Fusion Podcast Pal: "*An Interview with Dr. Sam Cohen*"

The Fusion Podcast
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Guest: Dr. Sam Cohen
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Host: Dr. Matt Moynihan
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Guest Bio:

Dr. Sam Cohen

For 44 years, Dr. Sam Cohen has worked as a physicist at Princeton University. He currently serves as director of the Plasma Science and Technology program at the Princeton Plasma Physics Laboratory. Since 1998, Sam has been performing research on plasma devices known as Field-Reversed Configurations (FRCs), studying their potential as power plants and rocket engines. In November of 2013, his small group announced that they had held an FRC stable for 300 milliseconds - a world record, by a large margin. Our interview covers a transition period in his career, from his time spent as a manager on the US ITER effort to his personal experiences rubbing shoulders with physics luminaries to the beautiful physics and practical aspects of field-reversed configurations. Dr. Cohen offers advice on how the US government could accelerate progress in fusion by re-invigorating research into small, clean fusion reactors, an activity now proceeding almost exclusively with venture capital support.

Transcript:

Can you introduce yourself to people who don't know who you are?

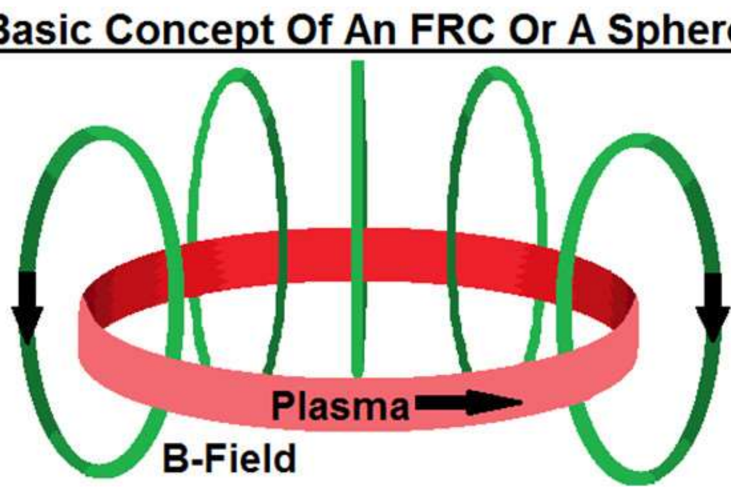
My name is Sam Cohen. I'm a research physicist at Princeton University's plasma physics laboratory. I teach plasma physics at the university and run a program for undergraduates and graduate students. I've been at Princeton for 44 years. My undergraduate and graduate work was done at MIT, a PhD in 73. Most of my work then was an atomic physics not plasma physics.

When did you make the transition from atomic to plasma physics?

When I got the job, when I came to Princeton, I knew no plasma physics. Now I know a little.

Your career spans a very long period of time and you've worked with a number of very famous luminary people throughout the fusion field, including Harold Furth. Can you talk about him?

The man was Brilliant. He had lots of ideas that he published and he also was able to give insightful advice concerning things that he just heard about that he hadn't paid much attention to in his own research. In my particular field of research, he actually made some seminal contributions. He is the person who I think gets credit for identifying the method that was first used to form FRC, plasmas, Field Reverse Configuration plasma, the ones that I work on now.



The basic idea of an FRC: moving current makes a magnetic field around itself. That magnetic field can self-contain the current. Field Reversed Configurations are loops of charged plasma. They make their own magnetic fields, self-containing themselves. On the inside of the loop, the plasma density is higher, leading to fusion. An FRC is a structure made from plasma.

He also, in his, introductory course to plasma physics gave, a homework problem which has been central to how I studied my plasmas' now. So that course was a wonderful course. I didn't realize how good it was until 20 or 30 years later. He also was extraordinarily honest and far sighted and understanding that tokamaks would be very, very big, very, very expensive, and he tried hard to find smaller approaches to fusion, smaller machines that one could experiment with, develop, implement and test. So he had great foresight to try to see what you could do besides the tokamak, which is currently the frontrunner for fusion research, in the world.

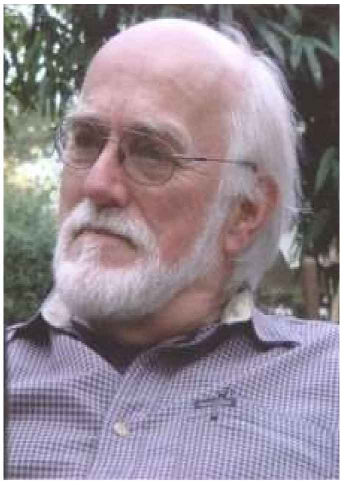
Left: Lyman Spitzer (1914 – 1997), Center: Harold P Furth (1930 – 2002) and Right: Dale Meade.

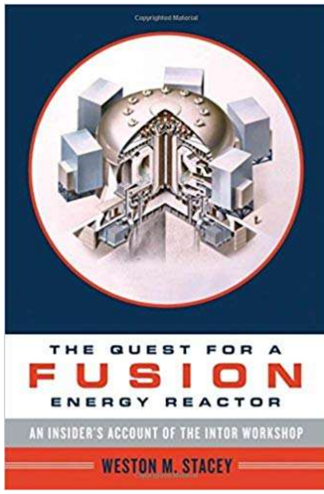
Did you career overlap at all with Lyman Spitzer?

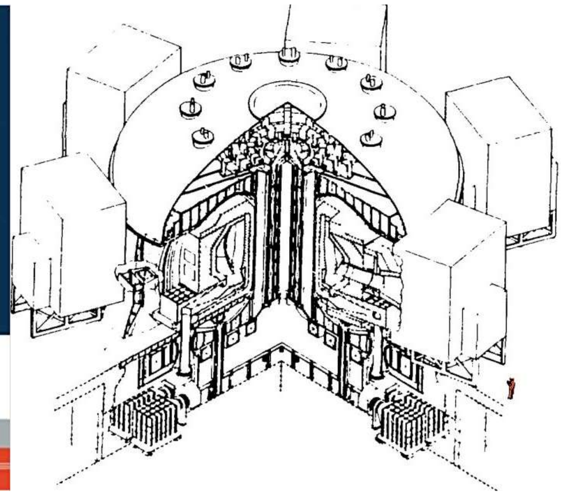
A small amount. That is: when I came to Princeton, he already left the Plasma Physics Lab and he was teaching back on main campus doing astrophysical research. I think it was either my first year or the beginning of my second year at Princeton that I decided to take a course with him. I lasted through about five or six lectures because there's so much in the course that was useful, that when I heard something I just went off to do research on it by myself. So I never did finish the course, but he inspired me to at least write one paper by what he taught in that course, it had to do with interstellar dust, clouds, gas clouds, and how light propagates through them and what atomic physics processes take place in those gas clouds. So I didn't know him except for the course. I probably sat through half a dozen lectures. The physicists who were at the lab at the time spoke of him as though he was a demigod. He had all the brilliant ideas. They had nothing but the highest respect for him.

You mentioned earlier about Harold Furth's' predictions about tokamaks. That's a good segue into the discussion about ITER. Could you walk us through your involvement with ITER back at the beginning?

I think the year was either, must have been around in 1987 and Dale Meade, who is at that time the head of the CFTR project (I can't remember his exact position) invited me to his office and asked me if I'd like to join the ITER team and work on the design of ITER, and I accepted. I thought that the activity would be interesting, but I really had no idea how really, really interesting it would be. I thought that the activity would be time consuming, but I didn't realize how completely encompassing and consuming it was. So I started working on ITER, roughly an 87 when the US first made an agreement. To work with the Russians and others on designing a machine. By the way ITER was not the first attempt at this it was preceded by INTOR, which was headquartered in Vienna, so the ITER project had a predecessor that probably lasted six years.

