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AN OVERVIEW OF FUSION ENERGY SCIENCE

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HEARING

BEFORE THE

SUBCOMMITTEE ON ENERGY

COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

ONE HUNDRED FOURTEENTH CONGRESS

SECOND SESSION

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C O N T E N T S

April 20, 2016

Appendix I: Additional Material for the Record

AN OVERVIEW OF FUSION ENERGY SCIENCE

WEDNESDAY, APRIL 20, 2016

House of Representatives,
Subcommittee on Energy,
Committee on Science, Space, and Technology,
Washington, D.C.

The Subcommittee met, pursuant to call, at 10:09 a.m., in Room 2318, Rayburn House Office Building, Hon. Randy Weber [Chairman of the Subcommittee] presiding.

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Weber. The Subcommittee on Energy will come to order.

And without objection, the Chair is authorized to declare recesses of the Subcommittee at any time.

And we want to welcome you to today's hearing entitled ``An Overview of Fusion Energy Science.'' I recognize myself for five minutes.

Today, we will hear from a panel of experts on the status of fusion energy science and learn about what can be done to advance this research and technology looking forward. We have two DOE national labs represented here today, as well as the ITER Organization. These experts represent the world's efforts to advance fusion energy science.

The Science Committee has bipartisan interest in fusion energy research and development, and we look forward to hearing from our witnesses today about the future of this very, very exciting research.

Fusion energy science is groundbreaking because researchers are working towards a goal that seems actually beyond reach: to create a star on Earth, to contain it, and control it to the point that we can convert the immense heat into electricity. Fusion clearly is high-risk yet high-reward research and development.

One of the Energy Subcommittee's key responsibilities is to maintain oversight of the research activities within the Office of Science. As the authorizing committee, we must also consider the prospects of future research investments.

The DOE's current budget request for fiscal year 2017 is approximately $398 million, a proposed cut from fiscal year 2016-enacted levels at $438 million.

Funding for fusion energy science has been on a downward trend over the past few years. This sends a signal of uncertainty to the fusion research community of America's commitment to lead in this science. Congress must decide how to effectively invest taxpayer dollars in basic research that provides the scientific foundation for technologies that today might seem impossible.

Today, we will hear testimony from Dr. Stewart Prager, Director of the Princeton Plasma Physics Laboratory, which is the nation's preeminent lab in fusion science. Under his leadership, Princeton's recent upgrade to its spherical tokamak--I keep wanting to say tomahawk, and I know that's not right--tokamak fusion reactor was completed on time and on budget. Dr. Prager, can you teach Congress how to do that with other programs?

I look forward to discussing with Dr. Prager what opportunities exist for the United States to play a larger role in fusion energy research and development.

I also look forward to hearing from Dr. Scott Hsu--am I pronouncing that right, Dr. Hsu--of Los Alamos National Laboratory. Dr. Hsu's work is a great example of how our experts responsible for maintaining the nation's nuclear weapons stockpile can apply their knowledge for an alternate use.

Of course, we're all interested to get a status update on--is it ITER or ITER? ITER, okay. With the complexity of a multinational collaboration like ITER, this project has faced more challenges than most. The Department of Energy will release its own assessment of this project in early May.

Fortunately, today, we have the opportunity to hear from the Director General of the ITER project directly, Dr. Bernard--is it Bigot?

Dr. Bigot. Certainly.

Chairman Weber. Okay. Dr. Bigot's track record as the ITER Director General thus far has been stellar and inspiring. Dr. Bigot, we look forward to your testimony today.

It is important that this committee continues to scrutinize the progress of ITER to ensure that it remains a good investment of taxpayer dollars. We must also consider the importance of access to the ITER reactor for American researchers and America's standing and credibility as a global scientific collaborator. If the United States is going to lead the world in cutting-edge science, we cannot take our commitments to our international partners lightly, and we must not undermine progress on these complex projects.

I want to thank our accomplished panel of witnesses for testifying on fusion energy research and development today, and I look forward to a productive discussion about this exciting area of basic science.

[The prepared statement of Chairman Weber follows:]

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Weber. I'll now yield to the Ranking Member.

Mr. Grayson. Thank you, Mr. Chairman.

I welcome this distinguished panel of witnesses here today to discuss a topic that is of critical importance to the future of our nation and in fact the entire world.

Fusion energy has the potential to provide a practically unlimited supply of safe, reliable, clean energy to us all. While we've yet to achieve a viable fusion reactor, I believe there's many paths that we have to do so. I also don't believe that we're doing nearly enough to ensure that we're pursuing the most promising approaches to achieve this goal quickly and effectively as possible.

Fusion energy can be an enormous global boon to every living human being, and it's going to happen. Whether it happens five years from now or 50 years from now depends on the decisions that we make and the work that you do.

That's why, while I appreciate the participation of both the ITER Director General and the Director of the DOE's only national laboratory dedicated to advancing fusion energy, I'm also particularly pleased that we have Dr. Hsu here on the panel this morning. He's the recipient of the largest award in the recently established ARPA-E program that's examining the potential for alternative innovative fusion energy concepts, this one called magnetized target fusion, which may achieve net energy production far sooner and with much lower capital costs than conventional existing approaches. I also look forward to hearing Dr. Hsu's thoughts on how the Department of Energy can better support and assess the viability more generally of a breakthrough approaches like this.

And I look forward to learning more about the progress that ITER has made under Dr. Bigot's leadership to address previously identified management deficiencies and to establish a more reliable path forward for the project.

And finally, I look forward to Dr. Prager's views on how we can and should regain or maintain U.S. leadership in fusion energy development moving forward.

I think that this panel today goes right to the heart of why we do the work we do in research in America through the U.S. Government and otherwise. It's going to happen. Sooner or later mankind will definitely, without any doubt, establish a means to generate fusion energy and meet our energy needs this way. The question is it's going to happen during our lifetimes and our generation or the next generation or the one after that. I prefer to see it happen in my generation, and I'll know that when that does happen, I will feel very proud that we sat here today, learned how to make that happen, and then did what we needed to do to go ahead and to deliver this breakthrough energy source to all mankind.

I yield back.

[The prepared statement of Mr. Grayson follows:]

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Weber. Thank you, Mr. Grayson.

I now recognize Chairman Smith, the Chairman of the full committee. Mr. Smith?

Chairman Smith. Thank you, Mr. Chairman.

And I appreciate both your opening statement and the Ranking Member's longstanding interest in fusion energy. And I tend to think he's correct; I hope it happens sooner rather than later.

Today, we will hear about the status of fusion energy research and development and the prospects of future scientific discovery in fusion energy. The basic idea of fusion energy is to create the equivalent of the power source of a star here on Earth. The same nuclear reactions that occur in a star would be recreated and controlled within a fusion reactor. The heat from these reactions would ultimately be converted into renewable and reliable electricity.

It has captured the imagination of scientists and engineers for over half-a-century. At the Princeton Plasma Physics Laboratory, the National Spherical Torus Experiment enables scientists from across the country to carry out experiments in cutting-edge fusion research. Someday, the results of this research may provide the scientific foundation for producing power through fusion.

Other DOE labs also support fusion research. At Los Alamos National Laboratory, our nuclear weapons researchers apply their expertise to the development of innovation--innovative fusion concepts.

The ultimate goal in fusion energy science is to provide a sustainable, renewable, zero-emissions energy source. We cannot say when fusion will be a viable part of our energy portfolio, but we should support this critical science that could benefit future generations.

One major step toward achieving this goal is ITER. The ITER project is a multinational collaborative effort to build the world's largest tokamak-type fusion reactor. The federal government should invest in long-term challenging science projects such as this, which will ensure America remains a world leader in innovation.

Today, we will hear from the Director General of ITER, who will provide an update on the project's advances and challenges.

Basic research, such as fusion energy science, provides the underpinnings for groundbreaking technology. This type of energy R&D is still in its early stages and requires commitment and leadership. Unfortunately, the President has not provided the leadership that is necessary and has repeatedly cut funding for fusion science. Despite the President's promises to support clean energy R&D, his lack of support for fusion energy is more than disappointing.

Fusion energy is the type of technology that could someday change the way we think about energy. To maintain our competitive advantage, we must continue to support the basic research that will lead to next-generation energy technologies.

Thank you, Mr. Chairman.

[The prepared statement of Chairman Smith follows:]

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Smith. Before I yield back, I want to explain to my colleagues and our expert panelists today that I have a Judiciary Committee markup, so I'm going to have to excuse myself but hope to be back. Thank you.

Chairman Weber. Thank you, Chairman Smith.

Our witnesses today--our first witness is Dr. Bernard Bigot, Director General of the ITER Organization. Dr. Bigot received his bachelor's degree in mathematics from Claremont McKenna College and his MBA from Harvard Business School.

Our next witness today is Dr. Stewart Prager, Director of the Princeton Plasma Physics Laboratory. Dr. Prager received his Ph.D. in plasma physics from Columbia University.

And our final witness today is Dr. Scott Hsu, scientist in the Physics Division of the Los Alamos National Laboratory. Dr. Hsu received his Ph.D. in astrophysical sciences from Princeton.

I'm now going to recognize Dr. Bigot, Mr. Grayson, for five minutes to present his testimony, and he's going to tell us when they're going to get the fusion problem fixed.

Dr. Bigot, you're recognized for five minutes.

TESTIMONY OF DR. BERNARD BIGOT,

DIRECTOR GENERAL, ITER ORGANIZATION

Dr. Bigot. Thank you very much. Thank you, Chairman Weber, Ranking Member Grayson, and distinguished--sorry. I would like also to recognize the full committee Chairman Smith, which was there a few minutes ago.

I'm grateful and deeply honored for this opportunity to present to you the status of progress on the ITER project. May I have the first slide?

[Slide.]

So you see on this slide the worksite, okay, we have something old, which is the steel frame just in front of you, and just behind is a tokamak pit. It was recorded in last September, and I hope you will be able to view the video we have prepared for you. It will show the real progress and the very short time.

Next slide, please.

[Slide.]

As you know, the project started in 2007, and after nearly ten years--it will be ten years, okay, on next January--it was obvious for many that we have some organizational shortcoming. And is why in a management assessment report, which has been provided by Bill Madia, Dr. Bill Madia in 2013, they point out some specific issue which have to be fixed.

