Introduction:

Fusion has the power to change the course of human history. It could curb climate change - a clear threat to human civilization. Fusion research is littered with great ideas that we have never tried. We have failed to support so many great ideas. This must change. We cannot wait. Our leaders must hear this call to action.

In August of 2015, Congressman Alan Grayson introduced HR.3440: The Fusion Innovation Act Of 2015. This bill calls for changes to federal US fusion funding. I decided to write his staff an open letter covering what changes we think are needed. Enjoy.

Executive Summary:

This letter calls for an overhaul of US federal support for nuclear fusion. It is addressed to Representative Alan Grayson, who introduced "The Fusion Innovation Act Of 2015" into the 114th Congress. Presently, federal funding is narrowly focused on laser fusion or tokamaks, neither of which will be commercially viable. This narrowing has happened because fusion shifted from practical to esoteric research, poorly communicated its' advancements, lost funding for a diversity of ideas and did not compare all of it's' approaches objectively. ITERs' high cost (~16 to 50 billion USD), size (~23,000 tons of steel), delays (over 7 years presently) and hazards make it unlikely to lead to commercial fusion. NIFs' failure to get ignition led Livermore to cancel the Laser Inertial Fusion Energy (LIFE) program in 2014. Meanwhile, fusion can be done cheaply by non-specialists, using common tools. Moreover, several outside groups have made progress in spite of a lack of government support, including: Tri Alpha Energy, Energy Matter Conversion Company, General Fusion, the Northwest Nuclear Consortium, Hyper V, Lockheed Martin and Helion. U.S. federal support for fusion must change; changes should start with rating machines using John Lawson's' original energy balance, not derived metrics like the triple product. Next, the mission of the Fusion Energy Sciences Office, should be both expanded and modified. Congress should refrain from calling for yet another review panel which will wastes time. Funding should be increased for shovel-ready projects such as EMC2s' polywell (30 million over 3 years) MITs' LDX (2 million per year) LANLs' PLX (tens of millions) the dynomak (35 million over 3 years). Additionally, an expanded SBIR program should be created that focuses on fusion startups. Government funding should be closer to venture capital; with a real possibility of losing funding. Finally, the Northwest Nuclear Consortium should be examined as a model for high school fusion education. For example, grants could be made available for high schools to build small fusors. The letter closes with a comparison between flight and fusion.

Introduction:

I am writing in support of H.R.3440. Your proposed bill, "The Fusion Innovation Act Of 2015" is the first fusion legislation we have seen in a long time. We need Congress to pass some kind of fusion research reform. For almost 10 years I have been working in fusion, either as a researcher or as a promoter. I cannot express to you strongly enough how lopsided the U.S. Government-funded fusion research is right now. Presently, a handful of concepts get all the talent, funding and resources. Ironically, these are arguably the furthest away from commercial viability. Meanwhile, many other approaches have been left out in the cold, with nearly no government support. We have been stuck in this rut for many decades. But today, despite inaction at the federal level, fusion research is advancing. This will likely continue, with or without, U. S. government support. Reform is needed so that the United States can stay on top of the newest technologies, concepts and developments. Fusion power would have serious implications for the economic, environmental, defense and moral might of the United States. This is not something we should let slip by. We need to lead on this issue, and we are not leading.

What got us are here:

Several overlapping trends have led to our current situation. Currently, the U.S. fusion world consists of a spectrum of universities, laboratories and companies. Most of these groups are focused on one of two concepts: ICF or Tokamaks. This makes for a very narrow environment. There have been supporters and critics of this narrowing approach over the past 20 years. Supporters argue that ICF and Tokamaks approaches are the most promising scientifically. Critics argue that this narrowing has devastated fusion research. There are several reasons why the critics have a stronger case. First, what is interesting scientifically may not be useful commercially. NIF and ITER are both interesting scientific machines but they will likely never lead to commercial viability (I show this below). Next, have these machines shown strong results because of their merits or because they have received hundreds of millions of dollars in funding?

This is tough to answer, because there is little data comparing all fusion concepts. There is no textbook that compares all of the approaches to fusion by net power, efficiency, beta, size, power balance or cost. Worse, there is no funding to pay someone to write such a book, nor is anyone trying to write it. This lack of feedback is another negative effect of the narrowing. When researchers are not threatened by other approaches they see no need to compare their work. This narrowing has also killed innovation. Today, most fusion innovations are merely plays off the existing ICF or Tokamak devices. For example, the ICF approach has direct drive, indirect drive, fast ignition and heavy ion beam fusion. Unfortunately, all of these concepts are based on the same platform, which means they all suffer from similar commercial and physical limitations. The narrowing approach also kills innovation by forcing all other programs to “show relevancy to ITER or NIF”. Projects must be tied to the flagship machines or their funding is cut. Researchers have also shifted away from focusing on practical, commercial concepts to esoteric publications and maintaining funding. People have put more of an emphasis on upholding their careers rather than finding commercially viable fusion.