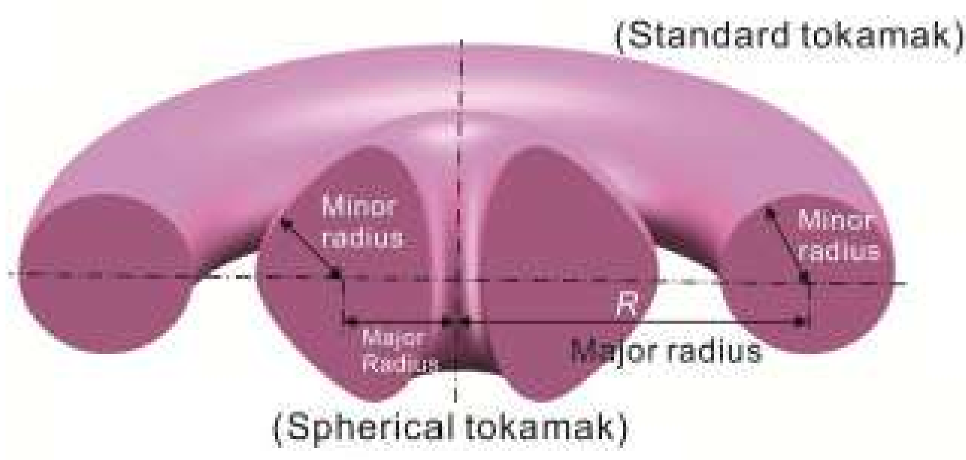






Dr. Weston Stacey (Left) from Georgia Tech and his book, featuring the INTOR design on the cover (INTOR was never built).

I think, Weston Stacie from Georgia was the US leader of that project. And I remember the first organizational meetings that I went to, in the US. One was held in Dallas and before I went to the meeting I was spoken to by Paul Rutherford, who was one of the leading theoreticians at Princeton, and he said that he and Harold were greatly concerned that the ITER project design was growing too large. I think at that time the ITER design was like three or four meters and major, major radius and I said you have to do everything you can to keep it from getting larger and try to make it smaller. But when I left the meeting, which was held in Dallas, the size of the machine, had changed from 3 or 4 meters to 5 meters and so it hadn't gotten smaller, it gotten bigger. And by the time my effort on this finished, which was roughly 1994, get a grown to 6 or 6.2 meters. So despite Harold's suggestions and urging, I was unable to do it because people understood that tokamaks needed to be big. The team I worked on for ITER was called the CDE, the Conceptual Design Effort. That was followed by a more detailed Engineering Design Effort and the machine actually grew in major radius from 6.2 to 8 meters, until they realized that the cost was well beyond what could be provided to them. Then they went back to the 6.2 meter machine that the CDA produced. The 6.2 meter machine might've been called ITER-lite, but I don't recall that exactly. I know the phrase ITER-lite was circulating around that time.



Profiles of a standard tokamak next to a spherical tokamak, with the major and minor radius marked.

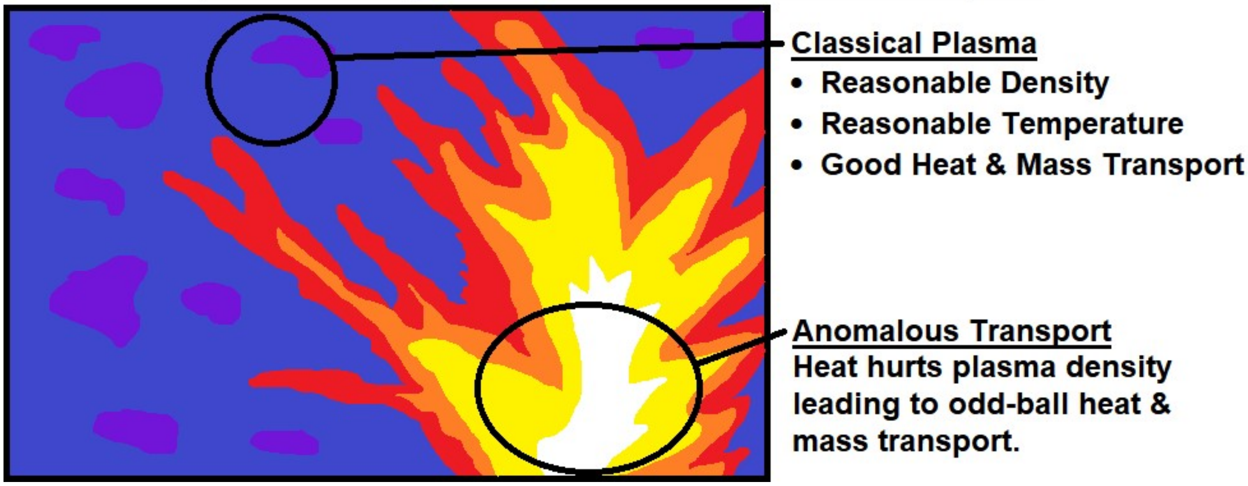
And there's a number of reasons why the size has to be so big, like Anomalous Transport for instance.

That's the main reason why. To give yourself a safety margin in the confinement, you want the machine to be bigger.

Can you explain anomalous transport?

Let's pretend that you have a cubic meter of Styrofoam. It has a certain thermal conductivity. If you put a blow torch on one side. It takes a long time for the heat to reach the other side. It could take a year. But sometimes you might end up actually destroying part of the Styrofoam and have channels flow through it, and maybe the heat from the blow torch could actually go through directly. The tokamak had that kind of problem, that there were ways that the energy can get from inside the tokamak to the outside, much quicker than what's called the classical theory of transport. So the classical theory of transport gives you a good confinement, but anomalous can ruin that and can make confinement 10 or 100 times worse. In fact, when Tokamaks were first built and stellorators were first built in the sixties, the anomalous transport was a million times faster than classical. Now tokamaks have achieved neoclassical confinement for the fast ions. Neoclassical means classical transport, but in a torus. For electrons, there's still some anomalous transport, so the heat in the electrons leaks out faster than ideally. It leaks out according to what people think they understand.

Artistic Depiction Of Anomalous Transport



That's an example of where theory and reality don't agree.

No, I think the old theory doesn't match it but, the old theory was classical transport. But lots of people have done really detailed calculations and they come very, very close to predicting the behavior that's observed. So, over the 40 years, 50 years that tokamaks have been pursued, the theory has come much closer to explaining the results.

So in '94, you decided to move away from the ITER effort. What was the thinking there?

I worked on a particular part of ITER called power and particle control. That is when the plasma starts to fuse and release energy, you have to take the energy out and convert it to something useful like electricity and sometimes the energy would leak out in huge bursts and both the steady state flow of energy and the bursts of energy were too great for conventional materials and conventional structures to handle. We tried to think of many ways to avoid both of these heat losses, but we really came up pretty empty and I think in fact some of the more recent explanations for heat transport are even less optimistic than our studies were in those times. One of the problems with this type of heat transport is if everything worked fine and dandy for a day or two or three in the reactor, but you suddenly get one impulse or loss of heat, it can destroy component and your machine is shut down for three months, six months or a year to fix it. So you can't really tolerate even a few of these accidental losses of confinement.

One example of the maintenance issues on fusion machines using superconducting magnets. Occasionally, superconductors can “go normal” releasing a huge amount of energy and damaging the reactor. This happened on CERN (depicted above) and is a possible issue on fusion devices using superconductors.

So that was one issue. That was a major issue. The second major issue was tokamaks burn a fuel mixture of deuterium and tritium and that produces a lot of neutrons. People like those neutrons because you need them to breed more tritium by absorbing them in lithium, but other people like me don't like those neutrons because they damage the structural material, and they also activate it. So not only do you have problems with the structure is you build don't last forever, and in fact, that might only last for a month or two or three before needed replacement and secondly you have this massive amount of radioactive material. So the neutrons were real big problem for me. I think those were the two main problems. The heat loads being very, very high and the neutron damage being very, very high. Occasionally people would have ideas how to solve some of these problems or at least ameliorate them and the trouble with the tokamak is that it is a huge machine, and to get any experiment tried, to have any new component built for it can take years and millions - tens or hundreds of millions of dollars - so I didn't see how to test things.

