

Report of the FESAC Subcommittee on the Priorities of the Magnetic Fusion Energy Science Program

Written in Response to Dr. William Brinkman's charge letter to FESAC of
April 13, 2012

Riccardo Betti, Michael Brown, Vincent Chan, Bruce I. Cohen, Steve Cowley,
Ronald C. Davidson, James F. Drake, Nathaniel Fisch, Charles M. Greenfield,
Sibylle Guenter, Ian Hutchinson, Mitsuru Kikuchi, Mark Koepke,
Li Jiangang, William J. Madia, Rajesh Maingi, Michael Mauel, Ronald Parker,
Donald Rej, Robert Rosner (chair), Carl Sovinec, Peter Stangeby, and Steven
Zinkle

January 22, 2013 (revised February 10, 2013)

Dr. Martin Greenwald
Senior Research Scientist



Massachusetts Institute of Technology
77 Massachusetts Avenue, Building NW17-107
Cambridge, Massachusetts 02139-4307

Plasma Science & Fusion Center
Phone 617.253.6053
Fax 617.253.0627
Email g@psfc.mit.edu

January 31, 2013

Dr. William F. Brinkman
Director - Office of Science, SC-1
U.S. Department of Energy
1000 Independence Avenue, SW
Washington, DC 20585

Dear Dr. Brinkman,

With this letter, the Fusion Energy Sciences Advisory Committee transmits its report addressing your charge of April 13, 2012 on priorities in the Magnetic Fusion Energy Science Program. We want to thank Prof. Rosner and members of the panel for their efforts in taking on this difficult task.

As requested, the report proposes a set of scientific priorities for the program to target over the next 5-10 years. It then attempts to map those priorities onto major program elements under the three funding scenarios presented in the charge. We also expect that the prioritization will provide useful input for the panel now assessing new facility opportunities. The challenge of meeting the priorities under the budget assumptions, especially the reduced funding assumed in charge 1 is spelled out and the negative consequences that would flow from that scenario are clearly outlined.

FESAC acknowledges that the answers to the charge are incomplete. But given the COI guidance, the panel as constituted was unable to provide greater detail. The report does propose an approach toward that goal in its recommendation under charge 1.

This study generated wide-ranging and lively discussion at the recent FESAC meeting after which the full committee voted 10-4 to endorse the report.

Sincerely,

A handwritten signature in purple ink that reads "Martin Greenwald".

Martin Greenwald
Chair, Fusion Energy Sciences Advisory Committee

Cc: Patricia Dehmer
Edmund Synakowski
Al Opdenaker

Table of Contents

Table of Contents	ii
1. Preamble	1
2. Summary of Findings and Recommendations	2
Charge 1 Response.....	2
Charge 2 Response.....	3
Charge 3 Response.....	5
3. Committee Considerations.....	7
3.1 Reflections on Program Direction	7
3.2 Committee Organization.....	8
3.3 Conflicts of Interest.....	9
3.4. Science Prioritization Process.....	10
4. Prioritization of the <i>ReNeW</i> Thrusts	11
4.1 The Highest Priority Thrusts.....	13
4.1.1 Thrust 2: Control Transient Events in Burning Plasmas	13
4.1.2 Thrust 6: Develop Predictive Models for Fusion Plasmas, Supported by Theory and Challenged with Experimental Measurement	15
4.1.3 Thrust 9: Unfold the Physics of Boundary Layer Plasmas	17
4.1.4 Thrust 10: Decode and Advance the Science and Technology of Plasma-Surface Interactions	18
4.1.5 Thrust 17: Optimize Steady-State, Disruption-Free Toroidal Confinement using 3-D Magnetic Shaping, and Emphasizing Quasi-Symmetry Principles.....	20
4.2 The Other Thrusts	21
4.2.1 Middle Category	22
4.2.2 Third Category	25
Appendix A: References and Key <i>ReNeW</i> Fusion Science Thrusts	29
A.1 References.....	29
A.2 The Key <i>ReNeW</i> Fusion Science Thrusts	30

A.2.1 Thrust 2: Control Transient Events in Burning Plasmas.....	31
A.2.2 Thrust 6: Develop predictive models for fusion plasmas, supported by theory and challenged with experimental measurement	37
A.2.3 Thrust 9: Unfold the physics of boundary layer plasmas.....	40
A.2.4 Thrust 10: Decode and advance the science and technology of plasma-surface interactions.	45
A.2.5 Thrust 17: Optimize steady-state, disruption-free toroidal confinement using 3-D magnetic shaping, and emphasizing quasi-symmetry principles	47
Appendix C: Acronyms and Abbreviations.....	55
Appendix D: Statement of Charge.....	60

1. Preamble

This report responds to the charge given to the Fusion Energy Science Advisory Committee (FESAC) in April 2012 by Dr. William Brinkman, Director of the Office of Science at the Department of Energy (DOE), and reflects extensive discussions of the subcommittee of FESAC appointed to respond to this charge.

The subcommittee was fully constituted by early July 2012, and initiated discussions on 18 July in Bethesda, MD. At this meeting, four breakout groups were set up. These covered basic fusion science, fusion science directed principally at the ITER, fusion science in the post-ITER era, and fourth group designated to provide feedback to the subcommittee on the policy implications of our recommendations. A second meeting of the subcommittee took place 31 July to 1 August, in Bethesda, MD; a planning meeting, with participation from the leads of the three breakout groups, took place at O'Hare Airport in Chicago on 31 August, 2012; a full subcommittee meeting took place on 10-11 September 2012 in Gaithersburg, MD; and a final meeting of the subcommittee took place in Gaithersburg on 10-11 January 2013. The subcommittee also took advantage of the regular APS Division of Plasma Physics meeting in Providence, RI to meet on October 28, 2012 to come together for detailed discussions. At the beginning of this meeting, the subcommittee was given guidance by the Associate Director for Fusion Energy Science (FES) regarding conflicts of interest within our subcommittee; in short, the subcommittee was instructed to adhere to the FACA rules because it is an entity of FESAC, which is a FACA committee.

Our panel received substantial input from the fusion and plasma science communities. 62 white papers were submitted, and two virtual workshops were organized and hosted by the U.S. Burning Plasma Organization (USBPO) during the deliberation period of our panel.

Responding to the charge was challenging because the context we found ourselves in is complex. The U.S. is widely regarded as one of the world leaders in both plasma physics science and fusion energy science, a position gained over the past five decades of substantial investments by the U.S. Department of Energy and its federal predecessors. The past two decades have however seen a substantial decrease in annual funding; and it must be recognized that much of the current strengths rest upon these historical investments. Thus, a key question for the U.S. fusion energy program is how to maintain its existing strengths in a much more constrained funding climate than the field has heretofore experienced. This question is particularly challenging to answer because the international fusion energy science community is currently engaged in a major transition, aiming to study for the first time a steady fusion-burning plasma at the ITER facility in France; and the U.S. is expected – and is expecting, based on its substantial investments in

ITER – to strongly engage in the science of ITER once it begins its research program operations in the early 2020s.

2. Summary of Findings and Recommendations

In this section, we summarize our Committee’s specific responses to Dr. Brinkman’s three charges. For each charge, we provide both our recommendation as well as justifications for our recommendations that link to the discussions of science priorities found in Sections 3 and 4 below, as well as in the more detailed prioritization discussions in Appendix A.

Charge 1 Response

Prioritize among and within the FY2013 elements of the non-ITER magnetic fusion portion of the Fusion Energy Sciences program, assuming the FY2013 Presidential budget request level of effort.

Under the FY2013 budget, the highest priority research topics are the five *ReNeW* thrusts¹ discussed in Section 4.1 and (in more detail) in Appendix A. However, we have concluded that the FY2013 FES Budget level is inadequate to address even the highest priorities in a timely way². Specific shortcomings include:

1. It is out of balance in its budget allocation to facilities operations (10%) and research (45%). It therefore fails to take advantage of major past capital investments. The typical Office of Science (SC) budget for each of its offices allocates at least 30% to facilities operations. [FY2013 Congressional request overview, page 14. See also this report, Appendix B.]

¹ These five *ReNeW* thrusts, discussed in both Section 4.1 and Appendix A, are Thrusts 2 (*Control Transient Events in Burning Plasmas*), 6 (*Develop Predictive Models for Fusion Plasmas, Supported by Theory and Challenged with Experimental Measurement*), 9 (*Unfold the Physics of Boundary Layer Plasmas*), 10 (*Decode and Advance the Science and Technology of Plasma-Surface Interactions*), and 17 (*Optimize Steady-State, Disruption-Free Toroidal Confinement using 3-D Magnetic Shaping, and Emphasizing Quasi-Symmetry Principles*).

² Given the conflict of interest issues discussed in Section 3.3 below, as well as time constraints on our deliberations, this Panel was not in a position to carry out a detailed, credible, re-distribution of funds among the FES budget elements for FY2013; to do so would have raised serious issues regarding either conflicts of interest (if the entire Panel participated) or adequacy and competence (if only a limited subgroup of this Panel participated in a funds re-distribution).

2. It jeopardizes ITER success because U.S. facilities are some of the best in the world to address urgent research needs. For example, capabilities in disruption mitigation, ELM control using non-axisymmetric fields or pellets, ELM-free operation, divertor and boundary issues at high heat flux, and world-leading diagnostics, make U.S. confinement facilities ideal vehicles for resolving ITER design and operational decisions.

3. It jeopardizes the U.S. ability to take advantage of ITER in the future, because it undermines our ability to attract top minds to the field. U.S. leadership is based predominantly upon the quality of our scientists and engineers. High quality students, who will become our future ITER researchers, seek a vibrant graduate research field in which there are dynamic opportunities at home as well as abroad. Moreover, at the proposed FY2013 domestic funding level even experienced scientists will leave the field.

4. It significantly weakens the preeminent capability of the U.S. program in innovative research and critical discovery science. Such areas range from advanced diagnostic development (*e.g.* plasma boundary, alpha particle, and Alfvén wave eigenmode diagnostics), to first-principles simulations of nonlinear processes that govern core and edge transport.

If this budget level persists, a thorough remapping between the high priority thrusts and the elements of the whole U.S. FES program must be undertaken.

As discussed in more detail in Section 3.1, we question the balance of the FY2013 congressional request that implements an “overall reduction in domestic research” while making “a modest increase in funding for scientific collaborations on major international facilities.” The overseas superconducting tokamaks are not yet ready to exploit their full long-pulse capabilities, nor are they as capable of addressing ITER’s urgent challenges as our domestic tokamaks are today.

Charge 2 Response³

Considering the same focus as in [Charge] (1), again prioritize the elements of the non-ITER magnetic fusion portion of the FES program, but assume a restoration of the budget to the 2012 level for that part of the program. New elements may be inserted in the prioritization after FY2012.

³ Only ‘non-conflicted’ subcommittee members participated in the discussions leading to our Charge #2; the specific participants were Michael Brown, James F. Drake, Sibylle Guenter, Mitsuru Kikuchi, Mark Koepke, William J. Madia, Michael Mauel, Robert Rosner, Carl Sovinec, and Steve Zinkle. The specifics of the Committee’s response to the issue of conflicts of interest are discussed in Section 3.3.

Our starting perspective in addressing this charge has been that even the FY2012 levels were insufficient to make full use of our nation's fusion research infrastructure or to allow timely completion of high-priority tasks, as outlined in Section 4 below.

If funding at the FY2012 level is made available (with approximately \$32M annually provided in addition to FY2013 levels), the following should be given priority:

1. We recommend that roughly one-third of the restored funds, \$12M, should be deployed for a three to five year period of operation of C-Mod to resolve high-priority topics on ITER-relevant boundary and divertor physics, and might include upgrades as required to accomplish these goals. This restoration is consistent with our highest priority thrusts 9 and 10, focuses on completing specific urgent research tasks relevant to ITER for which C-Mod is uniquely suited, and treats C-Mod as an critical experimental device in the preparations for ITER, but not as a long-term facility. Once C-Mod has completed its critical ITER-relevant tasks, it should be closed down so that funding can be re-directed toward other high priority science goals, as discussed under Charge 3.
2. We recommend that \$10M be allocated to increased utilization of DIII-D, covering operations and research focused on achieving faster progress on the urgent, high-priority research that DIII-D is carrying out for ITER preparations; this work (on disruption prediction, avoidance and mitigation, and ELMs) has been identified by us as part of the highest priority thrust work (e.g., Thrust 2).
3. We recommend that \$10M be allocated to a highly targeted support of theory and simulation. This support needs to be focused on the high-priority research thrusts discussed earlier, advancing specific new physics topics, and where appropriate building tools that are ultimately aimed at allowing broad use by the community. This allocation should not be viewed as a "general increase" of theory and simulation. One possibility might be to build teams that focus on issues falling within our highest-priority physics topics, and that might involve an experimental/observational component. Scientific progress should be closely monitored.
4. We expect that on a time scale of order half a decade, there will be a considerable evolution of the domestic major facilities, including closure of C-Mod and the completion of the NSTX upgrade. Consistent with this evolution, the program will need to consider next steps in the fusion major facilities portfolio; possible alternatives might include upgrades to DIII-D and a stellarator. We expect C-Mod scientists to play an active role in the formulation of these plans. Given likely timelines, it will be important to start planning for a major new facility as soon as possible.

While the prioritization of the non-magnetic fusion portion of the FES program is beyond our charges, the Committee nevertheless recommends that funding for FES's stewardship role for plasma science be restored to the greatest extent possible.

Charge 3 Response

Prioritize the elements of the non-ITER magnetic fusion portion of the Fusion Energy Sciences program for the five-year period following the roll-off in ITER project construction funding, assuming a 50% increase over that provided in the FY2013 budget in non-ITER-project magnetic fusion level of effort following the peak in ITER funding. Assume that research on fusion materials science and harnessing fusion power will capture much of this increase.

