on the Polywell

The Polywell Blog, http://thepolywellblog.blogspot.com/

October 20, 2009

1. What is your formal background? When did you start working at EMC2? When did you leave EMC2?

I call myself an engineering technologist. My present company calls me an engineer (I object, but it does no good). My two BS degrees from Virginia Tech are in Biology (with a Health Physics option) and Electrical Engineering Technology.

I did some undergrad research in time-of-flight mass spectrometery that allowed me to instantly understand fusor basics. In fact, I came up with the notion for the Hirsch-Farnsworth fusor back in college, circa 1975, unaware it already existed.

My health physics professors said it would never work.

I was about to quit my old job and start a consulting business, when the old company was contacted by an R. W. Bussard, who wondered if they could leak-check a vacuum chamber for him. I obtained the address, having instantly recognized the name. I wanted to know what he was up to (if this was really the R. W. Bussard). At the time, the company was in Manassas Park, VA.

Figure 1 — PXL-1

I interviewed with Dr. Bussard in the fall of 1995, while EMC2 was shut down for lack of funding. During the interview, the vacuum chamber in question arrived, and I helped carry it in to the lab. That was PXL-1.

He received a trickle of funding that December and I started working for him part time. As more funding came available, I was able to increase my hours. I went full-time a little before the move to San Diego. I agreed to move out there temporarily to help get the new lab going. Temporarily turned into over two years. By early 2001, the company was well-staffed by people better qualified than me, and I came home to Virginia.

2. Can you describe your experiences while working at EMC2? A history of events?

My first glance at the data acquisition system they had been using revealed serious flaws. My first order of business was to overhaul it, eliminating noise problems and frequent blowouts. We then got WB-2 back up and running, while I built up a large power supply for PXL-1 (10 kV at an amp).

After WB-2 did everything it was expected to do, we hooked it up to the big power supply to see what it couldn't do, pushing it to failure. It reached about 4500 volts before experiencing irreparable damage. At that point, we pulled the WB-2 magrid out of the chamber, and replaced it with a pair of spherical grids to make a Hirsch Farnsworth machine called DG-2 (Double-Grid 2). We ran this machine with deuterium, in part to "join the neutron club," but also to prove the old neutron counter still worked and gain some experience with it.

Figure 2 — WB-2

We then put PXL-1 together and started running it. It was essentially a miniature version of the earlier HEPS experiment run in the 1980's, but designed to run for half a minute or so at a time. We tried several configurations with it, and it revealed some intriguing behavior. The machine fascinates me. While it proved a dead end for fusion, I'd love to see it investigated further. It is a remarkably effective high-energy electron storage device, and will gladly prove it to you if you turn off the magnets too quickly.

While running PXL-1, I built up WB-3. This involved an all-new vacuum system and a coil-winding machine I built. We had hopes for pushing WB-3 to fusion conditions, but it stubbornly refused to "clean up" enough to run at fusion potentials, breaking into a Paschen discharge instead at about half the voltage we wanted to reach. WB-3 was set back up in San Diego after the move, and was the run for over a year out there. It had square cross section coils and the "funny cusp" loss problem.

Figure 3 — WB-3

While in San Diego, we hired more help. I designed a new vacuum chamber, the one shown in the WB-6 experiments, for WB-4. Mike Skillercorn engineered the WB-4 magrid (water-cooled with custom copper conductors) and the machine to wind it. WB-4 did make measurable fusion, but way below the desired rate. It shared the flaws of WB-2 and WB-3, the square cross section coils and funny cusp loss points.