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\begin{array}{llllllll}
{}^{2}_{1}\mathrm{D} & + \; {}^{3}_{1}\mathrm{T} & \rightarrow \; {}^{4}_{2}\mathrm{He} & (\;3.5\ \mathrm{MeV}\;) & + \; n^{0} & (\;14.1\ \mathrm{MeV}\;) & & \\
{}^{2}_{1}\mathrm{D} & + \; {}^{2}_{1}\mathrm{D} & \rightarrow \; {}^{3}_{1}\mathrm{T} & (\;1.01\ \mathrm{MeV}\;) & + \; p^{+} & (\;3.02\ \mathrm{MeV}\;) & 50\% & \\
 & & \rightarrow \; {}^{3}_{2}\mathrm{He} & (\;0.82\ \mathrm{MeV}\;) & + \; n^{0} & (\;2.45\ \mathrm{MeV}\;) & 50\% & \\
p^{+} & + \; {}^{11}_{5}\mathrm{B} & \rightarrow \; 3\,{}^{4}_{2}\mathrm{He} & & & & & + \; 8.7\ \mathrm{MeV}
\end{array}
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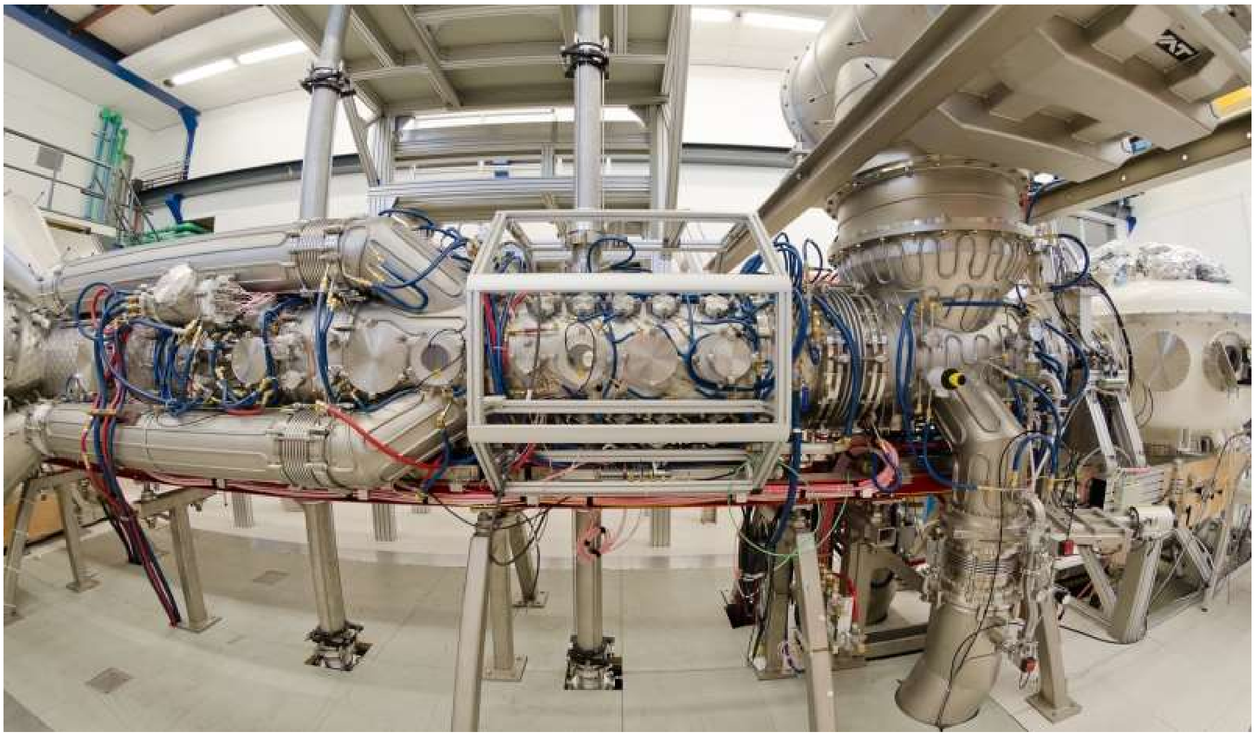
Above, are listed possible fuels for a fusion reactor (deuterium, deuterium-tritium and boron-11). Two of these reactions make neutrons, which is bad. Neutrons mess things up (swelling, cracking, radioactivity, etc). In ICF they make the chamber walls out of tungsten-carbide, which is durable to handle the "threat" from an exploding fusion event, spaced far enough away. The neutrons can also be absorbed in a molten lead-lithium blanket (General Fusion does this) and used to breed more fusion fuel. Aneutronic reactions make *almost* no neutrons – making boron-11 basically the holy grail of all energy sources. If we could get this to work we would have a zero-carbon, almost-zero-radioactivity and plentiful energy source for all mankind.

We had come to a point that people were going to go ahead and build a machine using ideas that were current in, you know, 1988, 1990, and if a wonderful solution came around 10 years later or 20 years later, you couldn't easily put it into the machine to test it. I didn't like the prospect of working with such a large machine where you weren't readily able to test new ideas, so I wanted something smaller where you could try new things and experiment with them. It was a big, big, big, big project. I should point out that I tried very hard to build a machine that could be used by ITER for testing the materials that we're going to be inside it and collaborating with a group of people from Princeton, from Livermore, from Grumman, we put together a large proposal for machine called ITER Diverter Experiment and Laboratory, and we made a proposal to the Department of Energy to build this machine was a long linear machine that would provide a huge amount of power load on surfaces.



An example of a linear test stand for exploring ITER materials, the Dutch Institute for Plasma Physics, 2011. Dr. Cohen helped to design a larger test facility for the US which was not supported by the DOE.

We can see if we could make components that would take the heat load that ITER was predicted to make. The machine was heated by radio frequency waves. The proposal was made to what's called FESAC (Fusion Energy Science Advisory Committee). The vote was taken by that committee and the vote was 17:0:1. 17 against, nobody in favor and one person abstained. Since then, a couple copies of this machine have appeared around the world. One in Holland. Not quite as ambitious as mine, but you know, probably within a factor of two. So I felt in some way vindicated. But, what I liked about the machine was its' design, it was a linear machine, so as much easier to build, to assemble, to do maintenance on. What I learned in the design of this machine helped me move forward in the design of the FRC that I work on now. It wasn't a wasted effort.

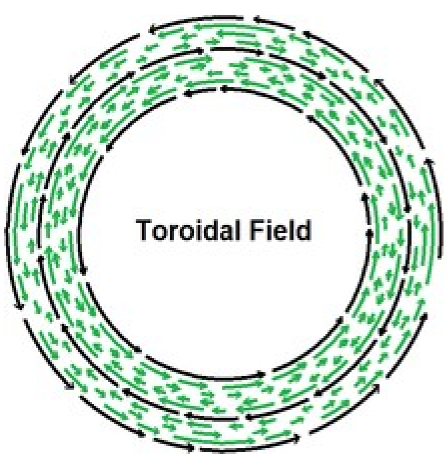
Well, unfortunately the FESAC committee did not support the proposal.

Fortunately they did not support it because it forced me to think of something else. Right.

Then you settled on what you're doing now?

The idea to work on the FRC, in some ways involved, Harold Furth also. So I heard about the FRC effort that was still being pursued, it was a linear machine, which I really did like. It was like a mirror machine, but it had one added feature. It had closed magnetic field lines inside which would improve the confinement. I remember a cocktail party one evening at the home of Robert Buttony and Harold Furth was there and he and I were chatting and I said, by the way, I'm quite interested in for FRCs because I think it has some benefits and I think a lot of progress could be made with them. And Harold said: oh yes, yes, I've heard there's some remarkable new results with them I strongly encourage you to go investigate them. And he says: why don't you talk with Lennon Zacharoff about it because he's the person that he had heard these wonderful results from.

So before I spoke with Zacharoff, I immersed myself more and more in FRC so I could hold an intelligent conversation with Zacharoff. And then I went to see him and I said: Leanne, Harold said I should talk to you about FRCs and what wonderful breakthroughs have been made in them recently. He said, no, no, no, not FRCs but RFPs. So Harold and I kind of miss hurt each other. But nevertheless, Harold gave me some encouragement to work on something different from what he really thought it was. But I had gotten deep enough into FRCs that I really liked them by that time.