This is why in early August 2014 I was questioned if I could consider to take some responsibility in order to help this project, and after nearly 12 years as a head of Atomic Energy Commission and Alternative Energies Commission in France, I consider such possibility. But I said I want to do it only after we have an agreement of an action plan to be sure that all the ITER members support the recovery plan we needed. And is why we tried to fix, okay, the organization.

We decide on--about effective decision process. We set up Executive Project Board. We gathered together project team in such a way we have an integrated, okay, way to proceed with domestic agencies, seven domestic agencies, which have to provide nearly 90 percent of the value of the project. And I am very pleased to say that we have made very important progress in this field.

The second important point was to freeze the design. When you are to build the machine now, you need to have really a full, okay, finalization of the design. And as you see on the vacuum vessel sector, nine of them are like this. There is many, many piece to assemble. So if you have no finalization of the design, it will delay the delivery. Now on the most important for me is ITER Organization as a design responsible and as the owner of the project must not be a limiting step on any progress for the project.

Also, we develop a large, okay, project culture, nuclear recognition of--it is a statement we have to do, and I am pleased to see that the whole staff now is moving on in this direction. But may be the most important for me is to have a schedule. And when I come in, I discover that, okay, many people don't feel that the schedule wasn't right. And it's why we tried to fix it. I am pleased to say that we have made it okay as of last November. The ITER council agrees on the first years and we set up some milestones.

Next slide, please.

[Slide.]

And so you see some of the milestones, and I don't want to depict it in detail, but really it's impressive how large the progress has been made once we free the energy of the suppliers

and have a clear plan.
Next slide, please.
[Slide.]
Some other milestones, as you see.
Next.
[Slide.]
Okay. The most important was for the ITER members to have an assessment of our proposal as a schedule, and is why, we have an independent review panel. And I'm very pleased to say that on time the panel has delivered its report. On last Friday, April 15, we've, as you see, quite a positive assessment on the way we are proceeding.
Next slide.
[Slide.]
Okay. And now we expect that on the basis of this report that we will be able to have a final decision, okay, on 27th of April I expect that the ITER council extraordinary meeting will be able to examine the finding of those independent report and give full guidance on the next steps. And on the next two ITER Council, we will have approval of the baseline in such a way we can move on to First Plasma first and after to DT, deuterium-tritium commissioning.
Next slide.
[Slide.]
Now, okay, why is the U.S. and the many ITER members has to stay in in this larger project is because I do believe it's worth for them to share their capacity. We've limited investment, nine percent, while it would be a 100 percent return due to the full sharing of intellectual property and operational know-how.
Next slide.
[Slide.]
As you know, the United States is largely contributing. with many national labs been involved in. And you imagine that if the U.S. alone or any other ITER members has to contribute all together, it will take much more time.
Next steps, okay.
[Slide.]
Not only we are developing technology for fusion but for many other cutting-edge technologies and superconducting materials under final distribution and all these things.
Next.
[Slide.]
you see here a map which show that many States in the U.S. is involved in the industrialization of this project. Nearly $800 million has been already awarded to the industry. Eighty percent are spent fully in the United States of taxpayer money from the United States and even more, all the partners are requesting the U.S. industry to deliver.
Next.
[Slide.]
Here is a full list of all the potential suppliers in the, okay, last time.
Next. Next. Okay.
[Slide.]
And we do believe it's important that it is agreed to global sense of urgency about the importance of fusion as you depict because whatever we do, we need to provide more energy due to the increase in population and also the increase in the level of livestyle now.
Next.
[Slide.]
Addressing also some environmental concerns, and you see we depict some possibility. And there is not a silver bullet. We have to make some innovation in order to be able to, okay,

fulfill the expectation of energy supply. And there's big players are not only the United States but also some others, and we have not able to move on. It will be difficult. And I'm very pleased to tell you that last weeks I was in China, and China is now pushing very hard in order to be able to deliver.

Last slide, I do believe.

[Slide.]

Fusion is really making the case, as you mentioned, clean, safe, abundant, and economic energy potential.

And last slide.

[Slide.]

Just to show you that we are now moving on, okay, with this picture. And if you agree, we could have this video just showing you, okay, how it is now in the last few days on the working site.

Thank you for your attention, and I'm ready to listen to any of your question.

[The prepared statement of Dr. Bigot follows:]

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Weber. Thank you, Dr. Bigot.

And at this time I recognize Dr. Prager for five minutes to present his testimony.

TESTIMONY OF DR. STEWART PRAGER, DIRECTOR,

PRINCETON PLASMA PHYSICS LABORATORY

Dr. Prager. Well, thank you very much for your opening comments--I appreciate them greatly--and also for the opportunity to speak to you today.

I direct the Princeton Plasma Physics Laboratory, PPPL, which is a national laboratory, a DOE national laboratory managed by Princeton University. I've been asked to describe PPPL, its activities and opportunities; and ITER, its importance in relation to the U.S. research program.

PPPL employs a staff of 500. It has the dual mission to develop fusion energy and to advance fundamental plasma science with its many applications. The core of a fusion reactor is a very hot plasma, a gas of electrically charged particles such as a flame or a star. Research at PPPL concentrates on ideas that are innovative, unique, and at the world forefront, key criteria for all U.S. fusion research.

Fusion energy research in Asia and Europe is escalating. For the U.S. to contribute competitively in the face of larger investments elsewhere, we must focus on activities with breakthrough potential. Research at PPPL aims for innovation in four major areas: the development of a fusion concept that might lead to a fusion pilot plant as a next step for U.S. fusion, the challenge of how one surrounds a 100-million-degree plasma by a resilient material, the use of large-scale computing for new insights into fusion systems, and physics research that is key to the success of ITER.

We're currently at a propitious moment at PPPL. We have recently upgraded our major facility and just begun operation of this new experiment, the National Spherical Torus Experiment-Upgrade, NSTX-U. It is a DOE-user facility with 350 researchers from 60 institutions. The experiment cuts across all of the four topics just mentioned. It is a design that could lead to a reduced-size fusion pilot plant, a facility that would demonstrate net electricity production from fusion. NSTX will tell us whether this exciting step is possible. To do so it will push the frontier of our understanding of fusion plasmas.

We are also developing a novel solution to the challenge of the material that faces the hot plasma. Most of the world is investigating solid metals. A complementary approach is to surround the plasma by a liquid metal. Liquids are not damaged by the hot plasma. This offers a breakthrough solution to a major challenge. Will it work? We aim to find out through research that combines plasma physics with material science.

Fusion today is being transformed by supercomputing. We can now solve the equations that describe fusion plasmas as never before. PPPL has developed complex computer codes that are generating innovations in fusion systems. All these activities yield key understanding to help guide the future of ITER.

Looking to the future, opportunities abound for new world-leading major initiatives in the United States and the PPPL. PPPL is an underutilized resource for the Nation. The physical infrastructure includes capabilities that are unexploited, but more importantly, the staff of PPPL and U.S. fusion labs in general has broad world-class expertise and ideas that are not being tapped fully. We can do much more.

I will mention three exciting paths for PPPL and the United States. First, if experimental results prove favorable over the next decade, the United States could possibly move to preparations for a fusion pilot plant, a transformational step.

Second, with the revolutionary advance in computing power, we are now optimizing the fusion system in ways that were nearly inconceivable 20 years ago. With significant reactor advantages, PPPL aspires to experimentally test such modern designs.

Third, if current research and liquid materials proves favorable, we could move to a definitive integrated test of that concept.

And PPPL aims, as the national lab for fusion, to coordinate the U.S. research team on ITER following a model we are developing for a U.S. team that's collaborating on a major facility in Germany.

This brings me to the importance of ITER. ITER will be the first experiment to demonstrate and study a burning plasma, a fusion plasma that is self-sustaining, kept hot by the energy from fusion. A burning plasma is an essential gateway to commercial fusion. ITER is the path to this crucial goal.

ITER will also test key technologies and generate 500 million watts of thermal fusion power. ITER will be a landmark experiment in science and energy of the 21st century. It will be the focus of the world fusion program, complemented by strong domestic research in each participating nature--nation.

It is imperative that the United States maintain active participation in ITER and a strong domestic research program. These two components are strongly intertwined. Without a strong domestic program, we will not be able to extract information from ITER, and a domestic program is needed to solve the remaining challenges that ITER is not designed to solve.

The U.S. fusion program consists of broad research at universities and national laboratories and three major tokamak facilities. The three major facilities are General Atomics, MIT, and Princeton, form a triad of complementary capabilities that have made seminal contributions. The Oak Ridge, Livermore, Los Alamos, and other national labs also make key contributions.

The university research community in the United States provides foundational and innovative contributions. Research at universities spans the full range of fusion challenges carried out through experiments on campus and through participation at user facilities. There's a very strong need to reinvigorate U.S. university research and in fusion energy, which has suffered losses in recent years.

The opportunities for the United States to accelerate the pace to fusion energy are enormous. This would strongly benefit the United States as well as the world.

Thank you very much for the opportunity to provide an opening statement.

[The prepared statement of Dr. Prager follows:]

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Weber. Thank you, Dr. Prager.

I now recognize Dr. Hsu for five minutes.

TESTIMONY OF DR. SCOTT HSU, SCIENTIST,

PHYSICS DIVISION,

LOS ALAMOS NATIONAL LABORATORY

Dr. Hsu. Chairman Weber, Ranking Member Grayson, Members of the Committee, thank you for your opening remarks, and also thank you very much for the opportunity to testify. I thank the Committee for its longstanding support of fusion energy and plasma physics research in this country.

I have been asked to describe the status of DOE support for innovative fusion energy concept development and to provide recommendations. I am pleased that the committee is considering these topics.

I also ask that my written testimony be entered into the record.

Chairman Weber. Without objection.

Dr. Hsu. My name is Scott Hsu. I was trained in plasma physics at the Princeton Plasma Physics Laboratory, and I am now a fusion research scientist at Los Alamos National Laboratory. As you know, Los Alamos had its storied beginnings in the Manhattan Project during World War II. Today, Los Alamos is focused on national security science, which includes our nation's energy security. In controlled fusion research, Los Alamos historically focused on many non-tokamak approaches, and thus, it is perhaps fitting that I appear before you today to discuss innovative fusion energy concept development.