Finally, the scientific community has done a poor job communicating developments in fusion, both internally and externally. They may communicate with technocrats in Washington, but do not explain themselves to the general public. This has gradually led to a lack of public awareness. Naturally, this has led to a lack of political leadership. Taken together, these effects have killed a diversity of approaches, driven researchers away and left the United States in a bad place. We are stuck with NIF and ITER – and neither is very promising.

Problems with ITER and NIF:

ITER is an interesting project, but it will never lead to a Tokamak-based power plant. The projected cost is anywhere from 16 to 50 billion dollars [1-4]. That amount is far too high for commercial uses. It is estimated that that the core will be 60 times more massive than a fission core - roughly 23,000 tons of steel [5]. That is too big to be commercially viable. Originally, the machine was supposed to open in 2016 [6]. It is presently over seven years delayed [7]. The machine can accidentally quench, creating sudden fires which release radioactive waste [5]. This warrants an expensive safety system. Realize that any commercial plant based on the Tokamak concept will likely be more complicated, costly, time consuming and hazardous than the ITER prototype. So any commercial device would indeed be monstrous. This machine would then need to be regulated by the NRC [5]. And finally, it would need to turn a profit. It is a fantasy to think that such a project will lead to a commercially viable reactor. It is too extreme for private industry to take on. Supporters will argue that even if it fails, ITER will lead to a raft of spin-off technologies. In fact, the spin-off technologies (materials, magnets, diagnostics and plasma heating) could be developed separately, decoupled from the ITER project. Moreover, ITER's existence spreads the false perception that fusion is big, expensive and ivory tower, when in fact, nuclear fusion can be done by teenagers in their basements using cheap fusors [8]. Bottom line: we do not need to wait seven years to find out that ITER will never lead to commercial power - and then back out. We know this right now.

The course taken by ITER bears a striking resemblance to the smaller National Ignition Facility (NIF) at Lawrence Livermore National Laboratory. The facility failed to get ignition after $3.5 billion was spent on it [9]. When it was turned on, NIF was so complex that researchers could not connect parameters to different results – a basic requirement for valid scientific experiments. A NIF implosion takes about 20 nanoseconds and occurs in a space of ~1E-7 cubic meters [10, 11]. Building the sixty tools to measure the implosion is a feat of science, but a NIF based system will never be commercially viable with such ridiculous constraints [10, 11]. It speaks volumes that Livermore quietly killed the Laser Inertial Fusion Energy (LIFE) program last year [12]. It was a quiet admission of a colossal misstep. A similar failure at ITER will be far worse because it will be more costly and more time consuming than NIF.

Lawson Criteria:

Both flagship ICF and Tokamak machines are failing to lead to commercial viability. But this is certainly not what you hear from ICF or Tokamak researchers. One figure of merit they use to prove that these approaches will work is the Lawson criteria. This is so widely used in the fusion that it is worth clarifying. In the 1950s John Lawson wrote a landmark paper about what it would take to get fusion power. Lawson applied the following energy balance across a plasma-base fusion machine [13].

Net Power = Efficiency*(Fusion – Conduction – Radiation)