The RFP, the Reversed Field Pinch is an internally switched tokamak fusion design. The field in the center goes clockwise, while the outside field goes counter-clockwise. Zacharoff was talking about RFP.

How would you explain the Field Reverse Configuration?

People talk about the tokamak being a donut. And, it's a special kind of donut because it has magnetic coils that encircled the donut. If you want to think of an FRC, the first thing you do is to throw away those coils and the second thing is your shrink the hole in the donut until it's not there. Maybe, it's better to think of the FRC is an orange. There's really no hold on the center of an orange. So I think an orange is a very good way of thinking of it. If you had your orange access vertically, the current is perpendicular to that and the magnetic field is mostly self-generated, by a very large current in the plasma. This orange is placed in a cylindrical container with another magnetic field around it. The linear part of this machine is that cylindrical container that contains the orange.

An orange with the current and field lines of the FRC, drawn over it.

And the orange is the magnetic field lines looping around the plasma? Correct. Exactly right. Okay, so the plasma is spinning in a loop?

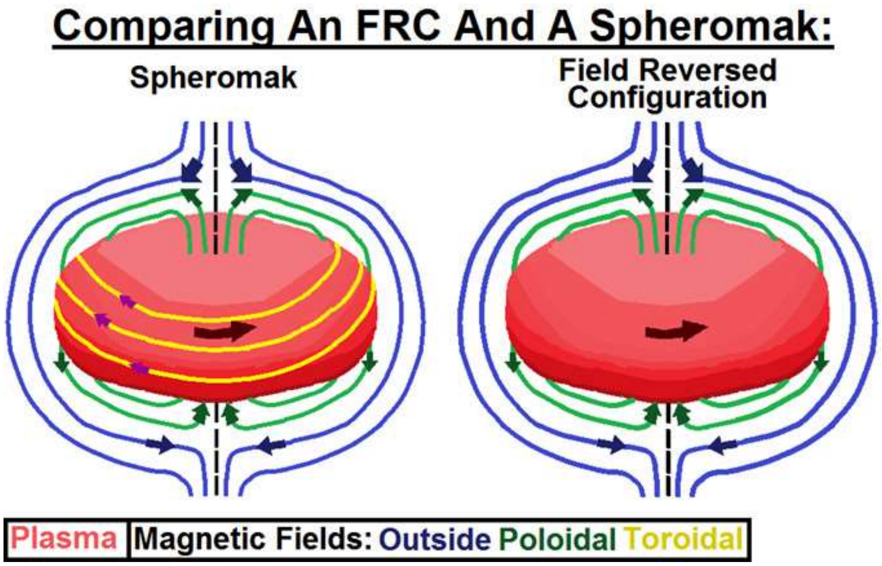
The electrons go one way and the ions go the other way. So there's spinning in opposite directions. There's not one kind of FRC. I want to make sure that that's clear. There are probably four or five, six different kinds of FRCs. So there's' a whole family, and when you say the FRCs will work, do you mean all FRCs work as fusion reactors? Or only one or two out of the family?

And that's a field reverse configuration? Correct. And that's different than a Sephromak?

A Spheromak is kind of like a second cousin. It's on the way between an FRC and the Tokamak.

What is the difference exactly?

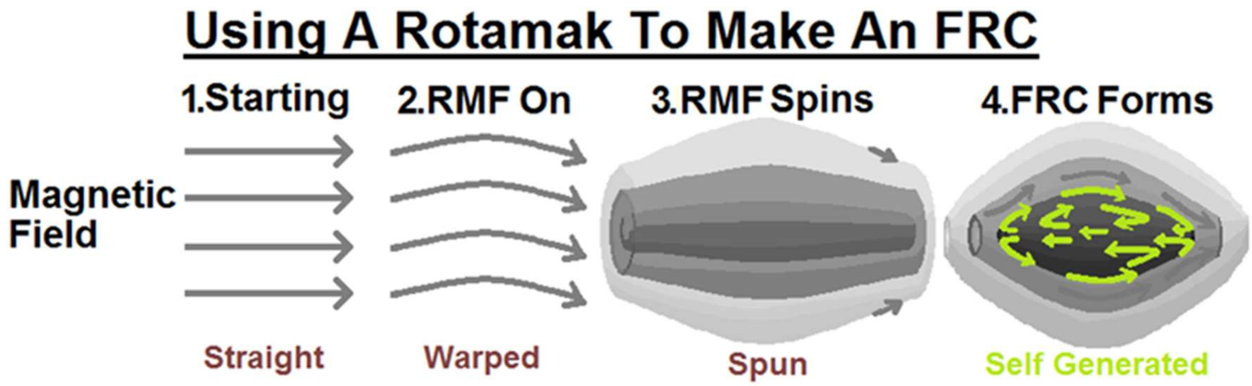
The most important difference is that a Spheromak has the addition of a Toroidal magnetic field. A magnetic field in the same direction as a plasma current. The FRC doesn't have that.



The difference between a spheromak and a Field Reversed Configuration. Spheromaks have one extra magnetic field on the outside that stabilizes the plasma loop.

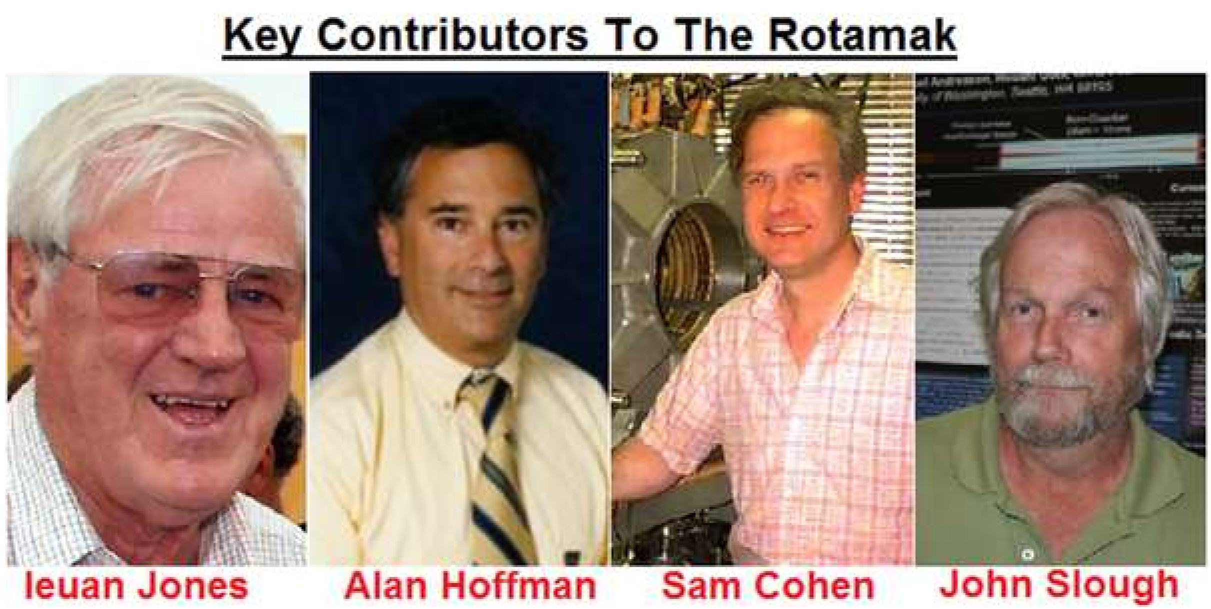
I've used the term Rotamak, but, I think I might've used it incorrectly.

The problem is this: the FRC name refers to how the plasma was made in the 1960s, 70's and 80's. While, the Rotamak refers to how similar plasmas made in the 80's, 90's and 2000's. So it's how the plasma is made, that really is the important thing. When they say Field Reverse Configuration, it really refers back to experiments that were first done in the Naval Research Lab and the way they made the plasma was they had a cylindrical magnet and they have current flowing in that magnet, and then they reversed the current in that magnet. And in fact, Harold Furth gets credit for identifying that process. So the way the FRC was originally made was you very quickly reverse current in coils. That caused the field in the plasma to reverse its' direction too. In the Rotamak, you always have the current in the coils - and the external coils - going the same direction and then you apply what's called a rotating magnetic field. And some people say that that causes current to flow because the magnetic fields rotation drags electrons around. That's probably an incorrect way to view it. But you know, it's a nice picture, so why not? And that's called Rotamak. That name I think was invented by Ieuan Jones who probably wrote 100 or more papers on Rotamaks.