The first point is that there are many credible approaches to fusion energy other than our two leading approaches, which are the tokamak such as ITER and inertial confinement fusion such as the National Ignition Facility. You may refer to figure 1 on my written testimony.

Many in the fusion community refer to the other approaches collectively as alternative or innovative concepts. These are specifically what I am discussing here today.

The main reason many of us are motivated to pursue innovative approaches is that they hold the potential for a smaller fusion reactor with less engineering complexity. Some of them could potentially cost much less to develop in a shorter time, perhaps in time to penetrate midcentury electricity generation markets. But these concepts are less mature, and more research is needed to tell us whether their performance can be improved to the point of enabling a power reactor.

The second point is that lowering the cost of fusion energy development is itself a worthwhile goal. The reason is that the stages of development of our mainline fusion programs are very costly, too costly for private investors and companies to play a significant role.

One potential way to lower the cost of fusion energy development is to strategically pursue a number of the most promising innovative fusion concepts that are inherently much lower cost than the tokamak. If federal support reduces early-stage risk for promising lower-cost innovative fusion energy concepts, then more companies such as Tri Alpha Energy or General Fusion may step into pursue fusion energy development.

The third point is that present DOE support of innovative fusion concept development is unhealthy with no new federal funding opportunities. As recently as 2010, DOE provided approximately $42 million per year to support innovative concept development. Today, the only such support is in the recently initiated ARPA-E ALPHA program, which is $30 million over three years, and is focused on a particular class of fusion approaches called magneto-inertial fusion due to its inherently low cost. This was also referred to as magnetized target fusion. You may refer to figure 2 of my written testimony.

So let me close with three primary recommendations. First, Congress and DOE should reassess innovative fusion energy concept development, which should be pursued in addition to our present fusion energy program elements, which Dr. Prager described very eloquently. DOE should consider implementing a new energy-oriented innovative concepts program with appropriate metrics to encourage lower cost and timely development of economically competitive fusion power. Progress is possible for a modest fraction of the overall fusion budget.

Secondly, any new program should enable and promote advances with regard to both the plasma physics challenges and the criteria for a practical fusion power reactor.

Finally, a federal funding bridge should exist for the entire innovative concepts development path from early-stage research to a logical handoff to private development. And this is depicted in figure 3 of my written testimony.

Thank you again for the opportunity to testify. I'm happy to take questions.

[The prepared statement of Dr. Hsu follows:]

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

Chairman Weber. Thank you, Dr. Hsu. And, Dr. Bigot, when you earlier had your comments about the video, I thought you were offering that--to send that to our office, but we've got time to watch that video now if that's what you'd like to do.

Let's play that video.

Dr. Bigot. Okay. Thank you.

Chairman Weber. You bet you.

[Video shown.]

Chairman Weber. Thank you, Dr. Bigot. I now recognize myself for five minutes.

Dr. Prager, I think you said that the fusion power may become actually a--you may have a pilot plant in ten years in your testimony? Elaborate on that.

Dr. Prager. So one grand goal of fusion on a step along the way is to build an energy-producing plant.

Chairman Weber. Right.

Dr. Prager. ITER will produce energy but it won't make--it's not intended to make net electricity. So there is a goal to do that, to demonstrate that you can make net electricity, produce more electricity than you can consume.

So in experiments at Princeton the design that we're studying at NSTX-U, if successful, can offer the possibility of doing that at a somewhat smaller size, not radically smaller but somewhat smaller scale than the conventional tokamak approach.

Chairman Weber. Would that be on site where you are now?

Dr. Prager. No. A--such a facility would involve tritium handling, which would not best be done in Princeton, New Jersey.

Chairman Weber. Okay. Well----

Dr. Prager. In ten years we could begin the design and construction of such a facility, and there are--this is--there are other ideas, for example, to use advances in magnets, current magnet technology also to seek reduced size pilot plants. So this is one aspiration for the future.

Chairman Weber. Well, that--I mean, you're putting it--to the gentleman from Florida's question about we don't do this in our lifetime, you're putting a ten-year time frame on it, which, whether or not it's realistic--but that is a goal for us to shoot at and I'm encouraged to hear that.

Dr. Prager. Yes. But just let me clarify, that's not ten years to a commercial reactor.

Chairman Weber. No, I get it.

Dr. Prager. So let me clear----

Chairman Weber. No, I get that.

Dr. Prager. Okay. If it's ten years where we would begin the----

Chairman Weber. Right.

Dr. Prager. --design and construction of a pilot plant----

Chairman Weber. I'm just hoping my friend from Florida will still be with us----

Dr. Prager. Yes.

Chairman Weber. --in ten years.

Dr. Prager. I hope so, too.

Chairman Weber. Yes. You bet. So that--I was glad to hear that.

And, Dr. Prager, would you support Department of Energy's development of high-performance computational tools that would be accessible to the researchers in the private sector, academia, and at the national labs to be useful to the fusion community? Do you think that would help shorten that time frame to where we could develop that commercial energy power plant?

Dr. Prager. Absolutely, yes. You know, there's been revolutionary advances in supercomputers that's revolutionizing all of modern science, fusion no less than any other. So with the supercomputing capabilities we can design concepts and test them on the computer and advance them in ways that we couldn't possibly do before. And there are new ideas on the table because of that.

It's also, I might say, critical for us in terms of interpreting ITER results. We need these advanced computation to understand as best as we can how ITER will behaved. So this is revolutionary for fusion.

Chairman Weber. Dr. Hsu, does that help you?

Dr. Hsu. Absolutely, yes. I should also add that for smaller projects such as Innovative Concepts, the resources generally are tough to come by to make use of our computational capabilities. So any assistance on that front would be tremendously useful for innovative concept development. Also in the inertial fusion side, access to some of the codes can be difficult for people not at the national labs, so----

Chairman Weber. Okay.

Dr. Hsu. --making codes more generally available would help

fusion energy development.
Chairman Weber. You're working on magnetized target fusion?
Dr. Hsu. Yes, that's correct.
Chairman Weber. That's part of this and that would help you in that endeavor?
Dr. Hsu. Yes.
Chairman Weber. Okay. Very good.
And, Dr. Bigot, I'm going to come back to you. Thank you, by the way, for your success--early success as Director General in setting a time frame and a guideline. We really appreciate your efforts in that manner.
The fact that you've had success should instill confidence, but how can we help you--what needs to be done to increase the confidence in ITER that ITER will be on--will continue on that steady pace to realize its goals? What are your plans to make sure that you continue that pace?
Dr. Bigot. It's clear that it's a long-term commitment for all the seven ITER members, and I do believe that the best would be for the ITER members to have referral and open discussion in such a way that any proposal we could make could be fully examined and supported. And it's why it is so important that the seven members feel fully committed to support what we call the "best technically achievable schedule" in such a way that we have all the milestones--we have now clearly a position on this road. We have the full support.
I really appreciate that you would give us the opportunity to make more largely--warn us, share among all the ITER members about the importance of continuous support.
Chairman Weber. Well, thank you for saying that. And we want you to view this committee as a resource because we want to be a source of encouragement and resource for you so that anything we can do to keep this project moving forward, we want to be able to be helped in.
I'm over my time. So thank you again for being here and your testimony.
And the Chair is going to recognize Alan Grayson.
Mr. Grayson. Thank you.
Dr. Bigot, it's been ten years already since the major governments of the world signed off on the ITER project. We now have 11 years to go before we start to do the major experiments involved, and there isn't even a plan to actually generate net electricity from ITER. That's not its design or its purpose.
Dr. Prager, you're talking about an alternative smaller-scale approach where we would begin construction ten years from now. Let's say hypothetically that mankind wakes up tomorrow morning and decides that we don't want to wait 10 or 11 years until we do the experiments or the construction, but we want a much quicker result that can lead to electricity generation net from fusion projects in a shorter time frame. What should we do? Dr. Hsu?
Dr. Hsu. I think we need to pursue many avenues at this point because we don't know the answer right now. ITER is the most mature--ITER is the most mature method, and I believe that is why we're pursuing and need to pursue it, but we should consider all our known options at this point.
Mr. Grayson. Well, consider, what do you mean by that? Pursue, you used that word also. What do you mean by that? What should we do?
Dr. Hsu. We should look at our other options alongside our maybe--you know, our most mature option.
Mr. Grayson. Do you want to enumerate some?
Dr. Prager. Well, so some of the examples I discuss in my written testimony and on the figure 1 of my written testimony, DOE has supported the development of those concepts in the past. There is room to continue advancing some of those

concepts, and some of those concepts, as I mentioned earlier, are attractive because they have less engineering complexity. But they--but I caution, they are at a less-mature state right now, so it's harder to provide a reliable path forward. We need to do the research to decide whether the performance is acceptable.

Mr. Grayson. Dr. Prager, if we don't want to wait 10 or 11 years simply to conduct more experiments or new construction but we actually want to see some positive result that benefits mankind in a shorter time frame, what should we do?

Dr. Prager. Well, we're resource-limited right now. I think if the United States wanted to commit more aggressively to fusion, there's lots that we can do. We can lay plans now for in the United States to build an energy-producing facility. Whether it's a pilot plant or something of reduced ambition, the scientific immunity would have to debate, but we can move forward on that. We could move forward on ideas that offer perhaps more attractive route or solve some of the problems that are confronting us.

I mentioned the computer is able to design machines we couldn't design before. There are designed on the table that the United States should be building that would have very attractive features. I think there is no one idea that's a magic bullet that will deliver commercial fusion power in 10 years.

Mr. Grayson. Okay.

Dr. Prager. I don't think that's going to happen. But we can greatly accelerate the pace, and no question, fusion can be developed in a timescale to have a huge impact on how we produce energy in the mid-part of this century.

Mr. Grayson. All right.

Dr. Prager. I do agree with Dr. Hsu's testimony. I think that we should be supporting ideas in fusion that span from the mainstream and there's a continuous spectrum all out to ideas that are very, very primitive at this time. But I agree with Dr. Hsu that we should have metrics and a systematic way to judge their progress and what should move forward and what shouldn't be.

Mr. Grayson. You say there are designs on the table that the United States should be building. What are they?

