THE NEXT GENERATION OF FUSION ENERGY RESEARCH

HEARING

BEFORE THE

SUBCOMMITTEE ON ENERGY AND ENVIRONMENT

COMMITTEE ON SCIENCE AND TECHNOLOGY

HOUSE OF REPRESENTATIVES

ONE HUNDRED ELEVENTH CONGRESS

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THURSDAY, OCTOBER 29, 2009

HOUSE OF REPRESENTATIVES,
SUBCOMMITTEE ON ENERGY AND ENVIRONMENT,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, DC.

The Subcommittee met, pursuant to call, at 10:03 a.m., in Room 2318 of the Rayburn House Office Building, Hon. Brian Baird [Chairman of the Subcommittee] presiding.

BART GORDON, TENNESSEE
CHAIRMAN

RALPH M. HALL, TEXAS
RANKING MEMBER

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Committee on Science and Technology
Subcommittee on Energy and Environment

Hearing on

The Next Generation of Fusion Energy Research

Thursday, October 29, 2009
10:00 a.m. – 12:00 p.m.
2318 Rayburn House Office Building

Witness List

Dr. Edmund Synakowski
Director
Office of Fusion Energy Sciences
U.S. Department of Energy

Dr. Stewart Prager
Director
Princeton Plasma Physics Laboratory

Dr. Thom Mason
Director
Oak Ridge National Laboratory

Dr. Riccardo Betti
Assistant Director for Academic Affairs
Laboratory for Laser Energetics
University of Rochester

Dr. Raymond J. Fonck
Professor of Engineering Physics
University of Wisconsin

HEARING CHARTER

SUBCOMMITTEE ON ENERGY AND ENVIRONMENT COMMITTEE ON SCIENCE AND TECHNOLOGY U.S. HOUSE OF REPRESENTATIVES

The Next Generation of Fusion Energy Research

THURSDAY, OCTOBER 29, 2009
10:00 A.M.–12:00 P.M.
2318 RAYBURN HOUSE OFFICE BUILDING

Purpose

On Thursday, October 29, 2009 the House Committee on Science and Technology, Subcommittee on Energy and Environment will hold a hearing entitled *"The Next Generation of Fusion Energy Research."*

The Subcommittee will receive testimony on research activities conducted by the Department of Energy (DOE) Office of Science's Fusion Energy Sciences (FES) program, as well as its collaborations with DOE's National Nuclear Security Administration (NNSA). In addition, the Subcommittee will examine the status of international partnerships in fusion energy research.

Witnesses

- **Dr. Edmund Synakowski** is Director of FES. Dr. Synakowski will testify on DOE's current fusion research activities and his vision for how the program should evolve over the next ten years.
- **Dr. Stewart Prager** is Director of the Princeton Plasma Physics Laboratory (PPPL) in Princeton, NJ and former Chair of DOE's Fusion Energy Sciences Advisory Committee (FESAC). Dr. Prager will testify on PPPL's current and future roles as a leading center of fusion energy research.
- **Dr. Thom Mason** is Director of Oak Ridge National Laboratory (ORNL) in Oak Ridge, TN. Dr. Mason will describe the current status of the ITER international fusion project and the role of ORNL as the headquarters of the U.S. ITER Project Office.
- **Dr. Riccardo Betti** is an Assistant Director of the University of Rochester's Laboratory for Laser Energetics in Rochester, NY and former Chair of the National Academies Plasma Science Committee. He was also Chair of a 2009 DOE report on *"Advancing the Science of High Energy Density Laboratory Plasmas."* Dr. Betti will testify on the status of inertial fusion energy (IFE) research and his vision for how DOE should steward IFE over the next ten years.
- **Dr. Raymond Fonck** is a Professor of Engineering Physics at the University of Wisconsin–Madison and former Director of FES. He was also Chair of the 2004 National Academies report *"Burning Plasma: Bringing a Star to Earth."* Dr. Fonck will testify on his experience as FES Director and his vision for a viable U.S. fusion program over the next several decades.

Background

Fusion is the process that powers the sun and the stars, and U.S. scientists have investigated ways to replicate this process here on Earth for over 50 years. Research into fusion for military purposes began in the early 1940s as part of the Manhattan Project, but was not successful until 1952. Research on creating controlled fusion devices to meet growing demands for new energy sources began in the 1950s, and continues to this day. In one type of this reaction, two atoms of hydrogen combine together, or fuse, to form an atom of helium. In the process some of the mass of the hydrogen is converted into energy, following Einstein's formula: E (Energy) = m (mass) times c (the speed of light) squared. The easiest fusion reaction to artificially recreate combines deuterium (a "heavy" form of hydrogen as it includes both a proton and a neutron[1]) with tritium (made up of a proton and two neutrons—the heaviest form of hydrogen found in nature) to make helium and a neutron. Deuterium is plentifully available in ordinary water, and tritium can be produced by combining a fusion neutron with the relatively abundant lithium atom. Thus, if its significant remaining scientific questions and engineering challenges can be overcome, fusion may have the potential to be a practically inexhaustible source of energy.

All nuclei in atoms are positively charged, so they have a natural electromagnetic repulsion pushing them apart. This is because, while opposite charges attract, like charges repel. Thus to induce the fusion process, hydrogen gas is typically heated to very high temperatures (100 million degrees or more) to give the atoms sufficient energy to overcome this repulsion and fuse. In the process the gas becomes *ionized,* meaning that atomic nuclei and their electrons have too much energy to stay bound to each other as neutrally charged atoms. Thus what is known as a plasma is formed. Plasmas are considered the fourth state of matter, after solids, liquids, and gases. Plasmas are unique from normal gases because large portions of them are either unbound electrons or charged nuclei (*ions*), so they can be manipulated by electric and magnetic fields. If a very hot plasma is held together (i.e., confined) long enough, then the sheer number of fusion reactions may produce more energy than what's required to heat the gas, generating excess energy that can be used for other applications. The sun and stars do this with gravity. Artificial approaches on Earth include magnetic confinement, in which a strong magnetic field holds the plasma together while its ions and electrons are heated by microwaves or other energy sources, and inertial confinement, where a tiny pellet of frozen hydrogen is compressed and heated by intense pressure so quickly that fusion occurs before the deuterium and tritium atoms can fly apart from each other. This level of pressure may be attained by utilizing a powerful laser or a heavy ion beam.

If successful, fusion devices for energy production are expected to be relatively environmentally friendly, producing no combustion products or greenhouse gases. While fusion is a nuclear process, the products of a fusion reaction are not intrinsically radioactive and cannot themselves be weaponized. Relatively short-lived radioactive material (~100 years, compared to thousands of years for some nuclear fission products) would result from interactions of the fusion products with the reactor wall. A long-term, large-scale geologic repository for waste from fusion would be unnecessary. Fusion also is not dependent on chain reactions that must be constantly monitored and regulated, so there should be no danger of a runaway process leading to a reactor meltdown.

The above are the major reasons why most industrialized nations pursue fusion research today. However, several significant questions in this field remain, including:

Can we adequately control a fusion plasma—meaning a plasma that receives a significant portion of its heat from its own fusion reactions?

Given the intense heat and neutron flux expected inside a reactor, what material(s) should be used in the first wall facing a fusion plasma?

Even if all fundamental technical challenges are overcome, how economical can a fusion reactor be in comparison to other energy options?

And specifically with regard to inertial fusion:

Can we actually build a system that perfectly implodes and recovers energy from ~10 pellets of hydrogen per second—the currently estimated rate necessary to produce significant net energy?

[1] See charter for hearing entitled *Investigating the Nature of Matter, Energy, Space, and Time* held on October 1st, 2009 for further explanation of "protons" and "neutrons," which are the primary constituents of an atom's nucleus.

DOE Office of Science—Fusion Energy Sciences (FES)

	(dollars in thousands)			
	FY 2008 Current Appropriation	FY 2009 Original Appropriation	FY 2009 Additional Appropriation[a]	FY 2010 Request
Fusion Energy Sciences				
Science	155,032	172,387	+56,511	176,067
Facility Operations	116,968	207,253	+34,512	221,742
Enabling R&D	22,933	22,910	—	23,191
Total, Fusion Energy Sciences	294,933[bc]	402,550	+91,023	421,000

Table 1: Budget table for the DOE Office of Science's Fusion Energy Sciences (FES) program. FY 2008 and FY 2009 are appropriated levels, and FY 2010 is the Administration's request level. The FY 2009 Additional Appropriation column represents the Department's plans for additional funding to be allocated from the American Recovery and Reinvestment Act of 2009, which includes $28.6M for upgrades to and enhanced operation of the three major U.S. magnetic fusion facilities, $31M for facilities that examine high energy density plasmas relevant to inertial fusion research, and $26.4M for basic plasma science research, student fellowships, and early career awards. $426M was appropriated for FES in FY 2010. FES has yet to announce how the additional $5M above its request will be allocated.

FES is the lead program in the Federal Government that supports research in the science and engineering required to magnetically confine plasmas for the purposes of generating net fusion energy. It is also the lead program that stewards basic research in plasma science, which has applications in a broad range of areas from microchip processing to astrophysics. In addition, FES examines the science underlying what are called "high energy density laboratory plasmas," or HEDLP, which are relevant to current and proposed inertial fusion energy facilities. However, the Federal Government currently has no official steward of research in inertial fusion for the purposes of energy generation. This will be described in greater detail in the section on the National Ignition Facility below.

ITER

ITER (pronounced "eater") is a major international research project with the goal of demonstrating the scientific and technological feasibility of nuclear fusion energy. ITER was originally an acronym for International Thermonuclear Experimental Reactor, but that title was later dropped due to the potentially negative popular connotation of the word "thermonuclear." The project's leaders now note that *iter* also means "the way" in Latin. The project is being designed and built by the members of the ITER Organization: the European Union, India, Japan, China, Korea, Russia, and the United States, with additional partner nations currently under consideration. The ITER Organization was formally established on October 24th, 2007 following ratification of the ITER International Agreement by all current members. The device will be built at Cadarache in southeastern France with the European Union serving as the host party, and it is scheduled to begin preliminary operations in 2018.

By roughly 2025, ITER is expected to generate fusion power that is at least 10 times greater than the external power delivered to heat its plasma. The project is designed to be the top scientific tool for exploring and testing expectations of plasma behavior in what is called the *burning plasma regime,* wherein the fusion process itself provides the primary heat source to sustain its high temperatures. A clear and comprehensive understanding of this type of plasma is needed to confidently extrapolate its behavior and related control technologies beyond ITER to a reliable fusion power plant.

The United States will primarily contribute hardware components and personnel during ITER's construction phase, with nearly all of these components being manufactured in the U.S. and then shipped to Cadarache. Throughout this phase, the United States is an equal, non-host partner responsible for about nine percent of its total construction cost, though this cost may decrease if additional partners are added to the ITER Organization. DOE currently estimates the total U.S. cost in as-spent dollars to be between $1.45 and $2.2 billion, with an official baseline expected to be determined and announced over the next year. However, the total international cost for the project has not been determined because different partners use very different accounting practices for their contributions. For example, many do not include contingency, labor, and in some cases not even inflation in their announced estimates.

The U.S. ITER Project Office is hosted by Oak Ridge National Laboratory in partnership with Princeton Plasma Physics Laboratory and Savannah River National Laboratory. Oak Ridge was chosen by the Department of Energy in large part because its recently commissioned Spallation Neutron Source facility is considered to be a major success in billion-dollar level project planning and execution, and the lab is employing nearly the same management and acquisitions team for the U.S. ITER contribution.

In FY 2010, the U.S. plans to provide contributions valued at $135 million for the ITER project, which is included in the Facility Operations budget line in Table 1.

Science

FES's Science subprogram includes several activities, much of which involve research in the leading configuration for magnetic fusion devices—including ITER—called the *tokamak.* Tokamaks, first conceived of by Russian scientists in the 1950s, are devices that are essentially toroidally (i.e., doughnut) shaped at their core. External coils induce magnetic fields which wind around the inside of the toroid and confine the hot plasma within. The U.S. hosts three major magnetic fusion facilities, two of which are tokamaks and one is known as a "spherical torus," which is essentially a uniquely shaped tokamak that, at its core, appears to be a ball which a narrow hole down its middle. These facilities include:

- **DIII–D** (pronounced "D. 3. D.")—This tokamak operated by General Atomics in San Diego, CA is the largest magnetic fusion facility in the United States. It is also geometrically the closest to the ITER configuration. DIII–D has unique capabilities to shape its plasma and provide feedback control of errant magnetic fields that affect the stability of the plasma.
- **Alcator C–Mod** (pronounced "ALKator See Mahd")—This facility at the Massachusetts Institute of Technology is the only tokamak in the world operating at and above the ITER design magnetic field and plasma densities. It also produces the highest pressure tokamak plasma in the world, approaching pressures expected in ITER, allowing for materials testing relevant to both ITER and an eventual fusion power plant.
- **The National Spherical Torus Experiment—**NSTX is a unique magnetic fusion device that was constructed by the Princeton Plasma Physics Laboratory (PPPL) in collaboration with the Oak Ridge National Laboratory, Columbia University, and the University of Washington at Seattle. Its spherical torus configuration may have several advantages over conventional tokamaks, a major one being the potential ability to confine a higher plasma pressure for a given magnetic field strength, which could enable the development of smaller, more economical fusion reactors.

In addition to direct research on these facilities, the Science subprogram also supports research in:

- Non-tokamak magnetic fusion concepts and experiments of various sizes and shapes at several universities and national laboratories
- High Energy Density Laboratory Plasmas (HEDLP), which are relevant to current and proposed inertial fusion facilities as well as the understanding of various astrophysical phenomena such as supernovae
- Theory and advanced simulation of fusion plasma behavior
- Basic plasma science

Facility Operations

The mission of the Facility Operations subprogram is to provide for the operation, maintenance, and enhancements of the three major fusion research facilities—DIII–D, Alcator C–Mod, and NSTX—to meet the needs of the scientific collaborators using the facilities. In addition, this subprogram is responsible for the execution of new projects and upgrades of major fusion facilities, such as installation of new diagnostics, in accordance with the Office of Science's project management standards and with minimum deviation from approved cost and schedule baselines. As noted above, Facility Operations also includes the U.S. contributions to the ITER project.

Enabling R&D

The Enabling R&D subprogram focuses on developing and continually improving the hardware, materials, and technology that are incorporated into existing fusion research facilities, thereby enabling these facilities to achieve higher levels of performance within their inherent capability. Enabling R&D efforts also develop near-term technology advancements enabling U.S. researchers, through international collaborations, to access plasma conditions not available in domestic facilities. In addition, this subprogram supports the development of new hardware, materials and technology that are incorporated into the design of next generation facilities to increase confidence that the predicted performance of these new facilities will be achieved.

National Ignition Facility and Inertial Fusion Energy Research

The National Ignition Facility (NIF), located at Lawrence Livermore National Laboratory in Livermore, CA, is the largest inertial fusion facility in the world. Its construction was certified complete on March 31, 2009, and the facility was officially dedicated on May 29, 2009 with experiments beginning in June. NIF's construction was supported entirely by DOE's National Nuclear Security Administration (NNSA), not FES. The total cost to build the facility was approximately $3.5 billion. Its primary mission is to produce data relevant to ensuring the reliability of the U.S.'s nuclear weapons stockpile through the study of controlled fusion events similar to the detonation of a thermonuclear warhead.

To do this, NIF's designers created the world's largest and highest-energy laser, which can be used to form 192 powerful laser beams. In 2010, NIF will begin experiments that will focus all of these beams on a BB-sized target filled with deuterium and tritium fuel. NIF's researchers believe that by 2012, they will be able to consistently implode these pellets, igniting the fusion process and creating the first man-made fusion system to produce more energy than it uses.

While this facility was not primarily designed for energy research applications, the achievement of net fusion energy production in NIF may become strong justification for a significant inertial fusion energy program. At this time, however, neither NNSA nor FES, nor DOE as a whole, has determined which (if either) subagency would take a leading role in developing such a program, nor determined how such a program would be stewarded in the future. Until FY09, a small inertial fusion energy research program had been funded solely through Congressional direction at NNSA. Recently, in the FES section of the Conference Report for the *Energy and Water Development Appropriations Act,* 2010, DOE was directed to review an inertial fusion energy research project at the Naval Research Laboratory and report on its findings within 60 days. The Conference Report also states: "The conferees encourage the Secretary to explore all possible opportunities to ensure that this program, which offers unique potential for long-term energy independence, is not abandoned for lack of a bureaucratic home."

Chairman BAIRD. This hearing will now come to order.

I want to wish everyone a good morning and welcome them to our hearing on the next generation of fusion energy research. Before we get started, we have Congressman Rush Holt of New Jersey with us in the Committee today. If there is no objection, I would ask unanimous consent that he join us on the dais. Hearing no objections, so ordered. Thank you for being here, wherever—where is Rush? Oh, hey Rush. Come on up. We will begin today—Rush, thank you for joining us. Your expertise will be much appreciated on this committee along with that of Dr. Ehlers.

Fusion energy has successfully powered the sun and the stars for billions of years, so it is no surprise that humankind has tried to recreate and harness this energy here on Earth. However, we all know that a working fusion reactor has been much more difficult to achieve than our Atomic Age scientists initially expected. Over the years, there were also some overly optimistic or even, in some cases, fraudulent proclamations by folks who skipped the peer review process and went straight to the media, which has further complicated the popular and political assessment of the extent to which the Federal Government should continue to support this research.

That said, however, according to recent reviews by the National Academies and the Department of Energy, there have been significant developments in the fields of advanced computing, engineering and plasma science over the last 20 years that have led to a far better understanding of how to create and control a fusion system. Within about three years time, the National Ignition Facility in California is expected to become the first fusion device in the world to produce more energy than it consumes, though only for at most a handful of brief experiments per day. In Cadarache, France, the large international fusion project called ITER is about to begin construction. This experiment is designed to produce five times more energy than it consumes for several consecutive hours—I think my children already do that, however, they are four and a half years old, and I swear they put more energy out than they consume—as well as 10 times more for at least 500 seconds. That is the expectation, at any rate.

If these new facilities are successful, they will represent a dramatic turning point in developing a viable commercial fusion reactor. Big questions still remain, however, such as how affordable fusion can be in comparison to other options that are known already to produce greater amounts of energy, and what the appropriate choices are for materials in a device which contains gases that can be hotter than the sun. But the U.S. fusion program needs to do all it can to ensure these successes and be ready to take advantage of them if and when they occur.

I look forward to learning more from this excellent panel of witnesses on how this program should evolve in light of recent developments.

[The prepared statement of Chairman Baird follows:]

PREPARED STATEMENT OF CHAIRMAN BRIAN BAIRD

Fusion energy has successfully powered the sun and the stars for billions of years, so it's no surprise that man would try to recreate and harness this energy source here on Earth. However, we all know that a working fusion reactor has been much more difficult to achieve than our atomic age scientists initially expected. Over the years, there were also some overly optimistic or even fraudulent proclamations by self-identified fusion researchers who skipped the peer review process and went straight to the media, further complicating the popular and political assessment of the extent to which the Federal Government should continue to support this research.

That said, according to recent reviews by the National Academies and the Department of Energy, there have been significant developments in the fields of advanced computing, engineering, and plasma science over the last twenty years that have led to a far better understanding of how to create and control a fusion system. Within about three years time, the National Ignition Facility in California is expected to become the first fusion device in the world to produce more energy than it consumes, though only for at most a handful of brief experiments per day. And in Cadarache, France, the large international fusion project called ITER is about to begin construction. This experiment is designed to produce five times more energy than it consumes for several consecutive hours, as well as 10 times more for at least 500 seconds.

If these new facilities are successful, they will represent a dramatic turning point in developing a viable, commercial fusion reactor. Big questions will still remain, such as how affordable fusion can be in comparison to other options, and what the appropriate choices are for materials in a device which contains gases that can be hotter than the sun. But the U.S. fusion program needs to do all it can to ensure these successes, and be ready to take advantage of them if and when they occur.

I look forward to learning more from this excellent panel of witnesses on how this program should evolve in light of recent developments, and with that I yield to our distinguished Ranking Member, Mr. Inglis.

Chairman BAIRD. We are waiting for Mr. Inglis but I would—how would you like to proceed, Vern? Do you want to make an opening comment or——

Mr. EHLERS. Thank you, Mr. Chairman. I am just sitting in briefly for the Ranking Republican on this committee, who will make a grand entrance shortly, I am sure.

But I really appreciate, Mr. Chairman, you holding this hearing. This is an issue that has really dominated long-range energy thinking for many years but has had very little public successes to back up the standing that they had hoped to achieve, and I hope, sincerely hope that we can learn a lot more about fusion and energy not only in this hearing but in the next five years and really be able to put it in its rightful place in the hierarchy of energy alternatives that we should be pursuing. It is clear to us that we have to take a different approach in our society in terms of the generation and use of energy. We know much of what we have to do to change our use of it. We even know a great deal about what we have to do to develop alternative methods of producing usable energy but we certainly don't know as much as we need to know about fusion energy and what role it can and should play in the future.

So I thank you for holding this hearing, and I will yield back.

Chairman BAIRD. Thank you, Dr. Ehlers.

If other Members wish to submit additional opening statements, those statements will be added to the record, and of course, when Mr. Inglis arrives we will accept his statement as well.

[The prepared statement of Mr. Costello follows:]

PREPARED STATEMENT OF REPRESENTATIVE JERRY F. COSTELLO

Good Morning. Thank you, Mr. Chairman, for holding today's hearing to examine the fusion energy research activities conducted by the Department of Energy's (DOE) Office of Science.

In order to develop a sustainable energy policy we must develop and demonstrate sources of energy that will reduce our dependence on foreign oil, improve our greenhouse gas emissions, and satisfy our energy needs. Fusion energy should play an integral role in providing a substantial amount of clean, domestic energy to our communities and industry without the risks of nuclear energy.

I am interested to hear from our witnesses today how this committee can work with DOE to ensure that we are using cutting-edge technology and providing appropriate levels of funding for fusion energy research. In particular, what timelines are in place to move current research efforts to the development and demonstration and eventually to large-scale commercialization. In addition, I would like to learn more about the international research collaborations on fusion energy and how this committee and the Federal Government can work with the international community on fusion research efforts while continuing to take the lead on these important efforts.

I welcome our panel of witnesses, and I look forward to their testimony. Thank you again, Mr. Chairman.

[The prepared statement of Ms. Johnson follows:]

PREPARED STATEMENT OF REPRESENTATIVE EDDIE BERNICE JOHNSON

Mr. Chairman I would like to thank you and the Ranking Member for holding this important hearing today on the future of fusion energy research.

I am please to welcome our witnesses, and look forward to their testimony.

Fusion energy is one of the most innovative and essential research projects occurring in this country and around the world.

As a safe, abundant and clean form of energy, the future of fusion is truly the future of energy independence in America.

Since the development of nuclear weapons in the 1940s, we've been working on research to harness that type of power into an energy source.

While fission has been successfully developed, fusion has proved more elusive.

There are currently several pivotal projects in fusion energy research.

One of those projects that I remain most interested and optimistic about is the international ITER (pronounced eater) research project.

The research they are doing in plasma behavior should prove essential in the generation of fusion power.

I am also interested in hearing about our domestic magnetic fusion research and facilities. They are playing a key role in many aspects of the future of fusion.

I look forward to hearing more about this project from our witnesses.

I am pleased that the Science Committee is holding this hearing today and believe we need to continue to take a proactive role in encouraging Congress and the Administration to invest more in energy research and development.

Witnesses, many of you represent the future of innovation in energy research. Once again, I welcome you and appreciate your contributions to today's hearing.

Thank you, Mr. Chairman. I yield back.

Chairman BAIRD. At this point it is my pleasure to introduce our extraordinarily distinguished panel of expert witnesses. Dr. Edmund Synakowski is the Director of the Office of Fusion Energy Science at the U.S. Department of Energy. Dr. Riccardo Betti is the Assistant Director of Academic Affairs for the Laboratory for Laser Energetics at the University of Rochester. Dr. Raymond Fonck is a Professor of Engineering Physics at the University of Wisconsin, and I will yield to our distinguished Chairman and then following that—well, I was our distinguished Chairman. I will yield to—oh, Bart, I didn't see you were here. It is our distinguished Chairman, Mr. Gordon. I didn't see you. Mr. Gordon is here, of course, to introduce Dr. Thom Mason, Director of Oak Ridge National Lab.

Chairman GORDON. Thank you, Dr. Baird, and, you know, as I have said before, you have been the workhorse, this has been the workhorse Subcommittee for our Full Committee, and I thank you for another very excellent hearing with an outstanding group of witnesses. Thank you all for coming.

I am pleased to have the opportunity to welcome to the Subcommittee today an adopted Tennessean and the Director of the Oak Ridge National Lab, Dr. Thom Mason. Dr. Mason earned his Ph.D. in experimental condensed matter physics, something that

Mr. Davis and I talk a lot about. After touring the world, he came to Oak Ridge in 1998, and in 2001 he was named Director of the lab's Spallation Neutron Source project, an impressive $1.4 billion project that was finished on time and under budget and at Oak Ridge now. Then later in 2007 he became Director of the lab at Oak Ridge. He is going to describe today his critical role in managing the U.S. contribution to the ITER reactor. They are in east Tennessee, I am in middle Tennessee, but Mr. Davis represents many of the folks that work there so I will yield to Mr. Davis to pander for a few minutes.

Mr. DAVIS. I am glad you said a few minutes. It takes me a long time to get words out. I speak a little bit slower than perhaps some folks.

Mr. Chairman, thank you for the work that you do on this committee and we in Tennessee are certainly lucky and very proud of the accomplishments that you have, and Bart and I represent an area that has in it the Cumberland Mountains, and our folks there say they are proud to say they have two Congressmen. I think they are prouder of Bart Gordon probably than of me since he is Chairman of this committee.

I would like to add to what comments you just made, and I would like to add that Oak Ridge is a pillar of the community in Tennessee, supporting world-leading research initiatives in energy, environment, national security and computing as well as providing good jobs and performing educational outreach to our students. We are lucky to have this critical scientific resource in our region with such an accomplished and dedicated scientist and leader as you are. Dr. Mason, I am very happy that you are here today to provide valuable insights on the future of fusion research, both at Oak Ridge and abroad. I look forward to your continued strong leadership at this laboratory in Oak Ridge. It has been great working with you, and as I travel to Oak Ridge to the lab, or whether its at the Spallation Neutron Source or the NNSA (National Nuclear Security Administration) at Y–12, I realize that this area of the world, this area of America, and this part of Tennessee, has been a valuable asset in scientific research and will continue under your leadership. Thank you for being here.

Chairman BAIRD. I thank our Oak Ridge boys for their introduction. I apologize to the Chairman. I hadn't seen you here, Mr. Chairman, so I was puzzled by what appeared to be a strange third-person self-reference here.

I would now be happy to recognize our guests at the Committee, Representative Rush Holt, to introduce our last witness, and Mr. Holt will be followed by Dr. Ehlers, who wishes to offer comments as well.