For the period following the roll-off in ITER project construction funding, with a 50 percent increase in non-ITER MFE effort, we recommend highest emphasis be given to science-rich feasibility issues that will directly impact the path to be followed to a DEMO fusion device. In particular, additional resources at this level would permit moving forward with a Fusion Nuclear Science Program (FNSP) and preparing the way for a Fusion Nuclear Science Facility (FNSF). A FNSF is a research facility that incorporates most of the technical components within the core of a future DEMO power plant, but built at minimum overall fusion power in order to enable fusion component testing and optimization at minimum tritium consumption and overall cost [Goldston, FESAC, 2003]. The FNSF allows research of high neutron-fluence plasmas that run reliably, without damaging transient events, and the evaluation of numerous fusion engineering issues including first-wall components that withstand fast neutron flux and the demonstration of tritium self-sufficiency.

At the present time, it is uncertain what materials would ultimately be selected for an FNSF, what would comprise the engineered components that make up the first wall, and what would be the magnetic configuration confining the burning plasma. The leading candidates for the magnetic confinement configuration are the advanced tokamak (AT), the spherical tokamak (ST), and the optimized stellarator. The research efforts that will inform the design of an FNSF facility were detailed in *ReNeW*. In addition to steady-state (Thrust 5), the Subcommittee recognizes especially that an expanded research effort in Thrusts 13, 14, and 15 (entitled “Theme 3: Harnessing Fusion Power”) is needed when the additional funds of this charge become available.

An expanded effort on materials research would enhance two broad categories of research: (i) fusion nuclear materials effects, and (ii) plasma surface interactions. A full 14 MeV D-T neutron spectrum test facility (e.g. IFMIF), lies beyond the financial capacity of the U.S. program alone. However, the recent FESAC [Zinkle] report on fusion nuclear materials identifies several medium-scale research initiatives that would directly address some challenges, albeit with less spectral accuracy. One or more of these initiatives would form a natural part of an expanded FNSP.

The FNS program would have experimental elements aimed at the grand challenges in the areas of “Plasma-materials interface, Conquering nuclear degradation of materials and structures, and harnessing fusion power.” [Zinkle report] Specific examples of program elements might include:

- Develop materials with micro-structures to mitigate transmutation produced helium and permeation of hydrogenic species
- Conduct neutron irradiation tests by leveraging domestic neutron sources, including neutron damage and tritium sequestration effects and evaluating designer materials above
- Participate in ITER Test Blanket Module program, should the opportunity arise
- Extend linear plasma devices, including appropriate upgrades from existing capabilities (e.g. tritium, liquid metals, rad. damage), to long time-scales, to help fulfill some of the critical FNS and PSI missions
- Initiate a comprehensive structural materials modeling program to address neutron damage, as part of a DOE-wide research program in this area

The plasma surface interactions category goes beyond present-day experiments both in the high operating temperature of the plasma facing components (at least 500 C) because of the need to minimize tritium retention and improve thermal efficiency, and in the week-long steady-state nature of the plasma exposure. It may be the best plan to establish the feasibility of such operation, which requires also reliable plasma sustainment, in a situation that is not complicated by the requirement for tritium handling and breeding. Such a facility, which does not yet exist, might be generically called a “pre-FNSF”; it is a high wall-temperature, high power-density, steady state, toroidal confinement facility.

- A pre-FNSF would be non-DT and would be a primary test-bed for developing a DT FNSF. [It might in fact be the first stage of an FNSF.]
- Much of the research work on a pre-FNSF would therefore necessarily be on confined plasma physics, including achievement and optimization of sustained current drive and identification and characterization of altered and new operating scenarios.

Axisymmetric (tokamak and ST) configurations are the best understood option for a pre-FNSF. The properties of the non-axisymmetric optimized stellarator are less well developed, but stellarators are inherently steady-state, operate at relatively high plasma density, provide greater design flexibility in their magnetic configuration, and may have less damaging off-normal events than found in tokamaks. Our Subcommittee cannot at this time specify the balance of funding to support an enhanced FNSP, nor determine the optimal steady-state plasma confinement configuration for a pre-FNSF. Provided sufficient progress is made in the world program that the plasma configuration can be reliably specified, then a greater share of the enhanced program can have a specific materials emphasis. In other circumstances, it might prove appropriate for the U.S. to initiate instead an experimental stellarator program through the construction of an

experiment with sufficient performance to establish the confinement of an optimized stellarator based on quasi-symmetry principles. This initiative could eventually lead to a steady-state nuclear facility based upon the stellarator, if it seemed more attractive.

3. Committee Considerations

3.1 Reflections on Program Direction

Our discussions were informed by three key goals for the U.S. fusion science program, which receive strong endorsement from the entire subcommittee:

1. Maintaining a strong fundamental plasma science program, which forms the base for all other efforts in the area of plasma and fusion science and technology
2. Insuring that ITER succeeds in meeting its science goals, which is a primary objective for demonstrating the *technical* feasibility of nuclear fusion as an energy source.
3. Establishing that fusion energy is a safe, environmentally sustainable, and *economically* feasible energy source, laying the basis for a transition of the present fusion science program to a fusion energy development program.

Accomplishing these three goals formed the basis for much of our deliberations. These discussions centered on two key issues: first, can the U.S. respond to the science and engineering challenges it has been tasked with as part of ITER construction and operation initiation? Second, is the U.S. in a position to carry out cutting edge research on ITER once ITER operations initiate? We believe the answers to these two questions are intimately related to the responses to the three charges we've been given.

The U.S. fusion science community has focused its ITER-related research activities at the major domestic magnetic confinement facilities with the expectation that our domestic research will help solve the ITER design and operational challenges in areas where the U.S. leads. At the highly constrained FY2013 Presidential budget request level, this expectation cannot be fulfilled. Alternative strategies for the U.S. program and for its stake in international fusion research will have to be developed. Due to the substantial reductions mandated in this scenario, finding a sound strategy for moving forward will not be accomplished simply or without loss. One proposal, suggested in the FY2013 FES budget request, is an "... overall reduction in domestic research ..." while making "... a modest increase in funding for scientific collaborations on major international facilities." We disagree, however, with this ordering of priorities. While we expect ITER will become a dominant experimental focus of the U.S. program at the end of its construction phase, the research facilities available today in the U.S. are in some areas uniquely equipped to tackle pressing challenges facing ITER design and operation. Although valuable

research opportunities will appear on non-ITER overseas experiments, cutting back on domestic programs where the U.S. is now making key contributions in the world's fusion effort will put at risk the success of ITER and diminish U.S. leadership. It will also impact both the existing workforce in fusion science, and our ability to attract and educate a new generation of fusion scientists and engineers that will exploit ITER and bring back its benefits.

The importance of maintaining a cadre of first-rate scientists and engineers in the U.S. capable of exploiting the science at ITER and recouping the substantial U.S. investments in this facility is a key point. Because the science activities at ITER will not initiate until the 2020s, and are expected to extend well into the 2040s, we will be training during *this* decade the cohort of scientists and engineers who will do much of this work – and lead the program – during those future decades. In our view, it is not only the actual funding levels as it is the uncertainties in available funding – and the consequent questions these uncertainties raise about the future of the field – that can prove to be damaging. We have seen similar issues arise in the past in other areas of science and engineering. In the case of fusion science, these difficulties are particularly vexing because we are expected to participate at a world-leading level in ITER science, once that facility transitions to full operations. We therefore view it as imperative to craft a national fusion science program that retains the inspiring spirit of discovery – and research activities – needed to attract the ‘best and the brightest’ in this era of ITER construction, that is focused on those science issues that we anticipate will be relevant in the coming ITER era. In this spirit, we emphasize the importance of maintaining strong experimental and theoretical elements in which graduate students in first-rate programs are directly involved in all aspects of plasma science.

With these considerations as background, the following chapter 4 and Appendix A discuss in some detail our views regarding scientific priorities, which formed the basis for the specific responses to the three charges posed by Dr. Brinkman to the FESAC, summarized in Section 2 above.

3.2 Committee Organization

Right from the start of our deliberations, we recognized that a coherent response to Dr. Brinkman's charge would need to start with a firm sense of the scientific priorities for the MFE program. We further recognized that these priorities could potentially change with time; and therefore we started by organizing ourselves along three distinct perspectives, aimed at prioritizing the science and technology presented in comprehensive form by the 2009 Research Needs Workshop (*ReNeW*) for Magnetic Fusion Energy Sciences.

Subgroup 1 (*Subgroup on foundational science and technology*) focused on the transcendent science and technology issues, that is, those issues that lie at the foundational level of plasma physics relevant to fusion energy science and technology.

Subgroup 2 (*Subgroup on ITER-critical science*) focused on the science and technology issues that will need to be addressed in order to ensure that the ITER succeeds as a science project.

Subgroup 3 (*Subgroup on post-ITER fusion science*) focused on those science and technology issues that will come to the fore after the ITER era, e.g., preceding the presumable transition era from MFE as a primarily basic science-oriented discipline to MFE as a primarily engineering-oriented energy discipline.

For obvious reasons, the perspectives defined by these three subgroups have been central to our analysis of the MFE program, and the specific responses to Dr. Brinkman's charge.

Subsequent analyses by our subcommittee, especially of the details entailed by the 18 research thrusts defined by the 2009 *ReNeW*, were then carried out by additional subgroupings. In this case, subgroup membership was much more fluid, and a number of subcommittee members contributed to several of the *ReNeW* thrust analyses. As already mentioned, the perspective defined by the Subgroups listed above have defined the structure of our thrust analyses.

3.3 Conflicts of Interest

As mentioned earlier, this subcommittee was instructed at the time of the APS Division of Plasma Physics meeting in Providence, RI (28 October 2012) to conduct our proceedings in a manner consistent with the FACA rules regarding conflicts of interest. Given the relatively small size of the MFE community, and the highly interactive nature of how it has been functioning, this is of course a challenging constraint for us. We have been addressing this serious issue in two ways. First, every member of the subcommittee has written – and circulated to the other committee members – a detailed declaration of their potential and real conflicts of interest. Second, we have systematically avoided touching on issues that potentially lead to conflicts of interest in our discussions since the Providence meeting. Furthermore, as we assembled the Report, the chair of this subpanel (R. Rosner) has been particularly careful about excluding written material generated prior to 28 October 2012 that might be interpreted as having been generated under conditions of conflicts of interest. Similarly, our response to the second charge (which potentially dealt with topics that are most likely to lead to conflicts of interest for some of the subcommittee member) involved only those subcommittee members with no actual or perceived conflicts of interest⁴; and the task of assembling the final report also involved only the

⁴ The specific 'non-conflicted' subcommittee members who participated in the discussions leading to our Charge #2 response were Michael Brown, James F. Drake, Sibylle Guenter, Mitsuru Kikuchi, Mark Koepke, William J. Madia, Michael Mael, Robert Rosner, Carl Sovinec, and Steve Zinkle. None of the

non-conflicted subcommittee members. For these reasons, we believe that we have been able to adhere to the FACA rules both in spirit and fact.

3.4. Science Prioritization Process

As the first step, the three subgroups of the FESAC Priorities Panel agreed to assess the 18 research thrusts of the 2009 Research Needs Workshop for Magnetic Fusion Energy Sciences, according to the perspective assigned to each group⁵. Our discussions of the 18 *ReNeW* thrusts were informed by five criteria for science prioritization. These are, does a given thrust

1. Provide the technical opportunities for breakthrough discoveries and excellent science?
2. Maintain or rebuild (especially at universities) critical skills, technologies, and competencies for plasma science and fusion research and development?
3. Enable U.S. leadership contributions to ongoing international fusion research?
4. Address, mitigate, and/or solve high risks to ITER performance goals?
5. Contribute to informing decisions about the future path of fusion development?

In addition, the context of this evaluation was provided by the broad scientific themes that define the field of basic fusion energy science, such as

1. Dynamics of plasma microturbulence and resulting transport of particles, momentum, and energy,
2. Plasma self-organization including (a) the formation, development, and structure of transport barriers, (b) dynamo effects in magnetized plasma, and (c) self-heating and self-driven effects in burning plasma,
3. Reconnection of magnetic fields and the associated energy conversion and acceleration of particles,
4. Wave-particle resonance phenomena, particularly in conditions that are thermodynamically out of equilibrium,

other Committee members participated in either the formulation of the second charge response, nor in any subsequent discussion regarding this response.

⁵ While scientific progress since 2009 might lead one to make minor adjustments to the 2009 *ReNeW* Workshop research thrusts, we believe that the 2009 *ReNeW* nevertheless continues to present a cogent community assessment of the main research foci for the ongoing magnetic fusion science program, and we therefore saw no pressing need to make any substantive modifications.

5. Cross-cutting, multiscale, multidisciplinary phenomena in extreme conditions that encompass plasma physics, materials and chemical sciences, and engineering sciences, and
6. Tradeoffs between symmetries and asymmetries in magnetic geometry.

It is evident that the three subgroups of our panel would rank the relative importance of the five prioritization criteria differently; thus, for example, Group 1 (Fundamental Science and Technology) focused primarily on the first 3 of these criteria. However, *our selection of the five fusion science thrusts that we have identified as the most important has been a consensus view developed by all three of the panel subgroups.*

In making the selection of the five ‘most important’ thrusts, we were quite aware of the conundrum that the remaining thirteen *ReNeW* thrusts contained program elements that could be viewed as comparable in importance to what we discuss in this section, albeit that these program elements do not suffice to make the thrusts in which they find themselves embedded rise to the top. Indeed, the key distinguishing element in these cases was not scientific importance, but rather *timeliness*, in the particular context of getting ready for ITER. For this reason, we will also discuss aspects of the remaining 13 thrusts that we view as important in Section 4 below.