Dr. Prager. So here you'll get different answers. Let me just preface it. You'll get different answers from different fusion scientists because there's a plethora of ideas and everybody has their favorite. So with that preface I'll tell you one of my favorites, okay? There are designs now that use magnets that look highly, highly asymmetric. If you look at how--the magnet structure, it's not nice, circular magnets. It's because we can design--we can optimize the shape of the magnetic bottle so that--to make the best physics performance we can possibly get.

So there are designs that go into the brand name of stellarators. They're studied in Germany and Japan, but there are unique U.S. designs that automatically run continuously for months on end and are extremely stable and well controlled. And that's one example, I think, of a modern design that we should be building. And there are others.

Mr. Grayson. Dr. Bigot?

Dr. Bigot. You know, I want to be very clear. We have not to oversell, okay, the schedule. For example, I just want to let you know that for many factorings a big vacuum vessel sectors. There are nine that I mentioned there. We know we could not do it before three years and eight months, okay? It takes time if we want to be able to really deliver.

The ITER project now want to demonstrate clearly that we will have, okay, massive production, a sustainable production, and it is leading-edge, okay, technologies and it takes time, okay. It's a nuclear facility. We need to absolutely work on quality and safety, and for me, I'm very supportive of the alternative, okay, development, which could be brought in because I do believe it will be worth once we have as ITER demonstration to integrate some of these things. But as long as we have not seen the real breakthrough with, okay, the yield of factor of ten and when compared with the energy it is consuming in order to heat the plasma it will be difficult to accommodate.

Again, we have to think about, for example, for developing the superconducting coils now, it takes nearly, okay, 30 years, okay, on all the best expert worldwide in national labs in order to secure an industry production, okay. So from my point of view we are not to raise too much expectation.

The most important for me is to keep now on schedule. We have a clear schedule, okay. It will take, as you say, okay, ten, eleven years to have the first plasma done, and I do believe it will be the best demonstration of the availability of this technology to be able to afford according to the best schedule we have.

Mr. Grayson. I yield back.

Chairman Weber. I thank the gentleman and now recognize Mr. Knight from California for five minutes.

Mr. Knight. Thank you, Mr. Chair.

So I'm just going to go down the kind of time frame here and who's involved. We have seven members involved, and we have about a 30-year process of where we started on this and now we're talking about another--maybe a ten-year process before we get to a--kind of a working model for lack of a better term here.

In every situation when we talk about a long-term project, we're always talking about cost, we're always talking about who's involved and maybe if we need to get more money, then we have to look at those members, or has anyone talked about bringing in other countries, other members into this agreement? Yes, sir.

Dr. Bigot. Yes. Clearly, I'm pleased to see that in the world many countries are looking to fusion, as you do yourself, and I'm very pleased to see some new companies starting in order to demonstrate if there is some innovation. And I'm clear to you that some country now are questioning us as to whether we could accept if they could join us as a new ITER member. So very soon I will discuss with some of these countries in order to see if they could fulfill the regulation and the rules to join the ITER.

Mr. Knight. Okay.

Dr. Bigot. And I will let you know as soon as, okay, we will be there. It could help us because, as you know, there is over-cost and so if these people bring in and are decided to be really a member, it will reduce the difficulty to find, okay, the financial request we are now facing.

Mr. Knight. Okay. Very good. And I'm to understand that the ITER project is the closest project in this form of technology anywhere in the world, is that correct?

Dr. Bigot. According to me, yes. We have been working for so long and all----

Mr. Knight. You're my expert.

Dr. Bigot. Okay. And we are--yes. And we are now benefiting of all the experience. You know, when the project start, it was quite difficult challenge to fix everything, but now after the ten years, I do believe if we have a proper management, we will be able to deliver on time.

Mr. Knight. Okay. And my last question is a question I always put to my son is that do you think we're smart enough to do some of the things today? And the answer is always we may or may not be smart enough to do some of these things. The fact of the matter is for the supercomputing that is happening today, we are way smarter today than we might have been ten years ago with the advancements in computers, with the advancements that we have done over the last ten years to get us to where we are.

So in ten years from now, hopefully, we have this model, hopefully, we are on schedule and we are hitting all the points that we're supposed to for this project. But over those ten years, computers are going to be infinitely better at what they do, compared to today. We are going to know an awful lot more in ten years than we know today.

So with all of that being said, I hear from all three of the panelists that it is very hopeful and possible that we will be there in ten years to have this project up and running. And I am getting that from all three of my panelists, is that correct?

Dr. Bigot. Yes. According to me, you are correct. It was really very well point out and underlined that computing facility is very, very important asset for the developing of these technologies.

As you know now, within the ITER we have what we call a broader approach. With Japan, for example, we have a computer specially dedicated to the modeling of the plasma and all of the, okay, operation and factors.

But I would say that now the ITER project is a really challenging engineering, okay, goal and is why it's bringing us so much to have this computing capability. And if you come onsite, you will see we have what we call a virtual room where all the engineers day after day are able to see how this piece will be fully assembled, how we can maintain them, how we could take advantage of the optimization of the process. So computing for me is really something which could help a lot in the future.

Mr. Knight. Thank you very much, Mr. Chair. And I think we were just invited to southern France.

Chairman Weber. All right. Well, when this hearing is concluded, we'll all get--go to the airport.

I appreciate the gentleman yielding back, and the gentlelady from Massachusetts is recognized.

Ms. Clark. Thank you, Mr. Chair, and thank you to the panelists for being here. And I understand that burning plasma science is just one of the areas that we have to address if we really want to deliver on fusion's promise of clean--as a clean energy source in a meaningful timescale as we look at climate change and the effects that it is having.

What--this is really a follow-up on Representative Grayson's question, but what does the United States have to do to establish leadership and accelerate the progress in plasma phasing materials research or in simulation and modeling of plasmas? For anyone.

Dr. Bigot. I could start. For me from my point of view as ITER, as everybody knows in the world, the United States has the most advanced, okay, in science and technology industry. So the best we could expect is to train excellent engineers, excellent scientists, and invite them to join the effort because the staff will be the best asset to move forward more rapidly.

And so for me it's very important that we have a clear long-term vision, okay, a real roadmap to deliver in such a way we built trust for the new generation to be involved in these works. As we discussed here, it's a few years ahead of us when we will be able to operate a facility, and the best is to have new generation to be involved in this field according to my views.

Ms. Clark. Thank you.

Dr. Prager. So in terms of your two questions on computation, I think we have the knowhow, we know what to do, we're building the codes, and we just need to be part of actually the presidential initiative in exascale computation. If fusion can partake in that, we can very directly move ahead in computation. That's--we're just--we're ready to go. And it's an area where a relatively modest investment can keep the United States on the leading edge in fusion computation.

You asked specifically about plasma-facing materials. Well, within the last year the U.S. fusion community got together and did some planning in that and came up with four possible next steps. They include building an experiment that's specifically designed to shoot a plasma into a material to develop material science. It includes a robust program in developing liquid metals as a plasma-facing component. It includes ideas to build a new but medium-sized tokamak that's designed specifically to learn how to exhaust the heat in a way that the material survives. And it also includes full utilization of the current facilities that are studying this. So the community did come together and lay out some near-term affordable opportunities in that area.

Ms. Clark. Great. Thank you.

Do you have anything to offer, Dr. Hsu?

Dr. Hsu. Yes, I like to say that I think to follow on Dr. Bigot's point about bringing young--bright young people into the field especially, there are things we can do. One of the things that I wrote about, about the excitement out some of the innovative concept work is that because it offers a tantalizing possibility of a faster development path, that that could help with exciting, you know, the new generation of fusion scientists.

The other thing is the advanced computational abilities you spoke about could really help the innovative concept aspect of the program because not as much has been applied to innovative concept research with our latest and best computational capabilities.

Ms. Clark. So if I understood you correctly in your testimony, you were talking about--I think you said that for the private sector a lot of these innovative technologies are too expensive to really have a meaningful investment. If we are not finding that funding in the United States, are there international competitors who are looking to fund this type of innovation?

Mr. HSU. I believe there is. I know that General Fusion, the Canadian company, has obtained funding from the Malaysian Government's sovereign fund. I've read that Tri Alpha Energy has received funding from a private equity vehicle created by the Russian Government. We know that China is building many if not most of the devices I showed in my figure 1, and I believe China is also pursuing magneto-inertial fusion, which is the focus of the Alpha ARPA-E program. So I--for some of these things international sources may become the main option.

Ms. Clark. Thank you. I see I'm out of time. Thank you, Mr. Chairman.

Chairman Weber. I thank the gentlelady, and Mr. Hultgren from Illinois is recognized for five minutes.

Mr. Hultgren. Thank you, Chairman. Thank you all so much for being here. This is an important discussion for us to be having. Fusion energy certainly is a very important research area that has the potential to completely transform our energy sector. It also is a massive undertaking that is emblematic of the internationalization of major research facilities. Our scientific communities have to work together because we can no longer just go it alone and expect to get anything done.

Dr. Bigot, I wonder if I could address my first question to you. First, I want to say that I really appreciate the work that you're doing, and from all that I've heard, the ITER project seems to be in a much better place than it has been in the past, and I think much of that is because of your leadership.

One question I'd like to ask you, and I hope you can be candid so that we can help to make America a better partner, I wanted to ask what the biggest hurdles that you face or that others face working with the United States. What are the first questions you ask yourself when we say that we're going to deliver on a project that is five or ten or fifteen years down the road?

Dr. Bigot. As you know, this project is so large that just one single country cannot afford it. You have to think about that we are building huge magnetic cages. The size of it is 20 meters, I would say, with a precision which is millimetric. So if we just considering one country, whatever powerful it could be, it will be too long to clearly demonstrate.

So for me it's very important again, as I stress the point, that the United States be committed on the long-term and could contribute--they contribute with their staff, as I mentioned. They could contribute also with many other technologies, okay.

The project--the ITER project, as any others, okay, fusion project, request a lot of different technology, cryogenics, electro techniques, materials, and all these things. And so all these part could be gathered, okay, and I expect, as it was told by your Dr. Prager and Dr. Hsu, that there is strong support for all these basic research which could contribute to accelerate the proper delivery of the fusion technology in the world in the very next two decades really.

Mr. Hultgren. Thank you. I hope that--I do see how important this partnership is, and I hope we can remain a reliable partner. That's something that we've got to struggle with and make sure that especially the funding side of things, that we are reliable there.