A rotamak uses a rotating magnetic field on the outside to drive the current in a loop, heat the plasma and stabilize the FRC. A more detailed explanation can be found here.

IR Jones, I read a number of his papers.



Key contributors to the rotamak and FRC technology. Ieuan Jones was a professor in Australia who worked on rotamaks from the 70's through the early 90's. Dr. Jones passed away a few years ago. Alan Hoffman and John Slough were both professors at the University of Washington and the Redmond Plasma Physics Lab in Seattle.

I think that he died five or six years ago, which is too bad. Wonderful person.

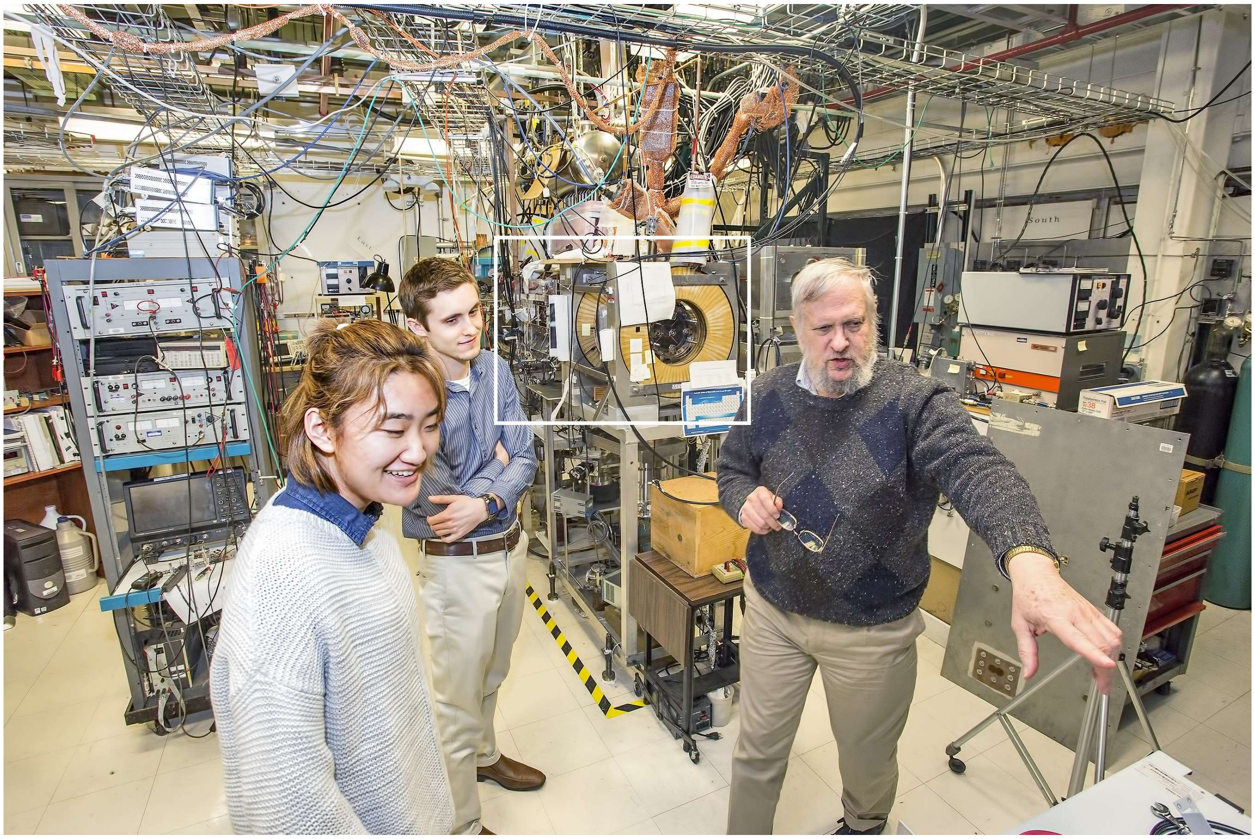
So you've built this machine today and you're doing experiments with it. Can you describe the machine you've got right now operating in the lab?

The machine is called PFRC - Princeton Field Reverse Configuration, and it's one of a series of machines, we think it would take 4 machines to reach net fusion power output. We built machine

1 starting in around 2001 and it ran for about seven or eight years and we achieved much, much better results than we hope for. And that we're running machine 2 and we hope that that will get to its goals in a year or so. The main problem has been the lack of manpower on it. In order to do experiments properly, you need professional diagnosticians, operators and technicians. Our funding is roughly enough for one half a person per year. DOE reduced our funding this year by a factor of two intending to stop the program next year. Luckily NASA jumped in and has given us twice the amount of money that DOE was giving us, although most of its aimed towards engineering studies for use of this for rocket engines. Less money in the physics, more in the engineering. I think that's really good because if it came up with a physics solution, but it was impractical for the engineering perspective, you would get nowhere. To do the engineering at this level and think is exactly the right way to go. We're doing this work with Princeton Satellite Systems, in fact they're the lead on the project because they have the engineering arm of the effort and it's been a wonderful collaboration. You asked what my machine looks like now. So there's the center of the machine and that's a plastic cylinder. It's a pipe, a plastic pipe, about 10 inches in diameter at about 40 inches long.

Made out of what?

The materials called Lexan, which is the trade name for a polycarbonate.



A picture of Sam Cohen's laboratory, the experiment is boxed in white. The experiment is a long tube with a plasma FRC formed inside it.

Why use polycarbonate?

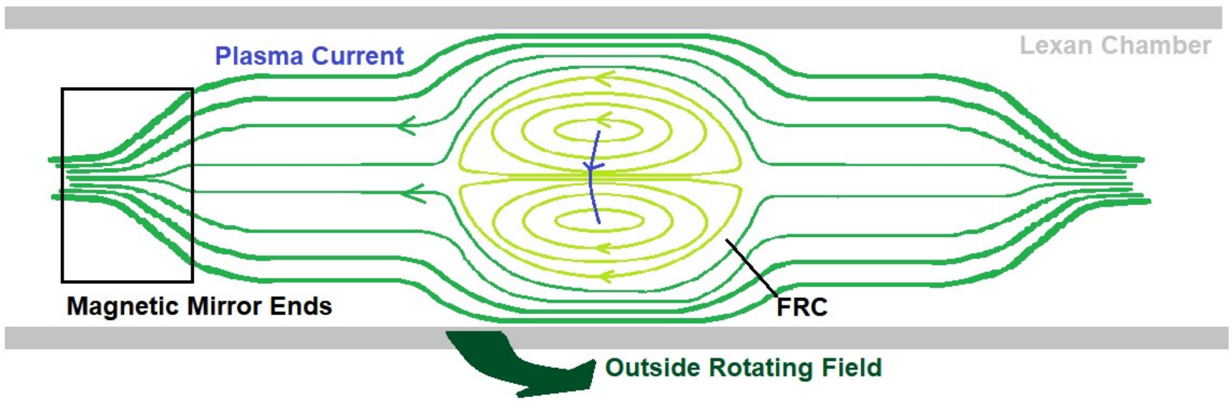
In the machine, we need to have our vacuum vessel be electrically insulating because our radio frequency, heating antennas are outside of the vessel. We want to keep the radio frequency antennas outside the vessel because it makes it much easier to apply the high voltages and high currents and to avoid breakdown. So we need an insulating vessel. Our first vessel was made out of glass (Pyrex) but we were afraid that it would chip crack implode. So we went to Lexan because it's a much, much tougher material and we did a lot of tests on it, machining tests and stress tests and out gassing tests and it's a wonderful material.

Why Pyrex?