Mr. HOLT. I thank the Chair and I am pleased to be with you and my distinguished colleagues today, and also with the distinguished panel. I have been asked to introduce Dr. Prager, and I could equally well spend time introducing and praising Dr. Synakowski, Dr. Fonck, all three of whom have been constituents in the 12th Congressional District in New Jersey, but more importantly, all three of whom have been leaders, world leaders, in advancing plasma physics and fusion sciences. They all have contributed to what we now see, the promise of fusion with essentially unlimited, globally available ingredients, with great environmental attractiveness, with no harmful emissions or high-level radioactive waste or connection to proliferations of weapons materials—in other words, a technology well worth undertaking. They have—they will be well prepared to address today how realistic and practical this may be, I would say at least possibly and I would go as far as to say probably. The progress has been great by any measure exceeding predictions. Certainly if you look at achievements per dollar spent, in power contained, millions of watt and plasma sustained, improvements by factors of hundreds of thousands to what amounts to an eternity in plasma lifetimes, development of an entire new field of science, plasma physics, with theoretical and practical contributions, not just to materials and engineering and science but to daily lives. The progress, the promise, the justification of spending taxpayer dollars, significant taxpayer dollars, have been recognized by domestic and international advisory committees, in some cases on which our panelists have served or which they have chaired, and also recognized by the actions of many other countries. It is worth noting as we talk about the fusion energy program in the United States that the United States was once for decades the world leader. We could be again. We should be again for a lot of reasons. If we are, it will be in part because of the work of some of these panelists.

Dr. Prager has worked at a number of the places where this significant work has been done: General Atomics in San Diego, Columbia University in New York, University of Wisconsin for many years, and he has chaired the Fusion Energy Advisory Committee, and he has chaired the Division of Plasma Physics of the American Physical Society. You will notice I spoke earlier of creating an entirely new field of science growing out of the work of Lyman Spitzer at Princeton 40, 50 years ago now. Dr. Prager also served as President of the University Fusion Association, so he represented the large academic contributions of this field as well. He is a recipient of the Dawson Prize and the Leadership Award of Fusion Power Associates. So I am pleased to introduce Dr. Prager but also commend to you, all of the panelists, whom I know personally and cannot praise highly enough. Thank you.

Chairman BAIRD. Thank you, Dr. Holt.

Dr. Ehlers.

Mr. EHLERS. Thank you, Mr. Chairman, just a brief comment in view of the comments of the two gentlemen from Tennessee sitting on the dais here and especially to our chairman, Mr. Gordon. When he was making—they were making their comments about Oak Ridge, it suddenly occurred to me, it has been at least 30 years since I have been to Oak Ridge. It has been probably 20 since I have been to Argonne and 10 since I have been to Fermilab. Members of the Committee, these are the crown jewels of our research effort, and I think Members of the Committee should be visiting these laboratories more often. So my plea to Chairman Gordon is, perhaps we could start organizing CODELs where Members of the Committee can go visit the national labs on a rotating basis. I think it would be extremely beneficial for the Members of the Committee. Yield back.

Chairman BAIRD. I hear there are visa problems for some of us to get into Oak Ridge.

We have been joined by the Ranking Member, Mr. Inglis, and we will recognize him. First we will welcome him. I know he had a panel he was attending prior to this so I appreciate his presence, and thank you, Dr. Ehlers, for so ably filling the role in the absence of Mr. Inglis. Welcome.

Mr. INGLIS. Thank you, Mr. Chairman.

Well, I think that Dr. Ehlers filled in very well and I thank you, Dr. Ehlers, for filling in. I am looking forward to hearing the testimony because it is very important and very exciting. The key question is how to make it work. So looking forward to learning more from you.

So thank you, Mr. Chairman, for holding the hearing.

[The prepared statement of Mr. Inglis follows:]

PREPARED STATEMENT OF REPRESENTATIVE BOB INGLIS

Good morning and thank you for holding this hearing, Mr. Chairman.

It's probably fair to say that when it comes to fusion, we're talking about the Holy Grail of energy. For the past 50 years, fusion has given us hope as an abundant, clean, secure, and safe source of energy. We've been investing in that hope, learning more about fusion and gaining critical technical knowledge. We've also identified more questions that need answering to turn fusion into the energy solution we're looking for.

Today our witnesses will help us understand where we stand on the road to fusion power. The recent capital investments in the National Ignition Facility at Lawrence Livermore National Lab and the international ITER project have been substantial. We need to understand what these investments will deliver and if these types of investments are getting the most out of scarce federal dollars. We also need to identify where the unique intellectual capital and innovative power of the United States should be put to work to crack the code to fusion energy.

I'd like to think that if we play our cards right, the materials, devices, and technologies necessary to turn fusion into electricity can be developed right here at home. That's why I joined Rep. Lofgren in co-sponsoring the *Fusion Energy Science and Fusion Energy Planning Act of 2009.* This bill will strengthen our fusion engineering research program and prepare the U.S. to lead on key research areas.

I'm hopeful that we'll find the way to practical fusion energy, but I also realize that it must be proved. I hope our witnesses can help us balance the marvelous prospect of a fusion-powered economy tomorrow with the responsibility to bring reliable forms of power to the market.

Mr. Chairman, thank you again for holding this hearing. I look forward to hearing from the witnesses and I yield back the balance of my time.

Chairman BAIRD. One of the things I greatly respect about Mr. Inglis is, he recognizes when everybody has said everything and he doesn't have to say it again. That is a rare quality in Congress, and with that, we will proceed to our experts and I will begin with Dr. Synakowski. Thank you very much.

STATEMENT OF DR. EDMUND J. SYNAKOWSKI, ASSOCIATE DIRECTOR FOR FUSION ENERGY SCIENCES, OFFICE OF SCIENCE, U.S. DEPARTMENT OF ENERGY

Dr. SYNAKOWSKI. Thank you, Mr. Chairman, Ranking Member Inglis and Members of the Committee.

I have been Director of the Federal Fusion Energy Sciences Program since June 7 of this year, and I am thrilled to join this office when the scientific readiness, opportunity and urgency of fusion are extraordinarily resonant.

The pursuit of fusion energy embraces the challenge of bringing the power of a star to Earth. Fusion's promise is enormous—nearly limitless fuel supplies, large-scale energy production, no greenhouse gas emissions. We are entering a new age in fusion science during which our knowledge base will be put to the test as researchers will undertake a fundamental set of new studies of fusion energy's viability.

At the heart of fusion energy in the stars and on Earth is the world's most famous equation, $E = MC^2$, which describes the fundamental relationship between mass and energy. The challenge is getting atomic nuclei of the fuel to bind together to form heavier elements, releasing enormous quantities of energy in the process. In the lab we use hydrogen isotopes as the fuel, and I have had the privilege of being part of experiments that have generated millions of watts of fusion power.

The science underpinning much of fusion energy research is plasma physics. Plasmas are hot gases, the stuff of stars, and over 99 percent of the visible universe, lightning, flames. Plasmas are routinely confined by magnetic fields and heated in laboratories to fusion conditions. The tokamak, a Russian invention from the 1960s, is studied worldwide and is the leading candidate "magnetic bottle" for creating fusion energy.

Dramatic progress prompted the National Academy of Sciences in 2004 to urge the United States to take a landmark step: it should participate in a fusion experiment in which the plasma burns, or generates more energy than is used to heat it externally and in large part, heats itself. In response, the United States agreed to participate in the ITER project to be built in Cadarache, France. We view ITER as a scientific instrument with the flexibility to reveal critical requirements for fusion's optimization. The seven members of ITER are China, the European Union, India, Japan, Russia, South Korea and the United States. Construction will take place over the next decade with burning plasma experiments slated to take place in the 2020s. The United States is committed to bringing a strong and effective approach to project management in ITER's design and construction.

Another approach to fusion is to compress the fuel extremely rapidly and to rely on its inertia to confine it long enough for fusion to occur. This is being studied by the National Nuclear Security Administration (NNSA) for stockpile stewardship applications and a joint program to study this extraordinary state of matter is being forged between NNSA and my office that will engage a broad array of laboratories and universities. Tests of this approach are being planned for the National Ignition Facility. If successful, they will be historic. The National Academy of Sciences has emphasized the importance of studying this plasma state to both energy research and to a rich array of scientific questions.

ITER's success, its chances of success and our prospects for deep scientific return are intimately interwoven with a broad domestic research program in the fusion-related sciences. In the United States, our multi-institutional program in experiment, theory and computation is rich in discovery and impact. It is globally respected for its depth, accomplishment and scientific aesthetic and has had a major impact on the ITER design and research plan. Research is supported in 38 states at national labs, private industry and about 60 universities. U.S. researchers participate in about 75 joint international activities and about 340 graduate students partake in fusion energy and general plasma science research.

Strategic planning is underway aimed at filling gaps in the world so as to assert U.S. leadership where it best advances fusion as a whole while maximizing U.S. scientific return. For magnetic fusion, the scientific challenges can be broadly stated as follows. First, understanding and optimizing the burning plasma state. Experiments, theory and simulation have significantly advanced our understanding of what to expect from a burning plasma, and will continue to do so, but ITER provides the only platform planned to directly test and expand our understanding of this complex physics.

Second, understanding the requirements for extending the burning plasma state to long times—days, weeks, and longer. Many aspects of this are pursued in the United States, and the second 10 years of ITER's operation will put our understanding to crucial tests. However, overseas fusion programs are set to assert leadership in part through new billion-dollar class research facilities in Europe, Japan, South Korea and China. We are exploring growing our collaborations to increase their impact and the knowledge returned.

And finally, third, advancing the materials science for enduring the harsh fusion plasma environment, for extracting energy and for generating fusion fuel in situ. We will be exploring what is required to develop a materials and fusion nuclear science program, one that addresses the necessary fundamental scientific issues, while weaving the results and advances into our best concepts of future fusion systems.

Thank you, Mr. Chairman, for providing this opportunity to discuss the Fusion Energy Sciences Program. This concludes my testimony, and I would be pleased to answer any questions you may have.

[The prepared statement of Dr. Synakowski follows:]

PREPARED STATEMENT OF EDMUND J. SYNAKOWSKI

Thank you Mr. Chairman, Ranking Member Inglis, and Members of the Committee for the opportunity to appear before you to provide testimony on the Fusion Energy Sciences program in the Department of Energy's (DOE's) Office of Science (SC). I have been Director of the Office of Fusion Energy Science since June 7th of this year. It is a privilege to lead the Nation's fusion energy sciences program following a career of scientific research and service at two national laboratories and in research collaborations with national labs and universities. I am thrilled to have joined this Office when the scientific readiness, opportunity, and urgency in fusion are extraordinarily resonant. I am pleased to share with you my perspectives on the status and the strategy for advancing fusion as we enter a new and critical age in its research and development.

Introduction

The pursuit of fusion energy embraces the challenge of bringing the energy-producing power of a star to Earth for the benefit of humankind. The promise is enormous—an energy system whose fuel is obtained from seawater and from plentiful supplies of lithium in the Earth, whose resulting radioactivity is modest compared to fission, and which yields zero carbon emissions to the atmosphere. The pursuit is one of the most challenging programs of scientific research and development that has ever been undertaken. A devoted, expert, and innovative scientific and engineering workforce has been responsible for the impressive progress in harnessing fusion energy since the earliest fusion experiments over sixty years ago. As a result we are on the verge of a new age in fusion science during which researchers will undertake fundamental tests of fusion energy's viability. The scientific community's excitement and optimism about our progress and readiness to enter this new era of fusion research is amplified by the high awareness worldwide of the need to fundamentally alter our energy landscape in this century. Fusion can be part of that landscape shift. But it is no secret that fusion on Earth is difficult. Establishing a deep scientific understanding of the requirements for harnessing and optimizing this process on Earth is critical, and the progress has been dramatic.

The Scientific Challenges of Fusion Energy

The science underpinning much of fusion energy research is plasma physics. Plasmas—the fourth state of matter—are hot gases, hot enough that electrons have been knocked free of atomic nuclei, forming an ensemble of ions and electrons that can conduct electrical currents and can respond to electric and magnetic fields. The science of plasmas is elegant, far-reaching, and impactful. Comprising over 99 percent of the visible universe, plasmas are also pervasive. It is the state of matter of the sun's center, corona, and solar flares. Plasma dynamics are at the heart of the extraordinary formation of galactic jets and accretion of stellar material around black holes. On Earth it is the stuff of lightning and flames. Plasma physics describes the processes giving rise to the aurora that gently illuminates the far northern and southern nighttime skies. Practical applications of plasmas are found in various forms of lighting and semiconductor manufacturing, and of course plasma televisions.

At the heart of fusion energy in the stars and on Earth is the world's most famous equation, $E = mc^2$, which summarizes our understanding of how mass can be converted into energy. Inside the sun, plasma pressures are high enough that hydrogen nuclei frequently collide and fuse into new atomic nuclei. The end product of these new fused systems actually weighs less than the original nuclei; the "missing" mass is converted into the motion of the byproducts of the collisions, releasing prodigious quantities of energy. The energy released by fusion is largest per unit mass for the lightest elements. Thus, scientists also choose hydrogen isotopes to achieve fusion on Earth.

On Earth, fusion is in fact routinely created and controlled in our fusion research laboratories—for example, I've had the privilege of being part of and of leading experiments that have generated millions of watts of fusion power for seconds at a time. In our vision of a working reactor, some of the energy will be captured by the plasma itself, and the plasma will self-heat, enabling more fusion to take place. The energy of the fusion reaction byproducts—energetic ions and neutrons—escaping the plasma will be captured and converted into heat. This heat will drive conventional power plant equipment to boil water, generate steam, and turn turbines to put electric power on the grid.

The leading challenge for fusion is stable confinement and control of the hot plasma. When a plasma gets hot enough for fusion to occur, its strong tendency is to expand and cool like any gas. If allowed to do this too quickly, the conditions that enable fusion are lost. If this same hot plasma strikes a material wall before fusion can take place, it also cools and fusion ceases. Thus the hot plasma must be confined for a long enough time away from a material container. The leading approach to fusion energy being pursued in the world is to confine the hot fusion fuel with magnetic fields. The insulating properties of magnetic fields, properly configured, can be extraordinary. In present experimental devices, temperatures of plasmas are found to increase tens of millions of degrees centigrade in a matter of a few centimeters—from the room-temperature vessel containing the hot plasma into the plasma itself. Another approach is to compress the fuel rapidly so as to reach fusion conditions and rely on the inertia of the fuel itself to keep it combined long enough for fusion to happen. This approach is being studied by the National Nuclear Security Administration (NNSA), and a joint program researching this state of matter is being forged between NNSA and my office.

A second great challenge for fusion is materials that can tolerate the extreme conditions of a fusion reactor. A plasma at a high enough temperature and density to undergo nuclear fusion in a reactor, while generating close to a billion watts of fusion power, will present a uniquely hostile environment to the materials comprising the reactor. The extreme heat fluxes inflicted on a reactor vessel's walls—at rates of tens of millions of watts per square meter—present significant materials challenges. Furthermore, in a fusion reactor the materials that will be near the burning plasma will bathe in a harsh shower of neutrons that can displace its constituent atoms and thus alter its strength and other material qualities. Advances in material science will be required to achieve reactor components that can withstand exposure to the enormous heat and neutron fluxes emanating from prolonged fusion burns.

In the last two decades, progress in our understanding of plasma systems and their control requirements has enabled the fusion community to move to the edge of a new era, the age of self-sustaining "burning" plasmas. For both lines of research described above, magnetic and inertial fusion, new experimental plans are being developed to make historic first studies of fusion systems where the energy produced by the fusion process itself is substantially greater than the energy applied externally to heat and control the plasma. In this testimony, I describe the current frontiers for the fusion energy sciences and describe how the research programs of the Office of Science contribute to scientific advances in these areas. I will discuss our program's relationship to international partners and the anticipated benefits of continued U.S. leadership, including benefits to science and to the Nation. I will also describe activities in our own program in the U.S. for building the science that is enabling us to enter the burning plasma era. To begin, however, I would like to briefly describe the origins and scientific breadth of fusion research.

A Brief History of Fusion Energy Sciences Research in the U.S.

The advent of the nuclear age in the mid-20th century led scientists to consider whether the nuclear fusion process could be harnessed on Earth for energy production. In the United States, interest in the possibility of controlled fusion dates back even prior to the end of World War II. From 1944 to 1946, frequent and lively discussions of the subject were held among scientists assembled at the Los Alamos Scientific Laboratory, particularly E. Fermi, E. Teller, J.L. Tuck, S. Ulam, J. Wheeler, and R.R. Wilson. In the wake of the Manhattan Project, optimism for fusion energy ran high. Many scientists, flush with excitement and confidence from the rapid success of fission research, expected similarly expeditious progress towards controlled fusion. Most of the basic principles of fusion, if not already known, were formulated at that time, and a number of suggestions were made for achieving controlled thermonuclear fusion conditions. While many of these early suggestions were highly ingenious, all failed to meet the basic requirements of a controlled fusion device. From 1951 until 1958, fusion energy research continued under a classified program named "Project Sherwood." By the mid-1950's, about 200 personnel were involved in the U.S. in magnetic fusion research, designing and testing various approaches for "magnetic bottles" to confine the hot plasma.

By the mid-1950s, it was apparent that the underlying physics of the plasma state was proving to be far more complex and difficult to control than had been anticipated. The research in magnetic fusion was declassified in 1958, and at that time it was seen that the U.S., Soviet, and British-led fusion research programs were neck-and-neck—and far from achieving a usable energy source. Each program was only capable of producing plasmas that were, according to a standard measure, about ten thousand times lower than required for fusion to generate more heat than was required to create the fusing plasma in the first place. Throughout most of the 1960's, research in fusion progressed through small-scale laboratory experiments and research into fundamental plasma theory. It became clear that cracking the nut of the fusion energy challenge was going to take far more basic physics research than predicted at the program's outset.

Much of the research through the 1960's focused on an approach where the magnetic field for confining the plasma was completely defined by the hardware of the experiment. In 1968, however, a major breakthrough was announced by Soviet researchers. They introduced a clever innovation wherein some of the magnetic field for confining the plasma was created by an electrical current passed through the plasma itself. This led to a dramatic simplification in the magnetic coils needed externally. The announced results were stunning to researchers—plasma performance measured in terms of confinement quality were said to be improved by an order of magnitude. In fact, the results were so surprising that many in the West did not believe them. In an event extraordinary for the times but emblematic of how science is best carried out, the leader of the Soviet fusion effort opened the door to British scientists in 1969. They brought their own measurement equipment to the Soviet Union and confirmed the Soviet claims—the plasma quality was far superior to any that had been created in any other experiment to date. The results led to the conversion of U.S. research facilities to this new concept called a tokamak, a name based on a Russian acronym for "toroidal (donut-shaped) chamber with a magnetic coil."

These developments expanded our view of what was possible in fusion research. In the 1970's, progress was rapid, and budgets for fusion research in the U.S. increased as a result of the energy crisis. New research facilities were built across the country, including those at the DOE national labs located at Princeton, New Jersey, Oak Ridge, Tennessee, and Livermore, California. A major industrial research endeavor was also begun through a contract with General Atomics in La Jolla, California. University research grew. The theory and computation efforts that accompanied and supported development and interpretation of these experiments grew as well. International research programs also were ambitious, with the largest facilities in the world being constructed in the United Kingdom and Japan.

Scientific progress was strong through the 1980's, despite declining budgets. Major choices were made in program direction, and the tokamak concept was selected as the leading contender to reach the promised land of creating a sustained, magnetically confined burning plasma on Earth. In the 1980's research began on the flagship Tokamak Fusion Test Reactor (TFTR) at Princeton, and mid-decade a remarkable achievement was realized. Temperatures of the plasma fuel reached over 200 million degrees Centigrade—ten times the core temperature of the sun—in these magnetically confined plasmas. The flexibility of this experiment proved to be of great scientific value in launching controlled research studies of this plasma state. The exciting TFTR results were joined by rapid progress at the DIII–D tokamak at General Atomics in La Jolla, and a healthy competition grew within the U.S. as well as internationally. At this time, complementary experiments were continued at MIT in compact devices of very high magnetic field. The Joint European Tokamak (JET) in England was the first to use the "high octane" mix of the hydrogen isotopes deuterium and tritium (D–T) that will be used in a first-generation fusion reactor. They soon announced to the world the generation of a few million watts of fusion power, enough to power thousands of homes. The race was on—TFTR at Princeton began its experimental campaign with the D–T fuel mix, and completed it with experiments in 1994 that generated over 10 million watts of fusion power. The JET experiment ultimately created a record 16 million watts of fusion power in 1997, a result enabled by the larger size of the device as compared to TFTR.

Notably, however, more power was used to heat and control the plasma in each of these cases than was used to create the fusion reactions themselves. The figure of merit used in magnetic fusion, Q, relates the fusion power created to the power used to heat the plasma. The JET experiment yielded a Q of about 0.6. A campfire analogy is that, to date in fusion research, we have been burning wet wood. Remove the external flame, and the fire goes out. Extending the analogy, we have learned a great deal during and since these research campaigns about how to make a fire and how to make a fusion fireplace in which the wood burns itself—in which we have a self-sustained "burning" plasma.

Today we have to build that fireplace and learn how to best manage the fire in a robust, attractive way. Results from the D–T TFTR and JET studies and those obtained worldwide in other experiments pointed to a common direction, one in which meeting the burning plasma challenge is going to require an increase in scale of the research device. The embodiment of these research conclusions is the design and new construction of the international project called ITER (Latin for "the way"), which is described more fully later in this testimony.

It is important to note for understanding the potential future of fusion research that at least two major research thrusts were developing in parallel to the magnetic confinement experiments that I have just described. First, a seminal paper in 1972 pointed out the potential of the laser, invented in 1960, to be used as the basis of a fundamentally different approach to fusion energy. This approach, called inertial confinement fusion, uses symmetrically-applied exceptionally high-power pulsed laser beams to compress a small pellet of fusion fuel to high enough densities and temperatures for fusion to occur. In this case, the inertia of the fuel itself is relied upon to keep the matter contained long enough for a fusion burn to take place. The National Nuclear Security Administration (NNSA) has been the primary supporter of this line of research, through its aim to develop critical tools for stockpile stewardship. The Office of Fusion Energy Sciences also has a keen interest in inertial fusion, both from the point of view of the richness of the plasma physics—more on this later—as well as its potential energy applications.

NNSA's recently completed National Ignition Facility at Lawrence Livermore National Laboratory is the world's leading experimental enterprise in this research, and its work in the emergent field of High Energy Density Laboratory Plasma (HEDLP) physics is supported stateside by related research at other national laboratories, the University of Rochester, and a wide range of university-scale experiments.

Second, the computer revolution had enormous impact on fusion research in both magnetic and inertial fusion. The fusion sciences have been transformed from a largely empirical enterprise to a theory-based dominated by vigorous interaction between those who measure the elusive qualities and behavior of the plasma state in fusion conditions, and those who develop its complex theory and represent that theory in computational models. Over the last twenty years, the scientific basis for our readiness for the next era of fusion energy research has been established through this interaction, anchored in flexible, inventive experiments, continuously growing computational horsepower, and rich physics challenges that have yielded many secrets of the plasma to our probing.

In both magnetic fusion energy science and the linked science of inertial fusion energy, we are at the edge of the burning plasma era. A burning plasma is fundamentally different from plasmas that have been created in research facilities to date; it is only in a burning plasma that the energy confinement, heating, and stability are fully coupled, and the scientific issues associated with creating and sustaining a power-producing plasma can be explored. The importance of moving into this era was strongly affirmed in a 2004 National Academy of Sciences review, *"Burning Plasmas—Bringing a Star to Earth."* This report recognized that a burning plasma experiment is essential to assessing the scientific and technical feasibility of fusion as an energy source. Its strongest recommendation was that the U.S. fusion science research program confront the rich and important scientific questions that will only be possibly by creating a burning plasma in the laboratory. Even since this report, our scientific basis for entering this new era has deepened.

Allow me to now describe for you the present fusion sciences research program in the U.S., with references to the world-wide effort that supports our entrance into this new age, and the enabling program of this new era—the ITER project.

The U.S. Research Program Today

In the United States, a broad, multi-institutional program in experiment, theory, and computation is executed through the Office of Fusion Energy Sciences. A national laboratory dedicated to plasma physics and fusion research is located at Princeton, New Jersey, and other national laboratories are funded to undertake research in the fusion sciences as well. Many university partners partake in fusion research at these laboratories and at their own campuses.

A major feature of the program is the research platform provided by three major experiments. These facilities and their predecessors have been crucial for developing the physics basis needed to justify a burning plasma physics program. Today the experimental research programs at the U.S. facilities are scientifically complementary.

These are the DIII–D tokamak at General Atomics, mentioned previously, the National Spherical Torus Experiment (NSTX), at the Princeton Plasma Physics Laboratory, and a compact, high magnetic field tokamak called Alcator C–Mod at the Massachusetts Institute of Technology. Researchers participate in joint experiments conducted between these facilities and are leaders in an international organization that develops joint experiments with facilities overseas as well. U.S. researchers participate in about 75 joint international activities at the present time. These activities have a common aim, namely, to develop the scientific basis for a sound and revealing burning plasma research program and to develop fusion plasma science more generally. The national laboratories are intimately intertwined in the research execution and program leadership at these sites. Significant student populations partake in research there, and their programs are intrinsically collaborative. In part through student participation (about 340 graduate students at this time participate in an aspect of fusion energy science research), these national programs have strong, productive ties with many universities across the Nation.

Our portfolio also includes a robust program in innovative plasma confinement concepts, which broadens the fusion program by exploring the science of confinement optimization and plasma stability through a variety of smaller novel devices. The breadth of this program is summarized by the fact that, taken together, these confinement devices allow scientists to study plasmas with densities spanning twelve orders of magnitude.

FES also supports a world-leading theory program, which provides the conceptual scientific underpinning of the magnetic fusion energy sciences program. This program focuses on three thrust areas: burning plasmas, fundamental understanding, and configuration improvement. Theory efforts describe the complex multiphysics, multiscale, non-linear plasma systems at the most fundamental level. These descriptions—ranging from analytic theory to highly sophisticated computer simulation codes—are used to interpret results from current experiments, plan new experiments on existing facilities, design future experimental facilities, and assess projections of facility performance. U.S. expertise and capabilities in theory and computation are a lynchpin of the transition to the burning plasma era.

The flagship program of this new era is the ITER project, an international fusion research project being constructed in Cadarache, France, that will realize magnetically confined burning plasmas for the first time. Burning plasma physics as it will be explored on ITER presents at once a grand scientific challenge in its own right and an undertaking of tremendous practical import. The goal of this international research program is to demonstrate the scientific and technological feasibility of sustained fusion power. In the United States, we place high importance on the potential of ITER as a flexible instrument for scientific discovery as well as a demonstration of fusion energy's scientific and technical viability. ITER's overarching goals are the creation of plasmas producing 500 megawatts of power with Q = 10 for hundreds of seconds, that is, ten times the fusion power generated by the burning plasma as compared to the power used to heat it, and plasmas of Q = 5 for durations of up to an hour. What we learn through ITER will guide our choices in the development of a subsequent demonstration power plant.