Dr. Prager, I wanted to talk briefly with you. First of all, it's good to see you again and I look forward to seeing you later with the Lab Day that's going on over on the Senate side this afternoon. But the privilege I have of representing Fermilab, I see a lot of similarities between our two labs being single-purpose, and I'll make sure that any measure of success our labs use, it takes into account the differences between these labs, and also our broader multipurpose labs like Argonne and some others.

When we do science, the science itself should always be the driver of the work we pursue, but it's always good for us to know the other side-benefits and application from our research. I wonder what other applications does your research have that can benefit the nation?

Dr. Prager. Thank you. And I do want to thank you for your broad support of the whole national laboratory complex, which is invaluable to us.

Mr. Hultgren. Thanks.

Dr. Prager. The applications of fusion energy science go far and broad. And there are two classes of applications. One is to other areas of science, and the other one is applications to society and industry in general. In science we only have to know that essentially all of the visible universe is made up of plasma. So if you want to understand how stars are formed, how black holes work, why solar flares occur, if you want to understand the space weather and the Earth's environment, that's largely a problem in plasma physics. And the synergy between fusion science and what we call plasma astrophysics is enormous. So the effect on astronomy is enormous.

In regard to your--one of your interest areas of Fermilab, there are plasma ideas to build new accelerators where you can accelerate particles much more quickly to high energy over much shorter distances than the present accelerator technology, and this is a very exciting application of plasma physics to particle physics, which is the focus of Fermilab.

In industry, plasmas have a nice property. They're kind of people-sized and they're pretty hot and you can use them to interact with materials in revolutionary ways. So plasmas are used to make semiconductor chips and have in part fueled Moore's Law. Plasmas are used to make new types of nanostructures that are revolutionizing various types of industry. Plasmas are used to burn up waste. There's a new area of plasma medicine where plasmas interact with biological systems. You can use plasmas to heal wounds and plasmas can affect the chemical reactions in biological systems. There are plasma rocket thrusters that are in use today. You can--instead of having a chemical rocket, you can shoot a plasma out of a nozzle and the rocket moves forward. So you can have rockets that are much more fuel efficient. So there's a remarkably broad array of both fundamental science and industrial applications of plasmas.

Mr. Hultgren. That's great. It sounds exciting and I'm looking forward to everything that comes out of this.

Thank you all again. My time is expired. I yield back, Chairman. Thank you.

Chairman Weber. I thank the gentleman. And, Mr. Lipinski, I think you're up.

Mr. Lipinski. Thank you, Mr. Chairman. Thank you for holding this hearing. It's very important.

As I'm sure everyone has talked about the--how critical it could be to--you know, producing energy in so many areas if we can figure this out.

I visited the NIF--I visited NIF at Lawrence Livermore a few years ago, but I'm going to leave that to Ms. Lofgren to talk a little bit more about that. I'm sure she has some questions and comments about that. But I want to look at what we've been doing over the past few years looking at promising alternative approaches to achieving a viable fusion reactor. They have emerged from some small and midsize startups, as well as academia and our national labs.

And Dr. Hsu, you know well ARPA-E recently established a three-year program to further explore the potential for some of these concepts, particularly on an approach called magnetized target fusion. But like all ARPA-E initiatives, this program is temporary. It does not cover the full range of emerging alternatives that currently receive no federal support.

So I want to ask Dr. Hsu and Dr. Prager, does the Office of Science's current fusion research program have the flexibility to shift resources to promising new approaches if they don't align with the conventional tokamak research pathway? And if not, what can we do to provide the office with the flexibility?

Dr. Hsu. Thank you for the question. I do not believe the flexibility current exists--currently exists for alternative concepts. At present, innovative concept development has no budget, nor new proposal solicitations from DOE, and I believe this omission should be addressed.

Mr. Lipinski. Dr. Prager, do you have----

Dr. Prager. I agree with Dr. Hsu. The budget--the fusion budget is very constrained financially, so there's been a decision made not to have a defined program to develop and consider fusion concepts that are different than what we call the tokamak and stellarator. And I do agree there should be a program and an opportunity within DOE, and these concepts, as Dr. Hsu said, should be subject to metrics, strict metrics moving forward. But I think as a--I would say as a matter of policy, the fusion program should be able to consider and, where meritorious, fund a variety of approaches to fusion.

Mr. Lipinski. All right. Thank you. I have to run off to a markup, so I yield back. Thanks.

Chairman Weber. The gentleman yields back.

And Mr. Rohrabacher from California, you are recognized for 5 minutes.

Mr. Rohrabacher. Thank you very much, Mr. Chairman.

I'd just like to get some numbers straight here. So over the last ten years we have spent $900 million on this project, is that right?

Dr. Bigot. Globally, globally, yes, with the seven members, yes. It is----

Mr. Rohrabacher. No, 700--how much has the United States spent on it?

Dr. Bigot. Okay. Right now, I do believe it's below two billion, but it is more to the U.S. ITER project office to speak about because myself, as the IO, have not the precise number because a different domestic agency has to provide in-kind, and I've not precise knowledge----

Mr. Rohrabacher. Well, how much money--we have spent how much money over the last ten years, the United States?

Dr. Prager. So I think it is--I don't have the exact number, but it is a good fraction of one billion dollars.

Mr. Rohrabacher. So about----

Dr. Prager. It's been typically funded in the range of $100 million a year----

Mr. Rohrabacher. Okay.

Dr. Prager. --you know, building up to where it is now----

Mr. Rohrabacher. So----

Dr. Prager. --so it's a fraction of one billion dollars.

Mr. Rohrabacher. So around about $900 million is----

Dr. Prager. In that range, yes.

Mr. Rohrabacher. Okay. And how much have our partners spent on this project?

Dr. Prager. Maybe Dr. Bigot can give the best estimate.

Dr. Bigot. Okay. It's quite difficult to give you a precise answer again as I explained to you because seven member has to bring, okay, their in-kind contribution, okay, each member, China, Russia, India, and so--and so the labor cost, for example, is not exactly comparable, okay. So again, I have no consolidation of the global cost which has already been spent. I can say clearly what has been spent, for example, in the ITER organization, where we are on the order of 250, 300 million, no more, okay, one billion per year on the last year, so this is below three billion, which has been spent already.

My expectation now, if we have some equivalency with what we call the European currency-because the European currency so far is used for measurement of the cost-altogether it will be spending including, commitments, on the order of twelve million--of twelve billion.

Mr. Rohrabacher. No, no, how much have they already spent is the question.

Dr. Bigot. Spent is no more than $7 billion according to my view.

Mr. Rohrabacher. They have already spent seven billion?

Dr. Bigot. Yes.

Mr. Rohrabacher. Okay. So we've spent $900 million, and they've spent seven billion on the project already, is that correct?

Dr. Bigot. Okay. I don't believe in the U.S. you have spent 9 billion, okay----

Mr. Rohrabacher. Nine hundred million.

Dr. Bigot. Oh, 900, okay, yes, okay. Sorry, I miss the point. Yes, I agree with you.

Mr. Rohrabacher. All right. So we've spent a grand total of perhaps--six billion on this project already has been spent, is that correct?

Dr. Bigot. Yes, it is of this order. As I explained to you, we've----

Mr. Rohrabacher. Six, seven billion dollars. All right. And we've spent nine billion. And we would expect to spend four-six billion more of our money in the next ten years, is that correct?

Dr. Bigot. Okay. As you know, we have made, okay, this best achievable schedule. We've come with some cost estimates, and the cost estimates, for the first plasma from the point of view of the ITER, okay, the central organization is on the order of four billion more.

Mr. Rohrabacher. We would be spending four billion?

Dr. Bigot. Yes. And so----

Mr. Rohrabacher. And how much----

Dr. Bigot. Because----

Mr. Rohrabacher. And how much would our--how much are our allies in this project expected to spend----

Dr. Bigot. Okay.

Mr. Rohrabacher. --more?

Dr. Bigot. Altogether it is an increase of four billion. And again, I don't speak about the in-kind which is, okay, the responsibility of the different ITER members. So----

Mr. Rohrabacher. Right.

Dr. Bigot. --altogether my expectation that the cost for this project ready for operation will be of the order of 18 billion of euro. I speak in euro.

Mr. Rohrabacher. Okay. The----

Dr. Bigot. Okay.

Mr. Rohrabacher. How much--so of the 18 billion, we will be spending four to six billion, and they will be spending the rest, is that right?

Dr. Bigot. Yes. Yes.

Mr. Rohrabacher. All right. That's what I'm looking for there. And this--we would--it's going to be ten years before we actually will be determining whether or not the project has been successful?

Dr. Bigot. Yes.

Mr. Rohrabacher. All right. And so the total price of what we're ending up talking about is what? I'm trying to add up the figures here. What, twenty billion?

Dr. Bigot. Yes, of this order, okay, I do believe you are--this is the right order.

Mr. Rohrabacher. Twenty billion dollars. And let me just note that--and what would you--you'd say the chances--after $20 billion, the chances of success and of reaching what theoretically is possible, what would you say the chances are of actual success in achieving that?

Dr. Bigot. According to me, the science is quite robust, taking advantage of all the work which has been done worldwide. The main challenge now is engineering and industrial----

Mr. Rohrabacher. Well----

Dr. Bigot. --and I do believe that, okay, more and more we are moving on. More and more we are confident that we will deliver.

Mr. Rohrabacher. The engineering--so it's possible, however, the engineering couldn't--I mean, for example, I understand that already there's been great progress made in the producing the advanced materials that--the actual material science has grown a long way, and you've achieved the goals--a lot--many of the goals that are necessary in the materials area. But that was possible that that may not have happened. I mean, we actually achieved a goal we didn't know we could achieve----

Dr. Bigot. Yes.

Mr. Rohrabacher. --and we achieved it. So you're going to have to lay odds on----

Dr. Bigot. Okay.

Mr. Rohrabacher. --all the engineering and all these things coming together. What are your odds?

Dr. Bigot. Okay. So the point is the following. As you know, the ITER project is a research project, and you're asked to demonstrate the, okay, capacity of materials, of good process, and all these things, and is why it will be a living project. In my expectation we have all the capacity of the scientists and engineers, okay, there is great chance that we will fulfill.