4. Prioritization of the *ReNeW* Thrusts

In this section, we describe the 18 *ReNeW* thrusts for the purpose of explaining our thrust rankings. These explanations provide the reasoning for our ranking. Since this document will be read by a broad audience, with quite disparate backgrounds in the field of fusion plasma research, the descriptions in this section are relatively high-level and short; we provide a more detailed and technical description of the highest-priority thrusts in Appendix A. The following table summarizes our *ReNeW* Thrusts ranking into the three categories “Highest Priority”, “Middle Priority”, and “Third Priority”; the order of thrust presentation *within* these three categories reflects the numerical ordering presented in the 2009 *ReNeW* Report, and not a separate scientific ranking.

Table 1: Summary of the Committee’s ReNeW Thrusts science priority ranking

ReNeW Thrust	Description
Highest Priority Thrusts	
2	Control Transient Events in Burning Plasmas
6	Develop Predictive Models for Fusion Plasmas, Supported by Theory and Challenged with Experimental Measurement
9	Unfold the Physics of Boundary Layer Plasmas
10	Decode and Advance the Science and Technology of Plasma-Surface Interactions
17	Optimize Steady-State, Disruption-Free Toroidal Confinement using 3-D Magnetic Shaping, and Emphasizing Quasi-Symmetry Principles
Middle Category Thrusts	
3	Understand the role of alpha particles in burning plasma
4	Qualify operational scenarios and the supporting physics basis for ITER
5	Expand the limits for controlling and sustaining fusion plasmas
14	Develop the material science and technology needed to harness fusion power
16	Develop the spherical torus to advance fusion nuclear science
18	Achieve high-performance toroidal confinement using minimal externally applied magnetic field
Third Category Thrusts	
1	Develop measurement techniques to understand and control burning plasmas
7	Exploit High Temperature Superconductors (HTS) and other magnet innovations to advance fusion research
8	Understand the highly integrated dynamics of dominantly self-heated and self-sustained burning plasmas
11	Improve power handling through engineering innovation
12	Demonstrate an integrated solution for plasma-material interfaces compatible with an optimized core plasma
13	Establish the science and technology for fusion power extraction and tritium sustainability
15	Create integrated designs and models for attractive fusion power systems

4.1 The Highest Priority Thrusts

The five *ReNeW* thrusts identified by us as highest priority rose to the top because they both represent highest priority science and have a critical timeliness element paced by the need to start and operate ITER successfully.

Although not explicitly called out as a high priority thrust, it was noted that research supporting steady-state scenarios is cross-cutting and has connections to each of the high priority thrusts. The U.S. FES program is currently world leading in this area, specifically with regard to the control of current density through auxiliary sources and bootstrap current toward the goal of extending pulse length. Development of these operating scenarios is prerequisite to ITER's steady-state mission, to a long-pulse fusion nuclear science facility, and indeed to the ultimate goal of fusion energy. This work should continue as an important part of each of the high priority thrusts.

4.1.1 Thrust 2: Control Transient Events in Burning Plasmas

4.1.1.1 Background

Avoiding unplanned transients is essential to the concept of MFE. Moreover, the normal power loading of steady operation in our first burning plasma experiment, ITER, will stretch present-day engineering capabilities; unmitigated transients that concentrate energy will not be tolerated. The two primary concerns for tokamak configurations, including ITER, are disruptions (rapid loss of plasma confinement) and edge-localized modes (ELMs, periodic bursts of energy from edge plasma during high-confinement operation). ELMs are also problematic for the stellarator configuration. The key issues and proposed research activities of the respective *ReNeW* thrust are organized into four elements:

1. *Prediction of disruptions*—characterizing, understanding, and sensing conditions where disruption is imminent,
2. *Avoidance of disruptions*—actuation of controls to maintain stability against disruption,
3. *Mitigation of disruptions*—actuation of safeguards against material damage from rapid termination of a discharge, and
4. *Avoidance of ELM-induced impulsive heat loads*—understanding ELM dynamics and developing techniques and operational modes that eliminate large, damaging bursts.

Since *ReNeW* there has been significant progress, particularly in elements 3 and 4, but there remain significant unsolved problems in all four.

Consistent with the criteria for scientific prioritization, the panel rates this research thrust among the set of highest priorities. The urgency and importance of this thrust are driven by the need for successful operation of ITER while meeting its research objectives. This is also an area where the U.S. has clear leadership within the worldwide fusion program. In addition, disruptions and ELMs reflect questions of macroscopic plasma stability where understanding needs to be improved at a fundamental level.

Categorizing research efforts into near-, medium-, and long-term priorities reflects the urgency with which problems need to be solved for ITER and for fusion energy development. The final design review for ITER's disruption mitigation system (DMS) is scheduled for 2016, so the near-term priorities identified below emphasize the mitigation research element. Much of the required research does not require long-pulse operation, but it does require experimental platforms that can withstand disruptions and ELMs. It is, therefore, important that current U.S. machines are utilized for near-term needs while they are operational. On the medium and longer term, the tokamak energy development path requires advanced operational regimes with robust avoidance of disruption, and research for this must be conducted in parallel with work on the near-term issues. [A more complete description of research tasks and priorities in this research thrust is provided in Appendix A.]

4.1.1.2 Priorities

Near-term (1-2 years)

- There are three areas that need to be addressed on the near term: (i) the dynamics of electrons driven to relativistic energies during disruption need to be understood and means to suppress their harmful effects need to be developed, (ii) the leading approaches for mitigating disruptions must be tested and verified, and (iii) the physics basis for approaches to suppressing ELMs needs to be completed.

Mid-term (3-5 years)

- Beyond the near term, we need to improve understanding and modeling of instabilities that lead to disruption for a range of operation scenarios. Control methods for advanced operation should be assessed for application to disruption avoidance. Methods of disruption avoidance that are compatible with conditions in advanced operation of ITER must also be developed. Understanding naturally ELM-free operational scenarios and their compatibility with ITER and future devices is also important.

Long-term (6-10 years)

- Research that is needed for energy development includes optimizing the tokamak configuration for disruption avoidance and developing ELM suppression methods for stellarators.

4.1.2 Thrust 6: Develop Predictive Models for Fusion Plasmas, Supported by Theory and Challenged with Experimental Measurement

4.1.2.1 Background

The essence of scientific understanding is the development and validation of predictive models based on first principles. This thrust describes the mechanisms for translating experimental observations into concrete knowledge. Further, it is the fundamental understanding of plasma behavior gained through modeling and validation that connects our field to the broader scientific community in space and astrophysics. This thrust therefore receives high priority. The U.S. is viewed as a leader in magnetic fusion energy (MFE) theory and computation and has taken initiative in applying validation methods for comparing models with experimental data. The tight coupling between theory/modeling and experiment that has significantly strengthened over the past decade has transformed our ability to gain knowledge from the complex dynamics of plasma behavior probed in experiment. Maintaining and strengthening this competency is an important priority for the U.S. program. Theory and validated modeling contribute vitally to ITER experimental planning and interpretation, and are essential for the further development of MFE beyond ITER. Furthermore, theory and validated modeling are critically important elements in understanding experiments and developing the predictive capability needed to obtain the greatest benefit from current and future experiments. Some examples of the importance of theory and modeling are the following:

1. Modeling of turbulent transport is critical in ensuring that designs of future experiments such as ITER perform as expected. Projections based on existing experiments by definition project beyond the existing database and are therefore inherently uncertain. Minimizing this uncertainty allows us to proceed with greater confidence in extrapolating to future experiments, which can accelerate the development of magnetic fusion energy.
2. Theory and modeling will enable the design of new magnetic geometries for containing fusion plasma such as exploiting magnetic symmetries in advanced concepts for stellarator configurations, or developing a more robust steady-state, high-performance tokamak mode based on progress in scientific understanding. Optimizing such designs must be based on modeling since there are far too many options to feasibly explore with experiment.

3. Robust models can help identify key variables that can be used to greatly improve plasma performance by controlling transport and suppressing instabilities that can lead to disruptive behavior.

At this stage of scientific development, it is the panel's opinion that the highest priority should be placed upon developing a detailed predictive understanding and improved modeling of key physical phenomena that have impact on fusion plasma performance and/or that have significant scientific importance. In areas where maturity has been established, a coordinated effort should be established to address the coupling of physics elements associated with equilibrium, stability, transport, auxiliary heating, fueling, and exhaust. This is needed to guide experimental planning and to design future experiments. Moreover, coupled physics leads to multi-scale challenges associated with modeling plasmas realistically with opportunities for new scientific discoveries. The validation effort implies a strong partnership among theory, modeling and experiment, the existence and utilization of a variety of experimental facilities, and continuing the development of advanced plasma diagnostics.

4.1.2.2 Priorities

The following is a partial list of high priorities in this important Thrust Area. A more complete list of priorities is provided in Appendix A.

Near-term (1-2 years)

- Develop improved predictive capability for Edge Localized Modes (ELMs) and disruptions in tokamaks.
- Develop a robust understanding of 3D edge pedestal physics and predictive capability for pedestal characteristics in tokamaks and stellarators.
- Provide modeling support for disruption avoidance and mitigation.
- Establish focused verification and validation programs to address specific case studies in high priority thrusts.

Mid-term (3-5 years)

- Develop improved predictive capability for L-H transition, core and edge transport and, plasma heating and fueling.
- Develop an understanding of the self-generation of rotation and its influence on turbulent transport, transport barrier formation and plasma confinement.
- Develop integrated advanced simulation tools addressing multi-scale plasma phenomena and coupling of physics models when appropriate taking into account some of the Fusion Simulation Project (FSP) planning report recommendations.

- Expand verification and validation programs addressing high-priority thrusts.

Long-term (6-10 years)

- Develop a robust program in surface material simulation for fusion plasma.
- Initiate a structural materials research effort to address neutron damage as part of a DOE-wide research program in this area.
- Pursue integrated, whole-device fusion simulation as re-defined through high priority mid-decade activities.

4.1.3 Thrust 9: Unfold the Physics of Boundary Layer Plasmas

4.1.3.1 Background

Magnetic confinement sharply reduces the contact between the plasma and the vessel walls, but such contact cannot be entirely eliminated. The boundary layer is the transition between magnetically confined plasmas and the plasma facing components. Its characteristics help determine whether we can tame the plasma material interface and whether fusion energy is practical [as addressed in the “Greenwald” and *ReNeW* reports].

The edge transport barrier and resulting pedestal region strongly influence core plasma pressure and generate the scrape-off layer (SOL) plasma that carries heat and particles to material surfaces. The synergy of multi-scale processes is fundamental science for MFE physics, and the relatively immature state of understanding requires breakthrough developments to predict conditions in ITER and future experiments. Edge plasma physics is an area where critical skills and workforce need development for ITER and for the future of MFE.

For the purposes of this Panel, the boundary layer is defined as the region of the plasma that extends from just inside the separatrix out to the Plasma Facing Components (PFC’s). An improved understanding of the physics of the boundary layer is required. The high rate with which new boundary effects are being discovered indicates that the controlling physics of the boundary has been only partially identified. The first task is identification of the missing physics. This will require increased effort on edge experiments and their interpretation and on significant increases in edge diagnostic capability. What is the nature of the perpendicular transport in the boundary layer? What can be done to increase perpendicular transport in the near boundary layer, i.e. spread out the divertor power; and how will that affect the pedestal and core transport, as well as plasma contact with the wall? What is the role of neutrals, both those recycling and those injected as fuel, e.g. in charge exchange sputtering of the walls? Present models cannot quantitatively reproduce the observed features of detachment – the divertor condition that ITER is counting on. The aim of answering the above questions is to enable development of a

reasonably complete model of the boundary layer, one that is at least on a par with the excellent progress being made in understanding and modeling of the pedestal and core plasma.

4.1.3.2 Priorities

Near-term (1-2 years)

- More comprehensive diagnostics in existing devices, to uncover the controlling physics
- Empirical scaling(s) consistent with critical gradient and other constraints, to predict the peak divertor power flux density
- Measurement of off-normal heat loads

Mid-term (3-5 years)

- Fundamental understandings of cross-field transport mechanisms, including the relation between the SOL and the region inside the separatrix
- Effects of non-axisymmetric magnetic fields on the boundary plasma
- Innovative ideas for improved divertors
- Explore the detailed effects of RF heating on the boundary plasma

Long-term (6-10 years)

- Develop reliable predictive capability for the boundary of high power tokamaks

4.1.4 Thrust 10: Decode and Advance the Science and Technology of Plasma-Surface Interactions

4.1.4.1 Background

Plasma-surface interactions (PSI) encompass scientific challenges that are among the most critical for fusion power, affecting: 1) plasma contamination by eroded material, 2) lifetime of PFCs, owing to sputtering and transient erosion, 3) dust formation and tritium co-deposition in eroded and re-deposited material. In steady state, the impurity particles enter and leave the plasma at the same rate and, integrally, there is no *net* erosion; however, the *local* net erosion rate may approach the local gross erosion rate. It is the net rather than gross erosion that primarily matters for lifetime of PFCs and tritium retention by co-deposition. The relation between net and gross erosion depends on material migration within the plasma, which is poorly understood even for single-element PFC systems. For mixed materials, such as ITER will employ, quantitative understanding of the processes is almost non-existent, which puts ITER operational reliability and availability at risk. The principal impediment to improved

understanding is inadequate surface diagnosis. Computer codes for interpreting or predicting material migration in MFE devices have been used for many years but have only been benchmarked against measurements in small, local-scale experiments involving the insertion and removal of a small test object into the plasma for a limited number of discharges, thereby achieving a controlled plasma exposure which is potentially interpretable. The reliability of these codes therefore needs to be established.