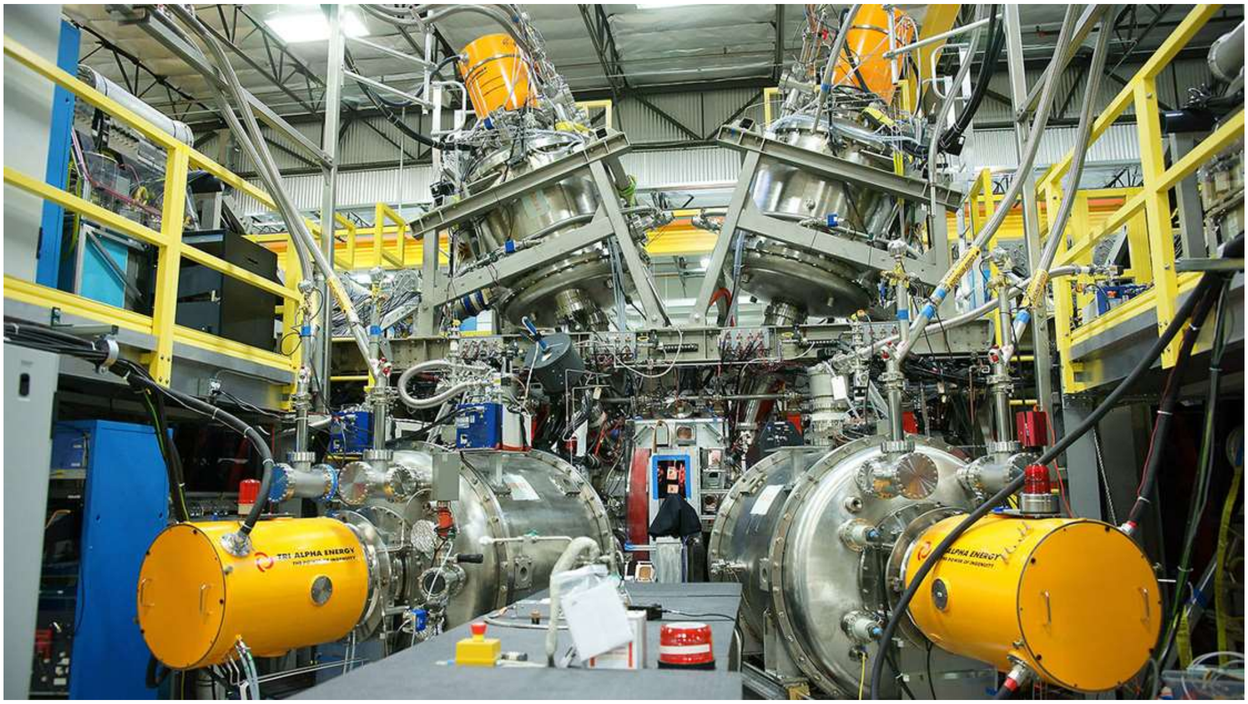
To get net power, we need to beat this equation. This should be a major focus inside the fusion community. At present, it is not. Most fusion researchers have taken this energy balance and adapted it to support their research. This has led to a focus on minimum triple products, experimentally achieved ignition thresholds and a multitude of abstract figures of merit [14]. *Fusion power will likely come from people who ignore all those derived metrics and just beat the energy balance directly.* There are likely unexplored avenues to do so.

Change starts at the FES:

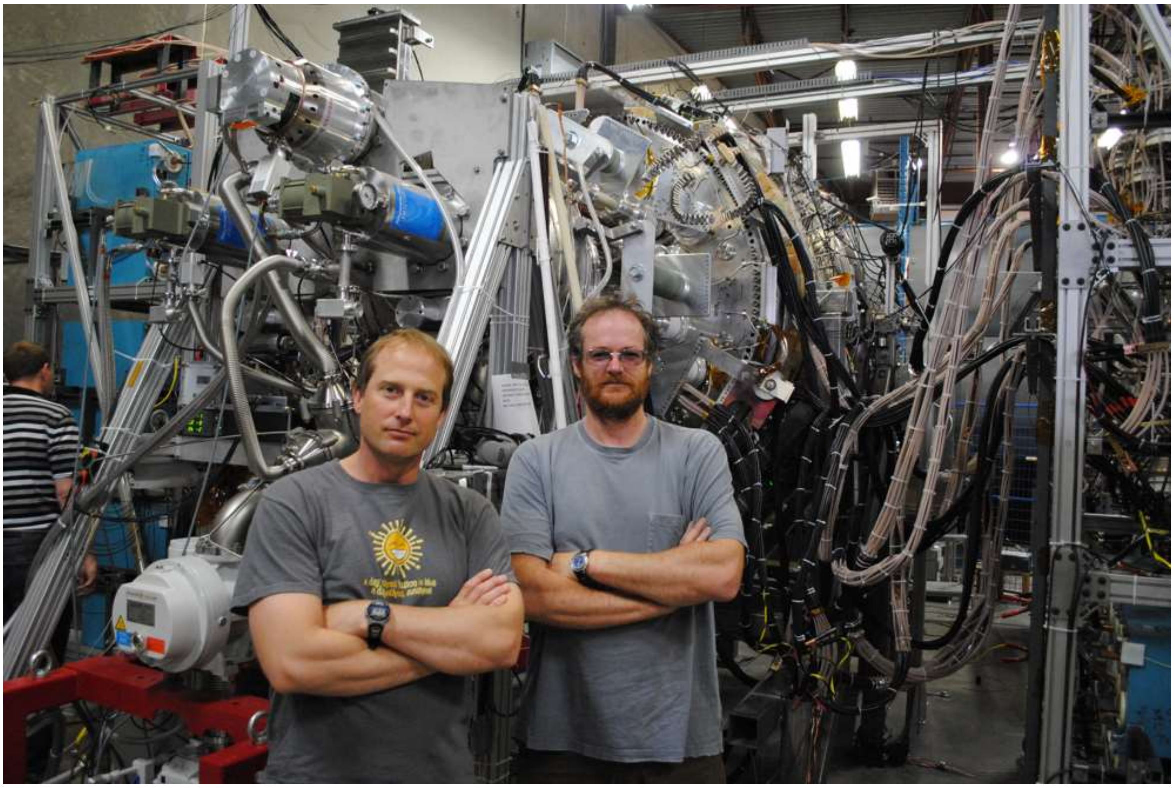
These problems must be hard to see from inside the beltway. For example, the Office of Fusion Energy Sciences can tout its many real successes. I do not want to detract from their efforts. They do get many things right. The office does a good job of fulfilling mandates from Congress. It also does a good job of maintaining the existing fusion facilities, staffers and concepts. But, it has become clear that this status quo will not lead to commercial viability. Tokamaks and ICF are not on a path commercial viability. This has not changed the behavior of the Office of Fusion Energy Sciences. If anything, it has made them far more dogmatic. One staffer explained to me that, “the alternatives will fade away and all that’s left will be ITER and NIF”. That would be disastrous. It would waste precious time and money, eliminate diversity and leave us with two options, neither of which are commercially viable – at a time when the human race needs zero carbon energy sources.