I found it on the shelf. Wonderful - I was looking to build the machine and I found this vacuum vessel on the shelf. It had a lot of ports so I could put diagnostics in and we could have used something else. Other people doing FRCs used quartz vacuum vessels, but they're much, much more expensive. They're $50,000 or $100,000. A Pyrex vessel can be a thousand dollars or $2,000 - much cheaper - but it give you worries. If somebody drops a wrench on it or scratches it, a crack can develop and propagate. So ever since we started using the Pyrex, we were always afraid of that particular problem. We wanted something that you could hit with a sledgehammer and it doesn't even say, ouch. A reactor would not have Lexan though. A reactor would be much, much hotter. We would replace the Lexan with something like boron-carbide.

It's a long tube and inside you have a vacuum? Right

Inside the vessel. We have rings of metal and these rings of metal are made mostly of copper. But we've embedded in them, high temperature super conductors and we cool the metal rings by flowing liquid nitrogen through them, so we can make high temperature superconducting coils inside the vacuum vessel. And that helps confine the plasma in the radial direction. On the outside we have our radio frequency heating antennas. They're unique because they have a special symmetry about them and the symmetry is critically important to virtually every aspect of the FRC's operation. The symmetry helps drive current in the plasma efficiently. Helps stabilize the plasma. It helps improve the energy confinement time in the plasma. We came up with this idea, roughly in the year 2000. A group up in University of Washington, tried our idea and improve their confinement by a factor of four or five. We tried it in our machine and it improved confined to a factor of 10 and we think we can do better, as the machines get hotter. So this particular symmetry would be called odd parody - is critically important to the machine's behavior and performance.



The basic magnetic fields used in a rotamak. Two magnetic mirrors face one another. A rotating field is applied on the outside of the plastic tube. This pulls the electrons in the plasma along – making a current. That current self-generates a magnetic field, forming the FRC. The rotating external field heats and maintains the FRC.

When you say RF heating I always describe it as microwaving a plasma, is that a fair statement?

That's not quite right, because in microwaves the wave lengths are small. The way of wavelengths may be anywhere, a few microns to tens of centimeters. But the wavelengths that we deal with are tens of meters, hundreds of meters. It's a little bit different. It is providing radio frequency energy. But, when you think of microwaves you think of waves propagating a long distance. Like for cell phones you have a tower a mile away and it beams a signal to your cell phone. With us, the antenna is very, very close to the plasma, so the wave doesn't have to propagate through space, you actually sense the electric fields and the currents of the antenna, directly. It's called near field, instead of far field.

And its' all a technique to heat the plasma?

Right, and to do it in a way that drives current, and a way that keeps the plasma well confined.
Now you talked about odd parity...

Parity describes the directions of the magnetic fields that are created by our antenna. Parity is a property having to do with mirror reflection. When you look in the mirror, your nose in your images knows could touch each other. If it came close enough to the mirror, that's an even parity mirror. If you have an odd parity mirror, you would see the back of your head.

Cool.

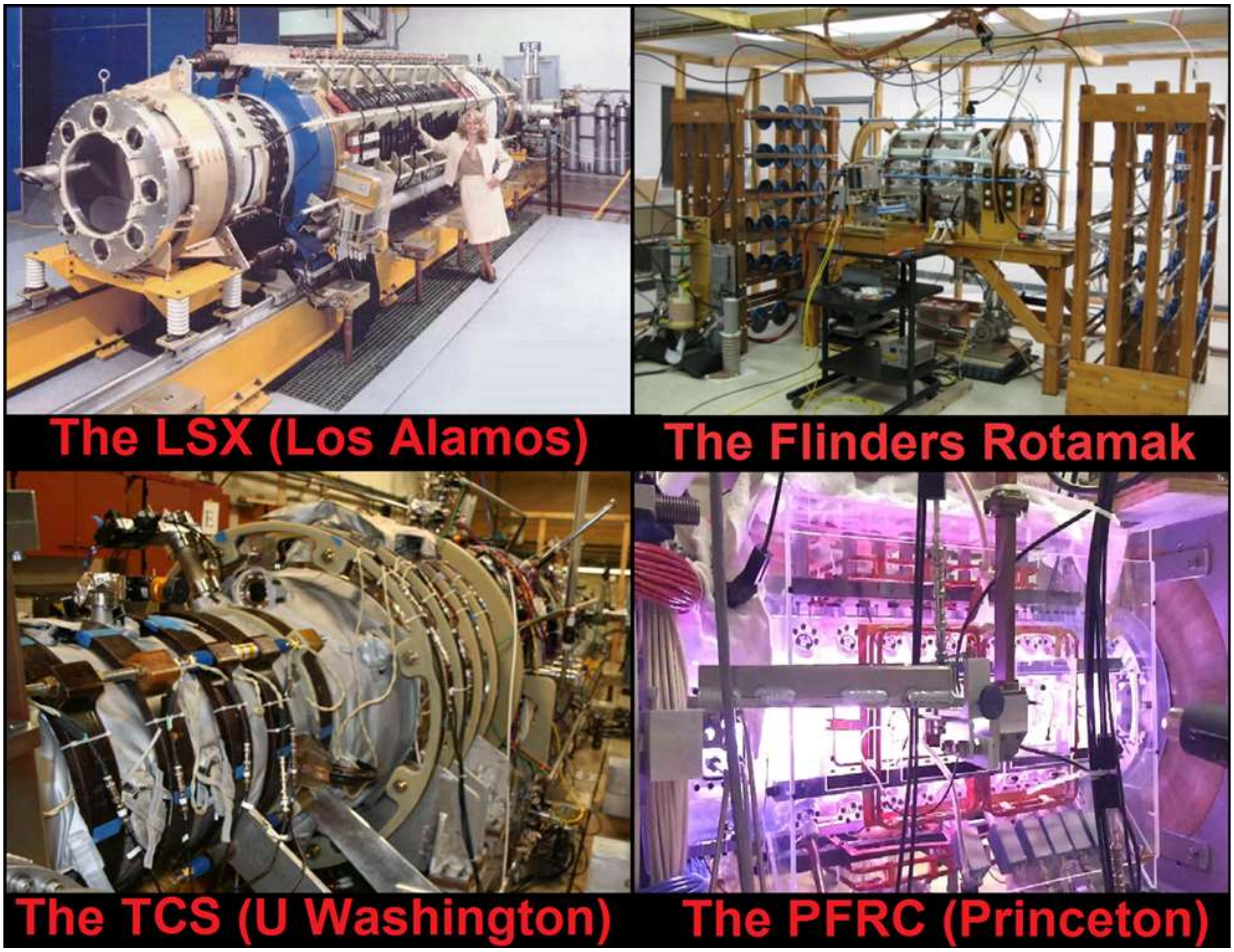
Not only that, but you would be upside down. You put out your right arm, your mirror image, we kind of its right arm. In an even parody mirror, when you put out your right arm, the mirror image presented his left arm

You can form a plasma. You can get it to bounce around between the mirrors and then you can get an FRC to form in the middle. Are you claiming that far or is there anywhere in that statement that you would stop?

We like to think we have an FRC because of a couple of less direct measurements. When we put power in the plasma, we measure how much magnetic field is generated by the currents in the plasma and that agrees with having an FRC. But, an FRC is a very precise mathematical and physical object. It has a skin over it, just like an orange has a skin, and that skin separates what’s on the inside and what's on the outside. We made one measurement about two years ago, which showed the skin very well. But we haven't been able to repeat the measurement partially because we're understaffed and not spending as much time on that. It would be nice to have that measurement done every time we turn the machine on. So do we really, really know we have an FRC? Well, the energy confinement looks like it's an FRC. The magnetic field strength generated looks like it's an FRC. But we have really shown we have that yet? I'm not sure

What would you need in terms of money, diagnostics or people to really prove it?