4.1.4.2 Program Elements

- Comprehensive theory-experiment comparisons, in well-controlled and well-characterized conditions
- Detailed investigations of material migration in toroidal geometry
- PSI evaluation of tungsten in appropriate plasma, thermal, and radiation damage environment, maintaining backup material PFC options as needed
- Plasma pulse lengths in relevant exposure environments to bridge the gap in pulse lengths between present experiments and FNSF/DEMO

4.1.4.3 Priorities

Near-term (1-2 years)

- Develop and improve first principles and reduced PSI models
- Implement real-time in-situ surface material diagnostics in toroidal and linear facilities for comprehensive theory-experiment comparisons, toward assessment of material migration, fuel retention, and plasma modification issues
- Characterize and evaluate tungsten and backup option plasma-facing components in toroidal and linear plasmas devices

Mid-term (3-5 years)

- Extend linear plasma devices, including appropriate upgrades from existing capabilities (e.g. tritium, liquid metals, rad. damage), to long time-scales for detailed PSI studies in well-controlled and well-diagnosed conditions
- Implement a coherent strategy using short-pulse U.S. tokamaks and long pulse international devices to extend material migration, fuel retention, and plasma modification studies
- Design and evaluate tungsten and backup option PFCs with high pressure helium gas coolant

Long-term (6-10 years)

- Pursue a dedicated limited activation toroidal PMI/PFC facility (and/or utilize an early phase of FNSF operation) to inform design of DT-FNSF.

4.1.5 Thrust 17: Optimize Steady-State, Disruption-Free Toroidal Confinement using 3-D Magnetic Shaping, and Emphasizing Quasi-Symmetry Principles

4.1.5.1 Background

While tokamak plasmas are nearly axisymmetric, strong 3-D magnetic shaping with external coils is the basis for the stellarator concept. Plasma confinement in stellarators does not require plasma current. They can confine plasma in steady state and with high pressure, and they do not suffer from virulent current or pressure-driven instabilities that abruptly terminate the plasma. Various types of stellarators have been proposed, and stellarators are leading alternates to the tokamak for magnetic confinement of fusion plasma.

Understanding 3-D magnetic shaping is also critical to the function and performance of all magnetic fusion configurations. ITER will use the controlled application of relatively weak 3-D magnetic fields to suppress edge instabilities driven by the H-mode pressure pedestal and to allow plasma rotation.

Because the magnetic field in a stellarator is not toroidally symmetric, energetic ions and charged fusion products may become unconfined. However, when the 3-D fields maintain particular symmetries, such as quasi-symmetry (QS), then energetic particles are predicted to be well-confined, and favorable bulk plasma circulation is unimpeded as in the tokamak. The QS stellarator is a relatively new transformational concept that maintains confinement properties of the tokamak while avoiding severe transient events and control issues that occur in high-pressure plasmas confined in tokamaks. The opportunity to explore the confinement physics of quasi-symmetry at larger, fusion relevant scales is significant.

4.1.5.2 Proposed Actions:

1. Investigate stellarator configurations with the goal of simplifying and making maintainable magnet systems and extending confinement parameters.
2. (a) Expand efforts in non-axisymmetric theory and computation, particularly aimed at understanding stellarator confinement. (b) Examine the merits of building a national QS experimental facility aimed at exploring a range of internal plasma current and demonstrating sustained, low-collisionality, disruption-free operation.

3. Design 3-D divertors compatible with QS geometry. Integrate with 3-D coil simplification.
4. Explore the addition of 3-D shaping to other magnetic configurations.

4.1.5.3 Priorities

Near-term (1-2 years)

- Advance the fundamental physics and broad understanding of 3-D magnetic fields through advancements in theory and simulation and by using existing university-scale experiments.
- Initiate theory and design efforts to explore stellarator configurations with simpler and maintainable magnet systems with practical 3-D divertors
- Partner with efforts to study and understand 3-D magnetic fields applied to tokamaks, especially to understand 3-D shaping applications to future ITER research scenarios.

Mid-term (3-5 years)

- Complete theory and design efforts to explore stellarator configurations with simpler and maintainable magnet systems with practical 3-D divertors
- Participate in experiments with the Wendelstein 7-X (W7-X) device in Germany and evaluate plasma confinement in quasi-isodynamic (*i.e.* non-quasisymmetric) stellarators

Long-Term (6-10 Years)

- Design and construct a new optimized stellarator research experiment to advance plasma confinement physics in an attractive 3-D magnetic configuration

4.2 The Other Thrusts

As discussed in the introduction to this Section (3), the five *ReNeW* thrust areas just discussed do not encompass the full set of science programs we view as important to the U.S. contribution to the international fusion science effort: recall that our criteria for ranking thrusts included the joint criteria of scientific importance *and* timeliness for ITER impact. For this reason, in order to complete the description of what we view as important components of the U.S. effort, we now turn to a description of these only nominally ‘secondary’ research program elements. In many cases, the scientific merit of these thrusts is equal to that of the highest-priority thrusts just discussed in Section 4.1, but share the attribute that they are not critical for the construction and/or initial operation of ITER. We also note that the order of thrust presentations *within* the

following Section 4.2.1 and Section 4.2.2 reflects the ordering presented by the 2009 *ReNeW* Report, and not a scientific ranking.

4.2.1 Middle Category

The second category of thrusts includes alpha-particle physics, the development of operations in ITER and optimization more generally, materials research, and development of alternate toroidal configurations.

Thrust 3: Understand the role of alpha particles in burning plasma.

Fusion-produced helium ions, called alpha particles, are the dominant source of heat in burning plasmas, and ITER will be the world's first experiment to investigate strong plasma "self-heating" and the resulting dynamics of energetic alpha particles produced by fusion. This thrust includes modeling to anticipate alpha particle effects in ITER and diagnostic development for measuring these effects. Although limited investigations of alpha particle physics occurred in early D-T experiments (TFTR and JET), full understanding of energetic particle physics as will occur in ITER and future fusion power devices is a foundational question for burning plasma studies. The U.S. is a leader in alpha-particle theory and simulation and, through neutral-beam and ICRF-driven experiments, continues to contribute to experimental work in this area. Research in this area includes: (i) identification of operational regimes in burning plasma devices that are stable to alpha-driven instabilities, (ii) determination of alpha transport, (iii) prediction of the alpha heating profile, alpha-driven currents, and impact on current drive requirements, (iv) exploration of the ambitious alpha channeling effect, thereby to retrieve alpha particle energy through collisionless means, and (v) incorporation of experimentally validated alpha physics transport models into integrated plasma simulation tools for the entire plasma.

Thrust 4: Qualify operational scenarios and the supporting physics basis for ITER.

Qualifying ITER operation by appropriately scaled integrated demonstration in existing tokamaks is an important way to validate the ITER design and ensure its success. The U.S. is very active in this work. The baseline (conventional H-mode) scenario has been experimentally demonstrated for the entire discharge, from plasma breakdown through non-disruptive termination. The U.S. has demonstrated leadership in advanced scenarios, including hybrid and advanced inductive scenarios that might provide an easier path to ITER's highest level goal of $Q = 10$ for hundreds of seconds. Research on steady-state scenarios to meet ITER's longer-term goal of fully noninductive operation for thousands of seconds with fusion gain $Q = 5$ is also essential. All three major U.S. devices have heating and current drive systems that support advanced scenarios, especially the noninductive operation. The committee rates Thrust 4 second priority. Although the work is important for ITER and reflects U.S. strengths, we do not expect

that it will impact immediate hardware decisions to be made for ITER during the construction phase. More progress on the baseline and hybrid/advanced inductive scenarios would be important for maximizing ITER's chances of success early in the research program. Decisions, needed by the early 2020s, on additional heating and current drive systems for steady-state operation of ITER, will be informed by our work on these scenarios.

Thrust 5: Expand the limits for controlling and sustaining fusion plasmas.

Developing efficient, high performance steady-state tokamak regimes is crucially important to the future of the tokamak as a platform for an FNSF and as an attractive reactor. Much of the high priority research outlined in this report addresses issues intimately connected to steady-state development, for example eliminating disruptions, taming the plasma-wall interface and expanding the knowledge base provided by theory and simulation.

The U.S. has made seminal contributions to steady-state research through the development of Advanced Tokamak regimes in which the majority of the current is provided by the self-generated bootstrap current and confinement is enhanced by optimizing the current profile. Although pulse lengths in U.S. facilities are relatively short, typically several seconds, they nevertheless can exceed the resistive current diffusion time and are then long enough to address the central issues of MHD stability and confinement consistent with the steady-state current profile. Thanks to past investments in heating and current drive systems, as well as in diagnostics and control, U.S. facilities currently have world leading capability to make ground breaking contributions to steady-state research. The panel believes that this capability should be fully exploited, with an eye toward transfer of the knowledge gained to facilities with substantially longer pulses possible in superconducting, off-shore tokamaks.

Thrust 14: Develop the material science and technology needed to harness fusion power.

The objective defined by this *ReNeW* thrust is to perform fundamental materials science and technology to establish much of the engineering feasibility of a fusion power plant. Thrust activities need to be fully integrated with those of Thrusts 9 – 13 and 15. The Panel ranked Thrust-14 in the middle priority category since it will likely not influence the ability of ITER to meet its performance goals, but nevertheless is critical for exploring the viability of potential blanket concepts that will provide the basis for an engineering design of a Fusion Nuclear Science Facility. The major scientific challenges include identification of approach(es) to mitigate the degradation of mechanical properties and dimensional stability of structures exposed to intense fusion neutrons, and to identify potential tritium sequestration mechanisms that may impact fusion safety. The lack of a fusion relevant neutron source for conducting accelerated single variable experiments was determined by the FESAC 2012 Materials Panel to be the largest obstacle in achieving a rigorous scientific understanding, and in developing effective strategies

for mitigating neutron induced material degradation. While a prototypic fusion neutron spectrum test facility (e.g., the International Fusion Materials Irradiation Facility or a volumetric D-T neutron source) lies beyond the financial capacity of the U.S. program alone, the Materials Panel did identify several medium-scale research initiatives to directly address Thrust-14 challenges. Finally, the need to conquer the neutron-induced degradation of materials and structures extends beyond the fusion energy mission. Coordinated FES partnerships with the broader DOE (Science, Nuclear Energy, and Defense Programs) offer a substantial leveraging opportunity.

Thrust 16: Develop the spherical torus to advance fusion nuclear science.

The spherical torus (ST) configuration is an axisymmetric configuration like the tokamak but has a much smaller aspect ratio. This allows the externally produced toroidal magnetic field to be reduced in strength compared with that of a tokamak. The resulting nearly spherical configuration is compact and has a number of favorable properties compared with a tokamak. The ratio of the plasma pressure that can be contained in the magnetic geometry of the device compared with the externally supplied magnetic pressure, measured by the plasma β is very high which has led to record-setting values for this key parameter in experiments. This property is very favorable for scaling to a burning plasma because the high β and associated high fusion energy production per unit volume leads to compact reactor designs. As a result the ST has been proposed as a candidate for a Fusion Nuclear Science Facility (FNSF).

The high β of the ST leads to an internal magnetic geometry that also has very favorable properties with respect to energy containment. A major success of the research on this configuration was the prediction based on theory that the dominant instabilities that drive energy transport in the conventional higher-aspect-ratio tokamak would be stable in this configuration. Remarkably, the theoretical predictions were confirmed in experiments – a new class of instabilities at much shorter spatial scales are the dominant drivers of transport. As a result, the ST also has very favorable energy containment properties even for a configuration with a very weak magnetic field; however the observed – and theoretically-unexpected – enhanced electron transport (due perhaps to the above-mentioned short wavelength modes) remains to be understood.

Because of the similarity in the geometry of the conventional tokamak and the ST, the comparison of their properties with respect to energy confinement and dynamics has produced a fruitful scientific test bed for benchmarking our theoretical models for large-scale stability and turbulence and associated transport. If theory and simulations are able to predict and/or explain the differences between the dynamics of these two similar yet distinct configurations, confidence in our fundamental understanding of the essential physics of stability and transport will be greatly increased.

Thrust 18: Achieve high-performance toroidal confinement using minimal externally applied magnetic field.

The objective defined by this *ReNeW* Thrust is to develop alternative approaches that avoid some of the practical challenges associated with tokamak and stellarator configurations. The panel does not expect that this research will have direct bearing on the burning plasma research in ITER, nor does it view the present low-field devices to have demonstrated sufficient performance for concept-development at this time. However, research in this area makes fundamental contributions to understanding self-organization, magnetic reconnection, and magnetic symmetries. It is an example of plasma stewardship in MFE that contributes to space and astrophysical research and to mainline MFE research (feedback stabilization and DC current drive being examples). Plasma relaxation and self-organization are central concepts in the operation of the reversed field pinch. Innovative approaches to steady-state operation include steady helicity injection in spheromaks and rotating magnetic field current drive in the field-reversed configuration. Much DOE-supported experimental research is conducted at universities and therefore makes important contributions to workforce development, despite its relatively modest cost. University-based experiments promote exploration, and these programs are incubators for potential breakthrough ideas. In addition, small-scale devices are well suited for validating modeling efforts over a range of parameters, which is essential for confidence in predictability. For these reasons, the panel supports continuing research in low-field plasma configurations, which represents a modest level of resources in the present program.

4.2.2 Third Category

Thrusts that appear relative low in our group's assessment include diagnostics for burning plasma, high-temperature superconductor development, burning-plasma integration, power handling, integrated power systems design, and tritium sustainability. Here, the group finds that the *ReNeW* thrusts do not emphasize foundational science and technology, are being addressed internationally, or do not have sufficient urgency relative to other research areas.