Seven members comprise the ITER partnership: China, the European Union, India, Japan, Russia, South Korea, and the United States. Under the formal international agreement that entered into force in 2006, the experiment is to be built in Cadarache, France proximal to a major French nuclear research laboratory. It will be the largest magnetic confinement fusion experiment ever constructed, with a radius of the magnetic donut over six meters, enclosed in structure close to 10 stories tall. The magnets will be superconducting so as to enable long pulses of fusion plasmas. U.S. researchers have played a significant role in identifying the design for ITER. As host, the European Union has responsibility for five-elevenths of the project cost. The remaining six partners, including the U.S., is each responsible for one-eleventh share. Contributions of the member states are primarily in-kind hardware components for the project. Annual cash contributions are also made to the ITER Organization (IO) in Cadarache that is responsible for assembling the device and the civil construction of the site. The data obtained from ITER will be shared by all partners.

The U.S. ITER Office (USIPO), located at Oak Ridge National Laboratory, reports to my office and manages the interfaces with the IO and the development of the hardware that are a U.S. responsibility. Most of the funds directed to the USIPO will be spent domestically in U.S. industry to design and fabricate the hardware needed to fulfill our obligations. Examples of what we will deliver include superconducting transformer coils that will reside in the center of the magnetic donut, superconducting strands of wire to be used in the construction of some the other magnets for ITER, and measurement instrumentation systems that will be installed on the device to measure and monitor many aspects of the burning plasma.

The schedule for ITER operations is being developed and refined; the first plasma experiments to commission the device are almost certainly at least 10 years away, with the first burning plasma experiments probably in the mid-2020's. This time scale is an acknowledged frustration of all parties given the urgency of the energy challenge and reflects both the immense technical scope of the project, the fact that the laboratory and its governance are being set up at a green field site, and the added challenges posed by a novel international collaboration. Importantly, the USIPO is vigorously engaged with the IO in Cadarache and other members' domestic agencies in implementing U.S. project management practices in ITER. The Office of Science takes most seriously the imperative that ITER be well managed in both its construction and research phases.

With respect to burning plasma physics and ITER itself, the U.S. research program has been particularly effective in improving the ITER design. For example, the "dynamic range" of the plasmas that ITER will be capable of creating has been significantly increased thanks in significant part to U.S. intellectual leadership. The U.S. fusion program's robust interplay among experimentalists, theorists, and computational researchers in developing complex simulation programs executed on the world's most powerful computers have been and will continue to be essential for preparing for the burning plasma era. This interplay is facilitated by the U.S. Burning Plasma Organization, a community-led endeavor of researchers currently headed by the chief scientist of the USIPO.

As described earlier, there is another form of fusion in the laboratory, inertial confinement fusion, whose science is being pursued and is also on the cusp of the burning plasma era. The National Ignition Facility is slated to explore whether a small pellet of fusion fuel can be ignited in a fusion burn by simultaneously heating and compressing it with the enormous radiant power of its unparalleled laser system. If successful, these experiments will be historic—analogous to achievement of the first spark ever in an internal combustion engine. Significant scientific and technological development will be required to achieve appreciable energy output per spark and the generation of many sparks per second in an attractive manner.

The branch of plasma physics at the heart of this endeavor, high energy density laboratory plasma physics, studies extreme states of matter known to exist otherwise only in extraordinary systems such as stellar interiors and exploding stars. The National Academy of Science has recognized the importance of this field to energy and the study of astrophysical systems, and has urged the formation of a coherent programmatic home in the Federal R&D portfolio. To this end, the Office of Fusion

Energy Sciences is now collaborating with NNSA in launching a research program in this branch of science for the sake of advancing both fusion energy science and the science of these extraordinary systems so as to further understanding of our universe.

Importantly, the U.S. fusion energy sciences program also has ambitions to develop and advance general plasma science in the broadest sense. A number of vigorous university-based programs are deployed across the country. Furthermore, my office supports over 30 joint research efforts with the National Science Foundation to advance general plasma science that extends beyond the immediate needs of the fusion goal. This science can be of high import in describing natural plasma phenomena and also has an impact on the economics of industrial plasma applications. Joint research centers with university-scale experiments are at the heart of these ventures and on shedding light on the phenomena governing plasma dynamics in settings ranging from the industrial to the solar corona.

The Office of Fusion Energy Sciences is currently engaged in a formal strategic planning process aimed at filling scientific gaps in the global research portfolio so as to assert U.S. leadership and maximize U.S. scientific return where it best advances fusion as a whole. For magnetic fusion, a Fusion Energy Sciences Advisory Committee recently identified gaps in scientific knowledge that must be filled so as to maximize ITER's scientific opportunities and to close the gaps between ITER and demonstrating fusion power on the grid. This formal gaps and priorities analysis was followed by a community-based activity that identified the research needs for making such an advance. This Office is developing a strategy by drawing upon this input and assessing strategic opportunities for partnership across the Department of Energy. Based on this input, the scientific challenges for magnetic fusion can be broadly stated as follows:

(1) Understanding and optimizing the burning plasma state. Experiments, theory, and simulation have significantly advanced our understanding of what to expect from a burning plasma, and will continue to do so. The U.S. domestic program will continue to play a strong and world-leading role in preparing for the burning plasma era. But ITER provides the only platform planned to directly test and thus expand and challenge our understanding of this complex physics. Both before and during experiments on ITER, we must strengthen the coupling between experiment, theory, and large-scale computer simulation so as to enable prediction of burning plasma performance beyond ITER's operating range and configuration.

(2) Understanding the requirements for extending the burning plasma state to long times—days, weeks, and longer. Many aspects of this are pursued in the U.S., and the second ten years of ITER's operation will put our understanding to crucial tests. However, in the next ten years overseas fusion programs are set to assert a stronger role and leadership in part through new billion dollar class research facilities in Europe, Japan, South Korea, and China. We are exploring growing our collaborations to increase their impact and the knowledge returned. And finally,

(3) Advancing the materials science for enduring the harsh fusion plasma environment, for extracting energy, and for generating fusion fuel in situ. We are beginning to outline our plans in these areas and to explore alignments with other energy-related fields in developing a materials and fusion nuclear science program. Common interests in materials research exist across both magnetic and inertial confinement fusion research. Beyond this, we will be exploring synergies in this area between fusion, fission, and defense-related research so as to assess the viability and requirements for a cross-office "Materials for Energy" effort that would make the most out of common needs and diverse resources.

Concluding Remarks

In the next ten years, the U.S. fusion research program will strive to be at the forefront of the burning plasma age, one in which research students grow a strong connection to fusion's future and potential. It will be an age where more is asked of advanced computation than ever, where computer simulations are relied upon to close the gaps between one research step and another, and reduce project costs and increase confidence. It will be an era where single purpose laboratories interact readily with multipurpose laboratories with common incentives and common purpose of advancing energy-related science for all. It will be an era in which the best combination of scientific depth and richness is combined with the highest sense of urgency to help the world address its energy challenges successfully to improve our quality of life.

Thank you, Mr. Chairman, for providing this opportunity to discuss the Fusion Energy Sciences Program at the Department of Energy. This concludes my testimony, and I would be pleased to answer any questions you may have.

BIOGRAPHY FOR EDMUND J. SYNAKOWSKI

Dr. Edmund J. Synakowski is the Associate Director of Fusion Energy Sciences at the U.S. Department of Energy (DOE). With an annual budget of over $400 million, the Office of Fusion Energy Sciences is the federal office supporting research to develop the scientific basis for fusion energy, and serves as a steward for plasma science. He joined the Office of Science on June of 2009 from the Lawrence Livermore National Laboratory where he was director of the Fusion Energy Program and Deputy Division Leader for the Physics Division of the Physics and Life Sciences Directorate. From 1988 through 2005, he was at the Princeton Plasma Physics Laboratory where he was Head of Research and Deputy Program Leader of the National Spherical Torus Experiment. He also performed extensive research and led research programs in fusion plasma confinement and control on the Tokamak Fusion Test Reactor. His service to the fusion community has included participation in the development of the initial research plan for the international ITER research program, chairmanship of the U.S. Transport Task Force, and membership of the American Physical Society Division of Plasma Physics Executive Committee. Dr. Synakowski received a B.A. degree in Physics from the Johns Hopkins University in 1982, graduating with Departmental Honors and receiving the Donald Kerr Medal for excellence in physics. He received a Ph.D. degree in physics from the University of Texas in 1988. He is a Fellow of the American Physical Society and received the American Physical Society award for Excellence in Plasma Physics Research in 2001 and the 2000 Kaul Foundation Prize for Excellence in Plasma Physics Research and Technology Development from Princeton University. He has published over 150 papers in the study of fusion plasmas and has performed research on all of the major U.S. fusion experiments.

Chairman GORDON. [Presiding] Dr. Prager, you are recognized.

STATEMENT OF DR. STEWART C. PRAGER, DIRECTOR, PRINCETON PLASMA PHYSICS LABORATORY

Dr. PRAGER. Well, thank you very much, Members of the Committee, for this opportunity to discuss fusion energy, and thank you, Congressman Holt, for the kind opening words and for your deep engagement and expertise in this topic. As he said, I am Director of the Princeton Plasma Physics Laboratory, which is a DOE national lab managed by Princeton University dedicated to developing fusion energy.

There are two complementary approaches to fusion; in one, as you have heard, powerful lasers compress a tiny pellet of fuel, releasing fusion energy in a flash. The National Ignition Facility will tremendously advance the physics for this approach.

I am here to discuss the approach known as magnetic fusion, in which the large, hot plasma is confined continuously by powerful magnetic fields. As I hear you already well recognize, fusion energy is one of the most challenging physics and engineering quests ever undertaken. It will be key to solving perhaps the most pressing problem confronting the world today: the absence of sustainable energy.

By any metric, we are far along the road to commercial fusion power. In the past 30 years we have progressed from producing one watt of fusion power for one-thousandth of a second to 15 million watts for seconds, and ITER will produce 500 million watts for 10 minutes and longer. Driving this progress has been the development of an entirely new field of science called plasma physics. Outside reviews continuously laud the progress of fusion. The most recent National Academy study notes remarkable progress in recent years. But my focus today is the future, the remainder of the journey to fusion power.

My comments are informed by the just-completed study by the U.S. fusion community commissioned by DOE known as the ReNeW Report. Two hundred fusion scientists undertook this one-year study that identifies the remaining scientific issues to resolve for fusion power. A fusion system consists of the hot plasma core and the surrounding material structure. We are ready to move forward on the two major challenges to better control the plasma and to develop new materials. The two problems are coupled since the plasma and the material structure interact with each other. Our ability to control the 100-million-degree plasma core is quite amazing, yet we have more work to do to sustain the plasma indefinitely and controllably. The sophistication of plasma science now offers new opportunities; for example, designs of magnetic configurations are possible now that were nearly impossible even to conceive 20 years ago. They are possible only with modern computers. Building upon the foundation of the mainline tokamak approach, these designs produce plasmas that persist indefinitely and are so well controlled as to reduce the severity of the materials challenge.

It is crucial that we establish a research program and materials for fusion. Materials must be developed to withstand the intense heat that emerges from the plasma. But full solution of the materials challenge ultimately requires study of materials in a true fusion environment with the intense flux of neutrons that are produced in the fusion reactions. It is time to lay the groundwork for such a facility, sometimes called a "fusion nuclear science facility," since it exposes materials to a nuclear fusion environment. If this facility were designed somewhat more aggressively, it could possibly demonstrate net electricity production. Design studies are required to identify the wisest next step in these directions.

The Princeton Plasma Physics Lab aims to solve a broad range of fusion science challenges. Our core capabilities in plasma physics enable us to attack crucial problems in the fusion plasma and in materials exposed to the intense plasma heat. The major experiment at our lab is laying the physics basis for a fusion nuclear science facility, is advancing fusion science broadly and is investigating novel material boundaries. We are contributing to the design and fabrication of ITER and are preparing for research in ITER. We hope to play key roles in a fusion nuclear science facility which would not be located at our laboratory, and we are developing plans to realize experimentally, at our laboratory, the new study state approaches to fusion energy that could prove so essential to the feasibility of fusion.

When I began my research career, the United States was the world leader in fusion with the best facilities, arguably the most innovative programs. Scientists from the world over flocked to our labs. Japan sent research teams to U.S. facilities to learn the trade. An alarming reversal of that flow of scientists is now underway. The United States has not built a major nuclear fusion facility in decades. The rest of the world is seizing the opportunities. Major facilities more ambitious than anything in the United States are starting operation or are under construction in China, Japan, South Korea, Germany and France. Our effort has dwindled to a fraction of that of Europe and Japan. The time is right for the United States to reverse its slide. Opportunities such as we are discussing today abound to restore the United States to world leadership and move us aggressively toward carbon-free, abundant fusion energy.

And I will just close by inviting all Members to please visit our laboratory, which is a short train ride up the coast, and with that, thank you very much.

[The prepared statement of Dr. Prager follows:]

PREPARED STATEMENT OF STEWART C. PRAGER

Mr. Chairman and Members of the Committee, thank you for this opportunity to discuss fusion energy.

I am Director of the Princeton Plasma Physics Laboratory—a Department of Energy national lab, managed by Princeton University, dedicated to developing the scientific foundation for fusion energy. Prior to nine months ago, I was a practicing fusion plasma physicist at the University of Wisconsin.

There are two complementary, compelling approaches to fusion energy. In one, powerful lasers compress a tiny frozen pellet of fusion fuel, releasing fusion energy in a billionth of a second. The anticipated demonstration of ignition in the National Ignition Facility will tremendously advance the physics basis for this approach.

I am here today to discuss the approach known as.magnetic fusion, in which a large, hot plasma (the hot gas that makes up the sun) is confined continuously by powerful magnetic fields. Fusion energy is perhaps one of the most challenging physics and engineering quests ever undertaken; its realization will be key to solving what is perhaps the most pressing problem confronting the world today—the absence of sustainable energy. By any measure, we are far along the road to commercial fusion power. My goal today is to talk about the future: the remainder of the journey to fusion energy.

My comments are informed by the just-completed study by the U.S. fusion community, commissioned by DOE and known as the ReNeW report. About 200 fusion scientists undertook this one-year study that articulates the scientific issues yet to resolve for fusion power, beyond those to be resolved in the landmark international ITER experiment. A fusion system consists of the hot plasma core—the "sun on Earth"—in which fusion reactions occur, and the surrounding material structure. We are ready to move forward to better control the plasma and to develop new materials. The two problems are coupled in that the plasma affects the materials and the material affects the behavior of the plasma within.

Our ability to control the 100 million degree plasma core is quite amazing. Yet, we have more work to do to sustain the fusion plasma indefinitely and controllably. The sophistication of plasma science now offers new opportunities for fusion. For example, new designs of magnetic configurations are possible now that were nearly impossible even to conceive twenty years ago. They are possible only with modern computers, enabled by new principles in plasma physics. Building upon the substantial experimental foundation of the mainline tokamak approach, these cousins of the tokamak produce plasmas that persist indefinitely and are so well controlled as to reduce the severity of the materials challenges.

It is crucial that we establish a research program in materials for fusion. Materials must be developed to withstand the intense heat that emerges from the plasma. This requires a basic materials research combined with materials studies in plasma experiments.

But full solution of the materials challenge ultimately requires study of materials in a true fusion environment—with the intense flux of neutrons that are produced in the fusion reactions. It is time to lay the groundwork for such a U.S. facility, sometimes called a fusion nuclear science facility since it provides study of materials in the nuclear fusion environment. If this facility were designed somewhat more aggressively—to produce net fusion power as well as neutrons, it would demonstrate electricity production. Design studies are required to identify the wisest next step in these directions, considering our level of physics and engineering readiness.

The Princeton Plasma Physics Lab is dedicated to solving the broad range of fusion science challenges. Our key capability in plasma physics enables us to attack crucial problems in the fusion plasma core, the interaction between the plasma and materials, and the properties of materials exposed to the intense plasma heat.

The major experiment at our lab is developing the plasma physics basis for a fusion nuclear science facility, advancing physics broadly applicable to fusion and

ITER, and investigating novel materials boundaries. We hope to play a key role in the physics and engineering design of a fusion nuclear science facility, which would not be located at our laboratory. We will continue our contributions to the design of ITER, and are preparing ourselves for participation in ITER research. And we are developing plans to realize experimentally, at our laboratory, the new steady-state approaches to fusion energy that could prove so essential to the feasibility of fusion.

When I began my research career the U.S. was the world leader in fusion. We had the best facilities and arguably the most innovative program. Scientists the world over flocked to our labs. The Japanese government sent research teams to then-modern U.S. facilities to learn the trade. An alarming reversal of that flow of scientists is now underway. The U.S. has not built a major new fusion facility in decades. The rest of the world is seizing the opportunities. Major facilities, more ambitious than anything in the U.S., are starting operation or are under construction in China, Japan, South Korea, Germany and France. The U.S. effort has dwindled to a fraction of that of the European Union and Japan. The time is ripe for the U.S. to reverse its slide. Opportunities abound to restore the U.S. to world leadership and move us aggressively toward carbon-free, abundant fusion energy.

Appendix I

Executive Summary of the Research Needs Workshop (ReNeW) for Magnetic Fusion Energy Science

Nuclear fusion—the process that powers the sun—offers an environmentally benign, intrinsically safe energy source with an abundant supply of low-cost fuel. It is the focus of an international research program, including the ITER fusion collaboration, which involves seven parties representing half the world's population. The realization of fusion power would change the economics and ecology of energy production as profoundly as petroleum exploitation did two centuries ago.

The 21st century finds fusion research in a transformed landscape. The worldwide fusion community broadly agrees that the science has advanced to the point where an aggressive action plan, aimed at the remaining barriers to practical fusion energy, is warranted. At the same time, and largely because of its scientific advance, the program faces new challenges; above all it is challenged to demonstrate the timeliness of its promised benefits.

In response to this changed landscape, the Office of Fusion Energy Sciences (OFES) in the U.S. Department of Energy commissioned a number of community-based studies of the key scientific and technical foci of magnetic fusion research. The Research Needs Workshop (ReNeW) for Magnetic Fusion Energy Science is a capstone to these studies. In the context of magnetic fusion energy, ReNeW surveyed the issues identified in previous studies, and used them as a starting point to define and characterize the research activities that the advance of fusion as a practical energy source will require. Thus, ReNeW's task was to identify (1) the scientific and technological research frontiers of the fusion program, and, especially, (2) a set of activities that will most effectively advance those frontiers. (Note that ReNeW was not charged with developing a strategic plan or timeline for the implementation of fusion power.)

The Workshop Report

This Report presents a portfolio of research activities for U.S. research in magnetic fusion for the next two decades. It is intended to provide a strategic framework for realizing practical fusion energy. The portfolio is the product of ten months of fusion-community study and discussion, culminating in a Workshop held in Bethesda, Maryland, from June 8 to June 12, 2009. The Workshop involved some 200 scientists from Universities, National Laboratories and private industry, including several scientists from outside the U.S.

Largely following the Basic Research Needs model established by the Office of Basic Energy Sciences (BES), the Report presents a collection of discrete research activities, here called "thrusts." Each thrust is based on an explicitly identified question, or coherent set of questions, on the frontier of fusion science. It presents a strategy to find the needed answers, combining the necessary intellectual and hardware tools, experimental facilities, and computational resources into an integrated, focused program. The thrusts should be viewed as building blocks for a fusion program plan whose overall structure will be developed by OFES, using whatever additional community input it requests.

Part I of the Report reviews the issues identified in previous fusion-community studies, which systematically identified the key research issues and described them in considerable detail. It then considers in some detail the scientific and technical means that can be used to address these issues. It ends by showing how these various research requirements are organized into a set of eighteen thrusts. Part II presents a detailed and self-contained discussion of each thrust, including the goals, required facilities and tools for each.

This Executive Summary focuses on a survey of the ReNeW thrusts. The following brief review of fusion science is intended to provide context for that survey. A more detailed discussion of fusion science can be found in an Appendix to the Report, entitled "Fusion Primer."

Fusion science

Fusion's promise

The main advantages of producing power from fusion reactions are well known:

- Essentially inexhaustible, low-cost fuel, available worldwide.
- High energy-density of fuel, allowing straightforward base-load power production without major transportation costs.
- No production of greenhouse gas, soot or acid rain.
- No possibility of runaway reaction or meltdown that could pose a risk to public safety.
- Minimal proliferation risk.
- Only short-lived radioactive wastes.

Few of these benefits are unique to fusion; what is exceptional is their simultaneous achievement in a single concept. For example, fusion's freedom from greenhouse gas production and chemical pollution is shared with, among other energy sources, fission nuclear power; in this regard the relatively mild radioactivity of fusion, whose waste is thousands of times less radioactive and long-lived than fission, is significant. On the other hand, compared to the non-proliferating renewable energy sources, fusion offers a steady, predictable energy source with low land use.

To be weighed against these advantages is the long and relatively expensive development path for fusion. Achieving the conditions necessary for appreciable fusion reactions to occur invokes substantial physics and engineering challenges. Yet the impressive progress achieved in addressing these hurdles must be acknowledged. One measure is the exponential increase in fusion power produced in laboratory experiments, amounting to some eight orders of magnitude (a factor of 100,000,000) since the mid-1970's. Indeed some fusion experiments have approached scientific "break-even," producing roughly as much fusion power as was externally supplied for heating the fuel. A more important if less easily measured avenue of progress lies in scientific understanding. Fusion scientists have developed a broad and sophisticated, if still incomplete, picture of what is happening in a magnetically confined fusion plasma. This advance now allows routine control of key plasma properties and behavior.

Magnetic confinement

Magnetic confinement (more accurately termed "magnetic insulation") allows the fusion fuel, which is necessarily in the form of ionized gas, or plasma, to retain sufficient heat to maintain fusion reactions. It acts by enforcing a relatively low plasma density at the plasma boundary, where vessel walls would otherwise cool the gas, and by inhibiting heat flow from the interior to the wall region. The essential ingredient is a magnetic geometry in which the magnetic field lines abide in a closed, bounded region.

During the last decades of the twentieth century, fusion research gained important scientific victories in plasma confinement: major advances in both the control of instability and the amelioration of heat transport. While significant confinement issues remain to be solved, and while most of the fusion scientific community looks forward to substantial further improvements, the present demonstrated level of confinement is sufficient to impart confidence in the future of fusion energy. One indicator of this scientific advance is the rapid confinement progress mentioned above. Perhaps a more significant consequence is the decision by the international fusion community to embark on the ITER project.

Breadth of fusion research

Fusion progress requires scientific research of the highest quality and originality. Such science is not an activity to be balanced against the energy goal, but rather an essential component of the quest for that goal. This Report emphasizes the goal-directed nature of the program, but it is also appropriate to mention that, like any deep investigation, fusion research has enjoyed broad connections with other domains of science.

Many connections are mentioned in the Theme chapters of Part I. Examples are:

- gyrokinetic simulation, used to understand transport and stability in magnetized fusion plasmas, has become an important tool in astrophysics and magnetosphere physics;
- magnetic reconnection, a key phenomenon in the stability of magnetically confined plasmas, has central importance in numerous solar, magnetosphere and astrophysical contexts;
- turbulent heat transport across the magnetic field, which plays a role in modern fusion experiments very similar to its role in the equilibrium configuration of the sun and other stars;
- unstable Alfven waves, whose effects in fusion experiments are closely similar to observed perturbations in the Earth's magnetosphere;
- the high-strength, ductile materials being developed for fusion should have wide application in industry, including aerospace and chemical manufacturing.

Research requirements

In the next two decades, the "ITER era," magnetic fusion will for the first time explore the burning plasma regime, where the plasma energy is sustained mostly by its own fusion reactions. We expect ITER to expand our understanding of fusion plasma science and to be a major step toward practical fusion energy. It will also, as the first burning plasma experiment, pose new requirements, including advanced diagnostics for measurement and control in a burning-plasma environment, and analytical tools for understanding the physics of self-heating.

To benefit fully from its investment in ITER the U.S. must maintain a broad research program, attacking fusion's scientific and technical issues on several fronts. We need in particular to acquire knowledge that ITER cannot provide: how to control a burning plasma with high efficiency for indefinite periods of time; how to keep a continuously burning plasma from damaging its surrounding walls—and the walls from contaminating the plasma; how to extract the fusion energy from a burning plasma efficiently and use it to produce electricity and a sustained supply of tritium fuel; and ultimately how to design economical fusion power plants. These requirements motivate a multi-disciplinary research program spanning such diverse fields as plasma physics and material science, and advancing a range of technologies including plasma diagnostics, magnets, radio-frequency and microwave sources and systems, controls, and computer simulation.

The key scientific and technical research areas whose development would have a major effect on progress toward fusion energy production were systematically identified, categorized and described in the three resource documents that form the starting point for ReNeW: the report of the Priorities, Gaps and Opportunities Panel, chaired by Martin Greenwald; the report of the Toroidal Alternates Panel, chaired by David Hill; and the report of the *Energy Policy Act* task group of the U.S. Burning Plasma Organization.

In Part I of the ReNeW Report the full panoply of fusion issues are summarized, and then examined from the point of view of *research requirements:* the facilities, tools and research programs that are needed to address each. The research thrusts presented in Part 11 are essentially integrated combinations of these research requirements. [NOTE: This paragraph is similar to the first paragraph on page 2.]

The ReNeW thrusts: a research portfolio

Thrust definition

The ReNeW thrusts listed below are the key results of the Workshop. They constitute eighteen concerted research actions to address the scientific and technological frontiers of fusion research. Each thrust attacks a related set of fusion science issues, using a combination of new and existing tools, in an integrated manner. In this sense each thrust attempts a certain stand-alone integrity.

Yet the thrusts are linked, both by scientific commonality and by mutual dependence. The most important linkages—for example, requirements that a certain thrust be pursued and at least in part accomplished before another is initiated—are discussed in Part 11 of the main Report. Here we emphasize that fusion advances along a broad scientific and technological front, in which each thrust plays an important role.

The thrusts span a wide range of sizes, from relatively focused activities to much larger, broadly encompassing efforts. This spectrum is expected to enhance the flexibility of OFES planning.

ReNeW participants consider all the thrusts to be realistic: their objectives can be achieved if attacked with sufficient vigor and commitment. Three additional elements characterize, in varying degrees, the ReNeW thrusts:

- Advancement in fundamental science and technology—such as the development of broadly applicable theoretical and simulation tools, or frontier studies in materials physics.
- Confrontation with critical fusion challenges—such as plasma-wall interactions, or the control of transient plasma events.
- The potential for major transformation of the program—such as altering the vision of a future fusion reactor, or shortening the time scale for fusion's realization.

Thrust organization

The resource documents used by ReNeW organized the issues into five scientific and technical research areas. Correspondingly, the ReNeW organizational structure was based on five Themes, each being further sub-divided into three to seven panels. The thrusts range in content over all the issues delineated in the five Themes.

Many of the ReNeW thrusts address issues from more than one Theme. For this reason the scientists contributing to most thrusts are from a variety of research areas, and key elements of a given thrust may stem from ideas developed in several Themes. In other words, the content of a typical thrust transcends that of any single Theme. Nonetheless, it is convenient to classify each thrust according to the Theme that contains its most central issues.

The ReNeW thrusts are:

Theme 1: Burning plasmas in ITER.

