Testimony of
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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 US 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 US 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 US leadership in selected areas that focus on the credibility and eventual economic exploitation of fusion as an energy source. In particular, an 8 to 10-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 US 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 US. 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 US 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 4 challenges of fusion energy development.

This, however, puts the US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 US 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 outsourcing 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.