Fusion Tribalism:

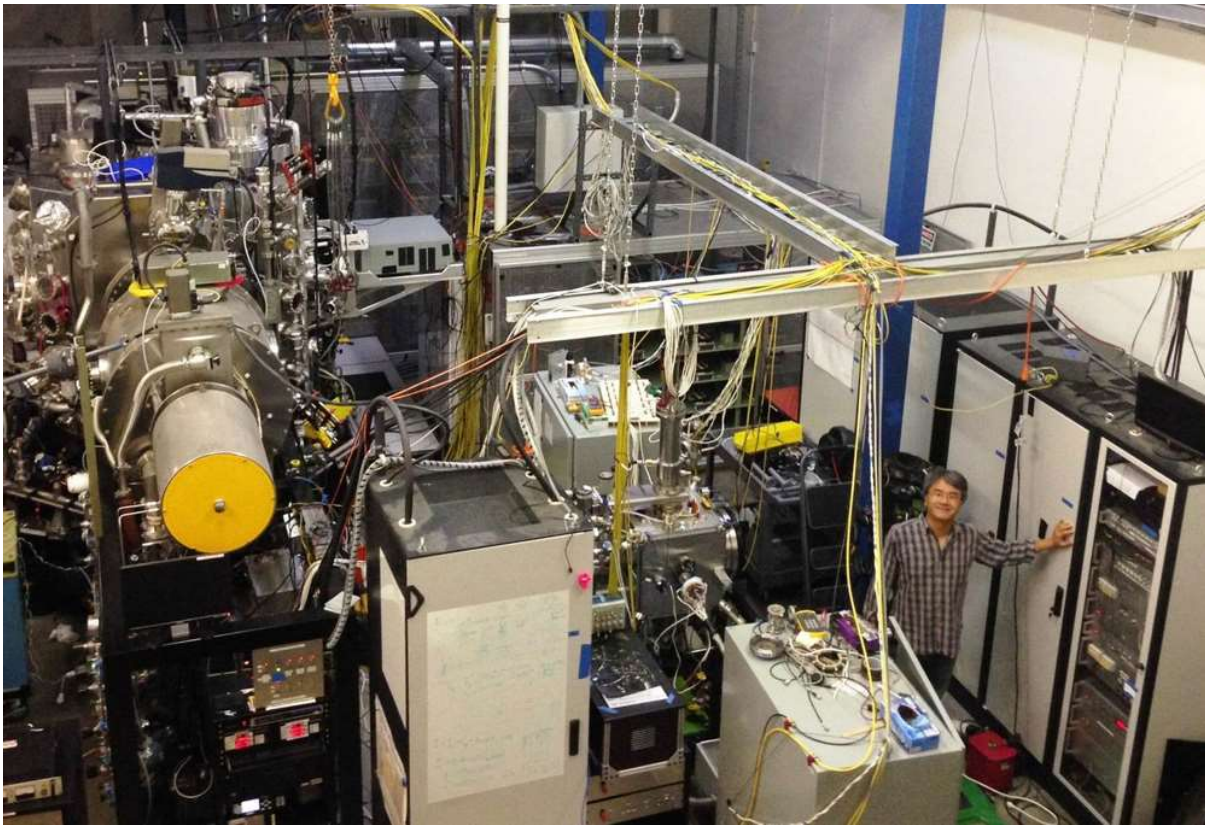
Fusion looks very different from outside the NIF and ITER communities. One person described this world as “fusion tribalism”. It consists of a multitude of disconnected ragtag groups supporting specific concepts. They are convinced that the world needs fusion power and that their approach will make it a reality. They work, in spite of government support, not because of it. Many feel that the DOE should expand its support to encompass these groups, or at the very least find ways to encourage their growth. One good suggestion would be an expanded SBIR program focused specifically on experimental fusion concepts. The reasoning for this is that commercial fusion power is more likely to come from one of these outside groups rather than the flagship machines. This is a high-risk investment and many of these teams and concepts will fail. However, one success would reap great rewards for the United States. Moreover, if fusion power was developed by a competing nation or group, it could be disastrous. Below is a list of some of the groups for your staff to review – it is by no means exhaustive.