Thrust 1: Develop measurement techniques to understand and control burning plasmas.

Sophisticated diagnostics are a hallmark of modern MFE experimentation to measure the extreme conditions of high-temperature plasma. They are critical for scientific discovery in core and edge plasma and for quantitative model validation. The U.S. is a leader in developing both the principles and the practical implementation of new diagnostic tools and has committed to providing some of the diagnostics on ITER. The importance of U.S. diagnostic development is therefore very great. The *ReNeW* Thrust 1 focuses on development for ITER and other burning plasma. The ITER diagnostics for which we are responsible include implementation of

IR/visible-light cameras, reflectometry, motional Stark effect polarimetry, electron cyclotron radiometry, toroidal interferometry/polarimetry, core X-ray spectrometry, and a residual-gas analyzer system. These are funded from the part of the FES budget devoted to ITER. For this reason, diagnostic support for burning plasma is considered lower priority for the rest of the U.S. program. Nevertheless, development of new diagnostics should remain a significant part of the domestic effort, particularly in support of the high-priority scientific topics, and in light of the fact that the measurement demands on a DEMO or FNSF are in many respect yet more challenging than on ITER.

Thrust 7: Exploit High Temperature Superconductors (HTS) and other magnet innovations to advance fusion research.

As articulated in *ReNeW*, the game-changing opportunities offered by HTS include the ability to optimize the magnetic fusion device for very high field plasma performance at reduced size and/or to operate the device at relatively high cryogenic temperatures. HTS can be used with any magnetic field configuration including 3-D shaped devices. These materials can operate at cryogenic temperatures approaching that of liquid nitrogen (77K), enabling the option to build electrical joints into the winding cross-section that can be connected, unconnected and reconnected on site thereby simplifying assembly and maintenance.

The FESAC Materials Panel Report summarizes recent progress and further issues confronting the practical development of HTS for fusion applications. Notable benchmarks include HTS magnet development with fields up to 35 T and current densities up to 500 A/mm², and recent operation of a 16 T magnet at BNL. The facility opportunities discussed in connection with Charge 3 would significantly benefit from advances in magnet technology. However, MFE R&D in this area has been substantially reduced due to budget exigencies, thus jeopardizing this opportunity. The Panel believes that HTS technology should be considered for use in any major new MFE facility and supports implementation of an accompanying HTS R&D program to underpin conceptualization and design.

Thrust 8: Understand the highly integrated dynamics of dominantly self-heated and self-sustained burning plasmas.

This Thrust focuses on maintaining control under conditions where most of the heat (alphas) and current (bootstrap) are self-generated. It is particularly forward-looking, and requires a facility operating steady-state at high fusion gain. Although such a facility is not on the horizon, it is generally recognized that a DEMO would need to operate in these conditions. Work in present day tokamaks is moving toward establishing appropriate operating scenarios, with high beta and high bootstrap fraction. In the more distant future, this Thrust might be addressed in several different ways: ITER, with appropriate upgrades, might be able to address this mission, but

probably not before the 2030s. Although FNSF is usually described as a low-gain device, it has been pointed out that a low-gain, high fluence, device may have unmanageable heat loads in the divertor. So it is conceivable that this mission might be folded into FNSF. If neither of these approaches is possible, this mission would have to be performed in a new device, perhaps a “pre-DEMO” or DEMO itself. Although no device in the world is currently capable of operating in burning plasma conditions, U.S. experiments have unique capabilities in current sustainment and high-beta and high-bootstrap operation to study regimes that could form the basis for DEMO operating scenarios. Since the “Thrust 8 experiment” is not envisioned before the distant future, there is little urgency. However, we recommend that work on these scenarios should continue. It should be noted that work for the basis of Thrust 8 lies along the same path of several other Thrusts that focus on preparing steady-state scenarios for ITER and FNSF.

Thrust 11: Improve power handling through engineering innovation.

The heat fluxes projected for many future devices, including ITER, are at the limits or even beyond the capability of present day heat removal technology. Moreover, as described in Thrust 9, there is substantial uncertainty in the projection of heat fluxes to future devices, i.e. the peak heat fluxes might be even higher. In addition to plasma engineering solutions described in Thrust 9, this thrust advocates engineering innovations to increase the technological limit on heat removal capability. Water-cooling technology used in ITER is inapplicable to a reactor that will operate with high-temperature solid walls or reactive liquid metals. For DEMO, either solid PFCs (cooled by high-pressure helium or liquid metals) or free-surface liquid PFCs (such as lithium or tin) could be used. Research in this area will be critical for fusion energy development, but the panel concludes the *ReNeW* thrust actions do not have the highest near term priorities in the present budget scenarios, as basic investigation of boundary-layer physics and plasma-surface interactions (Thrusts 9 and 10) are more pressing.

Thrust 12: Demonstrate an integrated solution for plasma-material interfaces compatible with an optimized core plasma.

Successful development of fusion energy will need to address the issues outlined for this thrust. There are fundamental science issues touching on the broader scientific themes of multi-scale synergies between surface conditions and the plasma core in extreme conditions, turbulent transport, and the formation of transport barriers. Actions for this Thrust include development of a new facility with a DEMO-relevant boundary, high power density and hot-wall operation, >500C, where recycling will approach 100% at all PFC surfaces, a condition where some advantageous confinement modes found in present tokamaks may disappear, while new advantageous confinement regimes may be enabled. This work must build on progress in the other thrusts in the plasma-material interfaces theme, namely boundary layer plasma physics, plasma-surface interactions, and engineering innovations (Thrusts 9, 10, and 11). This

sequencing suggests a lower priority for this thrust under the constraints of the 1st and 2nd charges; also existing facilities have yet to realize their full potential in this area, and the panel prioritizes their work higher. However, such a new facility constitutes a possible response to the 3rd charge, see Section 4 below.

Thrust 13: Establish the science and technology for fusion power extraction and tritium sustainability.

The objective defined by this *ReNeW* Thrust is to develop the scientific foundations of practical, safe and reliable processes and components that harness the heat produced by fusion, create and extract the tritium from lithium-bearing media, and manage tritium that circulates in the plant. An issue recognized in the community for some time is that in realistic geometry, the tritium-breeding ratio for leading reactor designs is only slightly greater than unity. Improved understanding of magnetohydrodynamic effects in blanket structures is needed to understand potential operating temperatures and static and cyclic thermomechanical stresses. While these issues may be critical for the viability of DT-based fusion development (and may exclude some or all of the current proposed breeding blanket concepts from future consideration), the panel views this area as a longer term need. Consequently, this Thrust was ranked, as a whole, in the third priority category with one notable exception. Studies of the basic properties of tritium retention and diffusion through materials should receive enhanced emphasis, as this will be critical in moving forward with a Fusion Nuclear Science Program that will provide a solid basis for self-sufficient tritium operation in a Fusion Nuclear Science Facility design.

Thrust 15: Create integrated designs and models for attractive fusion power systems.

This *ReNeW* Thrust focuses on further developing and utilizing methodologies, built up through years of FES investment, to identify fusion system integration issues and to optimize facility configurations. Such studies can be applied to extend the operating parameter space for future fusion facilities to meet reliability, availability, maintainability, and inspectability (RAMI) goals, as well as safety and environmental requirements on the path to fusion power. They can also guide the R&D on high-leverage, high-payoff issues in fusion and nuclear technology. The U.S. has been a leader in carrying-out reactor design studies, and maintaining the skills and corporate knowledge is important for fusion development. Under the lower budget scenarios in the charge, this Thrust is considered not to be time-urgent. However, this capability would be valuable for the planning of FNSF. The area is also not rated highly in terms of foundational science and technology.

Appendix A: References and Key *ReNeW* Fusion Science Thrusts

A.1 References

A major activity of our Subcommittee was to focus attention on the *Report of the Research Needs Workshop (ReNeW) for Magnetic Fusion Energy Sciences* held at Bethesda, MD from June 8 to June 12, 2009 and sponsored by the Office of Fusion Energy Sciences (FES) in the U.S. Department of Energy. The Workshop was the culmination of a ten-month activity involved some 200 scientists from universities, national laboratories and private industry, including several scientists from our international partners. The *ReNeW* task was to identify (i) the scientific and technological research frontiers of the fusion program, and, especially, (ii) a set of activities that will most effectively advance those frontiers. According to DOE/FES instructions, *ReNeW* was not charged with developing a strategic plan for the conduct of research or a timeline for the implementation of fusion power.

Following the Basic Research Needs model established by the Office of Basic Energy Sciences (BES), *ReNeW* presented a collection of eighteen discrete research activities, called “thrusts.” Each thrust was motivated by an explicit science question, or coherent set of questions, on the frontier of fusion science. The thrusts are comprised of compelling research elements that can be followed to find needed answers. Each thrust combines intellectual and technological tools, experimental facilities, and computational resources creating an integrated, focused subprogram. The thrusts did not necessarily have equivalent scales of effort; however, at some level, all eighteen thrusts were viewed important. The thrusts were viewed as building blocks for a comprehensive fusion research program.

The assessment of these eighteen building blocks, or “thrusts”, was the first step of our Subcommittee’s prioritization process.

The *ReNeW Report*, with detailed technical descriptions for each research thrust, is available at

http://science.energy.gov/fes/about/~media/fes/pdf/about/Magnetic_fusion_report_june_2009.pdf

In addition to the *ReNeW Report*, the Subcommittee made use of

- *Opportunities for and Modes of International Collaboration in Fusion Energy Sciences Research during the ITER Era*, Report of the Fusion Energy Sciences Advisory Committee (FESAC), DOE/SC-0150, February 2012. Available online:

http://science.energy.gov/~media/fes/pdf/workshop-reports/20120309/Intl_Collab_Final_SCSC-PRINT.pdf

- *Materials Science and Technology Research Opportunities Now and in the ITER Era: A Focused Vision on Compelling Fusion Nuclear Science Challenges*, Report of the Fusion Energy Sciences Advisory Committee (FESAC), DOE/SC-0149, February 2012. Available online:

<http://science.energy.gov/~media/fes/pdf/workshop-reports/20120309/FESAC-Materials-Science-final-report.pdf>

- *Plasma Science: Advancing Knowledge in the National Interest*, Report of the Plasma 2010 Committee, Plasma Science Committee, National Research Council (2007), available online at:

<http://www.nap.edu/catalog/11960.html>

- *Fusion Simulation Project*, a research needs workshop sponsored by DOE/FES in May 16-18, 2007. Available online:

http://science.energy.gov/fes/about/~media/fes/pdf/about/Fusion_simulation_report_may_2007.pdf

- *Priorities, Gaps and Opportunities: Towards A Long-Range Strategic Plan for Magnetic Fusion Energy*, Report of the Fusion Energy Sciences Advisory Committee (FESAC), October 2007. Available online:

http://science.energy.gov/~media/fes/fesac/pdf/2007/Fesac_planning_report.pdf

- *A Plan for the Development of Fusion Energy*, Report of the Fusion Energy Sciences Advisory Committee (FESAC), March 2003. Available online:

http://science.energy.gov/~media/fes/fesac/pdf/2003/Dev_report_2003.pdf

A.2 The Key *ReNeW* Fusion Science Thrusts

A major activity of our subcommittee was to focus attention on the five *ReNeW* thrusts we agreed ranked most highly among those discussed by the 2009 Research Needs Workshop; and in this Appendix, we describe our conclusions regarding the key science elements we view as central to the U.S. effort in the international magnetic fusion research program discussed in Section 3 above in considerably more detail.

A.2.1 Thrust 2: Control Transient Events in Burning Plasmas

Energy production from burning, magnetically confined plasma will require sustained operation lasting many orders of magnitude longer than discharges in contemporary experiments. Thus, avoiding unplanned transients is essential to the concept of MFE. Moreover, the normal power loading of steady operation in our first burning plasma experiment, ITER, will stretch present-day engineering capabilities; unmitigated transients that concentrate energy will not be tolerated. The two primary concerns for tokamak configurations, including ITER, are disruptions (rapid loss of plasma confinement) and edge-localized modes (ELMs, periodic bursts of energy from the pedestal of pressure that occurs in edge plasma during high-confinement, i.e. H-mode, operation). Recognizing the importance of transients and the urgent needs for ITER, *ReNeW* devotes one of its research thrusts to this area. The key issues and proposed research activities of the thrust are organized into four elements:

1. *Prediction of disruptions*—characterizing, understanding, and sensing conditions where disruption is imminent,
2. *Avoidance of disruptions*—actuation of controls to maintain stability against disruption,
3. *Mitigation of disruptions*—actuation of safeguards against material damage from rapid termination of a discharge, and
4. *Avoidance of ELM-induced impulsive heat loads*—understanding ELM dynamics and developing techniques and operational modes that eliminate large, damaging bursts.

All of the elements involve scientific research in addition to engineering development. Since *ReNeW* there has been significant progress, particularly in elements 3 and 4, but there remain significant unsolved problems in all four.

Among the three breakout groups that weighed *ReNeW* thrusts during the first part of the panel's activities, the two that considered all thrusts rated Thrust 2 among their top priorities for the U.S. MFE program.⁶ The group assigned to the U.S. role in ITER prioritized this thrust based on the urgency and importance for successful operation and scientific progress with ITER. The U.S. leadership role in the thrust's research elements was another important factor. The group assigned to foundational science and technology prioritized this thrust based on the fundamental nature of the plasma dynamics that occur during disruptions and ELMs and the need for a predictive understanding of plasma stability. It also recognized the U.S. leadership role, the urgency for ITER, and the potential for international collaboration.

⁶ The group assigned to fusion energy development did not consider any of *ReNeW*'s first four thrusts, which fall under its Theme 1, Burning Plasmas in ITER.