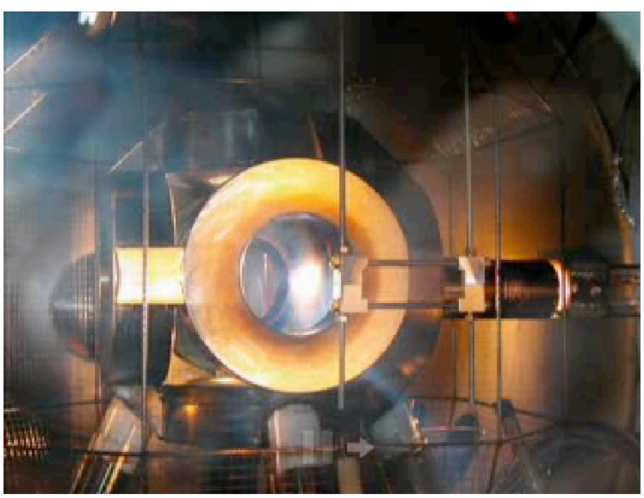

Figure 4 — WB-4

Running in parallel with WB-3, we set up a chamber to run the "NPG" machines. These were Polywell magrids made from a "single turn" of copper tubing, purchased at the local Home Depot. The NPG configuration had low field strength, but did have circular cross-section "coils" and lacked the funny cusp loss points. The magrid surface area was also inherently small. They did measure some fusion from these devices once they worked out some early issues with insulators.

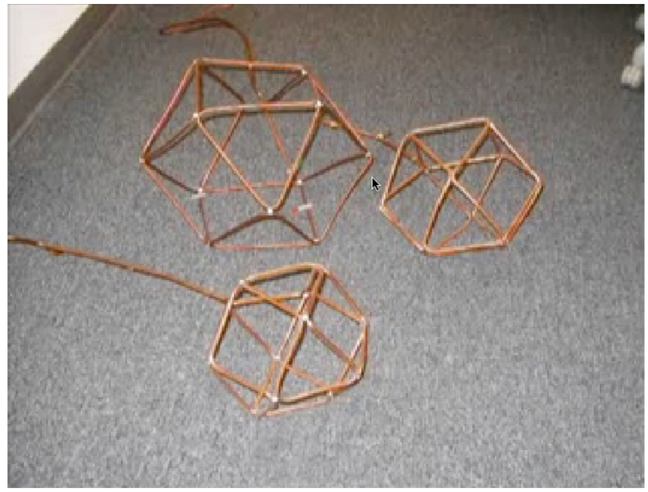

Figure 5 — NPG

After I left, they continued to push WB-4 until finally giving up on it. WB-5 was built and tested, but it was built along the lines of PXL-1 (external magnets instead of a magrid) and it was finally determined to be a dead end.

WB-6 was built in a hurry, based on an understanding that loss mechanisms of the earlier magrids, especially the "funny cusp" losses, were previously seriously underestimated. This inspired tweaking the design to space the coils apart (avoiding the "funny cusps") and making the cross sections circular. And the rest is history.

Figure 6 — WB-6

WB-7 is almost identical to WB-6, except for a few dimensional tweaks and attention to the insulation problems that lead to the short life of WB-6. WB-6 is the machine that produced apparent neutron bursts in late 2005, leading Dr. Bussard to conclude it was finally working as his models said it should.

3. What was it like working with Dr. Bussard?

Intense and challenging. Sometimes he would leave me alone to work on things for several weeks, and sometimes he would be in the lab driving me nuts. He was especially prone to tinkering with the designs, wanting to try new things. The experience was something like being a pinball. Every now and then, one of the direction changes resulted in loud noises and flashing lights.

The experience was the grandest opportunity of my life (with a certain cautious note here that I don't make my wife jealous!).

4. What is the idea behind the Polywell? How does the fusor work? What is the difference between the Polywell and the fusor? The steps to make the Polywell work? Forming the whiffle ball, the reactions inside the core ...

Fusion occurs when certain light nuclei collide hard enough that their short-range attractive forces pull the nuclei together into a new, larger one. A little mass is lost in the process, converted to energy by Einstein's famous $E = mc^2$. But since nuclei are are positively charged, they tend to repel each other. This is called Coulomb repulsion, and it is a bear to overcome. "Thermonuclear" approaches use heat to generate enough particle velocity to do this, but velocity is really the key factor in overcoming Coulomb repulsion. We discovered the fusion reactions using electric charges to accelerate nuclei in machines called linear accelerators.