I think we need what was had in the Tokamak program in the 1970s. In the 1970's, in the US alone, there were over a half dozen tokamaks. There was maybe two at MIT. At Princeton they were two the ST tokamak was replaced by the PLT Tokamak. The ATC tokamak, the PDX tokamak. Oak Ridge had Oramak. General Atomics had Doublet D3. I think there was another Tokamak at UCLA. And each one of these tokamaks was staffed with roughly 10 physicist who really, really knew their business and can really make detailed measurements and they would measure temperatures of electrons by two or three methods - never trust one measurement. They would make measurements not at one point in the machine, but at a 100 points in the machine. So you'd really be sure that you knew where the energy was. So we need roughly 10 professional physicists and each physicists probably needs about a half a technician of support and maybe a quarter of an engineer's support, roughly that scale. 15 to 20 full time people working on it. And the funding we have now, we have probably three full time people worked on this. So a factor of five increase in budget would be roughing necessary.



Current a former FRC machines. The LSX machine was at Los Alamos, which was defunded in the 1980's. The Flinders' machine was Ieuan Jones' main experimental device in the 1980's. The TCS machine was worked on by Hoffman and Slough at the University of Washington in the late 1990's. The PFRC was developed by Sam Cohen at Princeton.

Given the money that you have now, what's your plan to the next year, two years? Experimentally speaking?

The main effort is to try to heat ions. We have electrons in our first machine and the PSRC 1. Our goal was to get to electronic temperatures of 100 electron volts and we published a paper where we got to 115. We also had been discharges where we got 200 to 350 eV. I think we even had one discharge which was 400 electron volts. 400 electron volts is 4 million kelvin. And, in the present machine we've gotten to about five or 600 electron volts. Fifty percent better. We want to get the electrons and other 50 percent higher up to 1000 electron volts. The amount of power we have should be more than adequate for that because we've only been putting in 10 to 20 kilowatts of power, but our power supply can go up to 200 kilowatts, so I'm very optimistic about heating the electrons to where we want to go, but we've never heated ions. We'd have to raise our magnetic field strength and in order for the ions to get hot, electrons have to get hot first. So the ion heating is going to be the most critical goal. That would be a fabulous goal to achieve in the next year or two.

I have two very important questions we need to clarify. Have you recorded fusion?

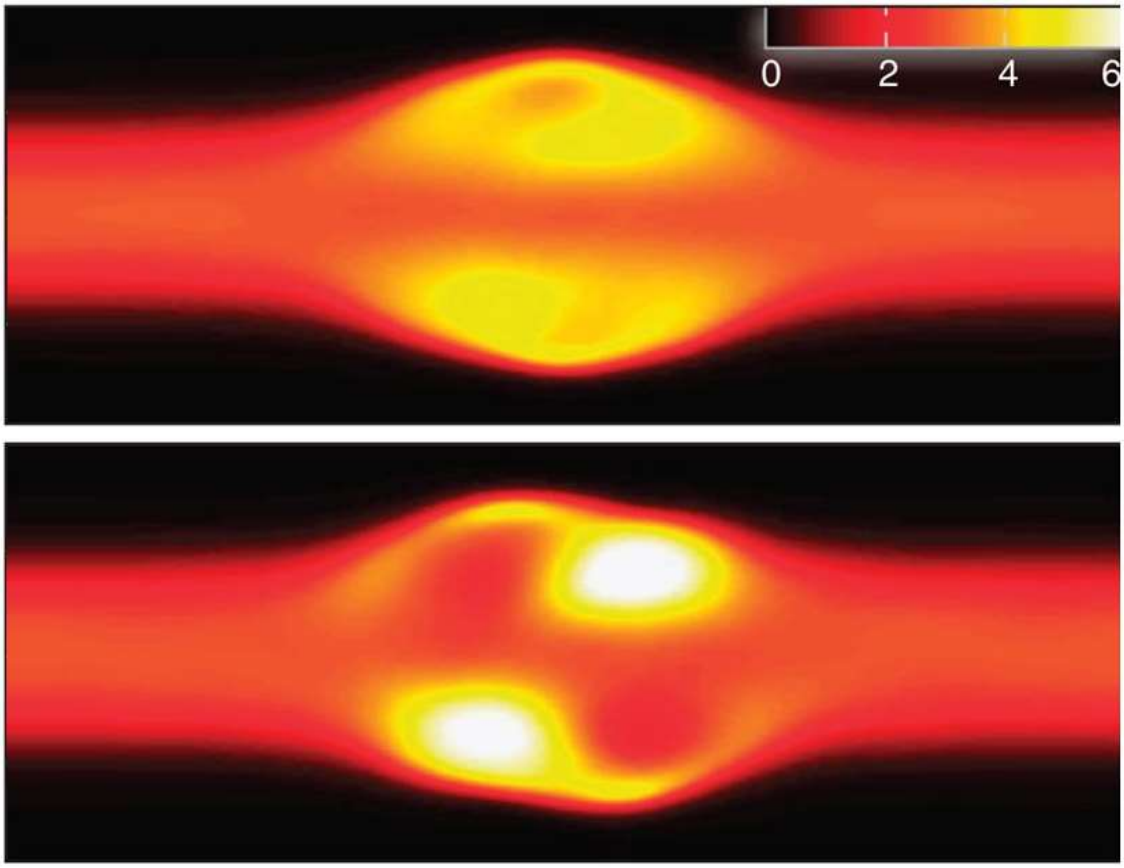
No. We're working purely with hydrogen. We have put other gases in. We've put deuterium, and helium and argon, neon, but we get the highest temperatures with hydrogen. The highest density is with Neon or Argon. But in this machine, we mostly run with hydrogen. In the next machine we will run mostly with hydrogen and hydrogen is extremely difficult to fuse, only happening with any abundance in stars.

And it would take a bunch to run.

We can run a deuterium. But deuterium we expect to perform less well in the machine. Being a heavier nucleus, its orbits are bigger and would be less well confined. We think we'd have trouble with deuterium that machine that we call PFRC 4, we would go to deuterium and helium 3 but the next machine after this one would still not be good enough to do deuterium burning.

The stability of the FRC?

In the late 1970, the greatest plasma physicist of his generation a fellow named Marshall Rosenbluth wrote a paper predicting that the FRC would be unstable. He actually addressed the stability of the spheromak, second cousin for the FRC - and he pointed out that the configuration was unstable to what's called a tilt mode.



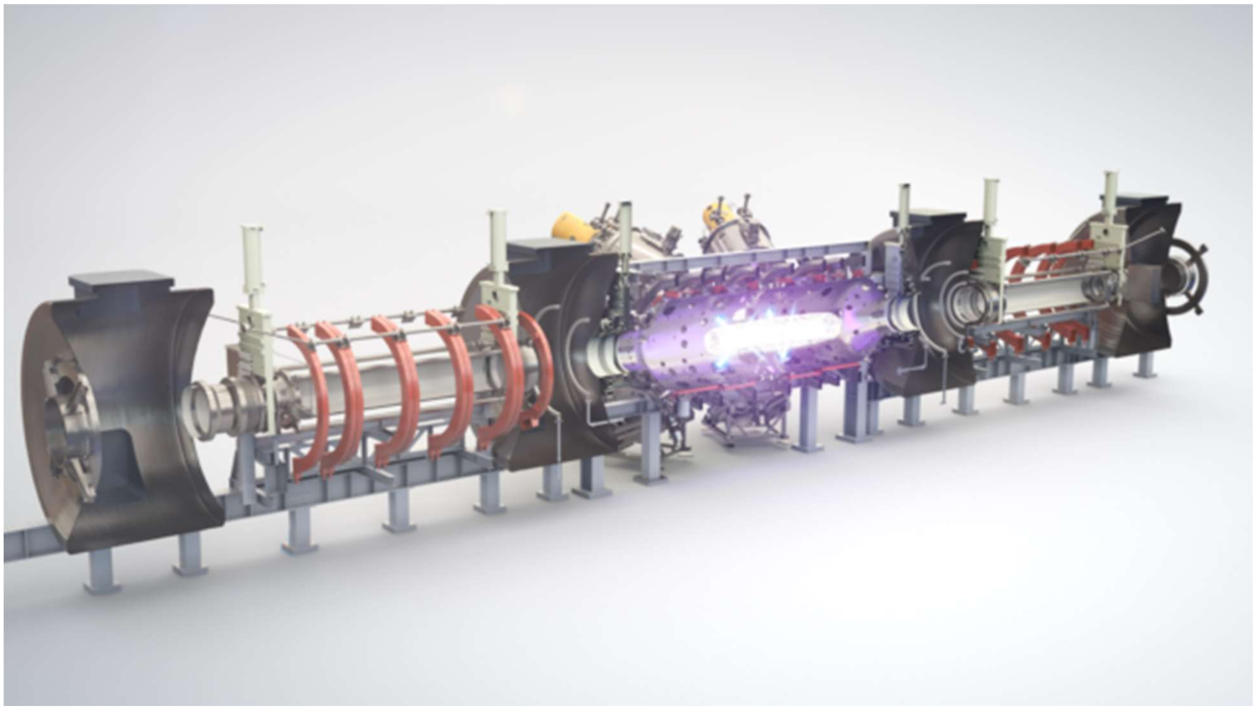
Plasma simulation of an FRC showing tilt mode, where the plasma structure starts to tilt, leading to its’ disintegration.