While the panel rates the research of this thrust as defined by *ReNeW* to be of highest priority, it also notes strong connections and opportunities associated with other research areas. The development of sensors and controls for advanced-tokamak (AT) operation is transferable to disruption avoidance. Three-dimensional plasma confinement in stellarators and magnetic perturbation of tokamaks (resonant, RMP, or non-resonant) for ELM suppression can be viewed as two extremes on a scale of toroidal asymmetry. Planned transients (startup and controlled shutdown) require consideration of effects such as runaway electrons that pose risks during disruptions. Plasma can also be unstable during current ramp-up and ramp-down phases, depending on, for example, q -profile and density.

The following subsection (A.2.1.1) briefly summarizes recent advances in the four research elements of this thrust. Subsection A.2.1.2 organizes the urgency of proposed actions into near-, medium-, and long-term efforts, and subsection A.2.1.3 revisits the scale of effort.

A.2.1.1 Advances since *ReNeW*

A.2.1.1.1 Predictions of Disruptions

Disruption prediction systems have been in use on DIII-D and JET at different levels of sophistication. The Advanced Predictor of Disruptions (APODIS) system on JET is now used to protect the recently completed, ITER-like metal wall from damage, which is representative of the situation for ITER. However, the systems in these experiments are based on machine learning with training sets assembled with signals from many previous discharges. Building a database that includes a number of disruptive discharges will not be feasible in ITER and larger experiments, where significant damage may result from each unmitigated disruption.

A recent investigation of data from NSTX seeks to develop a more physics-based approach. Correlation of several measurable properties, such as shaping, rotation, and current distribution are used to characterize the propensity for disruption. Combining output from multiple diagnostics with model performance in a tunable algorithm shows promise in terms of having few late predictions and few false positives.

Recent studies of internal transport barriers (ITBs) find that local transport reduction associated with ITB tends can cause disruption. To predict such disruptions, it is important to measure and calculate stability in real time to understand closeness to disruption. This is especially important for AT operation, which is susceptible to barrier-localized mode (BLM)-induced disruption and also neoclassical tearing mode (NTM)-induced disruption. Sometimes ITB-induced disruption occurs after the bifurcation of transport state such as curvature transition, hence characterization of such transport bifurcation is also important for predicting disruption.

A.2.1.1.2 *Avoidance of Disruptions*

There are two general approaches to avoidance, active control of instabilities and active control to avoid instabilities. Both approaches need to be developed, though most recent efforts focus on control of tearing modes, a class of non-ideal macroscopic instability. Applying electron cyclotron current drive (ECCD) to suppress magnetic islands and prevent locking of magnetic perturbations has been demonstrated in ASDEX-U, DIII-D, and TCV. Recent work in DIII-D includes active positioning of ECCD deposition using movable mirrors. Another technique developed on DIII-D uses internal coils to move locked perturbations into position for ECCD suppression. Improvements to modeling and feedback of resistive-wall modes (RWMs) in NSTX lead to better avoidance of disruptions of AT discharges. The characterization of NSTX discharges with respect to disruptions, noted in A.2.1.1.1, is an example of one aspect of research needed for active control to avoid instability.

A.2.1.1.3 *Mitigation of Disruptions*

Mitigation of disruptions is the "plan B" when efforts to avoid disruption fail. Even with mitigation, disruptions in a fusion nuclear science facility or in a demonstration reactor must be extremely rare occurrences. Nonetheless, given the present state of prediction and avoidance and concerns over material damage, mitigation is currently identified as the highest priority physics area for ITER. The international community has responded with the U.S. taking the leading role. The technical objective of all mitigation systems is to add particles to (a) radiate away the plasma's energy content and (b) suppress the formation of a relativistic runaway electron (RE) channel to avoid its damaging effects on the first wall. Significant progress since *ReNeW* has been achieved in the following areas:

1. Experimental results (mainly from DIII-D) indicate that the density needed to avoid RE damage is considerably lower than the theoretically predicted "Rosenbluth density." This is encouraging, because achieving the theoretical prediction is a technical challenge and would severely strain ITER's particle handling capabilities if it were achieved. More work is required to better understand the density requirements in present devices and how they extrapolate to ITER.
2. Insufficiently symmetric radiation of plasma thermal energy can melt first-wall surfaces during mitigation, and several experiments have worked on multiple injectors. Alcator C-Mod is the first to test injection from more than one location and has investigated the effects of delay between actuation times. Planned experiments in DIII-D will investigate another spatial arrangement of two gas valves. Numerical modeling with impurity transport and radiation coupled to magnetohydrodynamics (MHD) finds that toroidal peaking of radiation results from MHD activity expelling the hot plasma core asymmetrically into impurities.

3. Different methods of particle delivery have been tested.
 - a. Massive gas injection (MGI) has been demonstrated on DIII-D, C-Mod, ASDEX-U, JET, Tore Supra, and other devices. It is the front-runner for a disruption mitigation system (DMS) on ITER.
 - b. Burst disks have been tested in Tore Supra.
 - c. Shattering frozen D₂ pellets off a plate has been demonstrated in DIII-D with some success in achieving core density that is larger than achieved with MGI.
 - d. Shell pellets filled with boron powder have been tested on DIII-D.
 - e. Other techniques, such as a nano-particle-fueled plasma gun, are being developed.
4. Disrupted discharges in tokamak experiments can reform closed flux surfaces sustained by RE current. Position- and current-control of a RE beam to minimize harmful impact to the device following disruption has been demonstrated in DIII-D. The significance for ITER is being considered.

A.2.1.1.4 *Avoidance of ELM-induced Impulsive Power Loads*

Although understanding of the nonlinear ELM cycle is still emerging, the peeling-ballooning MHD instability that initiates large bursts is now well accepted. Efforts to control the size of bursts either alter the free energy of the profiles, by asymmetric magnetic perturbation or by change of operational mode, or initiate events at a larger frequency than the natural cycle. Since *ReNeW*, there has been considerable progress in laboratory demonstration of ELM control and in the understanding of how 3D magnetic perturbations lead to suppression. Internal coils to generate RMP fields and a pellet pacing system are included in the ITER baseline design.

The RMP method was first demonstrated on DIII-D early in the last decade and has since been tested in a number of different devices, including JET, ASDEX-U, NSTX, and KSTAR. Recent efforts have expanded the operational space for ELM suppression. Understanding how RMP alters edge transport and stability is essential for developing confidence in extrapolating to ITER. A recently developed phenomenological model for the effects in DIII-D is consistent with measurements and theory, but the predictions for magnetic topology are based on vacuum-field computations. Theoretical efforts aim to quantify the topological changes using either linear or nonlinear responses at various levels of modeling sophistication. A group within the U.S. is comparing results from different approaches with each other and with DIII-D measurements for validation. There is also international work to quantify screening, applying nonlinear reduced-MHD with some neoclassical and two-fluid effects.

Pellet pacing was first demonstrated on ASDEX-U approximately ten years ago. Recent efforts include injection of small pellets into ITER-shaped plasma in DIII-D at a frequency of 60 Hz. This increases the frequency of ELMs and decreases the size of each burst by more than a factor of 10. The discharges show little degradation of core plasma performance. Another ELM-

mitigation technology that works on the same principle as pellet pacing is supersonic molecular beam injection. It is being developed on the KSTAR and HL-2A tokamaks.

There has also been recent progress in developing operational modes that have H-mode performance without ELMs. Recent developments for the QH-mode, first obtained in DIII-D, include description in terms of peeling-ballooning theory. The new understanding implies that strong rotational shear near the boundary is the critical ingredient. This opens possibilities for demonstrating QH-mode in ITER using magnetic perturbations and neoclassical toroidal viscosity, and an effort to test this is underway at DIII-D. The I-mode of operation, discovered on Alcator C-Mod, has a temperature pedestal but no density pedestal and does not exhibit ELMs. It has recently been obtained on ASDEX-U.

A.2.1.2 Urgency of U.S. efforts

Categorizing research efforts into near-, medium-, and long-term priorities reflects the urgency with which problems need to be solved for ITER and for fusion energy development. The final design review for ITER's DMS is scheduled for 2016, so the near-term priorities identified below emphasize the mitigation research element. Much of the required research does not require long-pulse operation, but it does require experimental platforms that can withstand frequent disruption for testing purposes. Similarly, ELM mitigation studies require testing where large ELMs can be tolerated. It is, therefore, important that current U.S. machines are utilized for near-term needs while they are operational.

The needs for avoidance are longer term. Nonetheless, the tokamak energy development path requires high plasma- β , high bootstrap-current operation with robust avoidance of disruption. We stress that the medium- and long-term issues listed here are of importance that equals the near-term priorities, and research for them must be conducted in parallel.

Near-term (1-2 years)

- Understand the dynamics of REs in disruption.
 - What density is required to avoid RE?
 - How is RE deposition affected by magnetic-field topology?
- Test and verify disruption mitigation approaches for required physical characteristics.
 - Do the injected particles penetrate sufficiently to affect energy deposition?
 - Is the radiated power spread over a sufficiently large area to avoid first-wall damage?
 - Can the approach reach the density needed to avoid RE damage?
 - Are damaging magnetic forces on walls and other components avoided?
- Complete physical characterization of ELM suppression schemes.
 - What physical effects influence screening of resonant perturbations?

- How are transport and stability affected by RMP?
- Is pellet pacing technology sufficiently effective and reliable to accommodate divertor-surface limitations?

Mid-term (3-5 years)

- Improve understanding and modeling of instabilities that lead to disruption for a range of operation scenarios.
 - Extend physics-based disruption prediction studies for application to future experiments where it will not be practical to allow disruption.
 - Develop real-time analysis for operational use in predicting disruption.
 - Demonstrate full disruption mitigation initiated by sensed disruption onset.
- Examine the use of AT control methods, e.g. pressure and current profile control, for disruption avoidance.
- Develop methods of disruption avoidance that are compatible with conditions in AT operation of ITER.
 - To avoid ITB-induced disruption, it is important to understand plasma responses and ways to control flow shear suppression of turbulence and also local heating profile as well as to increase effective control knobs for plasma profiles.
 - When plasma in a steady phase is close to disruption, it may be necessary to terminate the discharge, for which we need to develop safe shut-down scenarios.
- Understand present ELM-free operation and develop operational modes that are compatible with ITER.
- Encourage innovation with respect to control, understanding and use of transient currents.
 - Are the means to control RE beams reliable, and do they scale from present experiments to ITER?
 - How can research on disruptive transients be used to develop superior startup scenarios?
 - Are high-efficiency quasi-steady state operations possible?

Long-term (6-10 years)

- Optimize the tokamak configuration for disruption avoidance.
- Develop ELM suppression methods for stellarators.

A.2.1.3 Scale of effort

The *ReNeW* thrust on controlling transients is defined succinctly, yet the scale of the required effort is substantial. Unplanned transients have been a part of tokamak and other MFE configurations over the history of the international experimental program, but only now are

solutions needed to protect scientific investments and to ensure safe operation. The U.S. has committed to supplying hardware for ITER's DMS. It is not solely responsible for developing the necessary science and technology, but it has established leadership in this area. In fact, while there are significant international efforts in all four of the Thrust 2 research elements, the U.S. has recognized leadership in disruption avoidance and ELM suppression, in addition to mitigation. Continuing our efforts there, in addition to accelerating work in prediction, will ensure that the U.S. will be a valued partner in collaborative international research.

The *ReNeW* summary statement with respect to proposed actions on transients remains valid today:

The U.S. fusion program is well positioned to carry out much of the required work in existing tokamaks, with modest upgrades to diagnostics and auxiliary systems, but substantial increases in experimental time and human resources. Further technology development will be required to extend these techniques to the burning plasma regime in ITER.

A.2.2 Thrust 6: Develop predictive models for fusion plasmas, supported by theory and challenged with experimental measurement

The essence of scientific understanding is the development and validation of predictive models based on first principles. This thrust describes the mechanisms for translating experimental observations into concrete knowledge. Further, it is the fundamental understanding of plasma behavior gained through modeling and validation that connects our field to the broader scientific community in space and astrophysics. This thrust therefore receives high priority. The U.S. is viewed as a leader in magnetic fusion energy (MFE) theory and computation and has taken initiative in applying validation methods for comparing models with experimental data. The tight coupling between theory/modeling and experiment that has significantly strengthened over the past decade has transformed our ability to gain knowledge from the complex dynamics of plasma behavior probed in experiment. Maintaining and strengthening this competency is an important priority for the U.S. program. Theory and validated modeling contribute vitally to ITER experimental planning and interpretation, and are essential for the further development of MFE beyond ITER. Furthermore, theory and validated modeling are critically important elements in understanding experiments and developing the predictive capability needed to obtain the greatest benefit from current and future experiments. Some examples of the importance of theory and modeling are the following:

1. Modeling of turbulent transport is critical in ensuring that designs of future experiments such as ITER perform as expected. Projections based on existing experiments by definition

project beyond the existing database and are therefore inherently uncertain. Minimizing this uncertainty allows us to proceed with greater confidence in extrapolating to future experiments, which can accelerate the development of magnetic fusion energy.

2. Theory and modeling will enable the design of new magnetic geometries for containing fusion plasma such as exploiting magnetic symmetries in advanced concepts for stellarator configurations, or developing a more robust steady-state, high-performance tokamak mode based on progress in scientific understanding. Optimizing such designs must be based on modeling since there are far too many options to feasibly explore with experiment.
3. Robust models help identify key variables that can be used to greatly improve plasma performance by controlling transport and suppressing instabilities that can lead to disruptive behavior.