The basic list of candidate fusion fuels is short. The easiest is deuterium-tritium, the reaction favored by tokamak and fusion bomb builders. The favorite of fusor builders and most lab researchers is deuterium-deuterium, which is about the second easiest, but far harder to initiate than DT. Deuterium-helium 3 is close to DD in ease, and produces less neutrons. There are a couple of interesting candidates involving lithium isotopes.

The darling of the lot is proton (hydrogen) with Boron 11. It is far harder to initiate, but wonderfully clean, producing only helium nuclei at high velocity, allowing direct conversion of fusion energy to high voltage by reversing the particle acceleration concept. The p-B^{11} reaction is also controversial, with many mainstream fusion folks convinced it is impossible due to a loss mechanism called bremsstrahlung radiation. Everybody wants the reaction to work. Not everyone thinks it will.

The fusor is a spherical particle accelerator, utilizing a negatively-charged inner wire grid to accelerate positively-charged ions to the center of the machine. Unlike other colliding beam approaches, non-fusion elastic collisions taking place near the center of a fusor do not necessarily lead to lost particles, as they would for earlier concepts such as the Maglich Migma. Because the accelerating potentials are spherical, all scatter directions are "uphill," and the particles can conserve energy and recirculate. However, the fusor grids intercept too many particles to allow net power production by a large margin, and normal operating condition of fusors cause limited recirculation.

(On video, a marble in a bowl might be a good illustration of an ion in a potential well. I could light off my fusor, although it is in bad shape at the moment. Maybe you could borrow a clip from someone at fusor.net?)

An Elmore-Tuck-Watson machine is similar to a fusor, but the grid charges are reversed. A high positive potential on the inner grid causes it to accelerate electrons to the center. Ions produced inside the inner grid then see a potential well made by the electrons. The ETW machines would suffer high losses of electrons to the inner accelerating grid.

A magrid-type Polywell is an Elmore-Tuck-Watson with a magnetically-insulated inner grid.

The underlying idea of the Polywell is that it is easier to confine and manipulate electrons with a magnetic field than it is to confine and manipulate ions, especially at fusion energies. The thought is to create a "potential well," that is, a region of negative electric charge, and use that charge to accelerate and confine ions. The name comes from the fact that the magnetic containment is configured in one of certain regular corner-clipped polygons, the lower orders being the truncated equilateral pyramid, the truncated cube, and the truncated dodecahedron. Polywells have been tried in two general forms: HEPS-type devices having external magnets, and magrid (magnetic grid) devices.

(I have not touched on all the issues in your question ... probably this needs to be broken up into more questions in a sequence.)

Some of the technical details I used to think I understood, but my discussions with Dr. Nebel and others over the last year or so convince me that the understanding of their operation has changed considerably since I left, and continues to do so. Wiffleballs remain, I'm told, unproven as yet, although something with similar net effect does seem to occur. The potential well seems to be a very steep-sided, flat-bottomed "potential bucket," something I remember Dr. Bussard sketching a decade ago. We used to talk about Debye lengths in these machines a lot, but that concept seems to be falling from grace as deeper understanding emerges. There is stuff going on at a small scale that seems to be allowing these machines to work at far higher densities than I would have guessed, and I'm no longer sure I should talk much about it as I'd probably be dead wrong. Some of my thinking about phenomena I observed in PXL-1, and the likely contribution of cold electrons in creating a wiffle-ball-like effect may be showing up in simulations.)

5a. Where did the research stand when you left EMC2? What major problems had been solved, what remained?

Outside the lab, there was a lot of software and theoretical work going on regarding the models. I was only very peripherally involved in these areas, and am not qualifed to say much about them. Dr. Bussard gives an idea of what the main components are in his "Valencia Report." From personal experience, I can say he would occasionally spend an hour or two at a time at the blackboard showing me calculations faster than I could fully absorb them, but almost all of it was straight from the NRL Plasma Formulary, and I was unaware of any hocus-pocus going on beyond the ken of known plasma physics.