The whole configuration would flip over, if the plasma axis was not perfectly aligned with the external coils. And when it flips over, it would destroy itself. He said this will occur if the FRC is operating in a mode where it's fluid-like. Where the plasma behaves like a fluid. We 100 percent agree with Rosenbluth and because we agree with him, we try to make our plasma where it's not a fluid.

To be a fluid, it has to be highly collisional, which means cold and dense. We try to stay away from the cold dense regime by making our plasma hot and tenuous and we've been very successful there. And the second way is you make the particles have gyro-orbits that are a large fraction of the machine size.

If you think of your plasma as a cloth, instead of being layers of cloth that can slide over each other, it's really a 3D weave, where particles from the inside, can go to the outside and, kind of, hold the whole structure together. We try to operate in this kinetic mode. So far our experimental results have exceeded Rosenbluths' prediction by a factor, in excess of 100,000. So, we've shown that we can do pretty well as far as the stability. When we go to larger and larger machines, we always designed the machines to be in the stable regime.

Other people go to bigger machines. Ah, there's a company called Tri Alpha in California, which is doing an FRC, which is much larger than mine. Heated by a different method, with a different fuel and whether they will have instabilities, I don't know. Right now, their pulse lengths are roughly 10 milliseconds. When our pulse led to 300 milliseconds, they're doing very well already. Whether they can do the same when they get a reactor we'll have to see.



Picture of the FRC formed inside Tri Alpha Energies' machine.

My question is, what do you think that the US can do differently to accelerate, expand and help Fusion in general?

There is a conception that governments are highly bureaucratic and because of that they're not very good at promoting research and that really research and development belongs in private industry. There was a book written by a professor in England whose last name this Mazzucato the name of the book is "The Entrepreneurial State" and she finds out how extremely well governments have helped to development of major technologies from the internet through solar power, wind power and so forth.

So I agree with her that governments can help a lot. Private industry really doesn't want to get involved until they can make money out of it. If you ask private industry, to contribute to the Tokamak, they would see a bottomless pit 10, 20, 30, 40, $50, billion dollars invested in something that's not going to make money for 100 years. I don't take what they would want to invest in the tokamak. But there are lots of private individuals who are making investments in FRCs. Because FRCs' can be smaller, hence their development can be quicker. This company in California, Tri Alpha has funding from a number of private venture capital firms including support from a guy named Paul Allen from Microsoft, and then there's another fusion startup in

British Columbia, which originally was going to use an FRC and they have funding from a guy named Jeff Bezos, and then there's a 3rd FRC company in Seattle with funding from a guy named Peter Thiel.

These are companies where the funding that's going in is somewhere between $5 and 15 million dollars a year, much different than the billion dollars a year going into Tokamaks. I think that there's room for both. The US used to support FRCs. The US supported FRCs, in the national labs, through 1988, and they shut down the program in Los Alamos. It was company in Seattle called Mathematical Sciences Northwest (MSNW). That started to do FRC experiments using rotating magnetic fields and they then became part of the University of Washington academic program and they were supported by the Department of Energy to about 2005, and then the DOE cut their funding. The same time that they cut their funding, they cut funding for several other innovative concepts. There was a mirror machine at the University of Maryland. There was a linear machine at Columbia University. There was the Levitated Dipole Experiment (LDX) at MIT. And DOE decided, gee, we're really into ITER. We need every penny we can get for ITER.

I think mine was the only surviving FRC in the US. There is a remaining Spheromak in the program at the University of Washington under Tom Jarboe, but you can't really support a fusion effort when you have a half a person working on it. You really need a group of professionals. Probably each machine should have five or 10 physicist. So I think that there are to be support for community of FRCs provided by the US government because when you have a single culture at one place, you're not competing against somebody else. I like competition. I want people to have different ideas, different cultures. I wanted to hear different sides of the story. I want to see different experimental tests and learn from each other. I think there would be three, four, five FRCs, in the US, each one staff that are funding that can support 10 professions.

The last question: is there anything else that you want to add that we didn't talk about?

Yes. Two things. I want to talk about aneutronic and I want to talk about small. As I said earlier, I left tokamaks, one of the reasons was the neutron problem. I think it's really important to try to make aneutronic fusion, where the fusion reaction, doesn't produce a lot of neutrons because the materials problems are extremely difficult. As much money and as much time has been spent already in the tokamak plasma physics research, I would bet that finding the materials and certifying them for use in Tokamak reactors will take as long, and be as costly, if not more. It's very difficult when you're building a commercial machine to certify every weld, every attachment, magnets, every bolt and nut, to be able to withstand the neutron, so you really have to work to support aneutronic fusion.

The second statement is about size. If the Tokamak works, it would be suitable for a central station power generation and most designed show that these power plants or five gigawatts of power, they're very complicated devices and if you look at which countries need these power plants, it might be countries like China, India, but the culture there for operating a machine like that, doesn't reside in the commercial sector, it resides in the military.

I really don't like the thought of having the military control civilian electrical power. And it's because these machines are so large, so complex and so radioactive. If we can make small FRCs, one per village, whether the villages in Africa, China, India, or in Idaho, you can see it being simple enough that the town's folks can run it, and safe enough so if you're at a problem, you can call in your Maytag repairman and have it fixed. So that's what we want. We have problems with our machine every day. Every time we run into something goes wrong. But, we're standing right next to the machine, we can stop and fix it in 20 minutes or an hour. And, we would try to make machine that's so low in radioactivity that you could be within 10 feet of the machine when it was running.

When do you think will have fusion power?

I'd like to see a power plant that is producing net power in 10 years. Is it possible to do it in 10 years? Not in a tokamak. When I started on ITER in 88, we thought we'd have ITER built by maybe 2010 or 2015 and now tritium won't be put in, until 2035. There's another device that's been discussed that possibly can help ameliorate one of the problems the tokamak has. The Stellarator. A Stellarator was designed and construction started at Princeton about 15 years ago and was plagued by major technical and cost problems. Not because the people weren't good. They were the best engineers and plasma physicists in the world, they had seen other Stellorators come into fruition. The Stellorators is a really tough machine because of its twists and contortions, to build. Now if you say, oh, but we built one in Germany, it turns out they were building this one in Germany in 1988. It took 20 years to build a machine that's not radioactive. Stellorators are a very tough machines to build. I think a simple machine has to be the guiding principle. The FRC really is much simpler then the toroidal ones.

A picture of the ITER construction site from July 2018.

Would it be fair to say that FRC shifts complexity from the engineering side to the physics side?

Yes. Physicists have to be the miracle workers. If you go to an engineer, and you say: do what's never been done. That's not what you want when you're making a product for people to use commercially. You want to give engineer's jobs that they can do. If you say, we want to take out the heat, I should say to you, we can tolerate heat loads of five megawatts per meter square. We can't tolerate 20 megawatts per meter squared, and if you tell them you got to do 20 megawatts per meter squared, He should resign his job and say you're giving us something that techniques cannot do reliably. So we want to make sure the engineers are given a chance to do their jobs properly. The physicists have to do the miracles. Once you make a miracle, one day becomes commonplace a week later. But, the physicists have to come up with ideas.