ITER participation will be a major focus of U.S. fusion research during the time period considered by ReNeW. The opportunities and challenges associated with the ITER project are treated in Theme 1.

Thrust 1: Develop measurement techniques to understand and control burning plasmas. This thrust would develop new and improved diagnostic methods for measuring and controlling key aspects of burning plasmas. The desired measurement techniques must be robust in the hostile burning-plasma environment and provide reliable information for long time periods. While initially focused on providing critical measurements for ITER, measurement capability would also be developed for steady-state burning plasmas beyond ITER.

Thrust 2: Control transient events in burning plasmas. This thrust would develop the scientific understanding and technical capability to predict and avoid disruptions and to mitigate their consequences, in particular for ITER. Also, tools would be developed to control edge plasma transport and stability, to minimize instability-driven heat impulses to the first wall.

Thrust 3: Understand the role of alpha particles in burning plasmas. Key actions would be developing diagnostics to measure alpha particle properties and alpha-induced fluctuations, incorporating validated theories for alpha particle behavior into integrated burning-plasma simulation tools, and expanding the operating regime of burning plasma devices through the development of control techniques for alpha-driven instabilities.

Thrust 4: Qualify operational scenarios and the supporting physics basis for ITER. This thrust would address key issues in forming, heating, sustaining, and operating the high-temperature plasmas required for ITER's mission. An integrated research campaign would investigate burning-plasma-relevant conditions with the use of upgraded tools for heating and current drive, particle control and fueling, and heat flux mitigation on existing tokamaks, along with a possible new facility.

Theme 2: Creating predictable, high-performance, steady-state plasmas

An economic fusion reactor will require a steady state with higher fusion density and greater fraction of self-heating than ITER. This Theme addresses a broad range of issues, including both plasma physics and engineering science, needed to demonstrate that plasmas with the needed conditions can be achieved and controlled. Predictive capability to enable confident extrapolation to a demonstration reactor is emphasized.

Thrust 5: Expand the limits for controlling and sustaining fusion plasmas. This thrust would integrate development of the diagnostic, auxiliary heating, current drive, fueling systems and control systems needed to maintain the nonlinear tokamak plasma state, seeking to maximize performance. The thrust will exploit existing experiments to test and develop new ideas and proceed with increased integration in upcoming steady-state experiments and alpha-heated plasmas in ITER, ultimately enabling the self-heated and self-driven plasmas needed for a fusion power plant.

Thrust 6: Develop predictive models for fusion plasmas, supported by theory and challenged with experimental measurement. Advances in plasma theory and simulation would be combined with innovative diagnostic methods and experiments to improve and validate models of confined plasma dynamics. Assessment of critical model elements would be provided by dedicated analysts, acting as bridges between theorists, code developers and experimentalists.

Thrust 7: Exploit high temperature superconductors and other magnet innovations to advance fusion research. Magnets are crucial for all MFE concepts. This focused thrust would perform the research necessary to enable revolutionary new high temperature superconducting materials to be used in fusion applications. Key activities include development of high-current conductors and cables, and integration into components of fusion research experiments, with great potential to improve their design options.

Thrust 8: Understand the highly integrated dynamics of dominantly self-heated and self-sustained burning plasmas. This thrust would explore scenarios where, as in a reactor, most heat comes from fusion alphas and most current is self-driven by plasma gradients. It would start by assessing potential advanced plasma scenarios and upgrades on ITER which could enhance its performance. In parallel, scoping/design studies would be done for a new US facility to explore the high fusion gain DEMO plasma regime. The studies would support actions to proceed with ITER enhancements, the construction of a U.S. D–T facility, or both.

Theme 3: Taming the plasma-material interface

Magnetic confinement sharply reduces the contact between the plasma and the vessel walls, but such contact cannot be entirely eliminated. Advanced wall materials and magnetic field structures that can prevent both rapid wall erosion and plasma contamination are studied in Theme 3.

Thrust 9: Unfold the physics of boundary layer plasmas. Comprehensive new diagnostics would be deployed in present confinement devices to measure key plasma parameters in the boundary region, including densities and temperatures, radiation, flow speeds, electric fields and turbulence levels. The results could vastly improve numerical simulation of the edge region, allowing, in particular, reliable prediction of wall erosion and better radio-frequency antenna design.

Thrust 10: Decode and advance the science and technology of plasma-surface interactions. Measurement of complex interaction of plasma with material surfaces under precisely controlled and well-diagnosed conditions would provide the information needed to develop comprehensive models to uncover the basic physics. These measurements would be made on both upgraded present facilities and new boundary plasma simulators capable of testing irradiated and toxic materials.

Thrust 11: Improve power handling through engineering innovation. Heat removal capability would be advanced by innovative refractory power-exhaust components, in parallel with assessment of alternative liquid-metal schemes. Materials research would provide ductile, reduced-activation refractory alloys, which would be developed into prototypes for qualification in high-heat flux test devices. Practical components would be deployed on existing or new fusion facilities.

Thrust 12: Demonstrate an integrated solution for plasma-material interfaces compatible with an optimized core plasma. Understanding of interactions between a fusion plasma core region and its boundary would be advanced and validated in a new facility. The facility would combine high power density, long pulse length, elevated wall temperature and flexibility regarding boundary systems, in a limited-activation environment. Knowledge gained from thrusts 9–11 would help guide the design of this facility.

Theme 4: Harnessing fusion power

Fusion energy from D–T reactions appears in the form of very energetic neutrons. Theme 4 is concerned with the means of capturing this energy, while simultaneously breeding the tritium atoms needed to maintain the reaction.

Thrust 13: Establish the science and technology for fusion power extraction and tritium sustainability. Fusion must create the tritium fuel it uses, and do so in the same systems that capture and extract the fusion energy. This thrust develops the scientific foundation and engineering of practical, safe and reliable processes and components that harvest the heat, create and extract the tritium, and rapidly process and contain the tritium. The thrust will culminate in a fuel and power handling capability on a scale needed for a demonstration energy system.

Thrust 14: Develop the material science and technology needed to harness fusion power. The objective of this thrust is to create low-activation, high-performance materials that effectively function for a long time in the hostile fusion environment. An essential requirement to fulfill the mission of this thrust is the establishment of a fusion-relevant neutron source to perform accelerated characterization of the effects of radiation damage to materials.

Thrust 15: Create integrated designs and models for attractive fusion power systems. Advanced design studies focused primarily on DEMO, but also on nearer term fusion nuclear facilities is one element of this thrust. These would lay out the scientific basis for fusion power and provide focus to the research efforts required to close the knowledge gap to DEMO. The other element comprises science-based predictive modeling capabilities for plasma chamber components and related systems.

Theme 5: Optimizing the magnetic configuration

Currently most large fusion experimental devices are based on the tokamak magnetic configuration, a design using a strong, axisymmetric external magnetic field to achieve operating parameters close to those in a fusion reactor. Alternative magnetic configurations are studied to investigate physics and technology principles that could optimize the design of future fusion devices. The most developed alternate toroidal magnetic configurations are considered in Theme 5.

Thrust 16: Develop the spherical torus to advance fusion nuclear science. Experiments on the small aspect-ratio tokamak, or Spherical Torus, would be extended to regimes of lower collision frequency, approaching values needed for fusion nuclear science applications. Plasma start-up, power handling, controlled stability, and sustainment issues in this regime would be studied in long-pulse experiments using stronger magnetic fields, improved heating and current drive, and advanced diagnostics, with strong coupling to theory and modeling.

Thrust 17: Optimize steady-state, disruption-free toroidal confinement using 3–D magnetic shaping, and emphasizing quasi-symmetry principles. Magnetic quasi-symmetry in 3–D configurations is expected to lead to excellent plasma confinement while ensuring stable steady-state burning plasma performance with minimal need for control. This thrust would conduct new quasi-symmetric experiments, which would, together with theory, engineering design, and targeted international collaboration, validate extrapolation to burning plasma applications.

Thrust 18: Achieve high-performance toroidal confinement using minimal externally applied magnetic field. This thrust advances a multi-faceted program of theory, simulation, and well-diagnosed experiments to resolve critical issues of confinement, stability, and current sustainment in magnetic configurations with minimal toroidal field. New devices with heating and current drive systems would enable scaling to high temperature and small ion gyroradius. Fusion system studies will guide productive directions for present and future research.

Appendix:

A Fusion Primer

Just as the heaviest elements, such as uranium, release energy when fission allows them to become smaller, so the very lightest elements release energy when they fuse, joining together to produce larger nuclei. (The dividing line between nuclei that are too light and want to fuse and those that are too heavy occurs at iron, the most stable nucleus.) The reaction that occurs most readily is the fusion of two isotopes of hydrogen: deuterium (D), whose nucleus consists of a proton and a neutron, and tritium (T), whose nucleus contains a proton and two neutrons. Fusion of these nuclei—the so-called D–T reaction—yields helium, an inert, non-radioactive gas whose nucleus has two protons and two neutrons. This helium nucleus or "alpha particle" carries 20 percent of the fusion energy production. It is contained by magnetic fields, and provides the plasma self-heating that sustains the very high plasma temperature. The remaining neutron is released at very high energy—energy whose capture provides 80 percent of the energetic profit of the reaction.

A reactor based on D–T reactions would have to breed tritium from lithium (which is plentiful), using the neutrons liberated in the D–T fusion process. More advanced fuel cycles would not require tritium breeding, but the D–T reaction has advantages with regard to accessibility and energy production. It is expected to be used in at least the first generation of fusion power reactors.

Because all nuclei are positively charged, they electrically repel each other. This "Coulomb repulsion" can be overcome only by bringing the reactants to very high temperatures; in the case of D–T the required temperature exceeds one hundred million degrees.

Far below thermonuclear temperatures the electron on each hydrogen atom breaks free from its nucleus, yielding independent ion and electron fluids. The resulting electrically active gas, called plasma, can carry enormous electric currents; it is strongly responsive to electromagnetic fields, while at the same time able to produce strong fields on its own. Thus the operating fluid in any fusion device is plasma, a form of matter more electro-dynamically active than any conventional liquid, solid or gas.

In summary, the key features of D–T fusion are:

1. an operating temperature in the hundred-million degree range, with the result that the working gas is necessarily in the plasma state;
2. an energy release primarily in the form of very fast alpha particles and neutrons, whose energy must be captured to provide the thermal output of the reactor;
3. the need to breed tritium from the D–T neutron and lithium.

Heating and confinement

Evidently the most basic tasks in constructing a fusion reactor are to heat a hydrogen gas to thermonuclear temperatures, and then to confine the resulting plasma for a time long enough for fusion reactions to take place, thus maintaining the high temperature. In most reactor designs heating is provided by a combination of driving electric currents through the plasma, directing energetic particle beams at the plasma, and energizing plasma particles by means of radio-frequency electromagnetic radiation, similar to the heating mechanism of a microwave oven.

Confinement is measured by the so-called energy confinement time, denoted by τ_E. Since both reaction rates and energy loss rates depend upon the plasma density n, the required value of τ_E depends on plasma density. It turns out that the critical parameter is the product $n\tau_E$; when density is measured in ions per cubic centimeter and τ_E in seconds, sufficient confinement has been achieved if the product exceeds about 10^{14} sec/cm^3 (the "Lawson criterion"). [NOTE: This paragraph is a little technical for a general primer, but it seems to work.]

One way to satisfy the Lawson criterion is to compress a hydrogen pellet to extreme density values, exceeding the density of conventional solids, while allowing relatively short confinement times. This is the approach taken by the *inertial confinement* program. The main arm of international fusion research uses much lower densities-lower even than the density of air at the Earth's surface. Thus the working fluid is a rarefied plasma, whose low density is part of the reason for the intrinsic safety of the device. The relatively long confinement time thereby required is supplied by magnetic fields, taking advantage of the plasma's strong response to such fields. This line of research is called *magnetic fusion,* although the phrase "magnetic confinement for fusion" would be more descriptive.

Magnetic confinement

Neon signs confine cold plasma in glass tubes. But a very hot, rarefied plasma—a fusion plasma—could not maintain thermonuclear temperatures if it had substantial contact with a material wall. At the densities used in magnetic fusion, plasma resting against a wall will quickly cool, bringing fusion reactions to a halt. So the confining magnetic field must protect the plasma from being quenched by contact with its bounding vessel. A magnetic field configured to provide this confinement is traditionally called a "magnetic bottle."

A magnetic bottle can work because charged particles—the ions and electrons that constitute a fusion plasma—spiral around the local field direction in helical orbits; the stronger the field, the tighter the helix. Thus, while motion parallel to the field is unaffected, motion perpendicular to the local field direction is strongly inhibited.

This inhibition of perpendicular motion has two effects. First, it allows the magnetic force to act against plasma pressure, pushing plasma away from the vessel wall. This profile control is especially effective when a *divertor*—a magnetic geometry in which the outermost field lines are diverted into an external chamber—is employed. In this case the layer of plasma near the vessel wall has especially low density, imposing a near vacuum between the inner plasma core and the wall.

The second insulating effect of the magnetic field pertains to dissipative transport. The inhibition of perpendicular motion affects plasma diffusion and heat conduction: transport in directions transverse to the field is sharply reduced, while transport parallel to the field is unaffected. For an appropriate field configuration this anisotropy markedly slows the conduction of heat from the fusion plasma core to the boundary region. Notice that this effect acts throughout the plasma volume, not only near the wall.

It is significant that while a magnetic bottle can reduce plasma contact with material boundaries, such contact is not eliminated. The residual contact is sufficiently tenuous to maintain a hot plasma interior, but still problematic because the wall material can be scarred. Aside from the obvious lifetime aspects of such erosion, plasma-wall interaction can allow impurities from the wall to enter the confinement region, with deleterious effects on both confinement and fusion reaction rates. Thus, significant materials-physics issues arise in the fusion quest.

A centuries-old theorem in topology shows that any closed surface on which the magnetic field does not vanish must have the topology of a torus: a magnetic bottle must be *toroidal*—donut-shaped. All the devices consider by ReNeW resemble donuts in this sense. (So-called "magnetic mirrors" get around the topological theorem by "plugging" the ends of a cylindrical field configuration; the mirror approach to confinement was not part of the purview of this ReNeW.) Since the only source of a magnetic field is electric current, magnetic confinement is based on electric currents flowing around or within some toroidal surface.

Most confinement devices employ a combination of external currents, in wire-wound coils, and internal currents, flowing within the plasma itself, to maintain the toroidal field structure. A prominent example is the *tokamak,* in which external and internal currents combine to yield a confining field that is symmetric with respect to a central axis. Other confinement schemes have yet to achieve the tokamak's level of performance but could bring operating advantages. For example, the stellarator deliberately breaks the field symmetry in order to simplify steady-state operation. And there are schemes under investigation that require relatively weak (and therefore less expensive) external magnetic fields.

Constructing a magnetic bottle does not solve the problem of confinement; there are essentially two additional hurdles. First, plasma currents, arising spontaneously from electromagnetic and fluid instability, can create magnetic fields that open up the bottle. Second, even when the magnetic configuration is stable with regard to gross distortion, localized "micro-instabilities" can produce fluctuations that degrade confinement. Common versions of such accelerated transport resemble boiling water on a stove: the water remains in the pot, but its turbulent motion rapidly conducts heat from the hot bottom to the cooler upper surface.

In the last decades of the twentieth century fusion research gained important scientific victories in plasma confinement: major advances in both the control of instability and the amelioration of turbulent transport. While significant confinement issues remain to be resolved, and while the fusion scientific community looks forward to substantial further improvements, the present demonstrated level of confinement is sufficient to impart confidence in the future of magnetic fusion energy.

Heating and confinement are the central, but not the only, challenges that must be faced before fusion power can be realized. Even a perfectly confined plasma at thermonuclear temperature must be fueled with reactant, it must be promptly cleansed of the helium that fusion produces, its thermal energy yield must be effectively retrieved, and so on. Such challenges occupy increasing research attention as the fusion program matures; they are the subject of major attention by ReNeW.

BIOGRAPHY FOR STEWART C. PRAGER

Stewart Prager is Director of the Princeton Plasma Physics Laboratory, a Department of Energy national laboratory, and Professor of Astrophysical Sciences at Princeton University. He received his Ph.D. degree in plasma physics from Columbia University in 1975. Following two years performing fusion energy research at General Atomics in San Diego he joined the University of Wisconsin Madison as an Assistant Professor of physics. Prager remained at the University of Wisconsin, as a Professor of physics, until 2009 when he assumed his position at Princeton.

Prager's research focuses on basic plasma physics, including applications to fusion energy and, more recently, applications to astrophysics. He has worked to advance the understanding and control of spontaneous plasma processes, such as turbulence, transport, and processes characterized under the umbrella of magnetic self-organization. While at Wisconsin, Prager was director of the Madison Symmetric Torus (MST) experimental facility supported by DOE. He also served as Director of the Center for Magnetic Self-Organization in Laboratory and Astrophysical Plasmas, established through the National Science Foundation program of "physics frontier centers."

Prager has participated in numerous scientific planning and advisory processes, including service as the chair of the DOE's Fusion Energy Sciences Advisory Committee, as Chair of the Division of Plasma Physics of the American Physical Society (APS), and as President of the University Fusion Association. He is also a co-recipient of the APS Dawson Prize for Excellence in Plasma Physics, a fellow of the APS, and a recipient of the Leadership Award of Fusion Power Associates.

Chairman BAIRD. Thank you.

Dr. Mason.

STATEMENT OF DR. THOMAS E. MASON, DIRECTOR, OAK RIDGE NATIONAL LABORATORY

Dr. MASON. Mr. Chairman, Ranking Member Inglis and Members of the Committee, thank you for the opportunity to appear before you today. My name is Thom Mason. I am the Director of the Department of Energy's Oak Ridge National Laboratory (ORNL), and unlike the other members of the panel, I am not an expert in fusion. But as Director of DOE'S largest multipurpose laboratory, I oversee a broad program of energy-related R&D that includes magnetic fusion, and it is from that perspective that I see fusion research as an essential part of the Nation's energy R&D portfolio.

You have heard how this is a promising source of energy that uses widely available fuel and produces no greenhouse gas emissions or long-lived radioactive waste. In fact, one could say that the fuel for fusion is smart people and high-end manufacturing, and so from that point of view, from the point of view of U.S. competitiveness and the type of energy source that is worth seeking, I think fusion is significant. Its science and technology base is now mature enough to warrant a significant investment in determining our readiness to advance to a prototype fusion reactor.

ITER is an international project to demonstrate the scientific and technological feasibility of fusion energy. It is being built at Cadarache in France by seven partners: the United States, Russia, the European Union, Japan, China, South Korea and India. Each partner is responsible for a share of the hardware, personnel and cash contributions towards common expenses. This international partnership presents an extraordinary number of technical and management challenges. ITER will be twice the size of the largest existing fusion experiment. It is a first-of-a-kind experimental facility made up of a large number of complex systems provided by suppliers all over the world and they must be integrated into a device that can function under extremely demanding challenges. The ITER organization has also faced the challenge of standing up and staffing a new multinational organization to provide coordination, project management, integration and engineering while overseeing efforts to finalize the design and supervise construction at Cadarache. Given these challenges, it is not surprising that there have been some teething pains. For example, in the United States we have struggled to secure funding for ITER during some very tough budget years, but now with the strong support provided by Congress in fiscal year 2009, for which we are very grateful, we are on a sound footing.

Today the ITER organization has two urgent tasks: completing the overall design and establishing realistic cost and schedule baselines. The U.S. fusion community is fully engaged in the execution of these tasks.

Oak Ridge has hosted and led the U.S. ITER project office since 2006. We are responsible for all U.S. activities supporting ITER construction. The estimated cost of these activities is between $1.4 and $2.2 billion, so this is a heavy responsibility and it is one that we take very seriously. The office was located at Oak Ridge to take advantage of project management expertise developed during the Spallation Neutron Source project, which as you have heard was a $1.4 billion neutron-scattering facility that was designed and built by a partnership of six Department of Energy national laboratories. It was completed on scope, on schedule and on budget in 2006. We are working with other national laboratories, industry and universities to deliver the U.S. contributions to ITER.

Recently, two contracts worth $34 million were awarded to U.S. companies: one in Waterbury, Connecticut, and one in Carteret, New Jersey. The New Jersey supplier has also received a contract from the European Union's ITER domestic agency. This speaks well of the ability of U.S. companies to compete internationally for work supporting ITER. More than 160 companies and universities in 33 states have worked directly on the project, and many others are interested in future procurements. The U.S. ITER team also provides substantial support to the international organization by developing systems engineering procedures, technical baseline documents, project management plans and so forth.

As ITER proceeds through construction into operation, Oak Ridge will continue to play a substantial role in fusion and the U.S. ITER project will remain a high priority. We will use our distinctive capabilities in materials R&D, nuclear technology and high-performance computing to advance fusion science, technology and engineering.

One specific focus will be a next-generation fusion nuclear science facility to answer questions that lie outside of ITER's scope. Our strengths at Oak Ridge position us to lead the technical and programmatic planning for this facility and we will work with the U.S. community to bring it into being at an appropriate pace.

ITER represents an opportunity for the DOE national laboratories, U.S. universities and U.S. industry. We are now positioned to make substantial contributions to ITER and to reap the rewards it will provide in terms of increased scientific knowledge, high-tech jobs that will help us rebuild U.S. manufacturing capacity, and training for fusion scientists and engineers who work on ITER and bring home what they learn. A sustained investment in ITER is essential to realizing the benefits of this extraordinary effort.

We also need a vibrant domestic fusion program to take advantage of the knowledge gained from ITER and to continue advancing toward commercial fusion power. ITER is a major step forward, but it will not answer all of our questions.

Congresswoman Zoe Lofgren has introduced a bill calling for a comprehensive plan to identify the R&D facilities needed to ensure the realization of practical fusion energy, and this is a vital step in setting the direction of the U.S. fusion program. The bill also calls for investing in U.S. capability in fusion engineering science. This will enable us to develop the materials and enabling technology needed to realize the full benefit of ITER and to take the next steps toward a fusion demonstration facility.

Sustained support for fusion engineering science and facilities is essential to successful development of this future energy source. As we search for sustainable energy solutions, we need a balanced R&D portfolio that includes both near- to mid-term improvements in energy efficiency, renewables and fission, along with electrification of our transportation sector, and fusion as a source of clean, safe and abundant baseload power in the long-term interest.

Thank you again for the opportunity to testify. I welcome your questions on this important topic.

[The prepared statement of Dr. Mason follows:]

PREPARED STATEMENT OF THOMAS E. MASON

Mr. Chairman, Ranking Member Inglis, and Members of the Committee: Thank you for the opportunity to appear before you today. My name is Thomas E. Mason, and I am Director of the U.S. Department of Energy's Oak Ridge National Laboratory (ORNL) in Oak Ridge, Tennessee. It is an honor to provide this testimony on the status of the ITER international fusion project, the role of ORNL as the headquarters of the U.S. ITER Project Office, and the way that fusion research fits into the overall portfolio of research and development (R&D) at ORNL.

INTRODUCTION

ORNL is the Department of Energy's largest science and energy laboratory. From my position as Director of a national laboratory with research encompassing fundamental science of relevance to energy through an extensive suite of energy programs—including energy efficiency; energy from renewable, fossil, and fission sources; and energy transmission and distribution—I view fusion as an essential part of the Nation's R&D portfolio. Fusion is a promising long-term source of energy whose fuel is widely available and whose emissions would include neither CO_2 nor long-lived radioactive waste. Its scientific and technological basis is maturing and warrants a significant federal investment, with the aim of advancing the underlying science and gaining understanding of the technology sufficient to enable future decisions on advancing to the level of a prototype reactor.

ORNL has been engaged in research on fusion energy since the early 1950s, when the Atomic Energy Commission launched Project Sherwood with the goal of developing a fusion analog to the fission reactor. From its earliest days, the Oak Ridge fusion program has drawn on the diverse resources afforded by ORNL's standing as a multi-program laboratory, and it has leveraged substantial investments by the Department of Energy in materials science, nuclear technology, and high-performance computing to deliver advances in plasma theory and simulation, magnetic confinement experiments, plasma heating and fueling, atomic physics, and materials development.

As soon as magnetic fusion research was declassified in 1958, the ORNL program initiated extensive collaborations with the international fusion community, which continue today. In particular, ORNL has been a key contributor to ITER since the inception of this activity in 1985.

The promise of fusion as a clean and abundant source of energy has driven extensive programs of R&D, at ORNL and other institutions throughout the world, for more than six decades. Impressive progress has been made in overcoming the challenges of harnessing fusion energy. From experiments in the United States and other nations, we have established the scientific and technical knowledge base for fusion, and we have reached a point at which the next step is to create a burning plasma: that is, an ionized gas in which the alpha particles produced by the fusion of hydrogen isotopes provide enough heat to keep the fusion reaction going.

With the potential to provide clean baseload electrical energy without a fuel resource constraint, fusion can be an important component of a long-term shift away from fossil fuels with the attendant environmental, economic, and national security benefits. The main cost lies in the intellectual content and high-end manufacturing, both of which are hallmarks of American industrial strength, so in addition to providing an attractive solution to our energy needs, fusion offers the potential to drive the development of a new industry.

THE ITER INTERNATIONAL FUSION PROJECT

The ITER international fusion project has been established to construct an experimental device that will demonstrate the scientific and technological feasibility of fusion energy and achieve sustained fusion power generation. The long-range goal is for ITER to produce at least ten times as much power as is needed to heat the plasma. It will test many of the key technologies needed to use fusion as a practical energy source, and it will provide industry with the opportunity to validate production techniques for components needed for future fusion power plants.

ITER will be constructed at Cadarache in southeastern France from components fabricated in the countries of the ITER Members: the United States, the Russian Federation, the European Union, Japan, the People's Republic of China, South Korea, and India. A Joint Implementation Agreement, finalized in 2007, governs the details of construction, operation, and decommissioning, as well as financing, organization, and staffing. Each ITER Member is responsible for supplying a share of hardware (including supporting R&D and design); personnel assigned to the ITER site; and cash contributions toward common expenses. The international ITER Organization established by the Joint Implementation Agreement is the legal entity responsible for project execution. It is governed by a Council that includes senior U.S. Department of Energy officials.

Each ITER Member was tasked with creating a Domestic Agency to fulfill the Member's obligations under the ITER Joint Implementation Agreement. The Domestic Agencies' role is to perform R&D and design and to procure each Member's in-kind (i.e., non-cash) contributions to ITER. The Domestic Agencies employ their own staff, have their own budget, and place contracts with suppliers. The United States was the first ITER Member to establish its Domestic Agency under the auspices of the Office of Fusion Energy Sciences within DOE's Office of Science. This is the U.S. ITER Project Office, about which I will speak further in a moment.

Under the terms of the Joint Implementation Agreement, the United States is a full Member of the ITER project. Our 9.09 percent share of the total cost gives us access to all scientific data and the right to propose and carry out experiments. It also creates opportunities for U.S. industry to manufacture the high-technology components that make up roughly 80 percent of our contribution.