When I left EMC2, we still had not achieved significant fusion. We had learned to control huge magnet currents developed by battery banks floating at high voltage. We had learned to work with some pretty impressive high voltage power supplies. We had learned to inject microwaves into the apparatus as an ionization aid. We had recognized the problem of false counts occurring on the neutron counters due to electric arcs or even stray static discharges to doorknobs, and eliminated it by hyper-shielding the counters against EMI. We had developed or purchased instruments and learned to use them:

- Photometry
- Interference filters
- Visible light spectrometers
- Residual gas analyzers
- Langmuir probes
- Gaussmeters
- Fiber-optic isolation of instruments floating at high potential.

When I left, the outstanding problems all boiled down to about the same problem, but appeared to be these main points:

Why did the machines seem to have so much trouble "cleaning up?" As fusion potentials were approached, the chamber pressure would shoot up, resulting in a bright glow discharge (with the larger power supplies on WB-4 and WB-6). Dr. Bussard characterized these as Paschen arcs. This loaded the power supplies down, dropping the voltage. Furthermore, the residual gas analyzer showed the pressure rise was typically hydrogen, which diluted the deuterium we were attempting to use for fusion to the point that no detectable fusion was likely.

5b. Why did the electron transport measurements seem to suggest the electrons were not being retained as well as Dr. Bussard expected?

Both problems were the product of electrons plowing in to metal magrid surfaces at high kinetic energy. Marks on the magrids suggested two loss areas:

- The the case corners of the square magnet cross-sections sticking out into the insulating fields.
- The line cusps where the magnets were joined together.

Of these loss paths, the line cusps may have dominated. These lines actually have very high flux strength, as they represent areas where the magnets are closest together and are repelling the most strongly. However, the orientation of the flux lines points directly into the region where the magnets join together, rather than making clean circles around the magnets as intended. The potential problem had been recognized years earlier, when one astute observer had mentioned they formed a "sort of funny cusp." The early thinking was that it might not be a problem because it was simply a short line segment at each joint, and lines have no area. Also, since the field was "doubled up" at this point, the electrons might simply go around to areas with less magnetic field.

Evidently, these suppositions were false hope. The lines do have area, as they are at least an electron gyro-diameter wide, and the electrons had a strong electrostatic incentive to continue down the funny cusps to the bare metal attracting them, and the orientation of the field lines did nothing to prevent it.

WB-6 changed to circular cross-section magnets to avoid the protruding corners, and spaced the magnets apart so the "funny cusps" don't point at magrid surface. WB-6 and WB-7 do, however, have a small interconnecting protrusion remaining across the funny cusp gap. I suspect it is the largest remaining loss path, and needs to be eliminated in future designs.

6. What is the biggest technical challenge facing the Polywell design? What are its advantages over other ideas?

Dr. Bussard liked to cite R^7 scaling. Basically, whatever the reaction rate of a machine of radius x, if you double the radius, he expected the reaction rate to go up by 2^7 power, or a factor of 128. That assumption is based on his engineering rule-of-thumb that magnetic field strength can be increased directly with magnet radius. The actual scale factor formula is B^4R^3. The R^3 factor, in a spherical machine is basically volume. The B^4 term also operates in tokamak scaling. Essentially, the machine scales with the same trend as tokamaks … the scaling law is probably quite reasonable, and, like tokamaks, says if the thing works at all at any scale, it should make dramatically more power if you make it big enough. The question then becomes, does it operate basically well enough to allow us to extrapolate to practical net power?

Polywell devices have always been intended to run continuously, not in pulses. My understanding so far is the fusion runs have been in short pulses. In order to run continuously, the generation of stray gas must be minimized, and elimination of it must be aggressive. It remains to be seen if good engineering can overcome these problems.

All of the runs so far have been fueled in an "accidental" manner. My analogy is running a gasoline engine using an eyedropper rather than a carburetor. Squirt in some gas and crank it, and you'll get a few barks, but it is exceedingly difficult to make an engine run this way. The Polywell machines need controlled ionization of fuel just inside the magrid.