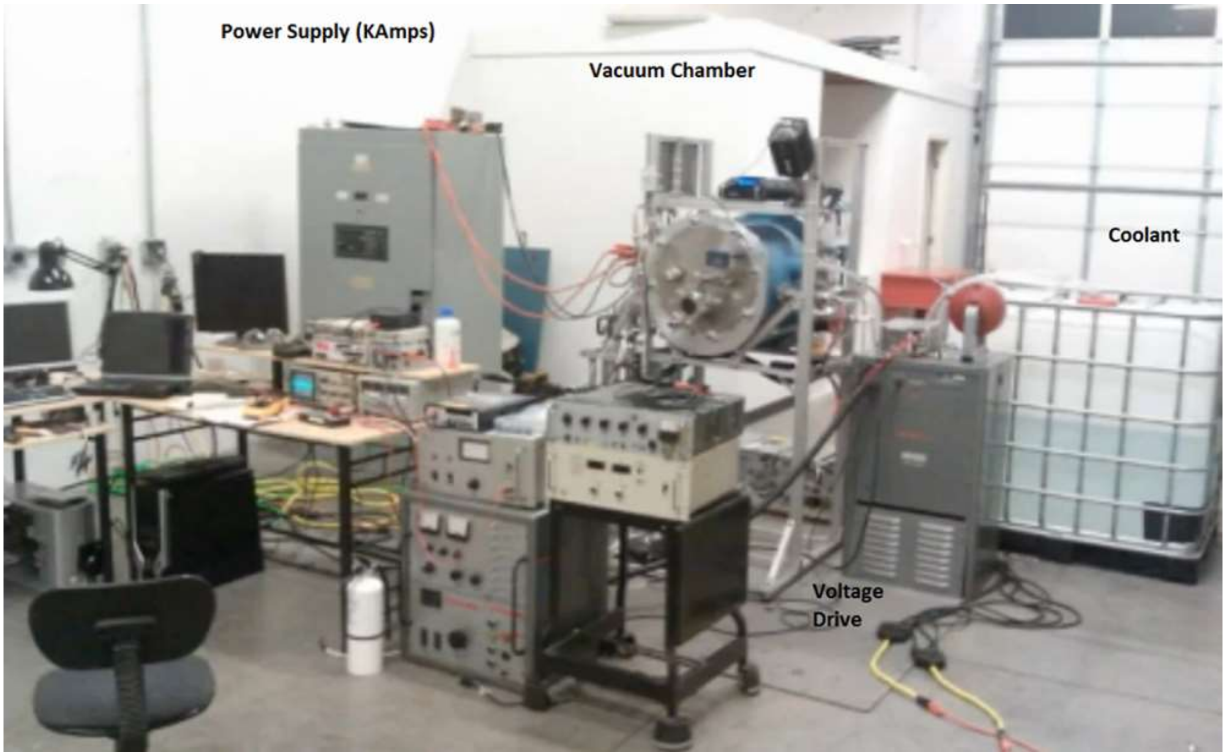
1. Tri Alpha Energy. This company is a poster-child for the problems created by a limited DOE vision. The company recently demonstrated the longest stable field reverse configuration (FRC) ever. They pulled this off with roughly the same concept they had when founding the company in 1998 [15]. This concept involves using particle beams to stabilize a field reversed configuration. Had the U.S. Government helped the company years ago, we would have likely gotten to these results much sooner. Tri Alpha is now gearing up for a much longer test.