In any case, I do believe this project could be so beneficial to the world that it is really worth to try and to demonstrate.

Mr. Rohrabacher. Okay. Let me----

Dr. Bigot. And again, we spoke----

Mr. Rohrabacher. Let me mention this. There are a lot of wonderful things that we can do in this world.

Dr. Bigot. I know.

Mr. Rohrabacher. Wonderful things, and----

Dr. Bigot. Including ITER.

Mr. Rohrabacher. Okay. And ITER maybe one of them, but what we do is we judge each one based on the cost and the chances of success. And I'm sorry, I've been through a lot of these hearings, and I still think that the money that we put into trying to develop fusion--had we put $20 billion in this same effort into perfecting fission, we'd be a lot--it's a lot greater chance for improving mankind.

But as we move forward, I wish you success because we want those dollars not to be wasted.

Dr. Bigot. Okay. When--again, I want to point out that the United States now has the sharing of nine percent, okay, and with all the effort made by all the other partners, you have good chance to have 100 percent, okay, rewarding with all the knowledge and the, okay, knowledge we bring in.

So, again, as you know, I have been working on energy for years and years. I do believe that in the world we'll be facing real challenge when we will see that fossil fuel we rely on more than 80 percent now will be depleting. We know. It is obvious. I don't know if it is in ten years or is a century, but it will be, and if we have no alternative technology in order to produce massively energy, okay, complementary with the renewable energy, we will--the world will face real difficulty.

So again, I do believe it is worth to go as far as we can in order to make full demonstration. Fusion has worked for years in the sun and stars, as Dr. Prager says, so why very talented scientists and engineers will not be able to deliver? My trust is that they will do so, provided that they have good support.

Chairman Weber. We're going to go ahead and move on.

Mr. Rohrabacher. Thank you.

Chairman Weber. I think the gentleman is yielding back. So I thank the gentleman, and we're going to move to Mr. Foster of Illinois.

Mr. Foster. Well, thank you. And thank you, Mr. Chairman, for allowing me to sit on this committee hearing.

I guess my first question is, assuming that ITER succeeds and that sometime around 2025, 2030, would succeed at everything including DT--the DT program, what are the--going to be the remaining unsolved problems A) to be able to design a production which--you know, something that is an energy plant, you know, what's on the list of things that will be unsolved problems?

And secondly, what will be needed to understand what the levelized cost of electricity from a tokamak of those dimensions might be? You know, those are the two things that have to succeed to make fusion succeed as--succeed scientifically and engineering-wise, and it has to succeed economically. And so what will be the unsolved problems in 2025 or 2030, assuming everything goes nominally? I'm happy to have--you two can split it.

Dr. Bigot. May I start? Yes. Okay. I do believe that the main problem which will have--okay, there is two main problem from my point of view. Once--okay, the ITER will have in delivery, okay, full demonstration that we could have, okay, 500 megawatt coming out of the 50 megawatt we will put in.

It is materials, okay. When we will have continuous production of plasma energies, with some energy flux with neutrons which are as large as 20 megawatt per square meter, when we know, for example, when many----

Mr. Foster. That's the power density on the diverter or not----

Dr. Bigot. Yes, on the diverter.

Mr. Foster. Right. Okay. Right.

Dr. Bigot. Okay. So all we could manage is some material which could be able to sustain such a flux continuously.

And the second, we know if we want to take full advantage of the investment of industry or tokamak, we've--okay, the superconducting coils which could last for very long because there is no real use with, okay, superconducting coils because there is no energy dissipation, as you know. And so it will be the remote handling. How could we change some of the piece, for example, okay, tiles which will be facing the plasma or we could make all this remote handling properly done in such a way that, okay, we could take the best investment and have a long lifetime, okay, expectation for the delivery.

So in order to come to the point you mentioned about the economy: it is a big investment, but if the operational costs in the long lifetime of the equipment are very low, it will be quite economical process.

Mr. Foster. And is that--are there actually designed studies where you say just, okay, imagine that you're not making one of ITER but you're making worldwide 100 of them? You know, how cheap could you imagine making all the superconducting coils? How cheap could you imagine making all the different components? You know, you can be optimistic there, but if you find that the levelized cost of electricity doesn't look--you know, doesn't look attractive, then you have to actually step back and maybe reallocate between more adventurous but potentially cheaper ones and straight ahead with the current plan.

And so what's the current state of knowledge of what the economics might be, just assuming everything works technically here?

Dr. Bigot. Okay. Right now, there are several studies. As we know, ITER is the first of a kind, okay, and we have a lot of equipment around, the technology and so on. So the people mentioned to me very recently that when we will be moving to a real industrial facility, maybe the cost will be down compared to the cost of the ITER facility----

Mr. Foster. Oh, unquestionably. And if you tell me you are optimistic it will be a factor of the--the unit cost will drop by a factor of five, it's not unthinkable, but then you still have to do the cost of electricity calculation and see if you're happy with the result. And that's--I wonder if--those sort of studies must have been done for different versions of fusion machines at different levels of accuracy. What's the current understanding for whether the ITER design point has a shot? I mean, that's the question I'm trying to get at.

Dr. Bigot. Okay. From my point of view all the studies I have seen so far we expect that the cost of the electricity which will come is--from such a facility will be around, okay, what we call 100 euro--I speak in euro, okay, which will be 100, okay, dollars, okay, per megawatt, as you have now, for example, with some of the, okay, windmills or solar energy.

Mr. Foster. That's----

Dr. Bigot. So it would be comparable.

Mr. Foster. --13 cents a kilowatt hour, right?

Dr. Bigot. Yes.

Mr. Foster. Yes. Stu, do you have anything?

Dr. Prager. I agree with everything Dr. Bigot said. I think for challenges, let me list three. I think one in the plasma science we have to learn how to hold the plasma in steady-state persistently. ITER will teach us about that but ITER will burn for about 8 minutes or so, and we need to learn how to have a burning plasma that lasts for months on end. That is in part a plasma science challenge, and there's research underway to accomplish that, number one.

Number two, as Bigot said, there's a whole--the whole issue of materials research, both the plasma-facing component and the structural material that has to manage the neutron bombardment, and that's a set of challenges, and there are ideas how to meet those challenges.

And third, while ITER is operating, we are working on how to make the reactor concept even more attractive economically. So ITER will teach us all about burning plasma science and then maybe by the time we get that, we'll have evolved beyond simply duplicating ITER for a reactor. So we can take that burning plasma science, ideas that have been developed in parallel maybe have a more highly optimized reactor.

On cost of electricity, over the years there have been--the best engineering studies that could be done taking the cost of materials, the cost of assembly and calculating, you know, capital cost and cost of electricity, they always come out to be competitive with baseload power generation of today. However, projecting economics 30 years into the future is highly theoretical.

We have an interesting data point with ITER, and we do ask ourselves the question, does the cost to construct ITER, is it consistent with the engineering calculations of what a reactor will cost? ITER is not a reactor, first of a kind, and so on.

And at PPPL we had the beginnings of a study to try to quantify that, try to quantify how much extra cost is in ITER because it's an experiment, it's the first of a kind, internationally managed. And so we're in the process of trying to get financial, if you like, data from the ITER partners so we can quantitatively answer your question.

Mr. Foster. Well, thank you. And, you know, that's very important to our--the strategic decisions that we're going to have to make.

I guess at this point I yield back.

Chairman Weber. Thank--you know, Bill, Yogi Berra said the problem with predictions is you're dealing with the future.

So the gentleman recognizes the gentleman from Georgia. Barry, you're up.

Mr. Loudermilk. Thank you, Mr. Chairman.

Dr. Bigot, you mentioned in your testimony that not only is ITER building a first-of-its-kind reactor but the organizational structure is first of kind--first of its kind. If you could go back and restructure the organization, would you do anything different, and if so, what would it be?

Dr. Bigot. Yes, you're right. It's quite a challenge to have these 35 different nation with different culture, different, okay, ways to proceed working altogether. But I do believe it's a precondition for the ITER to move forward because, as I said, it's a large investment, okay, large industrial capacity. If we have not all these partners around the table, it will be difficult.

If I would start from scratch with return of experience we have, I do believe that it would have been much better if what we propose in the action plan was accepted from the very beginning, which mean the DG--the Director General has full, okay, power to take any technical decision which is needed for the project even though the partners are making in-kind contribution, which is good because it allows the industry to develop, okay, to, okay, foster innovation in many fields.

I do believe the key point is the decision-making process. In the beginning it was not clear enough that it is an industrial project, and we have to empower the Director General with all, okay, the support and agreement of the ITER council members that he has capacity to decide. And I'm very pleased that I was able to convince the seven ITER members, when I elaborated and developed this action plan that they understood that, and they really support me during the past 12 month on this matter.

Mr. Loudermilk. Okay. Dr. Prager, did you want to--I didn't know if you were--you had something to add.

Dr. Prager. Well, I don't know. I mean, Bigot--Dr. Bigot is the expert on that. I think the international arrangement has been well recognized to have provided--be problematic, and I think Dr. Bigot is having a remarkable effect on fixing that.

Mr. Loudermilk. Okay.

Dr. Prager. So I think the whole fusion community is very delighted with the progress over the last year.

Mr. Loudermilk. Okay. Well, Dr. Bigot, are you considering requesting any changes to the organizational structure going forward?

Dr. Bigot. Oh, no. I do believe that we have now, okay, tried it to change the culture not from just the top managers--

Mr. Loudermilk. Right.

Dr. Bigot. --but down to all the staff in order that we work in what I call an integrated way. Everybody has to feel that they are the owner of this global project, and fully accountable for its progress. This is why it is so important to have a schedul and to stick to the schedule-with many clear milestone in such a way that everybody feels fully committed to deliver.

Mr. Loudermilk. Okay. Well, thank you.

Dr. Prager, fusion energy has often been described as being 50 years away. What do we know now that we didn't know ten to fifteen years ago that will give us the confidence that we are making some progress?

Dr. Prager. Yes, I think the joke is 30 years away.

Mr. Loudermilk. Okay.

Dr. Prager. There's been a lot of progress over the last 15 years in various ways. One, scientifically, a big challenge is how do you control 100 million degree plasma, keep the heat in effectively, keep it from what we say going unstable and kind of blowing out like a tire blowing out. And in the last 15 years we've controlled it in ways that I couldn't imagine when I started to work in this field.