At this stage of scientific development, it is the panel's opinion that the highest priority should be placed upon developing a detailed predictive understanding and improved modeling of key physical phenomena that have impact on fusion plasma performance and/or that have significant scientific importance. In areas where maturity has been established, a coordinated effort should be established to address the coupling of physics elements associated with equilibrium, stability, transport, auxiliary heating, fueling, and exhaust. This is needed to guide experimental planning and to design future experiments. Moreover, coupled physics leads to multi-scale challenges associated with modeling plasmas realistically with opportunities for new scientific discoveries. The validation effort implies a strong partnership among theory, modeling and experiment, the existence and utilization of a variety of experimental facilities, and continuing the development of advanced plasma diagnostics.

A.2.2.1 Key Issues

1. How well can the complex, multi-scale phenomena of fusion plasmas be understood through first-principles models and compared in detail to experimental measurements?
2. What are the appropriate methods and steps for integrating multi-physics and multi-scale effects that are needed to increase the fidelity of practical computer models?
3. How can reduced, integrated models be constructed that support rapid exploration of operating scenarios and plasma control on experiments, especially ITER? How is the reliability of reduced models to be ascertained?
4. What innovations in measurement techniques or experiments should be pursued that would facilitate comprehensive tests (validation) of these models?

A.2.2.2 Proposed Actions

1. For each high-priority Thrust area:
 - i. Identify the key issues, assess whether current theory models have the correct physics

- under conditions of interest;
- ii. Identify what improvements to models may be needed and if coupled multi-physics models are required for quantitative prediction of phenomena of interest, and assess readiness of models for validation against experiment; and
 - iii. Recommend a coordinated theory and simulation program that addresses the Thrust area.
2. Strengthen the basic theory program to address areas where current physical models are inadequate or incomplete, particularly as the research addresses the important themes (see the discussion of Themes in the *ReNeW* Final Report).
 3. Develop a spectrum of powerful, robust, well-verified computer models shared by a large user community. In a less constrained budget climate, the Fusion Simulation Program (FSP), if funded beyond the program definition phase, would be a very important but not exclusive component of this effort.
 4. Develop innovative diagnostic techniques to enable measurements critical for validation.
 5. Establish a spectrum of experiments including both large and small facilities, a range of confinement concepts and adequate run time dedicated to model testing.
 6. Conduct a rigorous set of validation activities that would assess critical elements of physical models and test them through careful comparison with experiments. These would help to guide research in theory and computation by identifying important gaps in current models.
 7. Recruit, train and support dedicated analysts, who would bridge the gap between theorists, code developers and experimentalists, providing unbiased assessments.
 8. Provide substantial computer time for code verification and model validation.

A.2.2.3 Priorities

Near-term (1-2 years)

- Develop improved predictive capability for Edge Localized Modes (ELMs) and disruptions in tokamaks.
- Develop a robust understanding of 3D edge pedestal physics and predictive capability for pedestal characteristics in tokamaks and stellarators.
- Provide modeling support for disruption avoidance and mitigation.
- Establish focused verification and validation (comparisons to data from experiments) programs to address specific case studies in high priority thrusts:
 - Plasma-wall interactions and scrape-off layer (SOL), e.g. SOL width, erosion and redeposition
 - Pedestal and edge localized modes, e.g. ELM stability, edge transport barrier
 - Disruptions (with emphasis on impacts, avoidance and mitigation), e.g. disruption stability boundaries, runaway electron dynamics; and

- Physics of non-axisymmetric configurations, e.g. stability and transport in 3D geometry.

Mid-term (3-5 years)

- Continue critical research on urgent near term tasks.
- Develop improved predictive capability for L-H transition, core and edge transport and, plasma heating and fueling.
- Develop an understanding of the self-generation of rotation and its influence of turbulent transport, transport barrier formation and plasma confinement.
- Develop integrated advanced simulation tools addressing multi-scale plasma phenomena and coupling of physics models when appropriate taking into account some of the Fusion Simulation Project (FSP) planning report recommendations.
- Expand verification and validation program addressing high-priority thrusts and ITER critical physics:
 - Core transport and magnetohydrodynamics;
 - Radiofrequency current drive and heating;
 - Energetic particle physics; and
 - Integrated modeling.

Long-term (6-10 years)

- Continued critical research on urgent near-term and mid-decade tasks.
- Develop a robust modeling program in surface materials simulation for fusion plasmas.
- Initiate a structural materials research effort to address neutron damage as part of a DOE-wide research program in this area.
- Pursue integrated, whole-device fusion simulation as re-defined through high priority mid-decade activities.

A.2.3 Thrust 9: Unfold the physics of boundary layer plasmas

The Panel endorses the basic plan of action recommended in the *ReNeW* Report regarding the challenge of unfolding the physics of boundary layer plasmas, specifically from page 301 of the *ReNeW* Report:

A thin boundary layer surrounds the hot core of all magnetically confined plasmas. The layer naturally mediates interactions between the confined plasma and material surfaces. The magnetic field structure of the region is complex. Furthermore, the plasma pressure that can be maintained at the core-boundary interface has a strong impact on fusion gain.

More than a dozen important new facets of boundary plasma behavior have been discovered over the past decade. Despite this progress, the basic processes that determine the local spatial scale lengths, and the heat and particle flow within the layer, are still not adequately understood. Hence, the heat and particle loads on plasma facing components, impurity intrusion, and core fusion gain are difficult to predict, making design requirements and operational strategies uncertain and necessarily conservative.

A.2.3.1 Key Issues

- Only a part of the physics controlling the boundary layer has yet been identified. How can we fully identify and characterize the physics controlling the boundary layer and resulting plasma-wall interaction (PWI) sufficiently for physics-based scaling to future devices?
- Models to predict the complex features of the boundary layer are immature. How can we accurately describe the highly turbulent boundary layer plasma with material erosion in comprehensive simulations to create simplified models?
- Specifications for active internal components, such as radiofrequency antennas and launchers, and passive diagnostics, are limited by our ability to predict plasma fluxes to those components, and erosion caused by the radiofrequency interaction with components at remote locations. How can the predictive capability of plasma edge modeling, including material interaction with internal components, be improved?
- The existing ITER design is projected to have little margin for managing the plasma heat load, and higher-power devices will require substantially increased power exhaust requirements. How can the magnetic configuration of the boundary region be modified to spread out the heat flux at the material interface?

A.2.3.2 Proposed Actions

- Develop and deploy new diagnostics in existing devices for comprehensive boundary layer measurements of plasma flow, density, temperature, electric field, turbulence characteristics, and neutral density in at least two dimensions and, as appropriate, three dimensions, to provide the data necessary to uncover the controlling physics.
- Increase the level of effort on validation of individual edge turbulence and transport codes, then expand this effort to involve more comprehensive boundary layer models.
- Develop measurements and predictive capability of the plasma fluxes to radiofrequency antennas and launchers; develop models for the self-consistent modification of the boundary layer plasma by the radiofrequency wave injection and other internal components.
- Design and implement innovations of the boundary magnetic geometry in existing devices to demonstrate optimized plasma heat exhaust that is within material limits, and design and implement such a configuration in a future fusion device.

Also from pg. 125 of the *ReNeW* Report:

Present edge/SOL diagnostic capability is seriously inadequate and is the main impediment to the identification of missing edge physics. On most tokamaks today, only n_e and T_e (without energy distribution information and usually only time-averaged) are measured regularly — at a few locations. A spatially extensive set of edge measurements is required. Understanding aerodynamic lift would have been impossible if the air velocity was measured at just one or two locations around airfoils; the SOL is much more complicated and present spatially sparse diagnostic sets cannot identify edge controlling physics Since most of these techniques are labor-intensive, a significant increase in the number of edge diagnosticians will be required.

Taking into consideration developments subsequent to the *ReNeW* study, including the FESAC Materials (Zinkle) Report, our Panel further finds as follows.

Description of Boundary Layer: For the purposes of this Panel, the boundary layer is defined as the region of the plasma that extends from just inside the separatrix out to the Plasma Facing Components (PFCs). In the case where the plasma exhibits a pedestal largely within the separatrix, as for example in H-mode, the boundary layer is defined as extending from the base of the pedestal, e.g., where the pressure has dropped to about 10% of its peak value, out to the PFC's. This then includes but is not limited to the Scrape Off Layer (SOL), i.e., the plasma confined by the open field lines outside the separatrix. Issues and actions associated with the pedestal and its relation to core confinement are dealt with separately in the report. The distinction made here between the Boundary Layer and the SOL is important because there is ample evidence that the physics of the pedestal influences SOL physics; for example, it is well known that the decay lengths for power, temperature and density change dramatically when the plasma undergoes an L-H transition. The formation of the density pedestal is due, at least in part, to the ionization source due to neutrals recycling from the PFCs, which depends on the transparency of the SOL. Another example is furnished by the results from the recent 2010 Joint Research Target (JRT) study on SOL thermal transport, which found a smooth behavior of scrape off lengths as the separatrix is crossed. The study also suggested the possibility that ballooning instability similar to that found in the pedestal may occur in the near SOL.

Importance of boundary-layer physics: The single most important issue for the boundary layer is reliable determination of the power deposition footprint on the divertor targets, i.e., the effective gradient scale length λ_p of the power in the SOL. Empirically there is evidence that $\lambda_p \sim a/I_p \sim 1/B_{pol}$, which is an unfavorable scaling for ITER, leading either to divertor power loads in excess of 10 MW/m² or to a restricted operational regime for achieving $Q = 10$. The solution adopted for ITER is to operate in the so-called detached regime, where recycling neutrals remove momentum and energy from the plasma streaming to the divertor targets and reduce the plasma

temperature and incident heat flux on the plates. Simulations for ITER show that this solution is plausible, at least in limited regimes of operation. However the physics captured in the ITER SOL simulations is incomplete and a more detailed understanding of SOL physics is needed, not only for ITER, but even more so for steps beyond.

A second area of importance is the link between boundary-layer physics and core performance. One example is the effect of impurities injected to radiate part of the SOL power. Another is the effect of divertor geometry. For example, it is known that the L-H transition depends on the effective length of the open field lines, with lower threshold power required for the L-H transition with a longer connection length to the divertor target. Models for the SOL are at an early stage of development and near term emphasis should be focused on identifying the key physics. However the boundary conditions for the SOL are determined by the edge of the core plasma, e.g., the pedestal in H-mode regimes. Therefore models of the SOL must ultimately be integrated with those describing the physics inside the separatrix.

Actions: Clearly, an improved understanding of the physics of the SOL is required. The high rate with which new boundary effects are being discovered indicates that the controlling physics of the boundary has been only partially identified. The first task in the achievement of better models is identification of the missing physics. This will require increased effort on edge experiments and their interpretation. It will also require significant increases in edge diagnostic capability. What is the nature of the perpendicular transport in the SOL? Is it representative of that occurring in the pedestal? What is the role of “blob transport” and its consequences, e.g. for loads on the main walls? What can be done to increase perpendicular transport in the near SOL, i.e., increase λ_p for the divertor power channel, while, if possible, decreasing perpendicular transport in the far SOL and thus loads on the main wall - and how will that affect the pedestal and core transport? What is the role of neutrals, both those recycling from the wall and those injected as fuel? Present models cannot quantitatively reproduce the observed features of detachment – the divertor condition that ITER is counting on. The missing physics needs to be identified. With this knowledge we will know how detachment should be implemented and how it can be reliably controlled to manage the divertor heat flux, including knowing how to implement actuators for semi-detached operation such as toroidally uniform divertor gas puffing, pellet injection, active control of pumping conductance, etc. The aim of answering the above questions is to enable development of a reasonably complete model of the SOL, one that is at least on a par with the excellent progress being made in understanding and modeling of the pedestal and core plasma.

It is important that there be a close, iterative partnership in boundary physics between experiment on the one hand and theory, modeling and simulation on the other. The latter can, when pursued in close partnership with experimentalists, help interpret the results of experiments so as to identify missing, controlling physics. When the latter is then added to the simulation

code-models their ability to reliably predict can be improved, the demonstration of which requires further close, iterative coupling to the experimental work, including the design and planning of benchmarking experiments.

While it is hoped that a more complete identification of the key physics controlling SOL performance will lead to methods of reducing divertor target heat loads, and improvement in plasma performance, it may be that fundamental changes in the divertor configuration for steps beyond ITER will be needed. Such configurations have been proposed, e.g., snowflake and super-X. More detailed studies are needed to assess their relative advantages and disadvantages. Assuming that a significant reduction in divertor target heat loads can be realized, their implementation in a reactor and impact on size, performance and cost needs to be assessed. Assuming continued favorable outlook, a flexible experimental facility aimed at exploration of advanced divertor concepts might be considered.

A.2.3.3 Priorities

Near-term (1-2 years)

- Develop and deploy new diagnostics in existing devices for comprehensive boundary layer measurements (e.g. plasma flow, density, temperature, electric field, turbulence characteristics, neutral density) to provide the data necessary to uncover the controlling physics, especially of cross-field transport
- Contribute key data to self-consistent empirical scaling(s) to predict the peak divertor power flux density, under highly radiative and partially detached conditions and also with mitigated ELMs, in ITER
- Measure off-normal heat loads in existing devices during major disruptions, both mitigated and unmitigated, VDEs, and fast H-L transitions, for improved extrapolation for ITER

Mid-term (3-5 years)

- Develop fundamental understandings of cross-field transport mechanisms responsible for the edge power and particle profiles, including the coupling between the pedestal and the near SOL, and also the far SOL plasma contact with the wall
- Understand the effects of non-axisymmetric magnetic fields on the boundary plasma
- Explore innovative ideas for improved control, mitigation, and management of plasma exhaust loads compatible with excellent core confinement" to the mid term, and suggest that an advanced divertor concept be implemented in a steady-state facility, either one that

exists or possibly in the design of one that is specifically aimed at resolving steady-state issues at reactor relevant power densities

- Explore the detailed effects of RF heating such as ICRF and LH, on the boundary plasma to help establish the compatibility of such heating approaches with high-performance plasmas and acceptable plasma facing component interaction

Long-term (6-10 years)

- Develop reliable predictive capability for high power tokamaks of: the detached divertor plasma state; impurity levels in the confined plasma; the coupling between the SOL and pedestal; turbulent transport in the SOL; the plasma interaction with radiofrequency antennas and launchers; the interaction of RMP fields with SOL properties; off-normal events; and far SOL plasma-wall contact.