2. General Fusion. This company was founded in 2002 by Dr. Michel Laberge in Canada [16]. The technology involves the compression of a Spheromak (presently), or a Field Reversed Configuration [17]. The concept was partially developed at Los Alamos National Laboratories in the seventies [18]. It was not pursued because the plasma compression could not be coordinated. Today this is possible. General Fusion has a staff of ~50 and they have raised over $55 million in investment [19]. They have put out dozens of presentations and about 10 papers. The strength of their technology is that the surrounding lead-lithium is well suited to deal with the fusion by-products.

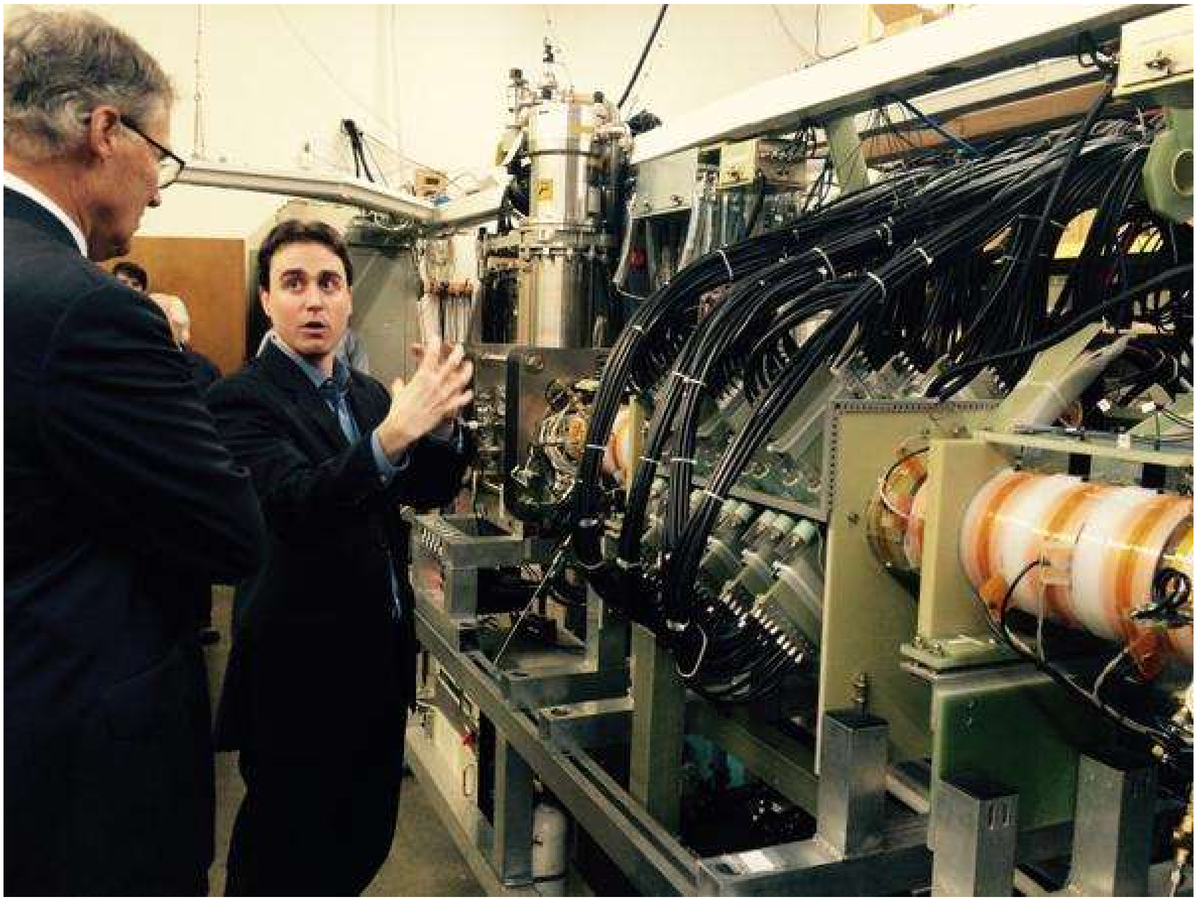


3. Energy Matter Conversion. This team just published a major paper in *Physical Reviews X*. The paper provides data showing that they made a cusp-confined plasma inside a magnetic device [20]. This is a theoretical condition where the plasma's own internal magnetic field rejects the outside magnetic field, leading to a powerful (beta = one) plasma trap [21]. If this research bears out, it would distinguish cusp confinement as radical alternative to trapping a magnetized plasma, which will have significant implications for fusion research. The group is now attempting to raise 30 million dollars over the next three years to develop this concept [22].