The ITER project presents an extraordinary number of technical and management challenges. Although the design of ITER is not yet complete, it is expected to be twice the size of the largest existing fusion experiment. It is a "first-of-a-kind" experimental facility comprising a large number of systems, some of which require innovative technologies. These systems, to be constructed by suppliers selected by the seven Domestic Agencies, must be integrated to produce a system that can perform under extremely demanding conditions.

The ITER Organization has also faced the challenge of standing up and staffing a new organization to provide coordination, project management, technical integration, and engineering while overseeing efforts to finalize the ITER design and supervising early-stage civil construction in Cadarache. A host of issues relating to finances, communication, intellectual property rights, conflicting national safety and import/export regulations, and other areas unique to this large-scale, high-visibility multinational scientific collaboration have had to be resolved to the satisfaction of all parties.

Given these challenges, it is not surprising that the project has experienced some "teething pains." We have not been immune to those teething pains in the United States as we struggled to secure funding during some very tough budget years; however, with the support provided by Congress in FY 2009 we are now on a sound footing and able to fully engage our international partners. The most urgent tasks facing the international ITER Organization today are completing the overall ITER design and systems engineering and establishing realistic schedule and cost baselines. The U.S. fusion community is supporting these tasks, while continuing to carry out an extensive program of work that is enhancing the physics basis and technology support for ITER.

THE ROLE OF ORNL AS HEADQUARTERS OF THE U.S. ITER PROJECT OFFICE

Since 2006, ORNL has hosted and led the U.S. ITER Project Office, which is responsible for project management of all U.S. activities to support construction of ITER. The U.S. share of the international ITER project construction has an estimated range of $1.4 billion to $2.2 billion, so this is a heavy responsibility and one that we at ORNL take very seriously.

All U.S. ITER activities are managed by the Department of Energy's Office of Science as a Major Item of Equipment (MIE) project and are subject to rigorous review. The project team under ORNL includes Princeton Plasma Physics Laboratory and Savannah River National Laboratory as partner laboratories.

The U.S. ITER Project Office was located at Oak Ridge to take advantage of the project management expertise developed during the construction of the Spallation Neutron Source. This $1.4 billion neutron scattering facility was designed and constructed by a partnership of six Department of Energy national laboratories, which I had the privilege of leading from 2001 to 2006. The project was completed ahead of schedule and within budget in 2006, and many members of the project team are now applying their expertise to the needs of the U.S. ITER Project Office.

The U.S. ITER team is engaging other national laboratories and industry and university partners across the United States in R&D, engineering, manufacturing, and fabrication of the U.S. contributions to ITER. Earlier this month, the U.S. ITER Project Office awarded two contracts totaling $33.6 million, one to a company in Waterbury, Connecticut, and the other to a company in Carteret, New Jersey, for components of the superconducting magnets that will confine the ITER plasma. It is noteworthy that in addition to these U.S.-funded contracts, a similar award has been made to the New Jersey supplier by the European Union's ITER Domestic Agency, which speaks well of the ability of U.S. industry to compete in this area on the world stage. To date, more than 160 companies and universities in 33 states have worked directly on the project, and some 140 have expressed interest in future procurements.

The U.S. ITER team is also providing substantial support to the international ITER Organization. Staff have contributed to the development of systems engineering procedures and technical baseline documents, assisted in the development of project management processes and procurement arrangements, and evaluated project risks and assisted with development and implementation of risk mitigation plans.

FUSION ENERGY RESEARCH AT ORNL OVER THE NEXT 20 YEARS

As the ITER project moves through construction and operation, ORNL will continue to play a substantial role, both through the U.S. ITER Project Office and through an extensive and well-integrated program of science, technology, and engineering aimed at supporting ITER and developing the understanding required for an attractive fusion energy source.

In particular, we will take advantage of ORNL's distinctive capabilities in materials R&D, nuclear technology, and high-performance computing to deliver the science and technology needed to realize the full potential of ITER and to exploit the knowledge gained from it in advancing toward a fusion power plant. Expertise in nuclear design and operations, nuclear materials science, ITER, fusion engineering, and project management positions ORNL to lead U.S. technical and programmatic planning for a next-generation fusion nuclear science facility. Such a facility and associated R&D programs could establish the scientific basis for fusion fuel self-sufficiency and reliable and efficient power extraction under realistic fusion power reactor conditions.

CLOSING REMARKS

The international ITER project represents an opportunity for the Department of Energy's national laboratories, U.S. universities, and U.S. industry to play a key role in a very challenging technical development and build a scientific and technical base for moving the fusion program from a science experiment to an engineering demonstration. The United States is positioned to make substantial contributions to the international ITER project and to reap the rewards that it will provide: increased scientific knowledge, high-technology jobs that can contribute to the restoration of U.S. manufacturing capacity, and training of fusion scientists and engineers who have the opportunity to work on this experiment with their colleagues from other nations and to apply the findings to the next generation of fusion systems. Sustaining the U.S. investment in ITER is essential to realizing the benefits of this extraordinary effort.

Our investment in ITER should be complemented by a vibrant domestic fusion program to ensure that the United States is positioned to exploit ITER for research, capitalize on the knowledge gained from ITER, and move forward along the way to commercial fusion power. While ITER represents a path-breaking advance toward the goal of practical magnetic fusion energy, it cannot address all of the questions that must be answered before we can proceed with a fusion power plant. For example, ITER is based on a magnetic confinement concept known as the tokamak, which was invented in Russia in the 1960s. This configuration was selected for ITER because of its maturity, but other configurations have properties that may make them attractive candidates for commercial power plants. Other challenges that lie outside ITER's scope include the development of materials and components that can withstand the intense conditions at the edge of a burning plasma and handle prolonged exposure to neutrons.

Legislation introduced by Congresswoman Zoe Lofgren, the *Fusion Engineering Science and Fusion Energy Planning Act of 2009* (H.R. 3177), calls for the development of a comprehensive plan to identify what the U.S. fusion community must do to ensure the realization of practical fusion energy. This is a vital step in determining the direction of the U.S. fusion program, and it has the full support of the program's leadership.

Congresswoman Lofgren's bill also calls for a targeted investment of $165 million over the next three years to enhance U.S. capability in fusion engineering science, in addition to the funding provided to the Department of Energy's Office of Fusion Energy Sciences for current programs. This would provide the U.S. fusion community with resources for developing the materials and enabling technology needed to realize the full benefit of the ITER project and to prepare for the experiments, such as a fusion nuclear science facility, needed to move beyond ITER to a successful fusion demonstration facility.

Some might argue that the investment of substantial sums in fusion R&D over the past six decades should have enabled us to reach the goal of fusion energy by now. In response to such an argument, I would make two points. First, controlled fusion has turned out to be a much more challenging scientific and technological problem than was originally thought. Optimistic predictions based on an incomplete understanding of the difficulties involved have haunted the program in the past. Today, however, we have attained a level of understanding that provides a solid foundation for ITER and for continuing efforts to find ways of meeting our energy needs with fusion.

Second, in 1972, federal funding for magnetic fusion energy was $33.3 million (about $172 million in today's dollars); it rose dramatically in response to the energy crisis, peaking in 1977 at roughly $1 billion in today's dollars, and then declined precipitously, to $230 million in 1997 (about $300 million in today's dollars) and has remained close to that level. The FY 2010 Energy and Water Appropriations bill passed by the Congress allocates $426 million for fusion energy sciences, which includes $135 million for the U.S. contribution to ITER. While much useful science and engineering has been accomplished at these funding levels, it is unlikely that we will be able to make the final leap to practical fusion power without sustained support for fusion engineering science and facilities for answering the questions that lie outside ITER's scope.

Ambassador Kaname Ikeda, ITER Director General, has pointed out that the current world energy market is about $3 trillion and growing. The amount invested in energy R&D generally (not just in fusion) is very modest when compared with the economic value of the market; this is in sharp contrast to the situation in industries such as information technology or health sciences, despite the fact that the benefits to society and the scientific and technical challenges are no less significant.

Perhaps even more important, most of the world's energy needs are now being met with nonrenewable fossil fuels that represent the primary source of the greenhouse gases that are contributing to climate change. As a safe and essentially inexhaustible source of baseload power that emits no greenhouse gases, fusion would be a sustainable energy solution for the long-term.

This is not to say that improvements in energy efficiency, renewables, and fission, combined with electrification of our transportation sector, are not key near-term to medium-term challenges that we must address. But given that there is no single element of energy R&D that will yield supplies sufficient to meet our overall objectives of reducing the environmental consequences of CO_2 and other emissions and the national security and economic consequences of a growing reliance on imported petroleum, fusion needs to be an element of a balanced energy R&D portfolio. Answering the remaining key science questions about the feasibility of fusion, which is a central focus of ITER, will enable us to shift our focus to the technological and engineering challenges of fusion as a power source.

Thank you again for the opportunity to testify. I welcome your questions on this important topic.

Biography for Thomas E. Mason

Thomas Mason is a native of Dartmouth, Nova Scotia, in Canada. He graduated from Dalhousie University in Halifax, Nova Scotia, with a Bachelor of Science degree in physics and completed his postgraduate study at McMaster University in Hamilton, Ontario, Canada, receiving a Doctor of Philosophy degree in experimental condensed matter physics.

After completing his Ph.D., he held a postdoctoral fellowship at AT&T Bell Laboratories in Murray Hill, New Jersey, and then became a Senior Scientist at Riso National Laboratory in Denmark. In 1993 he joined the faculty of the Department of Physics at the University of Toronto.

Thom joined Oak Ridge National Laboratory (ORNL) in 1998 as Scientific Director for the Department of Energy's Spallation Neutron Source (SNS) project. In April 2001 he was named Associate Laboratory Director for SNS and Vice President of UT–Battelle, LLC, which manages ORNL for the Department. In 2006 he became Associate Laboratory Director for Neutron Sciences, leading a new organization charged with delivering safe and productive scientific facilities for studying of structure and dynamics of materials. In May 2007, Thom was named Director of Oak Ridge National Laboratory.

Thom's research background is in the application of neutron scattering techniques to novel magnetic materials and superconductors using a variety of facilities in North America and Europe. He is co-author of more than 100 refereed publications and an Associate of the Quantum Materials Program of the Canadian Institute for Advanced Research. In 1997, he was awarded an Alfred P. Sloan Foundation Research Fellowship. Thom was named a Fellow of the American Association for the Advancement of Science in 2001 and a Fellow of the American Physical Society in 2007. He received the Distinguished Alumni Award for the Sciences from McMaster University in 2008.

Thom and his wife, Jennifer MacGillivray, also a native of Nova Scotia, live in Oak Ridge with their two sons, William and Simon.

Chairman BAIRD. Dr. Betti.

STATEMENT OF DR. RICCARDO BETTI, PROFESSOR, MECHANICAL ENGINEERING & PHYSICS AND ASTRONOMY; SENIOR SCIENTIST AND ASSISTANT DIRECTOR FOR ACADEMIC AFFAIRS, LABORATORY FOR LASER ENERGETICS, UNIVERSITY OF ROCHESTER

Dr. BETTI. Mr. Chairman, Ranking Member Inglis and Members of the Committee, I am Riccardo Betti, Professor at the University of Rochester. Thank you for inviting me to testify today about the status of inertial fusion energy research and a vision for the future.

Inertial fusion uses the same thermonuclear reactions and the same hydrogen fuel as magnetic fusion. Like gasoline in the cylinder of a car engine, fusion fuel must be ignited in order to produce useful energy. An ignited fuel can produce fusion energy that can greatly exceed the input energy. If the energy output is greater than the input, then we have an energy gain and only then fusion becomes an energy source. Thermonuclear ignition has been a scientific quest since the 1950s. Like no other time in history, we are now close to demonstrating ignition and energy gains in the laboratory.

The path towards economically viable inertial fusion energy involves three crucial elements: first, the demonstration of ignition; second, the demonstration of high energy gains; and third, the development of the technology for a power plant. In the near future, the National Ignition Facility, the NIF, at Lawrence Livermore National Laboratory, by far the world's largest laser, is expected to achieve the first demonstration of thermonuclear ignition in the laboratory by compressing a tiny pellet of solid cryogenic hydrogen fuel using lasers.

The current status of inertial fusion energy research in the United States is dominated by the National Ignition Campaign with a goal of achieving ignition on the NIF. The National Ignition Campaign is funded for reasons of national security by the Stockpile Stewardship Program under the National Nuclear Security Administration, the NNSA. In parallel to its national security mission, the National Ignition Campaign will be able to address many aspects of the physics principles of inertial fusion energy including ignition and energy gain. The National Ignition Campaign involves many institutions and major NNSA facilities. It is crucial to provide adequate funding to the National Ignition Campaign because achieving thermonuclear ignition in the laboratory is a milestone in the development of science and energy security. This goal should not be undermined by lack of funding. Not now, since we are so close to achieving ignition.

The next step after ignition is the demonstration of high energy gain. For a viable power plant, the fusion energy output must greatly exceed the input energy to the fuel by more than 100 times. It is unlikely that the NIF will achieve the high gains required for inertial fusion energy. The current configuration of the NIF will test one approach to inertial fusion, the indirect drive approach. Other inertial fusion concepts like direct drive, fast ignition and others funded through the Office of Fusion Energy Sciences, the OFES, and NNSA have the potential to generate the gains needed for inertial fusion energy. Some of these concepts can be tested on existing NNSA facilities.

But unfortunately, the very limited access to these facilities constitutes a serious impediment to progress in this important area and to achieve the wide energy gains for inertial fusion energy. OFES and NNSA have already formed a joint program to support high-energy-density physics research. This partnership should be strengthened to increase access to NNSA facilities to study high-gain inertial fusion energy concepts.

Achieving ignition and high gain does not imply that economically attractive fusion energy is just around the corner. Major technological and engineering challenges will still remain even after ignition. Before starting a major energy development program, it is prudent to undertake an assessment of the different options. This can begin immediately with a small exploratory technology program. A power plant requires a driver to compress the pellet, a target chamber and many other systems. The driver is the most complex and expensive component to the power plant. Several drivers have been proposed. Lasers are the most developed drivers. Other drivers would likely require longer development paths. An exploratory technology program should be started with the goal of assessing and selecting the most attractive driver in order to move quickly towards an expanded energy development program once the National Ignition Facility has demonstrated ignition and energy gain.

Thank you, Mr. Chairman, for allowing me to testify on the next generation of fusion energy research.

[The prepared statement of Dr. Betti follows:]

PREPARED STATEMENT OF RICCARDO BETTI

EXECUTIVE SUMMARY

Nuclear fusion powers the sun and other stars. Harnessing fusion energy has been a scientific quest since the 1950s. Inertial and magnetic confinement fusion are the main approaches to fusion energy pursued in the U.S. Both approaches use a 50–50 mixture of hydrogen isotopes (deuterium and tritium) as fuel. Like all advanced energy sources, inertial fusion requires a scientific demonstration of validity of the concept and a technology program to develop a viable power plant. The path to inertial fusion energy (IFE) involves three elements:

- The demonstration of the physics principles of controlled inertial fusion: thermonuclear ignition and burn of deuterium-tritium (DT) fuel
- The demonstration of high energy gain from DT fuel
- The development of the technology for an IFE power plant.

Demonstration of Ignition and Burn: *In the near future, the National Ignition Facility (NIF) at Lawrence Livermore National Laboratory (LLNL) is expected to achieve the first demonstration of thermonuclear ignition and moderate energy gain in the laboratory using lasers.* In the indirect-drive approach to inertial fusion, the laser is used to heat a small metallic enclosure (a "hohlraum") to high temperatures. The heated metal of the hohlraum wall emits x rays that irradiate a tiny pellet of cryogenic solid DT fuel. The pellet implodes, achieving extreme pressures and temperatures that turn the solid DT into hot dense plasma producing copious amounts of nuclear fusion reactions (what is called "a burning plasma"). Thermonuclear ignition is a thermal instability that causes the plasma to self-heat through a runaway process where fusion reactions increase the plasma temperature that in turn induces more fusion reactions. An ignited plasma can produce fusion energy that can greatly exceed the input energy required to produce the plasma. The process of laser irradiation, pellet implosion, thermonuclear ignition and energy gain is usually referred to as "target physics." Demonstrating thermonuclear ignition and energy gain in the laboratory has been a goal of fusion energy research for the past five decades, and is widely considered a milestone in the development of fusion energy, as well as a major scientific achievement.

The current status of IFE research in the U.S. is dominated by the National Ignition Campaign (NIC). The NIC is funded for reasons of national security by the Stockpile Stewardship Program under the National Nuclear Security Administration (NNSA). In parallel to its national security mission, the NIC will be able to address many aspects of burning-plasma physics relevant to inertial fusion energy and will demonstrate the physics principles of IFE. The NIC involves many institutions (LLNL, LLE, LANL, General Atomics and SNL) and major NNSA facilities (NIF, OMEGA and Z). Many diagnostics and experimental setups are validated on smaller facilities (mostly on OMEGA at the Laboratory for Laser Energetics) before installation on the NIF.

> **Recommendation:** *It is crucial to provide adequate funding to the National Ignition Campaign. Achieving thermonuclear ignition in the laboratory is a milestone in the development of science and energy security. This goal should not be undermined by lack of adequate funding.*

Demonstration of High Energy Gain: The next step in target physics after ignition is the demonstration of high energy gain. For a viable IFE power plant, the fusion energy output must greatly exceed the input energy to the plasma. Energy gain is the ratio between energy output and input. It is unlikely that the NIF will achieve high gains (> 100) in the laser indirect-drive configuration.

The 2009 FESAC report[1] states that "Alternative IFE concepts [laser direct-drive, fast ignition, heavy ion fusion and others] funded through OFES and NNSA have the potential to generate the gains needed for IFE." *Present research in alternative IFE concepts is funded by DOE's Office of Fusion Energy Sciences (OFES) and NNSA, with NNSA providing limited access to their facilities. Limited access to the NNSA facilities constitutes a serious impediment to progress in this important area and to the achievement of high energy gains for inertial fusion energy.* While the NIF is currently configured to fully validate the scientific principles of the laser indirect-drive approach, it can also test the laser direct-drive approach with very modest changes to the existing laser system. The direct-drive approach is simpler since the laser directly irradiates the solid pellet. It is also more efficient since it eliminates the need for the intermediate process of conversion of laser light into x rays.

> **Recommendation:** *OFES and NNSA have already formed a joint program to support high energy-density physics research. This partnership should be strengthened to increase access to NNSA facilities for research in the area of high-gain inertial-fusion-energy concepts. Experiments on the NIF should be carried out to demonstrate ignition and energy gain with the laser direct-drive approach.*

Development of the Technology: *Achieving ignition and high gain does not imply that economically attractive fusion energy is just around the corner. Major technological and engineering challenges will still remain even after the demonstration of ignition. The development of a viable fusion power plant requires large scientific and financial investments.* Before launching a major energy development program, it is prudent to undertake an assessment of the different driver options. This can begin immediately with a small exploratory IFE technology program ("small" here is used for comparison with the "large" science program of the National Ignition Campaign that received $458M in the FY10 Appropriations bill).

Several drivers have been proposed: solid-state and Krypton-Fluoride (KrF) lasers, Z pinches and heavy ion beams. The driver compresses the pellet and is the most complex and expensive component of an IFE power plant. Drivers are part of an integrated system including a target chamber, injection systems and other components. Drivers must operate with relatively high repetition rates to produce enough average power output. *Lasers are the most developed drivers. Small-scale high-repetition-rate KrF and solid-state lasers have been built and operated. Research in target physics for laser drivers is also the most advanced. The current experimental campaign will explore ignition with lasers implying that the target physics issues will only be resolved for laser drivers. Other drivers will likely require longer development paths for both the technological development and target physics.* An exploratory IFE program should be started with the goal of assessing and selecting the most attractive driver option in order to move quickly towards an expanded energy development program once the NIF has completed the ignition campaign and reliably demonstrated fusion-energy gains. Such a program should also assess the viability of fusion-fission hybrid systems where a blanket of fissionable material surrounding the fusion reactor is used to amplify the fusion-energy output. Funding for research in IFE technology has been eliminated in 2009 and no plans are in place to support it in the near future.

> **Recommendation:** *It would be beneficial to immediately initiate an exploratory fusion technology program in parallel to the ignition campaign to assess the viability of the different driver options. If successful, such a program will select the most attractive driver by the completion of the ignition campaign on the NIF.*

Status of Inertial Fusion Energy Research and Vision for the Future

Nuclear fusion powers the sun and other stars. Fusion involves the merging (e.g., fusing) of light elements. Harnessing fusion energy has been a scientific quest since the 1960s. Inertial and magnetic confinement are the main approaches to fusion energy pursued in the U.S. Both approaches use a 50–50 mixture of hydrogen isotopes (deuterium and tritium). Deuterium is abundant and can be extracted easily from sea water. Tritium must be obtained by breeding with lithium, and lithium is a readily available light metal.

Like all advanced energy sources, inertial fusion requires a scientific demonstration of viability of the concept and a technology program to develop a viable power plant. The path to inertial fusion energy (IFE) involves three elements:

[1] Fusion Energy Science Advisory Committee (FESAC), *Advancing the Science of High Energy Density Laboratory Plasmas,* U.S. Department of Energy, Office of Science, January 2009.

(1) The demonstration of the physics principles of controlled inertial fusion: thermonuclear ignition and burn of deuterium-tritium (DT) fuel

(2) The demonstration of high energy gain from DT fuel

(3) The development of the technology for an IFE power plant.

1. Demonstrating Controlled Thermonuclear Ignition and Burn

The demonstration of ignition and burn is the goal of the National Ignition Campaign (NIC). The NIC is funded for national security reasons by the Stockpile Stewardship Program under the National Nuclear Security Administration. The NIC involves many institutions (LLNL, LLE, LANL, General Atomics and SNL) and major NNSA facilities (NIF, OMEGA and Z). Many diagnostics and experimental setups are validated on smaller facilities (mostly on OMEGA at the Laboratory for Laser Energetics) before installation on the NIF.

Finding: *The National Ignition Campaign aims at demonstrating ignition and moderate fusion-energy gains in the next few years on the National Ignition Facility (NIF). Preparatory work is under way and the first attempts to ignition are set to begin at the end of FY10 on the NIF.*

Two recent highlights of the National Ignition Campaign are worth mentioning.

(1) Early experiments on the National Ignition Facility have shown good performance of the NIF laser and good coupling of the laser energy to the target. The NIF has already delivered energies exceeding one megajoule (one megajoule = one million joules) and is on track to proceed with the first attempts to ignition using the indirect drive approach.

(2) Using the laser direct-drive approach, recent experiments on OMEGA have achieved world record performance in terms of DT plasma compression and attained the required densities for fusion. It is likely that, within the next few years, OMEGA will also demonstrate the temperatures that will scale to those required for ignition. If successful, OMEGA will validate many of the physics principles of the direct-drive approach (with the exception of ignition and burn).

The direct-drive approach is a straightforward alternative to indirect drive. First, it is simpler since the laser directly irradiates the solid pellet and the targets do not require metallic enclosures (hohlraums). Second, it is more efficient since it eliminates the need for conversion of laser light into x-rays. For these reasons, the direct-drive approach offers better prospects for energy applications. While the NIF is currently configured to fully validate the scientific principles of the laser indirect-drive approach, it can also test the laser direct-drive approach with very modest upgrades to the laser system.

Recommendation: *The results from OMEGA can and should be used to field experiments on the National Ignition Facility to demonstrate ignition and energy gain with the laser direct-drive approach. This is a necessary step that will resolve most of the target physics issues for the direct-drive scheme and will determine if laser direct-drive is a viable option for fusion energy.*

The NIC is currently funded at the level of $458M for FY10. To the best of my knowledge, some of the key institutions involved in the NIC are operating under very tight budgets. With the first demonstration of ignition expected within the next few years, this is not the time to underfund the ignition campaign. Even small budget increases could significantly improve the prospects for success.

Recommendation: *It is crucial to provide adequate funding to the National Ignition Campaign. Achieving thermonuclear ignition in the laboratory is a milestone in the development of science and energy security. This goal should not be undermined by lack of adequate funding.*

2. Demonstrating High Energy Gain

The next step in target physics after ignition is the demonstration of high energy gain. For a viable IFE power plant, the product of the efficiency of the driver (the ratio of the "wall plug" energy to driver energy produced) and the target gain should exceed 10, e.g., a 10 percent efficient driver requires a gain of 100. The target gain is the ratio between the energy output and the energy input on target. It is unlikely that the NIF will achieve high gains (> 100) in the laser indirect-drive configuration—and so an alternative approach may be required. The 2009 FESAC report[2] states that "Alternative IFE concepts funded through OFES and NNSA have the potential to generate the gains needed for IFE . . .. [The] alternative concepts in IFE will play a crucial role in the development of inertial fusion energy, since high gains and high driver efficiencies are required features of an economically viable IFE power plant." Present research in alternative IFE concepts is mostly funded by DOE's Office of Fusion Energy Sciences (OFES) and NNSA, with NNSA providing limited access to their facilities. Limited access to the NNSA facilities constitutes a serious impediment to progress in this important area and to the achievement of high energy gains for inertial fusion energy.

There are several options for achieving the gains required for IFE using lasers: direct-drive, fast ignition and shock ignition. Heavy ion fusion requires a heavy ion accelerator, and Z-pinch fusion requires a pulsed-power device.

Heavy ion accelerators are attractive drivers from the standpoint of wall-plug efficiency. Recent theoretical work has indicated that heavy-ion fusion (HIF) could achieve high gains through direct irradiation of the target. However, there is little or no experimental work on implosion physics with heavy-ion drivers. Since there are not existing HIF implosion facilities, it is not possible to easily acquire critical experimental data to make a valid assessment of the target physics requirements for HIF. An IFE development path for heavy-ion fusion will inevitably require both a target physics and a technology development program. With little available experimental data on heavy-ion fusion implosions and the lack of HIF implosion facilities, it is likely that an IFE development path based on heavy ion fusion will be lengthy and uncertain.

Z-pinch fusion uses the indirect drive approach and requires high-gain targets (gains of 100 or more). Current *Z* pinches such as the *Z*-machine at Sandia National Laboratory have demonstrated reasonable single-shot performance and high x-ray yields. The rate of progress in target physics is mostly limited by the low shot rates of large *Z* pinches. Theoretical work indicates that it may be possible to design high yield targets that can satisfy the requirements for inertial fusion energy. *Z*-pinch fusion requires driving large currents through massive transmission metal lines that are partially destroyed at every shot. Since the cost of replacing the transmission lines would exceed the value of the fusion-energy output, a *Z*-pinch based IFE power plant will require recycling the large amounts of metal of the transmission lines. While some interesting ideas have been put forward to address this issue, a technology development path for *Z*-pinch fusion is highly uncertain.