Presuming a high fusion reaction rate can be achieved, there are a number of practical problems in dealing with the fusion products. High-energy fragments colliding with the magrid are sure to have a bad effect, sputtering off metal or blowing off hydrogen. Can good engineering produce a design which can run continuously at net power levels? By the way, the same arguments apply to tokamak walls.

If only pulsed power is possible, can useful power plants still be made by running arrays of them? This might be possible, considering the compactness of the reactors compared to tokamaks.

7. What are your thoughts on Dr. Bussards claimed data?

The calculated fusion rates for the four tests in the fall of 2005 were all Dr. Bussard had to work with, and he milked them for all they were worth. He was painfully aware of the lack of statistical significance of tests producing 1-3 counts. He was heartbroken that there was no opportunity to make replicate tests, which were obviously needed. He made the strongest claims he thought he could, but the intent was to have the opportunity to start back up and get better data, and have it stand peer review.

Given the almost non-existent statistical significance, all he could state was order of magnitude. If you calculate the fusion rate from his raw numbers, you can see he was stretching to claim 10^9 fusions per second, but he did so fairly. The rates were more like 1e9 than 10^8, and inserting even one digit ahead of the order of magnitude was unwarranted by the low counts. Even so, applying the presumed R^7 scaling, WB-6 did not scale up to net power at $R = 1.5$ meters. I think his peak fusion rate on WB-6 works out to under a milliwatt of fusion.

I don't have access to the full extent of the data, and did not have a chance to discuss the topic with him before he passed away. I believe he had a number of reasons to believe WB-6 was running far below its peak potential in those four runs. I'll speculate on a few possibilities.

The fusion burst seems to correlate with the onset of a rapid pressure buildup that culminated in a Paschen arc. Since there was no control of the fuel ionization, and since his model expected a fairly narrow parameter space in which robust fusion will occur, I would expect the pulse of fusion occurs as the machine passes quickly thru the correct parameter space. Considering the number of parameters involved (ion density, the location of the origin of the ions relative to the walls of the potential well — this controls the energy they can gain, and neutral species density are important), I actually found it remarkable that the machine was able to do even this much without ionization controlled in rate and location.

The RGA typically shows quite a bit of hydrogen present when operating, and the level builds rapidly as it runs. The reaction rate of deuterium with hydrogen, or hydrogen with hydrogen, is for all practical purposes zero in a machine of this sort. If you run equal hydrogen and deuterium, the reaction rate falls to 1/4 of the pure D rate. With ten hydrogens to a deuterium, the reaction rate falls to 1/100. The RGA scans are too slow to know exactly the ratio in these tests, but there is every reason to expect there was a strong dilution effect present.

Neutral gas is the enemy of the machine. He probably had an estimate of how much was present, but I don't.

Obviously there is a lot of speculation regarding the little bit of data. Dr. Bussard was probably stretching to say WB-6 had proved net power was possible at 1.5 meters using DD. In fact, that figure is what his models have been saying for years. I suspect he estimated WB-6 was performing sub-optimally by a large margin.

Is this enough to prove he succeeded? No, and he knew it. He understood perfectly well how sparse the data was, and that they must be confirmed by further experiment. However, he was also convinced, and I think rightly so, that he had demonstrated the program was well worth reviving. He believed, very sincerely, that WB-6 was finally the right design, and had shown why the previous attempts had been so disappointing.

When he lined up Drs. Nebel and Parks to do the WB-7 work, the idea was to have them independently confirm, then continue, the WB-6 work. Nebel and Parks are experienced electrostatic fusion researchers with whom Dr. Bussard had corresponded for years, but they were not EMC2 insiders. They had not invented the Polywell concept, and had no reputation to protect if it did not work. They could simply go back to their old jobs at Los Alamos. At this point, while we don't know the full results of their work, they have apparently built and successfully operated a machine very much like WB-6, done so extensively, and have had the results peer reviewed.