There's aspects of the plasma that, when I started to work, we just had to accept that it existed like bad weather, particularly turbulence in the plasma. Now, we, through--partly through experiment and through competition and theory we have ways that we can actually control the turbulence in the plasma.

And therefore, this gives us greater confidence that ITER will succeed and that we can design a successful fusion reactor.

You've heard a lot about the problem of surrounding 100 million degree plasma by a hot material. Well, in experiments over the last 15 years there's been ways to magnetically channel the heat out and spread it out over surfaces to alleviate that problem. Computation has been spoken about a lot, that we have much better predictive capabilities.

Looking a little bit into the future, there are new breakthroughs in technology outside of fusion that could have a big impact such as magnets they can make very strong magnetic fields. So there's been very good steady progress that's not solved anything by any means but bolstered our confidence that will move well in the future.

Mr. Loudermilk. Well, thank you. I'm out of time so, Mr. Chair, I yield back.

Chairman Weber. I thank the gentleman.

And the gentlelady from California is recognized.

Ms. Lofgren. Well, thank you very much. This is really important hearing, I think, and I'm hoping it's not the last hearing that we have on this subject.

You know, I remember when I first started working on fusion issues that people who were looking at magnetic versus IFE, it was like a religion. And I think we've actually moved past that now where people are seeing it's a--you know, we need to have a broad examination of the entire field, and I'm certainly in that spot. So I hope that my questions about the NIF will not be misconstrued as being only on the IFE pursuit.

But, as you know, Dr. Hsu, we've talked before about the National Ignition Facility, which obviously is a critical facility for this national Stockpile Stewardship Program, but it's also an important element of our science community. The National Academy report in 2013 outlined some efforts that might accelerate progress, including additional investments, better coordination--you've read the report. I won't recite everything.

I'm not--I keep mentioning this, and when the Department of Energy folks come, they cite things that the report didn't say, and I'm working with Dr. Moniz to have clarity on that.

But given the recommendations that they made, the National Academy made in terms of pursuing expanding NIF to include the direct drive and alternative modes of ignition, crafting and coordinating the joint plan for IFE research, Scientific Advisory Committee, and the like, can you comment whether that would actually improve the situation at--with IFE at the NIF in particular? Would it enhance the billions of dollars investment we've already made?

Dr. Hsu. Yes. I agree with those findings and the original rationale for standing up the HEDLP program. NIF is indeed meant--its primary mission is indeed stockpile stewardship, but as you say, it's an impressive and world-class facility that we've invested in. I believe there are opportunities on it. The three lab directors--Los Alamos, Livermore, Sandia--have stated that fusion is a critical need for stockpile stewardship and that the United States must be the first to achieve laboratory fusion.

I believe that over its lifetime NIF should explore, if the physics warrant, all the laser-based approaches. That includes direct laser drive, indirect x-ray drive, as well as magnetized approaches.

Ms. Lofgren. Right. Well, I'm just--you know, I have some level of frustration that obviously the stockpile stewardship mission was the primary mission. But the--and we have increased the number of shots dramatically, as I'm sure you're aware. But the facility itself is an underutilized resource, and that's not to take away from what we're doing with ITER in other areas. I mean, I--and when you think about what we spent on imported oil alone in 2013, an estimated $388 billion for that year on only imported oil, you know, investments in fusion science research to me is a bargain.

Now, we can't--you know, I think we made a huge mistake by setting a deadline on which we'd get ignition. How do you ever do science? That is ridiculous. I don't know who thought that up but it wasn't me. But, you know, I'm not so worried about the development. If we--once we get ignition--when we opened the National Ignition Facility, I had the chance to speak at the opening, along with many others, and I remember saying, once we get ignition, all the rest is just engineering. And, you know, people laughed but I actually have a high degree of confidence that things will take off once we clear that science.

And so really I think our effort ought to be on supporting the scientists to achieve that either, you know, we ought to ramp up at the NIF but also support the other efforts so we can achieve that incredibly important scientific milestone and then see where we go from there. And it's not just an energy source, but when you take a look at where we are and where we're going to be shortly in a shortage of water, how do you do desal without, you know, a limitless source of energy? I mean, we are going to need this as a source of energy in the near future.

So I'm about out of time but I just--playing cleanup, I just want to thank the three of you for your incredibly important work, and I hope--you know, Dr. Foster is the only physicist in the House. I am so glad that he is here. I hope that you will look at our committee as a source of support and that you will be in touch with us frequently, whether in formal hearings or informally because I think there is bipartisan interest in what you are doing.

And I yield back, Mr. Chairman.

Chairman Weber. I thank the gentlelady.

And I think the gentleman from Florida has some more questions.

Mr. Grayson. Looking back historically, we had a working net-energy-producing fission reactor before we actually had the first fission weapon detonated a couple of years earlier actually. So now here we are. It's been 64 years since the first fusion weapon was detonated, and we still don't have a fusion reactor that produces net energy, nor are we apparently even close to it. What's the problem, gentlemen? Let's start with Dr. Bigot?

Dr. Bigot. The problem is to be able to have sustainable production, you know, as you speak about the weapon-okay, we are able to deliver a huge amount of fusion energy, but to make it in a sustainable, fully controlled way is much more challenging as you could expect.

So again, this technology is very challenging. Requiring many different advanced technologies in cryogenics, in electromagnetics, and so on. Quite recently, I visit China where they have been able to assess and clearly demonstrate that we have what we call the feeders, which are the cables which will provide electricity to the coils--to the superconducting coils. They succeed to demonstrate that we could have as much--as many as, okay, nearly 70,000 amperes flowing through that, okay. It's really challenging. We push the technology very, very advanced, and making all this work as a system is really challenging.

I don't, okay, believe that it was a minor achievement, with the weapon as you mentioned, 64 years ago. But again, is something really different to master these technologies over the long-term, to have a consistent continuous production of energy.

Mr. Grayson. Dr. Prager?

Dr. Prager. Why has it taken so long? The fundamental answer is that this is one of the most challenging scientific and engineering enterprises ever undertaken by humankind, period. It's really hard. But the difficulty is matched by how transformative it will be when we succeed.

It required the development of a new field of science, the field that--what we call plasma physics. So when the pioneers in this field started out in the late 1950s, early '60s, this field didn't hardly exist. In the last 50 years a new field of science has been produced and developed, which is an enormous accomplishment. This has shown up in progress in fusion. If you look at fusion quantitative figures of merit, it beats Moore's Law. By our key figure of merit, we've gone up a factor of 30,000 in the last 30 years or so. We have another factor of six to go for commercial fusion.

It's taken long because you can't prove fusion on a tabletop. You just can't do it. The science doesn't allow it. We need machines like ITER, the major facilities in the United States. It just takes time to build a major facility. So all this stretches it out, and it's all--the overarching message also is that it's all been underfunded over the years so we could have gone faster.

So for an array of very understandable reasons, it's taken a long time. But if you look at how far we've come, I think it gives good basis for why the fusion community and scientists that look at this problem are very confident that we will get there.

Mr. Grayson. Dr. Hsu?

Dr. Hsu. Yes, I think drawing on your weapons analogy, I mean, we--like Dr. Prager said, we have come a very long way. We're almost to the point of detonating that first weapon. And I myself am interested in further work of miniaturizing it. That's the analogy. But we've--I think the main point is that it's a hard problem. We've come a long way. We're almost there to demonstrating it and to put the extra plug in that there are other ways we should be looking at that have the potential of not needing such a huge facility, but we need to do that work to know the answer.

Mr. Grayson. Let's say if the President of the United States announced that by the year 2025 he wanted to have fusion facilities all around the country as reactors providing net energy, in other words, a sort of Manhattan Project for fusion. What would that project actually look like, Dr. Prager?

Dr. Prager. You would parallelize. You would take more risk and you would look--you would develop--you would solve problems in parallel. Right now, we're doing it all serially, which stretches everything out. We would begin a study to build--my opinion--for example--it's going to be hypothetical. We would design a facility which would be a pilot plant and demonstrate net electricity production.

There would be some risk associated with it. It would be a risk that it might not work or will work partially, but if you really want a Manhattan Project, that could be the centerpiece of the program. At the same time, you would have satellite facilities that would solve the materials problems. We know what facilities we need to build, and you would have a program to develop more attractive fusion concepts. You would parallelize and do many things in parallel if you wanted to have a Manhattan Project.

Chairman Weber. Let me--Dr. Prager, let me break in here. You said satellites----

Dr. Prager. Yes.

Chairman Weber. --to solve the material problems.

Dr. Prager. Yes.

Chairman Weber. Could that be done in existing labs?

Dr. Prager. No. So, for example, just to give one example, in order to really study, as we would like to, how materials behave when bombarded by neutrons that the fusion reactions produce, you need a facility that can generate the neutrons. We know what that facility is. We can design it and we can build it. In round numbers it will cost $1 billion. So we can do that in parallel with this pilot plant, as one example.

Chairman Weber. I yield back.

Mr. Grayson. Let's just continue. Who would like to go next? What would that project--that Manhattan Project or a pilot project, what would it look like?

Dr. Bigot. Okay. I do believe it is what was said, very highly coordinated project with all the piece in order to move forward. And again, if the President of the United States and the other ITER members decide to have, okay, first fusion producing by 2025, 2028, according to the best of all knowledge now we have after five--after ten years of the ITER project, I do believe it's feasible if we have a highly coordinated way.

And I agree: now we know what we have to do and we could accelerate. But again, I don't want to oversell. Okay. It takes time if we want to do it, okay, right, safely, okay. When you have so many piece to assemble, okay, and it is very requiring--again, I stress the point that you have to move large piece, which are the same size as the one you are moving in the shipyard and to put them with millimetric precision-you can not rush so rapidly.

So again, do it straight in order to have this demonstration facility but in parallel to have some more which could consolidate the reliability of the installation in the facilities.

Mr. Grayson. Dr. Hsu?

Dr. Hsu. I agree with all that. I want to add a couple things, though. One is I think for a true fusion crash program you'd want to consider what the integrated reactor is going to look like at the end. I mean, to build the capabilities and the scientific understandings, you can study those things on separate facilities, as was mentioned, but ultimately, a fusion power plant has to tie everything together, and you would want to consider that earlier in the process. So the integration is important.