Lasers are the most developed drivers and the target physics for laser fusion is the most advanced. Laser drivers are used for direct drive, fast ignition and shock ignition. **Laser direct drive** has been pursued in the U.S., Europe and Japan for over 30 years. According to theoretical analyses, laser direct drive offers the possibility of achieving high energy gains. Since existing laser drivers have poor efficiencies, gains in excess of 100 are required for fusion energy. The conventional approach to laser direct drive uses a single step with a single laser pulse driving the compression and the heating of the thermonuclear fuel. This approach is currently under investigation at two implosion facilities: the OMEGA laser at the Laboratory for Laser Energetics of the University of Rochester, and the GEKKO laser at the Institute for Laser Engineering of Osaka University in Japan. Both OMEGA and GEKKO use glass laser technology. Until recently, target-physics studies on laser direct drive were also pursued at the NIKE laser facility of the Naval Research Laboratory (NRL). NIKE is a Krypton-Fluoride (KrF) gas laser producing laser light with a wavelength shorter than the other large glass lasers. KrF lasers are more efficient than glass lasers. Their short wavelength light efficiently couples the laser energy to the target and allows operation at relatively high laser intensities. While short wavelength light improves several aspects of the target physics, it poses more severe technological constraints on the optical components of the laser system. The NRL IFE program did not receive funding in the FY09 Omnibus Appropriations bill and its future is uncertain.

A wealth of experimental data is available on direct drive implosions. The data includes surrogate targets (mostly made of plastic shells) and cryogenic solid deuterium (D_2) and deuterium-tritium (DT) targets. The latter are the targets of most interest to inertial fusion energy. To date, cryogenic DT targets have only been used for implosion experiments on the OMEGA facility. Recent cryogenic implosion experiments on OMEGA have achieved high compression of thermonuclear fuel. While

[2] Fusion Energy Science Advisory Committee (FESAC), Advancing the Science of High Energy Density Laboratory Plasmas, US Department of Energy, Office of Science, January 2009

the required densities have been achieved, further progress needs to be made to raise the temperature (by about 50 percent–70 percent) and the fusion yield (by about two to four times) from the compressed DT fuel. Only when all these requirements (density, temperature and fusion yield) are simultaneously met in cryogenic implosions on OMEGA, can one achieve a full understanding of the target physics and full validation of the predictive capability. Achieving an experimental validation of the predictive capability is an important requirement for the design of robust high-gain targets. OMEGA is close to achieving such an experimental validation (with the exception of the validation of ignition and burn physics that requires experiments on the NIF).

Achieving gains in excess of ~100 with the conventional approach to direct drive requires very large lasers. An IFE laser driver should deliver a few megajoules of ultraviolet light to the target at a rate of about 10 shots per second. Krypton-Fluoride and advanced solid state lasers offer the promise of high efficiency and high repetition rates, but even in the most optimistic scenario, a power plant based on the conventional direct-drive approach will require large megajoule-class lasers and targets with gains above 100. The need for large high-repetition-rate laser systems is the main difficulty in the development of the conventional laser direct-drive approach to inertial fusion energy.

Fast ignition is a relatively new concept that separates the compression and the heating of the thermonuclear fuel. The compression is driven by a conventional system (laser or other driver), and the heating is induced by a beam of energetic electrons produced by the interaction of a short-pulse ultra-high-intensity laser beam with the target. Fast ignition research is actively pursued in the U.S., Europe and Japan. Theoretical analyses indicate that fast ignition may lead to energy gains well above the gains of conventional direct drive. However, such theoretical calculations are incomplete and the physics principle concerning the interaction of intense light with matter and the transport of energetic electrons in plasmas are poorly understood. While fast ignition may require a relatively small compression laser (a sub-megajoule laser), it is likely that providing the necessary external heating power will involve a large high-power laser (~100 kilojoule petawatt-class laser—one petawatt = 1000 trillion watts). Presently, the largest petawatt lasers are the OMEGA EP laser (2.5 kilojoules) at the Laboratory for Laser Energetics and the FIREX laser (10 kilojoules) at Osaka University.

Since little experimental data on the target physics for fast ignition is available, it is difficult to make an assessment on its viability as an option for fusion energy. In the past, the lack of experimental facilities with a dual integrated laser system (the compression and heating lasers working together) has prevented the acquisition of the necessary data. However, the U.S. and Japan have recently completed the construction of two integrated facilities that can explore the fast ignition concept. Such integrated laser systems are OMEGA at the Laboratory for Laser Energetics, and FIREX–I at Osaka University. A third integrated facility will soon be available at Lawrence Livermore National Laboratory. The OMEGA facility includes the OMEGA compression laser and the OMEGA–EP high-power laser, the FIREX–I facility includes the GEKKO compression laser and the FIREX high-power laser, and the NIF will soon include the ARC high-power laser. These three facilities have the potential to rapidly advance the target physics for fast ignition. The main obstacle to such advances is the very limited access granted to fast ignition studies on the U.S. integrated facilities OMEGA and NIF. For example, only five days of the OMEGA facility were devoted to integrated fast ignition experiments in FY09. With such a limited time allocation, it is difficult to make meaningful progress in fast ignition. The reason for this limitation is that such facilities are funded by NNSA, whose primary mission does not include fusion energy development. Inadequate access to the integrated NNSA laser facilities is currently the main obstacle to acquiring the necessary experimental data required to validate the fast ignition scheme. The lack of experimental data on the target physics as well as the complexity of the scheme and targets renders highly uncertain the development path of fusion energy based on the fast ignition concept.

Shock ignition is a very new concept introduced in 2007. Similarly to fast ignition, shock ignition is also a two-step process where a strong shock wave is used to heat the thermonuclear fuel previously assembled by a compression laser. An advantage of shock ignition is that the shock can be launched by the same laser used for the compression, and therefore it requires a single laser. Much of the target physics for shock ignition is a straightforward extension from laser direct drive. However, launching strong shock waves requires relatively high laser intensities and there are concerns about the coupling of the laser light to the target and other negative effects that occur at high intensities. Most of the theoretical work on shock ignition to date comes from computer simulations carried out at the Laboratory for Laser Energetics, the Naval Research Laboratory, Lawrence Livermore National Laboratory and the Centre Lasers Intenses et Applications in Bordeaux (France). This work shows that high energy gains may be possible with shock ignition using a sub-megajoule driver. Recent experiments on the NIKE laser, target design work and computer simulations from NRL have indicated that Krypton-Fluoride lasers are particularly suitable for shock ignition because they provide a more effective drive for the shock and reduce the risks (to the target) of operating at high intensities. This interesting research stopped in 2009 when the NRL program did not received funding in the FY09 Omnibus Appropriations bill. While the simulation results are promising, there is not sufficient available experimental data on the target physics to make an assessment of shock ignition as a viable scheme for fusion energy. The only available implosion data on shock ignition comes from a few experiments on the OMEGA laser. Acquiring meaningful experimental data requires access to the NNSA laser implosion facilities OMEGA and NIF. Like fast ignition, access to these facilities for shock-ignition research is very limited. For example, only one day of operation of the OMEGA facility was devoted to shock ignition in FY09. Inadequate access to the NNSA laser facilities is currently the main obstacle to acquiring the necessary experimental data required to validate the shock-ignition scheme. Due to the lack of experimental data on the target physics, the development path for shock ignition is uncertain.

Finding: *Laser drivers are the most developed drivers for inertial fusion. The target physics for laser direct drive is also the most advanced. Because of the relatively low driver efficiency, laser-based inertial fusion energy requires high gain targets (with gains above 100). Laser direct drive, fast ignition or shock ignition may provide such high gains. A power plant based on conventional direct drive will likely require large and expensive megajoule-class lasers. Fast and shock ignition may require a significantly smaller driver than conventional direct drive. However, little experimental data is available for fast and shock ignition to make a valid assessment of their viability for fusion energy. Heavy-ion drivers are more efficient than lasers but little or no experimental data is available on implosion physics for heavy ion fusion and there are no plans to acquire such data in the near future. Z-pinch fusion uses the indirect-drive approach and requires high gains (about 100 or more). Z-pinch research has made progress in target physics but serious questions remain on the viability of Z pinches as fusion-energy drivers.*

Existing NNSA facilities have the capability of exploring the physics principles of direct- and indirect-drive laser fusion, as well as fast and shock ignition. Fast and shock ignition research is currently funded by the OFES. Access to the NNSA facilities for fast and shock-ignition experiments is currently very limited since NNSA's mission does not include fusion-energy development. This limited access is currently the main obstacle to acquiring the necessary experimental data required to validate high-gain IFE concepts.

Recommendation: *OFES and NNSA have already formed a joint program to fund high-energy-density physics research. This partnership should be strengthened to increase access to NNSA facilities for research in the area of high-gain inertial fusion energy concepts.*

3. Developing the Technology for Inertial Fusion Energy

Achieving ignition and high gain does not imply that economically attractive fusion energy is just around the corner. Major technological and engineering challenges will still remain even after the demonstration of ignition. The development of a viable fusion power plant requires large scientific and financial investments. Drivers compress the pellet and are the most complex and expensive component of an IFE power plant. The driver is part of an integrated system including a target chamber, injection systems and other components. Drivers must operate with relatively high repetition rates to produce enough average power output.

Finding: *Several IFE drivers have been proposed: solid state lasers, Krypton-Fluoride lasers, Z pinches and heavy-ion beams. Drivers are part of an integrated system including a target chamber, injection systems and other components. While the technology of some drivers is more advanced than others, none of them offers a development path free of major engineering and technological challenges.*

Therefore, before launching a major energy development program, it is prudent to make an assessment of the different driver options. This can begin immediately with a small exploratory IFE technology program ("small" here is used for comparison with the "large" science program of the National Ignition Campaign).

In the past ten years, the High Average Power Laser Program, funded by NNSA under congressional mandate, was engaged in IFE technology development for KrF and solid state lasers. This program was not funded in the FY09 Omnibus Appropriations bill and no funding is currently provided for IFE technology.

Lasers are the most technically advanced drivers. Small-scale high-repetition-rate KrF and solid-state lasers have been built and tested. Research in target physics for laser drivers is also the most advanced. Furthermore, the current experimental campaign will explore ignition with lasers implying that all the target physics issues will only be resolved for laser drivers. Other drivers will likely require longer development paths for both the technological development and the target physics. An exploratory IFE program should be started with the goal of selecting the most attractive driver option in order to move quickly toward an expanded energy development program once the NIF has demonstrated ignition and energy gains.

Because of the engineering and technological difficulties involved with fusion energy, it is important to assess/explore all possible schemes including fusion-fission hybrids. A fusion-fission hybrid power plant consists of a fusion reactor (the "engine") surrounded by a blanket of fissionable material. This concept has been recently promoted by the Lawrence Livermore National Laboratory (LLNL). The fissionable material is depleted uranium or spent nuclear fuel, while the fusion engine is based on the laser indirect-drive approach. Since the fission blanket amplifies the energy output from the fusion engine, a relatively low-gain laser indirect-drive (or direct-drive) scheme may suffice in its role as neutron source. Advocates argue that the LLNL approach to fusion-fission hybrids offers the shortest development path for inertial fusion energy since the target physics and the required target gains are essentially the same as the ones explored by the NIF within the next few years.

In light of these possible advantages, an exploratory IFE technology program should also assess the viability of fusion-fission hybrid systems and make a determination on the benefits of such systems and the possibility of a shorter development path.

> **Recommendation:** *It would be beneficial to immediately develop an exploratory fusion technology program in parallel to the ignition campaign to assess the viability of the different driver options. If successful, such a program will select the most attractive driver by the completion of the ignition campaign on the NIF.*

Additional technical information, findings and recommendations can be found in:

- Fusion Energy Science Advisory Committee, *Advancing the Science of High Energy Density Laboratory Plasmas,* (Chapters 7, 9, 11), U.S. Department of Energy, Office of Science, January 2009.
- Fusion Energy Science Advisory Committee, Review of the Inertial Fusion Energy Program, U.S. Department of Energy, Office of Science, March 2004.

BIOGRAPHY FOR RICCARDO BETTI

Dr. Riccardo Betti is currently Professor of Mechanical Engineering & Physics and Astronomy at the University of Rochester. He is also Director of the Fusion Science Center for Extreme States of Matter, Senior Scientist and Assistant Director for Academic Affairs at the University of Rochester's Laboratory for Laser Energetics.

Dr. Betti has conducted research in plasma physics, inertial and magnetic confinement fusion for over 20 years. He is Vice-Chairman of the Department of Energy Fusion-Energy-Science Advisory Committee (FESAC), and Steering Committee Member of the High-Energy-Density Science Association. He was Chairman of the Plasma Science Committee of the National Academies in 2006–09, and Chairman of the FESAC sub-panel on High-Energy-Density Physics in 2008–09. Dr. Betti is a Fellow of the American Physical Society for his pioneering work on ablative fluid instabilities in inertial confinement fusion and energetic particle instabilities in magnetic confinement fusion. He is a recipient of the Edward Teller Medal for his seminal contribution to the theory of thermonuclear ignition and implosion physics for inertial confinement fusion. Dr. Betti received a *"laurea cum laude"* degree in Nuclear Engineering from the University of Rome, and a Ph.D. in Nuclear Engineering from the Massachusetts Institute of Technology. He has co-authored over 100 refereed articles in plasma physics, magnetic and inertial confinement fusion.

Chairman BAIRD. Thank you, Dr. Betti.

Dr. Fonck.

STATEMENT OF DR. RAYMOND J. FONCK, PROFESSOR OF ENGINEERING PHYSICS, UNIVERSITY OF WISCONSIN–MADISON

Dr. FONCK. Mr. Chairman, Ranking Member Inglis and Members of the Committee, thank you for the opportunity to testify today. I got the instruction from your letter to say what our vision for the next 10 to 20 years is, and I thought about it and said the biggest vision is: we should not have this hearing again in 20 years. We should not be talking about fusion energy science; we should be talking at that point about fusion energy development, and there is a crucial difference. So to answer your question, I will give you an example of a path we could take to pursue in the next 10 to 20 years to get in that direction.

In spite of the scientific progress we have made, it is no secret there is skepticism on the credibility or timeline of fusion energy but much of that can be traced to the fact that the full range of technical challenges is not being addressed. These challenges are—some of them have been mentioned—demonstrating and exploring the burning plasma state, creating predictable, high-performance continuous plasma, taming the plasma material interface and harnessing fusion power from the very energetic neutrons released in fusion. Addressing these four challenges would provide the knowledge base to establish the credibility of fusion as an energy source and motivate a decision to establish a fusion energy development program.

There has been outstanding progress in fusion energy science, as has been mentioned here already, under the auspices of the Department of Energy. Most of this is focused on the properties of the extremely hot fuel or plasma required for fusion energy and reactions to occur. But it is very important, and it must be emphasized, that fusion science is not just plasma physics. The frontiers of fusion science research are moving to the critical issues of the last two fusion challenges: the plasma-wall interactions and harnessing fusion energy. At the same time as these frontiers are moving, our experimental facilities are aging. Our leading experiment is over 20 years old. The next-generation state-of-the-art facilities and capabilities are being developed outside the United States. The fact that we have not positioned ourselves to lead in addressing the first two challenges because of these aging facilities, and that we haven't built anything in 20 years, puts us in a unique position, however, of being able to address more aggressively the last two elements of the fusion challenge. An emphasis on the complex processes occurring in the plasma material interfaces, their integration with the systems with that extract energy from the fusion system and the effects of the fast neutrons on those processes, should be the focus of the domestic U.S. program in the ITER era. This program and ITER together will address most of the critical issues underlying the credibility of fusion energy. Just as importantly, it starts the United States on the path to benefit economically from its long-term investments in fusion science research. Indeed, the intellectual property rights that accrue from the development of fusion will concentrate in these areas, not in the plasma sciences directly.

So it is time to create a plan to put the U.S. fusion program on a trajectory towards leadership in the next generation of fusion research. To accommodate realistic budgets, specific programs and facilities in our program will need to be redirected or completed to free resources for these new directions. Domestically, the program should move to address the pending nuclear and energy-related issues that fusion will present. These scientific challenges will be addressed first in small-scale studies, material studies, computational modeling, et cetera. But this effort should culminate in a national, integrated fusion nuclear science test facility as the central fusion facility in the United States. It will provide the needed integrated test of our understanding of the coupled plasma-wall energy conversion systems. Whatever form this facility takes, this centerpiece experiment in the United States should be a deuterium-tritium facility to access the full range of fusion nuclear issues.

The transition of the domestic program to an increasingly strong focus on fusion nuclear sciences can be executed over the next decade or so concurrent with the construction and initial operation of ITER. As ITER construction winds down, the roll-off of those funds could be applied to this new national facility to meet the new challenges. Pursuing this program would vault the U.S. program into leadership of critical areas of the overall fusion challenge. In the ITER era, the research activities on ITER and this U.S. program would arguably define the centers of gravity of fusion science and engineering development and will expedite the decision whether to develop a demonstration fusion reactor either by the U.S. Government or industry or some combination thereof.

So there is a pressing need for plans, A, to evolve—a world-leading fusion nuclear science program under realistic budgets, and B, to develop the technical case for an evolution of the program into a fusion energy development program as soon as it can. To support developing those plans, the planning of scientific missions and conceptual designs of requisite facilities to match those missions should begin immediately. Support of H.R. 3177, the *Fusion Engineering Science and Fusion Energy Planning Act of 2009,* would provide funding to start this transformation of the program.

Finally, I just want to comment on inertial fusion, because I have been concentrating on magnetic. If its the campaign to demonstrate ignition of fusion plasmas via inertial confinement in the National Ignition Facility is imminent. The achievement of ignition in NIF will be exciting and historic. It will rightly demand a reexamination of our national position on inertial fusion energy, or IFE. As the ideas and proposals for moving forward towards an IFE program evolve after results are obtained from NIF, it would be valuable to have a disinterested expert panel outside the community evaluate the prospects for inertial fusion energy to inform and motivate any decision about moving forward to a new inertial fusion energy science program.

Again, thank you for the opportunity to address the Committee.

[The prepared statement of Dr. Fonck follows:]

PREPARED STATEMENT OF RAYMOND J. FONCK

Mr. Chairman, Ranking Member Inglis, and Members of the Committee, thank you for the opportunity to testify today. In my testimony I will try to describe how the U.S. Fusion Energy Sciences program has been quite successful, but has been, through historical and artificial constraints, unable to address key issues that must be resolved before practical fusion energy can be reached. I will also suggest one possible path along which these issues can be resolved within a reasonable budgetary envelope.

Research on the properties of high-temperature plasmas, the fuel for fusion reactors, has made tremendous strides in the past decades. In the future, the scientific frontiers of fusion will increasingly move to the complex interactions among the cooler plasma edge, the materials of the surrounding chamber, and energy extraction systems, and the role of neutrons in modifying those interactions. To address these critical issues and motivate a future fusion energy development program, it is time to start building a fusion nuclear science program in the fusion R&D portfolio. It will start with modest activities in materials and related research, and should have a longer-term goal of deploying a new national fusion nuclear science research facility as the centerpiece of the U.S. domestic experimental effort in magnetic fusion in the ITER era. The transition to these new efforts will be gradual and must be funded during ITER construction in large part by completing existing programs. Strategic plans for the evolving program need to be developed. In addition, the anticipated success of the ignition campaign on NIF should motivate an examination of proposals for a new program in inertial fusion energy science and/or engineering. Support of H.R. 3177, the *Fusion Engineering Science and Fusion Energy Planning Act of 2009,* would provide funding to assist the start of necessary transformations in the program.

Progress in Plasma Sciences Motivates a New Phase of Fusion Research

Fusion is the nuclear process that produces energy in the interior of the sun and stars. Developing fusion power in the laboratory truly means capturing the power of the sun here on Earth, and is a grand challenge of science and technology. The path to producing useful energy through the fusion process here on Earth is complex, and the quest is not complete.

With readily available fuel and significant environmental advantages, fusion energy is a candidate for significant carbon-free, base-load energy production in the second half of this century. However, major new energy technologies can require decades to strongly penetrate the market after introduction. To offer the possibility of fusion power in a useful timeframe, we need to move as quickly as we can now to exploit and complement the advances in fusion energy R&D that are expected in the next decade or more.

Historic achievements have been made and others are eagerly anticipated in the world of fusion energy sciences research. Past demonstrations of 10–20 MW of fusion power production in the TFTR (in the U.S.) and JET (in the E.U.) experiments confirmed the promise of magnetic confinement of fusion plasmas in the 1990's. The U.S. subsequently entered the ITER project to allow U.S. scientists to explore magnetically confined burning plasmas. A burning plasma exists when the power released by the fusion nuclear reactions is roughly 5–10 times larger than the power injected to sustain the fusion process. All of those experiments are based on the tokamak concept, which is a type of donut-shaped magnetic bottle that holds the hot fusion fuel away from any material walls.

In addition to the magnetic confinement approach with tokamaks, the demonstration of ignition in inertially driven fusion targets in the National Ignition Facility is planned for the near future. This relies on powerful lasers to compress solid fusion fuel pellets to heat them to fusion temperatures and create a very short, powerful release of fusion energy.

There has been outstanding progress in fusion energy science research under the auspices of the Department of Energy Office of Science programs. Most of this has focused on the properties of the extremely hot fuel, or plasma, required for fusion reactions to occur. Our understanding of the extraordinarily complex problem of small-scale plasma fluctuations that lead to increased heat losses, and hence inhibit the ability to achieve the fusion state, has evolved to the point where these fluctuations can often times be suppressed. This leads to increasing plasma temperatures and fusion power. The understanding and predictability of fusion-grade plasmas have been refined to the point that the plasmas can be actively controlled to avoid damaging large-scale instabilities. Techniques to heat and manipulate these plasmas to finely tailor the plasma state and thereby optimize the potential to produce fusion reactions are being successfully developed. Similar progress has been made in understanding inertially confined plasmas in defense-related DOE programs. With all of these accomplishments in plasma sciences and supporting technologies, we are resolving some of the major plasma physics issues in the overall challenge of establishing the base for fusion energy.

These developments represent the culmination of decades of research in high temperature plasma sciences, and motivate us to confront the additional challenges remaining to making the case for fusion energy. Hence, it is indeed timely to consider

"The next generation of Fusion research," and it is time to start broadening the scope of the programs to expedite decisions on a commitment to fusion energy development.

Broadening the Fusion Research Portfolio to Enable a Future Energy Development Program

The DOE fusion science programs have, somewhat of necessity and somewhat due to artificial constraints, concentrated on studying many of the relevant plasma science questions that arise in moving towards fusion energy conditions. However, the fusion challenge is much broader than high temperature plasma science and its attendant enabling technologies. The development of the knowledge base for fusion energy requires a variety of topics to be addressed, including basic high temperature plasma science, measurement sciences, materials, the effects of nuclear interactions, and the engineering technology challenges of capturing and converting fusion energy. In fact, the full range of issues is well known, and only a fraction of them are addressed in the present program.

The research and development needed to establish the foundation for fusion energy development were identified in plans for fusion energy research in the 1970's, acknowledged in repeated reviews and planning documents since then, and most recently restated by a major Fusion Energy Sciences Advisory Committee study that was charged to identify the gaps in our knowledge that remain, assuming successful completion of the ITER burning plasma program. While the details vary, the general issues identified through the years have not changed, mainly because they are driven by the physical challenges of attaining and exploiting the fusion state.

From the most recent assessment of fusion, the fusion R&D enterprise must at least address the following four challenges.

FUSION CHALLENGES:

- Demonstrating and exploring the burning plasma state
  - *Creating and controlling a fusion plasma that releases several 100 MW of energy, and understanding the effects of very energetic fusion-created particles, is a grand challenge of fusion science research.*
- Creating predictable, high-performance, steady-state plasmas
  - *A continuously burning plasma that behaves predictably and is highly efficient is needed for economical fusion reactors*
- Taming the plasma-material interface
  - *Magnetic confinement sharply reduces the contact between the plasma and the containment vessel walls, but such contact cannot be entirely eliminated. Advanced wall materials and magnetic field structures that can prevent both wall erosion and plasma contamination are required.*
- Harnessing Fusion Power
  - *Fusion energy from deuterium-tritium (D–T) reactions appears in the form of very energetic neutrons. The understanding of the effects of these neutrons on the surrounding materials and the fusion plasma, and the means of capturing this energy, while simultaneously breeding the tritium atoms needed to maintain the reaction, must be developed.*

The first two challenges are addressed by research focused on understanding the high-temperature plasma properties in the hot central core region of these magnetically confined plasmas. This research has been very successful, and will remain a vibrant field well into the future.

However, the scientific frontiers of fusion are inexorably moving to examine the critical issues of the plasma interactions with the material chamber, and methods of extracting the energy from the fusion process. These topics are the focus of the last two challenges. For example, it is now clear that the processes in the edge plasma region, where the hot plasma interacts with the surrounding material chamber, profoundly influence the overall behavior of the plasma in the central hot region. The processes that occur in the plasma-chamber-energy conversion systems increase in number and complexity in the presence of a high-energy neutron flux, where the properties of the materials and their interactions with the plasma edge, can be significantly altered. This interacting plasma-chamber-energy conversion system will eventually need to be examined in integrated tests. This will encompass the entire fusion system, and complement the burning plasma studies to address all four fusion challenges.

It is no secret that there is skepticism on the credibility or timeline of fusion as an energy source, and much of it can be traced to the fact that this full range of challenges is not being addressed. Nevertheless, in those areas that have been addressed in detail (mainly concerning 1 and 2 above), the progress has been steady, impressive, and acknowledged. Outside evaluations of the science developed by the fusion research program have affirmed the high quality and integrity of that scientific enterprise. However, few resources have been focused on addressing the last two fusion challenges listed above, and hence progress there has been slow, which in turn undermines the argument for accelerating the development of fusion energy.

With the entry into the era of burning and ignited plasmas, it is time to broaden the fusion research enterprise to address, at appropriate levels, the full range of fusion challenges. ITER will provide us unique tests of the physics of the high-temperature core of a fusion system and some reactor-relevant technology. An emphasis on the complex processes occurring in the plasma-material interfaces, their integration with the systems that extract energy from the fusion system, and the effects of neutrons on those processes, should be the focus of the domestic U.S. program in the ITER era. These two efforts together will address most of the critical issues underlying the credibility of fusion energy. This will then provide the government and industry the information needed to decide any future commitment to fusion energy development as soon as possible.

Most present fusion-energy related research is in the portfolio of the Office of Fusion Energy Sciences in the Office of Science of DOE, and is concentrated on the magnetic confinement approach. It is establishing the scientific basis for fusion energy, but it is natural to expect that at some time in the future this program will evolve to a dedicated fusion energy development program, either inside or presumably outside of the Office of Science. This evolution will occur as the credibility of fusion energy is established through focused research activities that address in part all of the fusion challenges above. Continuing basic science studies to support this focused energy development program would continue in the Office, similar to other programs there. Indeed, this is precisely what the National Academics recent Decadal Study for Plasma Physics suggested will be the natural evolution of this program.

A major challenge of the present fusion research program is to establish the credibility of fusion energy to expedite this transition to an energy development program. To that end, DOE and the research community soon need to develop a long-range strategy to both justify and smoothly effect this transition towards an energy development program, assuming success in the present science program. Moving in this direction can be done within reasonable funding levels and will attract a new generation of researchers.

BROADENING THE FUSION PORTFOLIO IN THE NEAR–TERM

While one can anticipate the future fusion energy development program, the ability to move the present fusion science program forward within realistic budget constraints is hampered by both externally and internally imposed constraints.