8. What form do you see the research taking? Publicly funded government research? private? Private industry research?

I know there are private individuals very interested in the idea, and it is not out of the question an "angel" might fund it, as Paul Allen is funding TriAlpha. However, Dr. Bussard once told me "you don't find angels, angels find you." The angels know if they are interested, and the rest of us can only speculate. Venture capitalists typically want more of an indication the idea will really be successful, and will return a profit in five years. They might jump on after harder proof the system will, in fact, make power. Completion of a net power demo reactor (as opposed to a fully operational electrical power plant) would probably make them sit up and take notice.

Private industry could do it, but like the venture capitalists, they'll want more of an assurance that it will work.

Dr. Bussard had some offers of the above types. I don't know the details, but I do know he turned at least one down due to the terms. He did not want this technology so closely owned that it would a monopoly designed to generate money for one person or a small organization. He wanted it liberally available, although with fair return to the original investors.

The right way, in my opinion, is government funding. Properly, this always should have been a DOE effort. When DOE was created, and the present national fusion program was set up (largely the work of Hirsch, Trivelpiece, and Bussard) their thinking was to direct about 20% of the fusion budget away from lasers and tokamaks to smaller efforts such as this, and they were specifically looking at Hirsch-Farnsworth fusors as a starting point. However, the monster they created developed a will of its own, and did not want to go that way. I don't know if the DOE can be trusted, at this point, to fairly run a program like this, but it is exactly what the organization was intended to do.

If the Navy is happy funding this, at least for now, I'm good with that.

Overall, the DOE has been criticized for spending ten billion or so on tokamaks over the last few decades, with nothing but promises decades away to show for it. Honestly, that's the wrong attitude. The Big Three automakers recently showed up asking for a bailout of three times that much so they can make it thru the next few months. We're bailing out the financial markets to the tune of seven hundred billion dollars. Frankly, the tokamak effort sounds like a bargain to me, and the shame is the budget for fusion research can't be made generous enough that a few competing ideas can't have some crumbs to find viable alternatives. I would fund EMC2, Tri-Alpha, heck, Focus Fusion too. Anything Paul Koloc comes up with is worth a good look, too (that is me paraphrasing Dr. Bussard's opinion circa 1997 or so). All these put together are chump change compared to the potential payoff.

But EMC2 is, in my opinion, the smart money.

9. Assuming the major technical challenges were solved, assuming this idea in fact, works, can you give us a sense of what this machine might look like?

Dr. Bussard typically cited "radius" when describing the machine size. I believe that is always magrid radius. Double that for the magrid diameter, and that is inside a vacuum vessel in which it fits comfortably. At a minimum, the basic reactor will be 3 magrid radii in diameter, more likely around four. Most likely it will be a sphere, possibly a truncated polyhedral matching the magrid configuration.

Superconducting magrids are highly desirable from a power balance perspective. But can superconductors run for long in close proximity to a DD reaction, and the energetic neutrons that reaction produces? Tokamaks would put the magnets outside the lithium blanket. A DD Polywell would need a neutron-absorbing blanket and steam generating equipment around it.

The p-B^{11} powerplants need to be larger. Once I found out how Dr. Bussard thought he could beat the bremsstrahlung problem with boron, the reason for the larger radius made sense. Two of the tricks were running about 8:1 hydrogen-rich (which will reduce the reaction rate), and limiting the central virtual anode to about 15% of well depth (meaning reducing the density of hot fuel ions in the center, and thus the reaction rate). Compensating for these reductions by the R^7 scale factor appears to require the jump from 1.5 to about 2 meters magrid radius, if I calculated correctly. However, that still does not account for the additional structure to do the direct conversion of fusion alphas to high voltage DC. My guess is the final reactors will be spheres of about 10 to 12 meters diameter, with some supporting equipment for the cryogenics and power conversion. They will produce a wiff of neutrons by a few stray paths, and at least 5% of the power will wind up as bremsstrahlung x-rays, so they'll need shielding. Five megawatts of x-rays is a lot of x-rays.

Either machine will be tiny compared to ITER. They would not be far off from the size of a typical fission power reactor or some fossil fuel boilers.