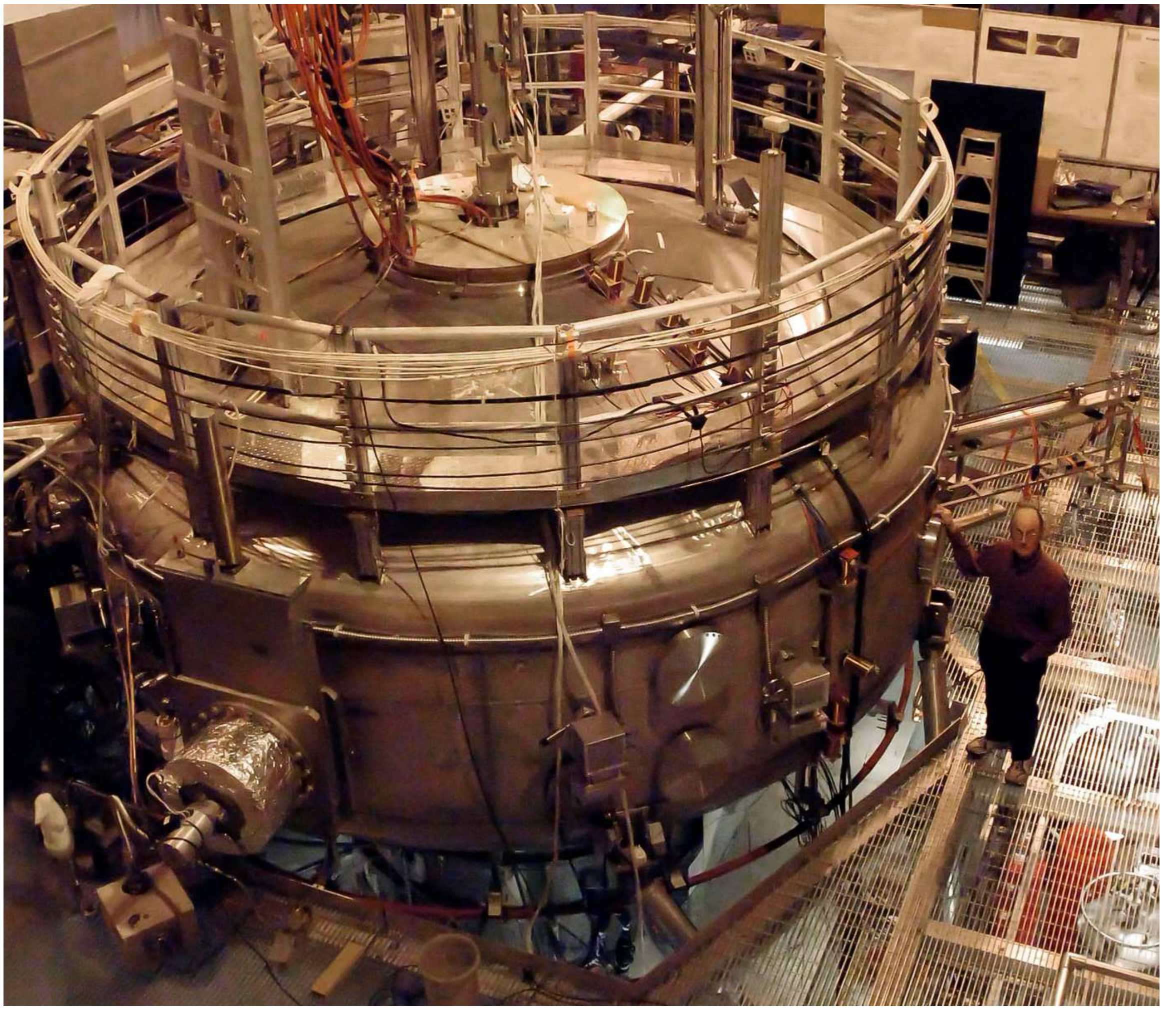


4. Convergent Scientific Inc. This a very small team working in Washington state. The company is developing a modified version of the Polywell. Raising and spending less than $150K, they were able to concentrate a plasma for 20 seconds inside a prototype device [23]. The group has also developed an innovative plasma simulation software. They are continuing to raise money and are gearing up for further containment experiments.

5. Lockheed Martin. Lockheed has been rightly criticized for not publishing their work in a peer-reviewed journal. What we know about their approach comes from three patents, several media stories and talks [24]. Their concept uses cusp confinement to trap plasma while heating it with neutral beam injection [24]. A good estimate is that Lockheed has $5 to 10 million invested in the project and a staff of 6 to 8 people.