The program is strongly focused on the underlying plasma science of the fusion plasma core. It does not address the rich array of scientific and engineering challenges that arise in the entire fusion system, and that must be addressed in the quest to demonstrate the viability of fusion power. Practically, this resulted from an external constraint on the program that there could be little research into the engineering sciences, material sciences, and technologies relevant to fusion energy until the whole range of underlying plasma physics issues is addressed.

While this constraint may have reflected priority setting in a resource-limited program and been used as a means of restraining the appetite for significantly increased budgets without clear priority setting, it is increasingly anachronistic. Without removing this constraint, we will miss the opportunity to develop the knowledge and skills in precisely those areas of the fusion problem that will lead to economic advantages from our long investments in fusion research. In considering the next phase of fusion research, I assume that this constraint is lifted and the Office of Fusion Energy Sciences will be free to allocate resources across the relevant broad range of issues to optimize the path to a fusion energy development program within available resources.

The fusion research community imposes another constraint on itself by seeing its resources as locked and concluding that there is little opportunity to move forward to new frontiers, which often means new facilities to access new physical states. This sense of insurmountable limits arises from real constraints on the amount of funding available, but also from an unwillingness to acknowledge clearly focused goals and make hard priority choices to achieve those goals.

This can be addressed by developing a plan for fusion R&D in the next decade and beyond that makes the hard choices needed to regain U.S. leadership in selected areas that focus on the credibility and eventual economic exploitation of fusion as an energy source. In particular, an eight- to ten-year plan that includes a growing activity in the critical fusion nuclear science and engineering issues that are relevant to exploitation of the energy-producing plasma should be developed and pursued. The goal of this plan would be to move smoothly over the next decade during ITER construction to include in the U.S. fusion program a world-leading fusion nuclear science program, with access to the requisite tools and resources to address the critical issues during the ITER era.

As mentioned above, the U.S. fusion science research program is addressing mainly the first two of the four main fusion challenges. However, the next-generation, state-of-the-art facilities and capabilities to address both of these challenges are being developed and located outside the U.S. The burning plasma program is now centered on ITER in France, and the large major tokamaks that are cited as necessary for ITER preparation and operation are located in the EU and Japan. Likewise, tokamaks with superconducting coils and world-class stellarator experiments will lead the research to resolve the issues inherent in steady-state plasma operations. The new superconducting tokamaks are located in China, South Korea, and Japan, while the large stellarator experiments reside in Germany and Japan. U.S. scientists, using older facilities, have certainly made seminal contributions to these various concepts—indeed, some of these facilities have benefited directly from U.S. developments. However, it is inevitable that research on these new facilities will guide fusion energy science developments in these areas in the future. Hopefully, our scientists will collaborate on these international facilities, but the net consequence is that the U.S. is off-shoring its ability to lead in the first two of the four challenges of fusion energy development.

This, however, puts the U.S. community in the position of being able to address more aggressively the last two elements of the fusion challenge. In particular, we have a unique opportunity to pursue world leadership in the new frontiers of fusion: plasma-wall interactions, materials, and harnessing fusion energy. These areas cover the problems inherent in handling, capturing, and converting fusion neutrons and heat created by the fusing plasma to useful power. The problems include: plasma, atomic, molecular, and nuclear physics; material sciences; neutron sciences; and associated engineering challenges. Starting to move the U.S. program in the direction of addressing these integrated problems complements the planned research on ITER and directly confronts major points of criticism of fusion power. Most importantly, it starts to position the U.S. to benefit economically from its long-term investments in fusion science research. Indeed, the intellectual property rights that accrue from fusion development will concentrate in these areas, since the plasma science knowledge to address the first two elements is openly developed and available.

A CONSTRAINED, AGGRESSIVE FUSION ENERGY RESEARCH PLAN

A fusion program with a properly expanded scope to include a growing focus on the underlying nuclear and energy science issues can be readily envisioned. One such scenario is outlined here, but it is only conceptual. Wide variations of this approach could emerge as planning goes forward. In any case, it must be constrained to realistic budgets, include milestone commitments, and contain sometimes-painful priority decisions.

I assume that the ITER construction will be supported, and U.S. domestic research funds will include the present level, with inflation escalation, and any increases that the program can successfully compete for as the Office of Science budget increases though pursuit of the goals of the *America COMPETES Act.* This funding profile will require that specific programs and facilities in the U.S. program be completed to provide resources for new directions of research.

The central activities addressing the first two elements of the fusion challenge will migrate to collaborative research on international facilities. That is, the research addressing the burning plasma and steady-state issues for fusion plasmas will be pursued overseas, and major U.S. facilities will be transitioned out as their programs are completed. As the new superconducting and steady-state plasma facilities come into full operation overseas, collaborative agreements will need to be developed or expanded to provide our scientists access to those capabilities that are not available in the U.S. Participation in ITER burning plasma studies will eventually require the development of a U.S. ITER science team. This team could also execute that collaborative research on other state-of-the-art tokamaks in anticipation of the ITER collaborations.

The stellarator (mentioned earlier) is a magnetic confinement concept that is similar to the tokamak but in a sense offers simpler plasma properties at the expense of more complex mechanical systems. It may provide a potential breakout concept for a fusion reactor concept, and international collaboration is also critical here. However, there may be a world-leading role for the US to pursue modest facilities to resolve critical issues. The domestic program in the U.S. should retain a viable research activity in this area to support informed decisions on future reactor concepts.

Domestically, the U.S. fusion science program should now begin to address the pending nuclear and energy-related issues that fusion will present. The scientific challenges of plasma-wall interactions can be addressed initially in present tokamaks, move to dedicated test stands to understand underlying physics, and eventually be a focus in the first phases of a central U.S. facility dedicated to fusion nuclear science issues. The fusion nuclear science program should ramp up over time to at least include: elemental material science studies and development of materials conducive to deployment in the fusion environment; materials tests using fission reactor irradiation; a materials test station to allow initial tests of small materials samples under intense energetic neutron bombardment; small-scale supporting test facilities as needed; and computational modeling of the integrated fusion system.

This effort should culminate in a national integrated fusion nuclear science test facility as the central fusion experimental facility in the U.S. It will provide the needed integrated tests and development of our understanding of the coupled plasma-wall-energy conversion systems. While the actual form that the fusion nuclear science test facility takes will depend on detailed development of its mission requirements and comparison of competing concepts, this next major confinement experiment in the U.S. should be a DT (deuterium-tritium) facility to access the full range of fusion nuclear issues. Such a facility would likely attract a substantial investment from other countries should the U.S. seek to lead this effort and pursue such partnerships. A phased development of the capabilities of this experiment will restrain costs and coincidently mitigate the impacts of our off-shoring our abilities to address the first two fusion challenges above.

The transition of the domestic program elements from the present configuration to one including the second two fusion challenges is required. It is important to recognize that this transition will take time, both to bring existing activities to successful closure and transition people and resources to new directions. Generally, the transition can be executed over the next decade or so, concurrent with the construction and initial operation of ITER.

As the ITER construction winds down, those roll-off funds should be applied to the new national facility to meet the challenges I have mentioned above. Some augmentation of those funds will be required to support a full DT implementation, but foreign collaborations might be solicited to help make up this gap.

To prepare moving in this direction, the planning of scientific programs and conceptual designs of requisite facilities to match chosen scientific missions must begin immediately. These will inform decisions needed in a few years. In the meantime, the near-term activities of the program will center on completing missions for existing facilities and programs as needed to begin a wedge of growth of a Fusion Nuclear Science Program component to the U.S. fusion program. There is especially an immediate need for initiating related materials research and developing trained fusion engineering science personnel.

Executing this transition of the program, and eventually deploying an integrated fusion nuclear science experiment, would vault the U.S. program into leadership of critical areas of the overall fusion challenge. In the ITER era, the research activities on ITER and this U.S. program would arguably define the centers of gravity of fusion science and engineering development, and will expedite the decision on proceeding to the development of a demonstration fusion reactor, whether by the U.S. Government, industry, or some combination thereof.

There are substantial risks to pursuing this program, and they must be recognized and managed. There is a real potential for loss of expertise and momentum as major U.S. facilities roll off and international collaboration becomes the norm for access to leadership-class facilities. If all or almost all of the major confinement experiments in the U.S. were terminated well before a new national experiment was initiated, there would likely be a loss of specialized machine designers. This in turn would make it increasingly difficult to start world-class programs in the U.S. as the international community moves forward. This has already happened in individual laboratories in the fusion community.

There is the danger of loss of interest by new young scientists without world-class U.S. facilities while waiting for a new national facility. There will inevitably be displacement of personnel, and long-term planning and scheduling will be required so that scientists and engineers know what is coming and can adjust accordingly. These changes will not necessarily be welcomed by the research community because they will almost inevitably include some reduction of the activities presently being pursued, and everyone can legitimately claim there is much more to do in any given area. Indeed, an additional risk is that many underlying science issues will receive less emphasis than may be called for. Finally, there is the risk that collaborations with U.S. scientists may be seen to be less valuable to foreign hosts when the U.S. has a decreasing number of world-class facilities and likely some declining domestic research capabilities.

These are serious consequences to a vital research program, and they are not suggested casually. They follow directly from the funding levels expected for the program and the scientific demands of the fusion enterprise. The program could be fatally damaged if these transitions are not managed adroitly.

However, there are corresponding risks to not evolving from the present program while our international partners and competitors aggressively advance their programs. We will either further, or possibly indefinitely, delay a decision on developing fusion energy. We would not be competitive as fusion energy and it commercial applications are developed elsewhere.

Thus, the program must focus and move forward to make the case for a breakout into a fusion energy development program as soon as it can. To that end, it may be useful to develop a technical contract among the fusion research community and DOE managers to define what minimal knowledge base is needed to establish the credibility of fusion and then confront the question of whether society wants to make the next level of investment for the development of commercial fusion energy. This contract should reflect the views of energy policy professionals on the criteria for the credibility of fusion as an energy source.

A COMMENT ON INERTIAL FUSION ENERGY RESEARCH

This discussion has focused on the direction of the magnetic confinement fusion research, given its prominence in the present OFES program. As mentioned earlier, the campaign to demonstrate ignition of fusion plasmas via inertial confinement with laser compression of solid fuel pellets on the National Ignition Facility is imminent. At present, there is no established program in the U.S. with a focus on developing the science and technology of inertial fusion energy (IFE). There is a modest research program in the related area of High Energy Density Physics, but it is quite broad and addresses some points of interest to IFE.

The achievement of ignition in NIF will be exciting and historic. It will rightly demand a reassessment of our national position on IFE. When ignition is demonstrated, there naturally will be increased interest in this approach to fusion production as an energy source. However, the challenges expected to move from this accomplishment to an energy source are as at least comparable to those in the magnetic fusion approach. While first concentrating on increasing the fusion gain to levels of interest to energy production, the issues of target development, laser development, and fusion chamber development will rise in interest. In addition, many of the materials and nuclear science issues to be addressed in the proposed fusion nuclear science program are common to both approaches to fusion energy.

As the ideas for moving forward towards an IFE program evolve after data is obtained from NIF, it would be valuable to have a disinterested expert panel evaluate the prospects and requirements for inertial fusion energy to inform any decision to embark on an inertial fusion science program or an inertial fusion energy development program.

SUMMARY

Significant progress in fusion science has been made in the past decade, and a solid scientific basis now exists to plan towards a fusion energy mission. The recognition that magnetic fusion energy research is at a mature stage for exploring burning plasmas and the expected achievement of high fusion gain in NIF for inertial fusion energy presage new eras for fusion research and development.

There is a pressing need to broaden the range of fusion research in the U.S. to prepare to explore the new frontier of fusion science, i.e., the integrated plasma-chamber-energy conversion system. To address this issue and position the U.S. as a world-leading source of expertise in the developing and harnessing of fusion power in the post-ITER era, it is timely to begin building a fusion nuclear science program. This will complement the advances made in magnetic confinement plasma sciences. It will start with modest activities in materials research and development of a new cadre of fusion engineers, and progress to the deployment of a new national fusion science research facility as the cornerstone of the U.S. fusion experimental effort in magnetic fusion.

The transition to these new efforts should be gradual and supported during ITER construction in large part by completing existing programs and out-sourcing many of our near-term activities to new facilities and programs presently being developed in partner states. Strategic plans should be developed to map the next decade or more to point to the initiation of a national fusion nuclear science test facility and to map the present fusion science program to a future fusion energy development program, with priority given to expediting that transition. This will necessarily be a very focused program, and hence contain risks of disrupting the existing infrastructure and missing other profitable avenues of research and development.

The highly anticipated success of the ignition campaign on NIF will rightly increase interest in evaluating the potential of inertially confined plasmas for energy applications, and should motivate a high-level review of proposals for a new program in IFE science and/or engineering.

Finally, support of the H.R. 3177, the *Fusion Engineering Science and Fusion Energy Planning Act of 2009,* would provide a modest level of funding to start this transformation in the program.

BIOGRAPHY FOR RAYMOND J. FONCK

Raymond John Fonck is a professor in the Department of Engineering Physics and the Steenbock Professor in the Physical Sciences at the University of Wisconsin–Madison. He received his Ph.D. in Physics from the University of Wisconsin–Madison in 1978 (atomic physics and laser spectroscopy). He has been involved in fusion energy science research for almost 30 years in the university and at national laboratories. He has also been involved in fusion science research policy, serving in several capacities for national advisory committees and for committees of the National Academies of Sciences and Engineering. He recently served as Associate Director of the DOE Office of Science for Fusion Energy Sciences.

Specifically, he was at the Princeton University Plasma Physics Laboratory from 1978 through 1989, where he was Deputy Head of the PBX–M Tokamak project and head of the spectroscopy group on the TFTR experimental team. He joined the Department of Nuclear Engineering and Engineering Physics at Wisconsin in 1989. He headed the Pegasus Toroidal Experiment and directed collaborative experiments on the DIII–D National Fusion Facility. He is a Fellow of the American Physical Society (APS), and served as President of the University Fusion Association for 1999–2000. He was chair of the Organizing Committee for the 2002 APS Topical Conference on High Temperature Plasma Diagnostics, and has served on APS Division of Plasma Physics organizing committees. He has served as a member of the Executive Committee of the Division of Plasma Physics. He has been a member of several Program Advisory Committees for large fusion science experiments, and is presently Chair of the Fusion Simulation Program Advisory Committee. He served on the Fusion Energy Sciences Advisory Committee sub-panel for U.S. participation in ITER. He also was a member on the Fusion Science Assessment Committee of the National Academies' National Research Council (NRC), and was co-chair of the NRC Burning Plasma Assessment Committee. He was a member of the NRC Board on Physics and Astronomy from 2003 to 2007, and was appointed a National Associate of the National Research Council of the National Academies in 2008. He presently is a member of the Fusion Advisory Board of the United Kingdom. Recently, he served as Associate Director of the Fusion Energy Sciences Program of the U.S. Department of Energy Office of Science. In that role, he led the fusion science program as it moves to exploring the burning plasma regime in the ITER international experiment. The program also supports investigations of magnetically confined plasmas, basic plasma science, high energy density laboratory physics, and fusion engineering sciences. Prior to his appointment to DOE, he was serving as the Director of the U.S. Burning Plasma Organization and Chief Scientist of the ITER Project Office. His research has been in experimental studies of high-pressure plasmas in toroidal geometries, plasma turbulence, and high-temperature plasma diagnostic development. He was awarded the 1999 APS Award for Excellence in Plasma Physics Research for his work on measurements of turbulence in high temperature plasmas. He was also awarded the Fusion Power Associates 2004 Fusion Leadership Award. He is the author of over 180 articles in publications on plasma turbulence, plasma confinement and stability, atomic physics, applied optics, and plasma measurement techniques.

DISCUSSION

Chairman BAIRD. I thank all the panelists for not only your testimony today but for your very distinguished careers and your service to our country as scientists, and I want to ask a number of questions and will recognize myself for five minutes to do so.

I want to put my questions into context. The context is known, and I often mention it regardless of the topic of this hearing, but it is worth putting forward. The hearing is in the context of a time when our country faces an \$11 trillion debt, a \$1.4 trillion deficit over the last fiscal year. That is our fiscal situation. Our energy situation is that we, if as I believe the evidence is compelling that we face global overheating and acidification of our oceans, the pace at which we need to make significant cuts in CO_2 and other greenhouse gases is much more rapid than any of the proposals currently moving through the Congress, and I think more ambitious than any of the proposals in terms of our reduction of greenhouse gases. That puts a budgetary constraint and a timeline constraint, and so with that as context, let me ask a series of questions so I can understand the sort of timeframe we are dealing with.

HOW FUSION ENERGY BECOMES A USABLE RESOURCE

Just first of all, how—when we speak of ignition, which I understand is when more energy is put out than put in, in layman's terms, what is the longest period of ignition achieved so far in any of our modalities in terms of time, how much time?

Dr. SYNAKOWSKI. Today actually no plasma has ignited. We have had plasmas with controlled fusion reactions. An analog I like to use is that it has been like burning wet wood. We have created a fire, we have controlled the fire, when we take away the external flame, the fire goes out.

Chairman BAIRD. Okay. Let us suppose we achieve ignition, so the challenge, I think once ignition is achieved—this is not an easy thing and you folks have dedicated your careers as brilliant as you are to this, and many others before you and I am sure after. So once we—if and when we actually achieve ignition, that is a eureka moment in a way but it is not like all our problems are solved because now we have got more energy coming out. The challenge is capturing that energy. You have got to somehow capture it. My understanding is that, and correct me if I am wrong, and this is not meant in any way pejoratively, but basically the way we are going to actually capture that energy is the good old-fashioned way of making water hot and turning turbines. Is that the model we are doing? And it is really great that we are still doing steam engines. I just love this. I don't mean that critically but it just kind of blows your mind that we are going to all this trouble to heat water up.

Dr. SYNAKOWSKI. It is a remarkable thing to go from $E = MC^2$ to boiling water in a turbine. That is essentially the train you are talking about.

Chairman BAIRD. So it is really important for this committee and I think for Americans and taxpayers to understand that just because we get ignition doesn't mean we can now expect that oh, then next week we are going to turn the lights on with energy produced by fusion energy, right? We have got to somehow then find a way to actually harness that energy, and the current model is again through steam-driven turbines, right? Is that—Dr. Betti?

Dr. BETTI. Yes, but there are two aspects. Ignition will prove the physics of fusion. Making energy after ignition requires the development of the technology, and often the development of the technology is faster than development of the physics. So that is why it is so hard, because we haven't proved the physics yet, and then we count on the technology to move a lot faster and turn the physics success into making electricity.

Chairman BAIRD. But there are some real challenges there, in terms of——

Dr. BETTI. Major challenges.

Chairman BAIRD. The material science of what kind of material can contain these reactions and sustain the bombardment of the neutrons, et cetera, right? And then so we have got some challenges ahead. Dr. Prager.

Dr. PRAGER. I think you are exactly correct, and a lot of the suggestions you have been hearing from the panel members get exactly at the questions you are pointing out need to be resolved. How one harnesses the fusion power is sometimes the expression used. So there are ideas to set up a research program to deal exactly with that question in addition to the remaining physics questions.

POTENTIAL CONSUMER PRICES FOR FUSION ENERGY

Chairman BAIRD. Then there is the next question. Okay, so let us suppose theoretically we can do it. We have solved the physics problem. We demonstrate that ignition is possible. We contain it with magnetic fields. We heat the water up. Then there is this little nagging problem of cost per kilowatt-hour. This seems to be a bit of a challenge. Any thoughts about cost per kilowatt-hour?

Dr. MASON. I don't want to offer up a number but I would sort of characterize it a little bit in the sense that with fusion the—you know, no one is talking about electricity too cheap to meter. I think everyone has learned the lesson that you shouldn't confuse fuel cost with what things actually cost. Fusion will be a fuel source that is dominated by capital costs because there is no ongoing substantial fuel cost, and of course, capital costs have to do with financing models and lifetimes and things that go well beyond the realm of physics and engineering. But if you look at the scale of plants that are being contemplated and the complexity of them, I would say it is not fundamentally different than the kind of cost model you see associated with fission. And what we have seen is that fission power is actually very cost-competitive, but because of the large upfront capital costs, it is difficult for risk markets to handle, and that is why things like the loan guarantee programs and so forth are very important. And so I think with the right sort of policy framework, it can be very cost-competitive, understanding that things like the programs that are in place now that are hopefully going to get us going with new starts in fission are likely models that would have to be explored, particularly at the outset when the risk is much higher.

Fusion as an Alternative to Other Energy Sources

Chairman BAIRD. Which leads to the question, you know, for me one of the challenges we face is, we have this imminent problem of global overheating and ocean acidification. We have this tremendous budget debt and deficit right now, and we have alternative technologies that today you can buy off the shelf that produce net energy output with the existing fusion energy thing called the sun at a known kilowatt-per-hour generation that if you invest, you know, with money being both finite and fungible, if we invest X amount of dollars now, we can lower our carbon output, et cetera. And so I think it is just really important for us to understand the win of this. So I have been talking costs and how and stuff. The win seems to me to be a good bit down the road. By win, when I say win, we could actually generate significant replacement of existing energy sources. I would be shocked if any of you would say less than 20 years and I guess it is more like 50. I may be wrong.

Dr. MASON. I alluded to this a bit when I talked about the portfolio of energy choices, and I would kind of divide it into three categories. In the very near-term, the most fruitful sorts of investments we can make are in energy efficiency. There are a lot of things that we know how to do that can immediately reduce demand, and the cheapest form of energy is the energy you don't need. In fact, most of the energy efficiency steps which represent about a third of what you need to achieve the types of CO_2 emissions that people are talking about cost you a negative amount of money. In other words, you save money by doing it. And so there is no question in the very near-term that is the low-hanging fruit that we need to go after aggressively. Things like renewables, wind and solar are—we know we can make them work but they are not yet cost-effective, although there are a lot of promising research directions that will improve that. And scaling up will bring down the cost. But these are intermittent, and so certainly when the sun is shining and the wind is blowing, we will want to be harnessing that energy and the environmental benefit that goes with it, but it is not a baseload generating capacity. We do need baseload capacity that we know will be on when people switch on the light switch and will allow us to buffer the intermittent renewables, and fusion has the possibility to offer a baseload generating capacity that does not have a fuel constraint. Right now most of our baseload capacity comes from coal, and in terms of timeline and risk, I would say our chances of being able to sequester CO_2 from coal-fired plants at the scale we need to is not greatly different from the challenge we face in fusion.

Chairman BAIRD. I don't dispute that.

Dr. MASON. And we can't be sure that either one will work.

Chairman BAIRD. I think that is a good point.

My time is up, past up, but I wanted to establish that line of evidence and questioning, and Mr. Inglis is recognized for five minutes.

Mr. INGLIS. Thank you, Mr. Chairman.

ARGUMENTS FOR PROMOTING FUSION RESEARCH

I wonder if others on the panel agree with Dr. Mason's assessment, that fusion is in the league of fission in terms of the capital costs and the involvement that we would have. Do you agree with that, Dr. Prager?

Dr. PRAGER. Yes, I would. One of the guiding principles, almost too much so, of the fusion program over the decades has been economic attractiveness. It has guided the kinds of plasmas we try to produce and has set goals for us, and throughout the years there have been many, many system studies of fusion systems in the future that as best can be done assess the cost, and the cost of electricity comes out to be competitive. There is a big "however." Those studies assume certain success in physics and technology so assuming that the physics and the technology research missions are accomplished, then they calculate that the costs are competitive. So I think what Dr. Mason says has been backed up by studies, and another caveat is of course when you try to project the cost of anything several decades into the future, it is fairly theoretical.

Mr. INGLIS. Does anybody else want to comment about that, about equivalency between the—are we in the ballpark of a fission kind of investment when we go to fusion if we make electricity that way?

Dr. FONCK. I will just back up what Dr. Prager said. There have been a lot of studies, and the answer is generally yes, these are large power plants. These are multi-gigawatt power plants typically to get the most efficiency out, and so you are in that ballpark in terms of the scale of the plant. Of course, the issues are different. Fission and fusion are quite different so the radioactive materials and the things you have to worry about are quite different, but the magnitude of the plant is about the same.

Mr. INGLIS. Let me make sure I understand that. What is the difference in terms of radioactivity in that?

Dr. FONCK. Well, fusion works with just deuterium and tritium. Tritium is just a gaseous fuel. There is not a lot in the plant and the radioactive waste you produce in fusion is mainly the structure that holds the plasma. There is no long-term highly radioactive waste that you get in a fission plant. Now, the fission people have ways to, if we ever get there, to transmute those wastes. But at the moment you are looking at very long-term, hundreds of thousands of years kinds of waste. Fusion doesn't have that. It has essentially a short-lived, hundreds of years, waste issue. You can imagine that, in a generation or two, it would decay away. And so it is a different radioactive profile. And that is one of the big advantages of fusion.

Mr. INGLIS. What are the other advantages? Why should the United States pursue fusion?

Dr. FONCK. I will throw in a few, and I am sure everybody else has their favorite. Well, there is one. The other of course is the ready availability of fuel. It is right out of seawater. All you need is deuterium and lithium. Lithium breeds the tritium. There is no danger of catastrophic failure of a fusion plant. The plasma state is essentially quite fragile. If anything happens, you get a leak in the chamber or something like that, the system just extinguishes itself. So it is quite safe, passively safe, if you will. The other thing of course is that it can be anywhere. It is a baseload energy source. I think to back up what Dr. Mason said, if you look into the future, not myself but energy experts, you only see two or three baseload possibilities in the future and it is fusion, fission, if you want to be carbon-free, of course, and possibly solar with storage, but that is a very hard proposition.

Mr. INGLIS. Anybody else want to add a reason to pursue fusion?

Dr. BETTI. I concur with my colleagues on both issues. In relation to your question, yes, fusion is clean and the fuel is basically unlimited. In the case of deuterium-tritium fuel is only limited by the supply of lithium. Of course, deuterium is abundant in seawater and it is unlimited. On the issue of cost, there have been several studies both about magnetic fusion energy power plants and inertial fusion energy. There have more studies on magnetic side than inertial side. The United States had a program until last year, the High Average Power Lasers Program, that were developing the technology of an inertial fusion energy- based power plant and so they were doing technology development, the cost estimates of this sort. To the best of my knowledge, the cost is competitive with fission-based nuclear power.

Dr. MASON. I would offer as an attribute of fusion that I think we in the United States should find attractive, the comment that I made about, you know, we talked about the fuel but another way to look at the fuel for fusion is that it is intellectual property and high-end manufacturing. The fuel is not something that you import from the Middle East. It is not something that you run out of and it is actually the essence of what our economy is built around, which is smart people and competitive industry, and so not only as a domestic supply of energy but as a possibly significant export market. If we can position our industry to lead in this field, there would be, I think, economic value for the United States.

Dr. PRAGER. Just getting into maybe a softer reason, since the fuel comes from the ocean's water and that should be accessible to all nations and you might speculate that the conflict over natural resources for energy between nations would be decreased with fusion driving the energy sector. So that is another reason. I mean, it is interesting, when most of us entered the field, what drew us in in terms of the application was that we were running out of energy. In the 1970s we were running out of energy, there were gas lines, an energy crisis, and now perhaps really what drives it, the dominant reason probably is no contribution to global climate change. So fusion somehow is almost the ideal and the dominating reason maybe just changes with the times as the problems that we confront become more clear.