And secondly, you want to consider the criteria for a practical power plant. Just because you can build it doesn't mean that everyone is going to use it. It has to be practical and usable and competitive. So thank you very much.

Mr. Grayson. I yield back.

Chairman Weber. Mr. Foster, I think you had some more?

Mr. Foster. Yes. I'd like to talk a little bit about the physics risk of different machines. I mean, we've just--in the case of NIF, you know, we saw a tremendous technical success, I mean, in terms of delivering the laser power to the objected succeeded--you know, I'm blown away by the--by, you know, the success of that from a technical point of view but unexpected--and despite having the access to the best supercomputers, the best codes, there was new physics uncovered because it was a big extrapolation from tested measured regimes of material.

And in the case of NIF they were very fortunate that there's a very good secondary mission to the National Ignition Facility, to the stockpile stewardship, all of the high energy density physics that is to be done there. And so it's a tremendous and ongoing successful facility.

In the case of ITER, you're building it to make fusion power. If there are unexpected physics of plasmas that are discovered that make the machine not work, that is a very different class of problem.

So my question was is our current state of understanding of the physics simulation of plasmas and the measurements made such that ITER is really going to be operated in an understood regime right now? Or are we extrapolating in ways that may have some physics danger in not achieving the goals?

Stew, do you want to give that a shot?

Dr. Prager. ITER is an experiment, and if we knew with 99 percent confidence that it would work as we hope, we wouldn't bother to build it; we would just move to the next step. So ITER is to teach us how to control burning plasmas.

What's the level of confidence that we will in fact succeed in getting a burning plasma 10 times more energy out than in and be able to control it? I think the confidence is high but it's not 99 percent or we wouldn't be doing the experiment.

So if you look at--you can step through the different physics issues. You know, will we be able to confine the energy? Well, that, we think so. There you can extrapolate pretty well from current experiments. Will the alpha particles that are generated in the fusion reaction cause instabilities that wreck the plasma? Well, we have good computation and we have simulated experiments in current facilities that lead us to think that it'll probably be okay. And on and on. But the challenge of a burning plasma is all these phenomenon interact at one time. It's a highly complex, coupled system, and when you start to burn, it changes.

So I think the summary statement is the fusion community has pretty good confidence that this will succeed for fusion power, but it is an experiment. That's why--if we--it's--every experiment is some reasonable extrapolation from the precursor.

Mr. Foster. Okay. Yes. But when there were difficulties encountered in the ignition campaign at NIF, there is no shortage of theorists to come out of the woodwork and say----

Dr. Prager. Yes.

Mr. Foster. --well, we told you in the initial design studies you needed 10 megajoules on target to make this----

Dr. Prager. Yes.

Mr. Foster. --certain to work, and we told you so. Are there a similar group of people standing in the background saying, look, there's a good chance that ITER is going to run into physics problems or really is there a much better consensus? Is that----

Dr. Prager. Both. I think there is a consensus that we well likely have the physics knowhow to succeed in ITER. At the same time, physicists are by nature--we're supposed to be skeptics so we are--every day we're pointing out problems that, you know, can kill ITER but won't really kill ITER. So they both go on all the time.

I think the extrapolation from inertial fusion facilities before NIF to NIF is greater than the extrapolation from existing fusion facilities to ITER. And so I think we have a pretty good shot that if we permit Dr. Bigot to complete the experiment that it will ultimately be successful.

Mr. Foster. Okay. And can the same be said when you're looking at stellarator designs, other magnetic geometries and so on, or are there a different class of uncertainties there?

Dr. Prager. Similar kinds of uncertainties, and when we speak about next-step stellarators, we're not at the present time thinking of a burning plasma but we're testing somewhat different magnetic configuration that's been tested before. So it's always an extrapolation, and whenever we build the new experiments, it's always a judgment call of how far you go, as you are saying, how far you extrapolate so that you'll do something exciting without going over the cliff.

And so I would say in the last 25 years or so in the United

States in magnetic fusion we've erred on the side of being too conservative.

Mr. Foster. And will a lot of the uncertainties be resolved with the data from the German machine in terms of stellarator or are there--or is that really not a ``modern design'' so you won't have that data?

Dr. Prager. It's a very--yes, so Germany has just this last few months started a new experiment. It's a fantastic, modernized, optimized stellarator design. It will be enormously informative. But in addition, the stellarator design has enormous design space. So, for example, the German one, as fantastic----

Chairman Weber. Dr. Prager, let me break in real quick.

Dr. Prager. Yes.

Chairman Weber. Didn't you say Germany and China was a----

Dr. Prager. Germany and Japan.

Chairman Weber. Japan, thank you.

Dr. Prager. So Japan has--there are two sort of billion-dollar-class facilities. The one in Japan has been operating for quite a while with extremely valuable information. The German one is an extremely highly optimized, modern facility, and it's fantastic. Well, one small but--though it extrapolates to a very large size reactor, probably bigger than ITER.

So, for example, there are ideas that we have in the United States to take all the advantages of the stellarator at have it be more compact, and that's what we'd like--one example of what we might want to do in the United States.

Mr. Foster. All right. Let's see. If I could have just a couple more minutes here?

Chairman Weber. Yes, sir, you bet.

Mr. Foster. And I'd like to sort of return to the painful, you know, project parts of the question here. You know, the United States, you know, a few years back signed up for nine percent of what was then--please correct me if I'm wrong--you know, roughly a $12 billion U.S. project. Is that roughly the understanding what the initial time that we signed up for ITER? And now it is--we are now carrying nine percent of something that is several times larger.

You know, that has caused a lot of pain in the Department of Energy Office of Science budget, and so that's one of the reasons why, you know, we're--you know, we're seeing, you know, what the Senate has done in the last few--has proposed in the last few cycles.

And so I was wondering, you know, what--you know, what--let's say that the Senate wins, you know, every--for the last few budget cycles the House has been restoring money that the Senate cut, you know, for ITER. And so I imagine in those circumstances you must have at least been starting to do contingency planning to find--to understand if that is a fatal blow for the ITER project if this time through the Senate wins. Is that--what can you say about that? Is that unquestionably a fatal blow or do you think that if you lose nine percent of the funding to the project it will still--you know, that you'll still find ways to work around it?

Dr. Bigot. Okay. Again I will stress the point. For sure money is important, but industrial and scientific capacity for me are even more important. And if the United States, okay, which are now the most powerful, as I said, in science and, okay, industry, will pull out from the ITER, it will be a real drawback for the project. It's not so easy to recover from expertise which has been developed in this country in the condition which was explained, okay, just in a few minutes.

So for me, again, I will really stress out that it is very important that all the ITER members and even, as it was said, some new one come in in such a way we get the best of the knowledge because we need absolutely frontiers, okay, expertise in many, many fields, and it was not easy to afford.

I just want to point a fact. When we start with the ITER, okay, the superconducting material, the superconducting material we need, it was 15 tons produced per year worldwide. In many different, okay, facility, we have no standard quality. We need this specific material 650 tons in order to be able to make, okay, ITER working. And so we have been coordinating the work, and if, okay, some partners was missing, we will fail. It takes six years to develop all this because now we have a 115-tons-per-year capacity. So, again, this project is so large due to the physics.

According to my point of view, you could not expect to deliver, okay, massive fusion, okay, power if you have not the proper size to do that. I could explain to you in more detail, you know----

Mr. Foster. I'm--I guess I am--I don't want to over-claim, but I think I'm probably the only Member of Congress that's designed and built a 100,000-ampere superconducting power transmission line, so I understand----

Dr. Bigot. So you know that. You know that.

Mr. Grayson. I haven't.

Mr. Foster. I understand--oh, I'm sorry, Ranking Member Grayson. My apologies. The--but this is--you know, I have massive respect for what you've accomplished on this superconductor front, you know, to get industrially produced superconductor on the scale needed.

On the other hand, when the United States signed up for the project, you know, the representation was made that this project was ready to go to an extent that in retrospect probably wasn't the case. And so this is, you know, one of the things that we have to understand is, you know, given this history of cost growth is this really it? Do we have a schedule and a budget that we can really plan around and understand? And that's--you know, that's one of the tough questions that we have to struggle with here.

Dr. Bigot. Okay. I want to make you fully aware that when I come in with my own, okay, professional experience, when I dedicate myself to something, I want to deliver. It's why I have been working very, very straight in order to have a best evaluation of the cost of the schedule we propose, and I'm very pleased to say that as an independent review panel with 14 best world expert has been going through all our schedule and, okay, cost estimate and I show on my slide they say it is complete, it is available, and we believe to do that.

And now I want all the, okay, IO staff, domestic agency, ITER organization staff, domestic agency staff, and suppliers to feel fully committed to deliver within budget and within, okay, schedule.

Mr. Foster. You know, your predecessors also, I'm sure, were equally committed to understanding the project cost, I would hope. Anyway, I don't want to get too much into history, but, you know, we have to be conscious of things.

And another possible risk is that the United States will fulfill its bargains, and another country that you crucially depend on will decide it does not have the resources to commit. And how do we--how should we evaluate that risk as well?

Chairman Weber. Does the gentleman intend to wrap up here in about a minute or so?

Mr. Foster. That's fine. I'm happy if that's my last question. If I can get that answer, though, in.

Dr. Bigot. So clearly, there is a large interest of fusion in the world. I expect that the United States will stay in. If not, for me the project is so important that we will have to go on and on, okay. But again, I am not really envisaging such hypothesis because I do believe if we are clear enough in what are the benefit for the United States to stay in, they will feel that it is worth to move on.

Mr. Foster. All right. And I really thank you. And I want to be sure I don't be seen as coming off not supportive of this project. I just want to understand the dimensions of the cliff that we're playing near when we talk about the United States pulling out.

Thank you. I yield back.

Chairman Weber. I thank the gentleman.

I thank the witnesses for their valuable testimony and the members for their questions. The record will remain open for two weeks for additional comments and written questions from the Members. The hearing is adjourned.

[Whereupon, at 12:02 p.m., the Subcommittee was adjourned.]

Appendix I

Additional Material for the Record

[GRAPHIC(S) NOT AVAILABLE IN TIFF FORMAT]

[all]