6. Helion. Helion was spun-off from Mathematical Sciences of the Northwest (MSNW) in 2010 [25]. Both companies were founded by Dr. John Slough, both are working on fusion and both have received government funding. This funding totals less than $25 million in almost as many years [19]. The company has heated plasma to a temperature of five thousand electron-volts, roughly a fourth of the goal set by the National Ignition Facility [25, 26]. They have also demonstrated a containment field of over one hundred Teslas in strength [25].



7. Levitating Dipole Experiment. This is an innovative machine that was pursued jointly by Jay Kesner at MIT and Michael Manual at Columbia. The dipole is a levitating magnetic donut, which is free floating inside a high-pressure plasma. Fuel (deuterium) is introduced at the plasma edge and is driven inwards by the so-called “turbulent pinch”. Fusion would occur in the high density inner region. The team was planning ion heating experiments which could lead to ignition experiments, but in 2010 the fusion funding for the LDX was cut [27, 28]. Today the principals are searching for funds to restart research.

8. Hyper-V and PLX. The Plasma Liner Experiment at Los Alamos has been sold as a middle ground between ICF and Tokamaks. First proposed in the late nineties, the idea is to inject a plasma structure (like a field reversed configuration) into a chamber and compress it with plasma cannons [29]. The idea builds on, but is arguably better than, ICF and Tokamaks. It is smaller, cheaper and is physically less constrained. Surprisingly, even such a mainstream concept still took over 10 years to get the funding for a full test [29, 30]. We should have accelerated its development.

9. Northwest Nuclear Consortium. This is not a company - but a group your staff should be aware of nonetheless. The NWNC is the only high school club in America with their own nuclear fusion device. The group was founded in 2010 by Carl Geinger [31]. Incredibly, these teens do fusion using a fusor every weekend in a home outside Seattle. They have won over $600K in college scholarships and came in 2nd place in 2013 at the Intel International Science and Engineering Fair [31]. Supporters want to duplicate this high school fusion program at other high schools around the country.

There are other companies that could be added here. They include small groups of people with very good ideas. This cluster of companies represents a fundamental shift inside the fusion research world. They are quite different from the firms which have come before because they exist in a hotter, more connected and technically advanced world. Collectively, they are still small. In 2014, I tabulated that among the dozen or so companies, there were 330 people working with $450 million invested [19]. Despite being small, they are innovative and very nimble. The government has not devoted the resources to staying on top of their technology. Moreover, far from going away, new firms are popping up every year - lured by the prospect for clean, cheap energy.

Changes Needed:

In a perfect world, the federal government should both increase and modify its support for fusion research. The government should avoid its habit of calling for yet another review panel, assessment or study. There have been many panels over the years. They waste time and tax dollars and they tend to come to the same conclusions. They typically call for increased funding, which is typically ignored. This delays research until a new administration calls for a new panel whose recommendations are also ignored. This cycle of inaction is very familiar to fusion researchers. Buck this trend. Take action right now. Funding should be increased immediately for several shovel-ready fusion projects: the EMC2 Polywell project ($30 million over 3 years) the Dynomak at the University of Washington ($35 million), the LDX at MIT (2 million per year), and the PLX machine at Los Alamos and Wisconsin's IEC research center. Wisconsin alone may not even be enough to deal with developments in fusors – a growing community amateurs are claiming higher neutron rates as of late and Australian researcher have started developing magnetically insulated fusors [32, 33]. There also needs to be an increase in the SBIR program for small fusion companies. In addition, the Northwest Nuclear Consortium should be examined as a model for high school fusion programs across the country. This would fit nicely into the broader push for STEM education. One suggestion is a grant program that enables schools to build their own fusion devices on campus. On top of this, the government needs to change the nature of its' funding. The U.S. government should fund fusion on a much more competitive basis – more like a venture capital firm rather than a blank check. This means including the real possibility of cutting funds for boondoggles and bad concepts.

In Closing: A Story

In 1903, powered flight was invented by two nobodies without a college education, federal funds or any university support. It was done at a time when powered flight was widely considered impossible, by both the general public and esteemed experts. Just nine days earlier, the highly visible federally funded program had been abandoned. Powered flight happened because there was a great need and a determined community willing to try. It was not just an innovation, it was an invention beyond our collective imaginations. Its creation came from a direction that no one would have predicted. Its impact was radical, unexpected and far reaching. History tells us that it is hard to pre-plan for these radical innovations. It is hard to know the shape of technologies before they exist. For fusion, this means never being 100% sure we know what fusion power looks like until it is real. Today, we must recognize that a similar community with similar motivations is budding around fusion. Today, there is a staggering need for this energy source and many more people eyeing this prize. And today, the technological barriers are coming down much faster than most people realize. The U.S. Government needs to be on top of all these developments. Make no mistake: we are not leading. This must change.

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