Mr. INGLIS. Thank you.

Thank you, Mr. Chairman.

Chairman BAIRD. Dr. Ehlers.

NATIONAL SECURITY AND TECHNICAL ELEMENTS OF PLASMAS IN REACTORS

Mr. EHLERS. Thank you, Mr. Chairman. Unfortunately, all the good questions have been asked already. But let me add one other item to our list, Dr. Prager, and that is national security. We are treading very tenderly in some treacherous waters with our current energy policies, and I really suspect that far too many people who should know that don't know it, and I am not just talking about gas lines, I am talking about the whip that other nations can hold over our nation just because we do not have the energy resources that we would like to have, and it continues to concern me. There are endless conversations that I get into both inside and outside the Congress. People say oh, well, you know, we have these new sources of natural gas, and in Pennsylvania we can get this new gas, we are all set for years and years and years. Yes, we are set for years but not years and years. And this inability to deal with reality is just fascinating to me. People just assume that somehow the scientists, the physicists, the engineers will find a way out of these shortages. And I have taken the opposite point of view. I have often said that natural gas is too valuable to burn. It is an incredibly useful feedstock for the petrochemical industry. I don't know of anything that is going to be as easy and cheap to use, and we are burning it. So there is a multitude of issues here, not just energy issues but resource issues, security issues, et cetera, and I don't think that we as a Nation are confronting them as adequately as we should. Having said that, I do wonder—I am just asking questions, things that I really haven't kept up with the field at all. How are we going to contain the plasmas and how easy or how difficult is that going to be to actually extract useful energy out of a fusion reactor? And I know it depends on the different types of reactors but can one or more of you just give me a quick summary of where you see this field going?

Dr. SYNAKOWSKI. That is a great and deep question. I think we have many elements of the solutions to both questions in hand, a much more mature understanding with respect to how are we going to contain the plasma and control it. There has been tremendous progress since actually the late 1960s, when there was a transformative event in the invention of the tokamak, which is a kind of magnetic bottle, if you will. It's twirly shaped. Just a little bit of science here. The plasma, if you can imagine a donut-shaped magnetic field, this is the heart of magnetic fusion with the plasma which is charged particles, ions and electrons. They do a very good job of moving along the magnetic field lines but have a very tough time crossing the magnetic field lines. But there are lots of subsidiary processes that go on in the plasma that can force the plasma, the hot fuel to make that migration across the magnetic field. What you are trying to do with that magnetic field is confine it for long enough so that you can heat it rapidly to get up to fusion conditions where the plasma pressure is such that the fusion between the nuclei takes place.

What has happened as the United States has really turned towards the science of the plasma I think has been a tremendous set of advances in understanding the basic physics of how the plasma is confined in this magnetic bottle, and what levers we can apply to the plasma to optimize that confinement. And I want to make two points. One, this is a very deep intellectual exercise, which is I think worthy of investment to obtain U.S. capital. These are great scientific challenges but they are not empty challenges, they are the best kind. They are the ones that are directed towards a purpose because the answers that we are finding with respect to the science, for example, of optimizing the confinement in this magnetic field, enables one perhaps to make a fusion reactor smaller which enables then for the vision of a fusion reactor to be more economically attractive. The science is intimately linked to the final product and so I think for those who are interested more directly in the final product, it is a compelling enterprise. The United States aesthetic I think has been extremely strong in terms of understanding that plasma science. It has been emphasized here though we think a major frontier resides in crossing that bridge from that magnetic bottle to boiling the water and generating the steam, and that is the material science question and the challenge of harnessing a fusion power.

And just as a footnote to all of this, a significant alternate approach that has been mentioned, especially by Dr. Betti, is that of inertial fusion. It is a fundamentally different process where you take a small pellet of fusion fuel. There are no magnetic fields in most versions of the vision. And you compress it very suddenly on relying on the inertia of the fuel itself to kind of tame itself long enough for the fusion to take place. But external to that, the transition to the fusion power and getting the power on the grid looks quite similar. Both of them again represent very deep scientific challenges but again I think they are the best kind of scientific challenges because they have direct bearing on the output and the attractiveness.

Mr. EHLERS. Thank you.

Dr. MASON. I could maybe add a little bit about the materials because I think that is a very important aspect is how you transition from the environment where fusion is happening, whatever form of containment you have and the environment where you are, you know, holding a vacuum and boiling water and so forth. It is a very challenging materials problem that falls into the general category that we like to refer to as materials under extreme conditions, and fusion is perhaps the most extreme of extreme conditions in terms of the temperatures, the radiation damage that the material is subjected to, the presence of hydrogen and the effect that it can have on materials. My background is materials science, so while I can't say too much in depth about fusion, in terms of materials, these are difficult problems, and they are a different sort of problem than some of the areas that we focus on right now. We at Oak Ridge and around the world are very excited about nanotechnology and things we can do with that and thin films for photovoltaics. The materials we are talking about here are different types of steel. It is the materials of heavy industry, and to be honest, the development of new steels is not something that as a nation we have been doing a lot of in recent times. In fact, you know, many of the materials we have now were developed decades or even in some cases centuries ago. They have served us very well, but they don't necessarily have the characteristics to survive under the conditions that we need, and that is why many of us have talked about the need to look at these materials issues, even as we resolve the remaining physics questions of the plasma, and many of those materials issues are the same whether it is inertial or magnetic confinement. In some cases, they may even be the same or similar to those faced by fast reactors that might be used in closing the fuel cycle. It is maybe not as sexy as nanoscience but getting better alloys is an important part of this equation.

Mr. EHLERS. Thank you very much. I would like to move on.

Chairman BAIRD. I know Dr. Fonck and others want to add but I want to recognize Dr. Bartlett for his line of questions.

FUSION VS. WIND AND SOLAR ENERGY

Mr. BARTLETT. Thank you very much. Whether or not we are successful in getting fusion power, and I am skeptical, I am still enormously supportive of this work because I think that we may find a lot of very other interesting things as we pursue this leading edge of scientific inquiries.

Dr. Mason, you mentioned efficiency as a major interest. I would like to suggest that before efficiency we can get huge gains from conservation. Conversation is two people getting in a car instead of one. Efficiency is getting in a Prius instead of an SUV. And we have enormous opportunities for gains in conservation, which could be immediate and free, really, really simple. You know, we have a huge reactor in the sun and I know that we disparage the use of solar and wind as baseload but I think it would be less technically challenging to make that baseload than it would be to produce fusion power. Wherever you have a topography difference, doesn't pump storage work very well for storing the excess energy you have when the wind is blowing and the sun is shining? And I suspect that creating huge banks of capacitors or enormous flywheels would be technically easier to do than trying to do what we are doing with fusion. By the way, I am a huge supporter. If the capability were out there, two and three times the amount of money that we appropriate for that, I would be happy to recommend that to the appropriators. But aren't there enormous opportunities for making solar and wind baseload?

Dr. MASON. Energy storage has huge leverage. If we could store even a small fraction of the grid for 24 hours, it would——

Mr. BARTLETT. You can store it all in pump storage, sir. Just pump it up to the mountain and a lake up there and then run it down when the sun is not shining, wind is not blowing through a turbine. It is really simple.

Dr. MASON. And in fact, in Tennessee TVA has something called Raccoon Mountain where they do exactly that, so it does work, but if you look at the capacity and efficiency of it, it is hard to see it scaling to the level that we would need. Now, if you push renewables as we should, you can probably get up to about 20 percent and still handle the intermittency. And you could push that farther if you had better energy storage, whether it is electrical energy storage in the form of batteries or compressed air. So I think there is tremendous leverage in storage. It is an area we should be and are investigating. But on grid scales, I believe it is a very challenging problem, and it is one that is maybe not quite as difficult as fusion but it is in the same league.

ELECTRIFYING TRANSPORTATION

Mr. BARTLETT. Dr. Betti, you mentioned something else that I think most people don't understand. You said that we should be electrifying our transportation, and you know, we use two kinds of energy. One is electrical energy, and I think the future for electrical energy is okay with nuclear, whether it is fission or fusion, with wind, with solar, with microhydro, for which there is considerable potential, and for true geothermal where we are tapping into the molten core of the Earth, I think that we can make about as much electricity as we ought to be using. But it is not true for liquid fuels because there is just no combination of substitutes out there for liquid fuels, and when gas and oil and coal are gone, and they will be, we are going to be living on electrical energy and so I think that too few appreciate the concern that you have that we need to be electrifying transportation because that is one essential use of liquid fuels. Some of the use of liquid fuels we can use electricity for, but for that one now, it is tough. We just tore up all our streetcars. We were proud that we were doing away with these antique things and we tore them all up. Now we need to be putting them back. Thank you for your recognition that we need to be doing that.

I welcome a second line of questioning, Mr. Chairman. I yield back.

Chairman BAIRD. Thank you, Dr. Bartlett. It looks like we are all going to probably not be able to do the second line with the votes being called, so I recognize Dr. Rohrabacher—Mr. Rohrabacher.

Mr. ROHRABACHER. Dr. Rohrabacher. That was quite a——

Chairman BAIRD. It is a frightening thought.

SKEPTICAL ARGUMENTS

Mr. ROHRABACHER. Let me just note, Mr. Chairman, that we heard that before back in the 1970s. We saw the gas lines and we were told that there was an imminent situation where there would be this massive shortage of energy and that was proven false, and now we are using an excuse of greenhouse gases which will cause global warming as an excuse to move forward on certain things, and quite frankly, none of the predictions of those people who have been advocating global warming have come true. In the last nine years there has been cooling, and in fact, there are now reports that the polar ice cap, the one pole or the other—the Antarctic was never contracting but the polar ice caps are now expanding. So this idea of greenhouse gases causing global warming is the basis of a lot of things but I would not use it if I was in the scientific community as an excuse for moving forward with fusion energy research because that I think is becoming something that again is another theory that will be proven false and it is being proven false by the way the world is acting.

When I was a young child, and I was actually in I think fifth or sixth grade, I saw a wonderful movie about fusion energy, and Mr. Science, I don't know if you remember those things, it was wonderful, and fusion energy was the energy of the future, and you know what? I am 62 years old now and I take it from what you have told us today that we haven't even had ignition yet after all of these years of research, and my calculation is that we have had $40 billion worth of research and we don't have ignition yet. Dr. Bartlett's observation is that research money—there is limited research money in this country—might well be put to better use in finding out how we can utilize the heat from the core of the Earth or the pumping technologies.

Mr. BARTLETT. I want to do both.

Mr. ROHRABACHER. Both. I do too. But we have limited research dollars. Why is it that fusion after all of these years and all of this money and with so little actual progress, meaning we haven't even had ignition yet, even for a second, I believe, why should we continue? Why shouldn't we transfer this money to some of these other technologies that perhaps would be cheaper? And one last thought, Mr. Chairman, and that is—and then I would like the panel to go to that question—and that is, I do believe that we should have at least one skeptic on the panel for every subject that we look at, and it is okay, I mean, all of you gentlemen have incredible credentials, a lot more than I do, that is for sure. But some of the issues that I am raising should be raised by people who have got Ph.D.s in this and be able to have a dialogue so that we will have something to decide here on the panel rather than just accepting one point of view.

I have made a couple points. Please feel free. I know you have got some things to counter there.[1]

Dr. SYNAKOWSKI. If I may, I think there are probably many facets that people might comment on. I think—actually I have a view that the urgent things and the opportunity we have is in fact to address the questions of credibility that you raise. Our science basis is such that I believe we have good confidence in what is required of our next step to get to the stage of what we call burning plasmas in magnetic fusion. I think that is what you are referring to when you are talking about ignition, where we are getting more energy out than we are putting in to heat it and control it. I think our understanding, the scientific basis for getting there is quite strong. And we understand it, and I will oversimplify it a little bit by saying it is a question of scale. We understand that the present devices that we have invested in are not appropriately scaled and don't have the control technologies that we need to reach the scientific regime that you are talking about. Having said that, in the early part of, I think it was in 2001, there was a technical assessment of several options in magnetic fusion that we could pursue to understanding this burning plasma state.

Also with respect to credibility, I think if you burrow down one more level, you get to the question that people had been raising here, and that is the question of materials and harnessing these fusion plasmas. I would be delighted, I would view it as a major accomplishment collectively in our careers to be able to point quite definitively to the answers to your questions demonstrably. I believe the scientific understanding is strong, that we have a confident and strong bridge to the demonstrations that you are talking about. Understand also that publicly there are many who desire exactly the sort of thing that you are talking about. I think we are in reach of doing that.

Mr. ROHRABACHER. Well, I have supported nanotechnology and these things, and I do support them, but they take research money,

[1] See response from Dr. Prager in Appendix.

and we have had $40 billion eaten up for fusion that perhaps had we put into nanotechnology or some of the other more that we have some actually demonstrations of progress, perhaps some of the other issues you are dealing with would have been solved.

Chairman BAIRD. My colleagues, we now have about eight and a half minutes left to vote, and what I would like to do is, I think rather than asking these gentlemen to wait 40-plus minutes while we go vote and come back and sometimes don't come back, what I would like to do is close, but Dr. Bartlett had some questions earlier that I know folks wanted to respond to. I would invite the witnesses, if you have additional responses, I know there some eyebrows raised about whether $40 billion is the correct number that has been spent, please feel free to give us some written comment.[2]

A FEDERAL AGENCY HOME FOR INERTIAL FUSION RESEARCH

I would just like to close with two questions. I want to make sure we get one issue on the record real quickly. My understanding is that the inertial fusion energy effort is, I am not sure the proper way to say it but does not necessarily have an official home within either NNSA or the Office of Science Research, and I am wondering is that is something we ought to consider addressing. I will give you a couple minutes. We are down to about seven minutes to get over to the vote. So any brief comments. Dr. Betti, you are in charge of that operation.

Mr. BETTI. Yes. So first I should just really briefly mention the fact that the physics principles of inertial fusion energy, what we call ignition, has actually already been demonstrated because that is how hydrogen bombs work. Okay. The problem is that to trigger ignition in hydrogen bombs——

Chairman BAIRD. It is a hell of a way to heat your house.

Dr. BETTI. We use an atomic bomb. No, but this is important. We use an atomic bomb. So what we are trying to do is to replace the atomic bombs with a driver, a laser, okay, but the physics principles have been demonstrated. What hasn't been demonstrated is that we can reproduce this in a laboratory. So that is an important distinction. In terms of the inertial fusion not having a home, inertial fusion energy doesn't have a home. Inertial fusion does have a home in NNSA for weapons——

Chairman BAIRD. Good point.

Dr. BETTI. Okay. So it is very important, I think, and very cost-effective to use the facilities that have already been built by the National Nuclear Security Administration for billions of dollars that are already there including the National Ignition Facility or mega laser and so on. We can use this to study the energy applications of inertial fusion and so that is why I think it is critical to have a home for fusion energy, inertial fusion energy, and use these facilities. We don't need a lot more facilities.

Chairman BAIRD. So you have got the physical home in terms of the infrastructure.

Dr. BETTI. The infrastructure.

Chairman BAIRD. Bureaucratically, where should the home be, NNSA or DOE, or both?

[2] See response from Dr. Prager in Appendix.

Fusion as an Unacceptable Substitute for Conservation

Dr. BETTI. Well, I mean, this is really not really for me to answer the question. I mean, I would think that the fusion energy development program should be within the Office of Fusion Energy Sciences but that is my personal preference.

Chairman BAIRD. Let me ask one last question and my colleagues are ready to go, quick question. If anybody were to say hey, we don't need to conserve—I want to really put the punctuation point on Dr. Bartlett's. If someone were to suggest we don't need to engage in conservation or renewable energy development because we have got fusion right around the corner, anybody agree with that at all?

Dr. MASON. I think this is not an either/or. We absolutely have to conserve and do energy efficiency, but we should not fool ourselves to think that that by itself will get us——

Chairman BAIRD. I get that, but it would be foolish to say that fusion is right is around the corner, it is going to solve all our energy needs. Mr. Davis is here. I am going to actually—very quick question. My colleagues are free to head out. But if you have other questions to ask, we will not be returning for this panel.

Mr. DAVIS. Just a comment and question, and Dr. Mason, I will probably converse with you later today about the question I am going to ask. There is a great deal of excitement about when the solar and other renewables being discussed now, if in fact as some believe that will supply our energy needs, why do we bother with fusion at all?

Dr. MASON. I think that the challenge is that in order to get to the sort of goals that I think we a nation have in terms of energy independence and emissions, we are going to need renewables, we are going to need storage, we are going to need baseload carbon-free generating capacity as well. And so we should certainly be using renewables as much and as quickly as we can, but they will not scale to meet all of our needs, and that is where a clean baseload generating capacity like fusion has the potential to be very valuable as part of our longer-term R&D portfolio, not to say that we shouldn't be pushing as hard as we can and as fast as we can on the things that we can do easily and quickly, like energy efficiency.

Mr. DAVIS. We get roughly 20 percent from fission nuclear energy today. What would be a—what would you see as a possible projection from fusion, nuclear fusion and the research we are doing?

Dr. MASON. Fusion can play exactly the sort of role in our electric grid that fission plays today and in fact in the end fission has a fuel supply need, so in the very long-term fission would be probably superseded by fusion.

Closing

Chairman BAIRD. Dr. Ehlers had a final comment.

Mr. EHLERS. I want to thank you for holding the hearing. It has been very, very useful to me, but I also want to congratulate you and the panel. I think this is the first hearing we have had on fusion that didn't result in questions about cold fusion. So we did have a little bit of disagreement about climate change but maybe we are making progress here. Thank you very much.

Chairman BAIRD. I want to echo Dr. Ehlers' comments. The frankness and honesty about both the potential and the limitations and the challenges have been very refreshing and much appreciated by myself and I think by my colleagues as well.

I again, thank the witnesses for their time and for their many years of service, and we look forward to continuing the dialogue. If you have additional comments, the record will remain open for two weeks to offer those, and with that, the hearing stands adjourned. Thank you very much.

[Whereupon, at 11:26 a.m., the Subcommittee was adjourned.]

Appendix 1:

Answers to Post-Hearing Questions

Responses by Dr. Edmund J. Synakowski, Associate Director for Fusion Energy Sciences, Office of Science, U.S. Department of Energy

Questions submitted by Chairman Brian Baird

Trade-offs on building a new large U.S. facility

Q1. Among the three major magnetic fusion facilities in the U.S., the youngest has operated for 10 years and the oldest for about 25 years. At the same time we have heard testimony from several of the witnesses for this hearing on the need for a new large fusion facility in the U.S., parallel to ITER, that can operate in a nuclear. environment for advanced materials research. ITER is not designed to fully address this research area, and you note that it would be critical for a future fusion power plant. Do you envision. closing down any or all of the major facilities we have today to achieve these new capabilities within a realistic budget scenario?

A1. The programs carried out at the existing major facilities are vigorous and strong; but, like all of our programs, they are always being evaluated in the context of the evolving needs of our national program. It is essential that the U.S. assert leadership in the fusion sciences where we can make fusion energy 'a reality as soon as possible. We have a clear understanding of the science and technology issues that must be resolved. So, upgrade, redirection, or orderly closeout of any element of our program are always options in maintaining the research portfolio necessary to make fusion energy a reality as soon as possible.

Q2. Do you see a point of diminishing returns for any of these current facilities on the horizon?

A2. The intellectual return on research performed on the major U.S. facilities is strong and puts the U.S. in a leadership position in many aspects of the fusion sciences. We will, however, continue to monitor these facilities as we go forward and, as the global fusion research landscape evolves, we will continue to assess their suitability for continuing to contribute significantly to fusion energy research.

Inertial fusion energy

Q3. Right now DOE's National Nuclear Security Administration (NNSA) stewards all of this country's major inertial fusion facilities for stockpile stewardship purposes, and your program within the DOE. Office of Science supports basic science on high energy density plasmas which may be relevant to inertial fusion energy. Still, there is no bureaucratic home in the Federal Government for inertial fusion research specifically for the purposes of producing energy. Should DOE wait until NIF achieves ignition to formulate a strategy to address this, or would it be wiser to have worked out a comprehensive plan and to have formally initiated a small, early-stage inertial fusion technology program ahead of such an event in order to immediately address the research opportunities it provides?

A3. It is reasonable and prudent to explore how inertial fusion energy research might be most effectively managed ahead of NIF ignition. While the details of such a plan will depend in significant part on the results obtained on the path to ignition, an in-depth strategic analysis of the challenges and potential of an IFE program would be helpful.

As presently configured, the respective SC and NNSA fusion offices have different technical strengths and different missions. Each, however, would play a significant role in IFE research.

It has been recently proposed that the National Academies undertake a study regarding the path forward for IFE in the event of NIF ignition. Such a study should highlight many of the science and technology issues that need to be addressed for an IFE program to succeed. This study, and our ongoing experience in SC/NNSA joint management of the high energy density laboratory plasma science program will help inform any future discussions and decisions regarding governance of a broader IFE science and technology mission.

Q4. Should the Office of Science or NNSA have the lead role in advancing this technology for energy?

A4. Both offices have expertise and resources needed to give inertial fusion energy the best chances of success. The advisability and governance of a possible future single program are policy issues that need to be assessed and determined going forward.

Q5. In view of the additional mission built into NNSA's authorizing legislation "to support United States leadership in science and technology," would it be more appropriate for that agency to continue stewarding advanced technologies that spin out of its weapons program, even if the final application is energy-related rather than weapons-related?

A5. The production of ignition will itself be a preeminent demonstration of U.S. leadership in fusion science and technology. Whether NNSA continues to steward advanced technologies that are spin-offs from the weapons program is a policy decision that needs to be fully considered and determined in the future. The National Academies study noted above should provide an initial framework for serious discussion?

Q6. Recently, in the Conference Report for the Energy and Water Development Appropriations Act, 2010, DOE was directed to review an inertial fusion energy research project at the Naval Research Laboratory and report on its findings within 60 days. The Conference Report also states: "The conferees encourage the Secretary to explore all possible opportunities to ensure that this program, which offers unique potential for long-term energy independence, is not abandoned for lack of a bureaucratic home." Please describe the Department of Energy's plans for this program, and what the impact to inertial fusion energy research would be if this program were officially terminated?

A6. A proposal for performing some of the Naval Research Laboratory work was received in the fall. It was peer reviewed in a process managed by my office. The proposal for funding was declined based on this review. The Department of Energy is currently analyzing the challenges and potential of an inertial fusion energy program. A proposed National Academies study should highlight the science and technology issues that need to be addressed for a successful IFE program.

Small fusion experiments

Q7. Your office manages an "Innovative Confinement Concepts" program that is essentially a collection of small facilities at universities as well as national laboratories. These facilities can be grouped into several smaller categories including: basic science, support of major facilities, and alternative fusion concepts of varying stages of development. Should all of these facilities continue to be lumped into a single grant competition within the Fusion Energy Sciences (FES) program every few years when their applications can be so different? Should these facilities be more explicitly aligned with FES's other more clearly defined subprograms in the budget .process as appropriate?

A7. As the new Associate Director for the Fusion Energy Sciences program, I am taking a fresh look at all of our programs, how they align with our overall mission, and how proposals are solicited for the Innovative Confinement Concepts (ICC) subprogram. As an important step, a recent ICC solicitation issued by my office makes a shift compared to past ICC calls. The current solicitation calls for proposals that have demonstrable connections to the science of burning plasmas in the laboratory, or that can enable this science to advance. This is appropriate as we enter the burning plasma era. I am fully committed to nurturing this scale of experiment so that it has maximum scientific impact both for fusion in particular and for the plasma and material sciences more generally. Our Office also understands the inherent benefits of this scale of research to students in building strong direct experiences with experimental fusion and plasma science.

Appendix 2:

Additional Material for the Record

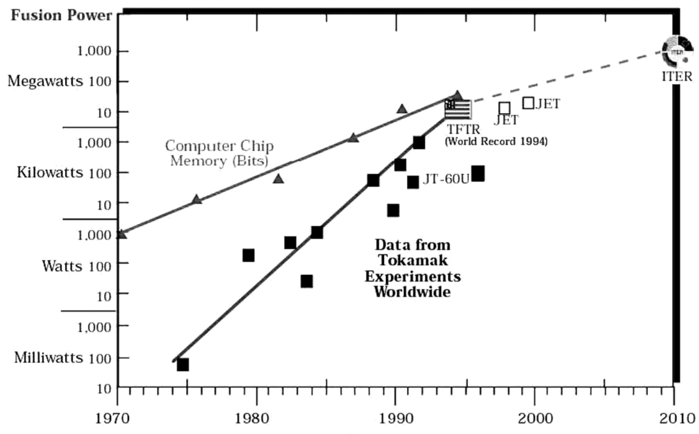
ADDITIONAL TESTIMONY BY
DR. STEWART C. PRAGER
DIRECTOR, PRINCETON PLASMA PHYSICS LABORATORY

At the fusion energy hearing on October 29, 2009 Congressman Rohrabacher raised three important issues that I wish to address briefly.

First, Congressman Rohrabacher stated that the fusion energy program has over the years acquired $40B in federal funding. This statement is incorrect. The total funding provided for fusion energy in the U.S. since 1953 is $11.5B (as spent) or $16.9B (inflation-adjusted) [source: S. Dean, Fusion Power Associates]. Current annual funding for fusion energy of $0.42B is close to, but slightly above the historical average.

Second, Congressman Rohrabacher asserted that there has been little progress in fusion energy. My response is confined to magnetic fusion energy. By any measure, the progress in fusion energy has been quantitatively enormous. Over the past thirty years, the fusion power produced in experiments has increased by a factor of 10 million, from 0.1 Watts produced for one-thousandth of a second around 1970 to 15 million Watts produced for seconds currently (see attached graph). Essentially every relevant scientific measure of progress, such as the fusion gain, has experienced an equally steady and steep advance. We routinely produce 100 million degree plasmas, and control them with unanticipated precision. Underlying this demonstrable and quantitative progress is the development of a new field of science—plasma physics. Fusion energy has both required and driven the development of plasma physics, which has had huge scientific and practical consequences beyond fusion—from understanding the cosmos to fabricating computer chips.

Third, Congressman Rohrabacher noted that despite large funding, we have not yet achieved ignition. For magnetic fusion energy, the approximate equivalent of ignition is attainment of a burning plasma. A burning plasma is self-heated by the fusion power itself. ITER will achieve this goal, as well as continue the advance of fusion power by producing 500 million Watts of fusion power for long periods of time. But, an historical note is also important here. About 20 years ago, the U.S. fusion community proposed an experiment called BPX (the Burning Plasma Experiment). BPX was endorsed by the DOE Fusion Policy Advisory Committee, which recommended construction. It was not funded. About 10 years ago, the community produced a design called FIRE, a modern experimental design for a burning plasma. Its mission and feasibility were affirmed by the DOE Fusion Energy Sciences Advisory Committee. It was not funded. Finally, ITER is funded to achieve this long-proposed goal. Had any of these earlier proposals been realized, we would now be studying burning plasmas. The scientific knowledge has existed for some time to achieve this milestone.



Fusion power produced (red curve) versus year. For comparison, the progress in computer chip memory is also shown, illustrating that fusion power has climbed even more steeply.