A.2.4 Thrust 10: Decode and advance the science and technology of plasma-surface interactions.

Plasma-surface interactions (PSI), aka plasma-wall interactions (PWI), encompass scientific issues that are among the most critical for fusion power, affecting: 1) plasma contamination by eroded material, 2) lifetime of PFCs, owing to sputtering and transient erosion, 3) dust formation and tritium co-deposition in eroded and re-deposited material.

In steady state, the impurity particles enter and leave the plasma at the same rate and, integrally, there is no *net* erosion; however, the *local* net erosion rate may approach the local gross erosion rate. It is the net rather than gross erosion that primarily matters for lifetime of PFCs and tritium retention by co-deposition. The relation between net and gross erosion depends on material migration within the plasma, which is poorly understood even for single-element PFC systems. For mixed materials, such as ITER will employ, quantitative understanding of the processes is almost non-existent. The principal impediment to improved understanding is inadequate surface diagnosis.

Computer codes for interpreting or predicting material migration in MFE devices have been used for many years but have only been bench-marked against measurements in small, local-scale experiments involving the insertion and removal of a small test object into the plasma for a limited number of discharges, thereby achieving a controlled plasma exposure which is potentially interpretable. The reliability of these codes therefore needs to be established.

A.2.4.1 Relevant Issues

- Plasma-surface interactions vary by orders of magnitude depending on temperature, incident species, surface material and exposure time. Can we reliably extrapolate conditions at the wall of today's pulsed confinement machines to future steady-state reactors?
- Transient expulsions of energy from the edge plasma cause unacceptable erosion of existing wall materials. Can we develop clever new concepts to extend the wall operational limits?
- Surfaces evolve when subjected to plasma, neutron and edge alpha irradiation. Is it possible to predict the impact of this evolution on an equilibrium plasma state and on plasma facing component lifetime during steady-state operation? Can more resistant materials and coatings suitable for use in diagnostics, or high-power radiofrequency and microwave components, be developed?

A.2.4.2 Program Elements

- Comprehensive theory-experiment comparisons, in well-controlled and well-characterized conditions
- Detailed investigations of material migration in toroidal geometry
- Evaluation of the plasma-surface interactions of tungsten as a leading PFC material in appropriate plasma, thermal, and radiation damage environment; due to open questions on tungsten melting and micro-structural evolution, a parallel effort should be maintained on back-up options
- Pursuit of opportunities to access plasma pulse lengths in relevant exposure environments in order to bridge the extremely large gap in pulse lengths between present experiments and FNSF/DEMO

A.2.4.3 Priorities

Near-term (1-2 years)

- Develop and improve first principles and reduced PSI models, e.g. through theory and SciDAC initiatives
- Implement real-time in-situ surface material diagnostics in toroidal and linear facilities for comprehensive theory-experiment comparisons, toward assessment of material migration, fuel retention, and plasma modification issues
- Characterize and evaluate tungsten and backup option plasma-facing components in toroidal and linear plasmas devices

Mid-term (3-5 years)

- Extend linear plasma devices, including appropriate upgrades from existing capabilities, to long time-scales for detailed PSI studies in well-controlled and well-diagnosed conditions. Three capabilities to be extended include tritium handling, liquid metal testing, and evaluation of irradiated materials.
- Implement a coherent strategy using short-pulse U.S. tokamaks and long pulse international devices to extend material migration, fuel retention, and plasma modification studies
- Design and evaluate tungsten and backup option PFCs with high pressure helium gas coolant

Long-term (6-10 years)

- In order to provide the technical basis for, and reduce the risk of, a DT FNSF mission, it will also be necessary to assess and pursue a dedicated non-nuclear toroidal PMI/PFC facility (and/or utilize an early phase of FNSF operation with hydrogen or deuterium operation). This will provide research access to the required days to weeks plasma duration in a Demo-relevant exposure environment; comprehensive data on the PMI/PFC response through diagnostic development and deployment will be simultaneously required.

A.2.5 Thrust 17: Optimize steady-state, disruption-free toroidal confinement using 3-D magnetic shaping, and emphasizing quasi-symmetry principles

While tokamak plasmas are nearly axisymmetric, strong 3-D magnetic shaping with external coils is the basis for the stellarator concept. Plasma confinement in stellarators does not require plasma current. They can confine plasma in steady state and with high pressure, and they do not suffer from virulent current or pressure-driven instabilities that abruptly terminate the plasma. Various types of stellarators have been proposed, and stellarators are leading alternates to the tokamak for magnetic confinement of fusion plasma.

Understanding 3-D magnetic shaping is also critical to the function and performance of all magnetic fusion configurations. ITER will use the controlled application of relatively weak 3-D magnetic fields to suppress edge instabilities driven by the H-mode pressure pedestal and to allow plasma rotation.

Because the magnetic field in a stellarator is not toroidally symmetric, energetic ions and charged fusion products may become unconfined. However, when the 3-D fields maintain particular symmetries, such as quasi-symmetry (QS), then energetic particles are predicted to be well confined, and favorable bulk plasma circulation is unimpeded as in the tokamak. The QS

stellarator is a relatively new transformational concept that maintains confinement properties of the tokamak while avoiding severe transient events and control issues that occur in high-pressure plasmas confined in tokamaks. The Helically Symmetric Experiment (HSX) at the University of Wisconsin is the only stellarator in the world that accurately approximates quasi-symmetry. The opportunity to explore the confinement physics of quasi-symmetry at larger, fusion relevant scales is significant.

The Subcommittee considers Thrust 17 among the highest priority because (i) plasma confinement in 3-D magnetic configurations is foundational science for MFE, (ii) the benefits of quasi-symmetry, demonstrated in a university-scale experiment in the U.S., offer opportunities for breakthrough discoveries, (iii) the international investment in stellarators is large, making the stellarator the leading alternative to the tokamak, and (iv) disruption-free performance and controllability of ELM activity make the stellarator promising for fusion development. Additionally, as a research element with high-priority Thrust 2, the application of 3D magnetic fields to alter ELMs and edge properties in tokamaks also holds promise for solving a critical need for ITER and for advanced tokamaks.

A.2.5.1 Research Elements:

Progress on stellarator experiments in Japan and Germany over the past two decades indicates that high-performance, sustained, disruption-free plasmas are attainable in 3-D configurations. This Thrust delineates activities to advance our understanding of quasi-symmetric (QS) stellarators to comparable high-level of performance.

Elements of this thrust also address the unique challenges of constructing the magnets of 3-D devices, including the implementation of 3-D divertors. Additionally, because tokamaks and other nominally axisymmetric toroidal systems exhibit nonsymmetric behaviors, this thrust also includes the advancement of the fundamental science of magnetic confinement from varying degrees of 3-D shaping and the application of analytic and numerical tools drawn from stellarator research to tokamaks.

The research needs for Thrust 17 are organized into four actions:

A.2.5.1.1 Action 1: New QS stellarator experiments for improved confinement in high-performance plasmas

Promising results obtained from the quasi-symmetric HSX experiment at the University of Wisconsin, combined with the theoretically predicted benefits of quasi-symmetry, motivate pursuing QS confinement experiments at a larger scale. The experiences from the larger (non-QS) stellarators (LHD in Japan and W7-AS in Germany) are also supportive of a research program in quasi-symmetric 3-D confinement at a scale relevant to the study of fusion

confinement science. This larger scale allows the integrated investigation of sustained, stable plasmas with hot ions, high pressure, and good confinement.

Stellarators offer significant flexibility in the arrangement of the magnetic configuration, and different types of quasi-symmetric 3-D shaping offer different trade-offs. Quasi-axial (QA) symmetry resembles the axisymmetry of the tokamak, and, with finite plasma pressure, gives rise to moderate levels of self-driven current parallel to the magnetic field (bootstrap current). A stellarator with quasi-axial symmetry is directly comparable to tokamak symmetry. QA symmetry may answer important questions like why the density and pressure-limiting behavior is substantially different in stellarators and tokamaks. Quasi-helical (QH) and quasi-poloidal (QP) symmetry exhibit lower levels of self-generated plasma bootstrap current than the tokamak-like QA configuration, and this may rendering these configurations less susceptible to current-driven instabilities and the need for external control.

Several key research steps are needed prior to the specification of an experiment at this scale. In addition to expanded efforts in theory and the initial results from the large superconducting stellarator, W7-X, now under construction at Greifswald, Germany, at intermediate-scale experiments are necessary in order to establish the scientific and technical knowledge and to inform considerations for QS stellarator development after ITER construction completes. Coordinated pursuit of the various stellarator optimizations, the quasi-helical (QH), the quasi-poloidal (QP), and the quasi-axial (QA) approaches, are needed to establish the confinement properties of QS stellarators. Results from intermediate-scale experiments will inform the choice of the optimal type of QS stellarator for further fusion energy development.

The *ReNeW* Report identified the following key research steps associated with this action:

- Construction and operation of intermediate-scale experiments to test the confinement physics of QA quasi-symmetry and a QH or QP experiment quasi-symmetry. These experiments would have sufficient pulse length and heating power to evaluate stability-limits at low collisionality to compare with theory and the confinement properties of tokamaks. These experiments differ in their magnetic field configuration, coil arrangement, and 3-D divertor design.
- Expansion of 3-D theory and modeling in the areas of turbulent transport, β -limits, impurity transport, nonlinear effects, effects of stochastic magnetic fields, energetic particle effects, effects of plasma rotation, and kinetic effects on equilibrium and stability with 3-D fields.
- Inclusion of 3-D theory and modeling in the effort to develop predictive simulation capability for fusion plasmas (as part of high-priority Thrust 6) applicable to both stellarators and tokamaks, in which small asymmetries of order $\delta B/B \sim 10^{-3}$ are known to affect the plasma behavior.

- Targeted collaboration with the nonsymmetric experiments LHD (Japan) and W7-X (Germany). These activities will focus on steady-state 3-D divertor performance, pressure-limiting mechanisms in stellarators, and integrated performance.
- Implementation of research program to extend the knowledge of QS plasma confinement to fusion-relevant conditions applicable to next-step experiment. The outcome will show the dependence of QS confinement on system size and plasma temperature that extrapolates to burning plasma studies.

A.2.5.1.2 Action 2: Design and construction of 3-D coil systems.

The 3-D coil sets required to produce stellarator magnetic configuration are more complex than those used in tokamaks. The goal is to reduce the technical risk and cost of constructing and maintaining large-scale stellarators.

Key research steps outlined in *ReNeW* include:

- Understand the interplay between the plasma confinement parameters and the detailed design of magnet coil.
- Investigation through modeling of different coil geometries to identify desirable QS configurations with simpler coils.
- Greater use of auxiliary trim coils to ease fabrication and assembly tolerances, and increase flexibility in the magnetic configuration.
- Innovative use of magnetic materials to simplify the shaping of the 3-D field.
- Exploration of high-temperature superconductors, leading to steady-state magnets with relatively low operating costs, improved maintainability (dismountability), and easier fabrication compared with conventional superconductors. (See Thrust 7).

The goal of this set of modeling and development activities is the development of a practical magnet system for QS stellarator experiments.

A.2.5.1.3 Action 3: Divertors for 3-D configurations

Magnetic field lines at the relatively cold edge of the plasma must be diverted to a region where helium ash from the fusion reaction and other impurities can be removed. The plasma temperature must be low enough to prevent rapid erosion of the plasma facing material in this divertor region. The high-density capability of even moderate-field stellarators makes a radiatively cooled divertor solution plausible, though the 3-D geometry makes the engineering design difficult. However, the understanding of divertor behavior in stellarators is less well-developed, and the adaptation of an effective divertor to 3-D geometry is more complex than in tokamaks. The island divertor concept employed on the W7-AS and LHD stellarators, and

planned for W7-X, requires control of the edge rotational transform. It also constrains the divertor to adjoin the main confinement region. Three-dimensional divertor designs that require less edge plasma control and allow for expanded exhaust with rapid pumping is highly desirable. Such designs must also be integrated with the optimization of the entire stellarator magnet system.

Key research steps detailed in *ReNeW* are:

- Designing advanced divertors that handle the necessary power and particle exhaust, and control neutral and impurity influx, while remaining compatible with QS 3-D shaping.
- Increasing participation on the large LHD and W7-X experiments in Japan and Germany in the area of 3-D divertor physics. This activity includes the benchmarking of 3-D edge physics transport modeling codes.

A.2.5.1.4 Action 4: Three-dimensional shaping for improved operation of other toroidal systems.

This action pursues the potential benefits of controllable levels of 3-D shaping to be pursued on tokamaks and other toroidal confinement systems. The application of 3-D magnetic fields (*i*) provide poloidal magnetic field for sustainment of the magnetic configuration, (*ii*) improve stability and prevent uncontrolled vertical displacements and disruptions, (*iii*) minimize the need for feedback systems, (*iv*) further our understanding of tokamak density limits, and (*v*) help understand the confinement improvements seen in nonsymmetric quasi-single helicity (QSH) equilibria observed in reverse field pinches (RFPs).

Of most importance today, 3-D magnetic fields suppress Type 1 edge localized modes (ELMs) in tokamaks (as explained in high-priority Thrust 2), and ELM suppression is required for ITER. The successful application of 3-D perturbation fields to control ELMs makes use of analytic tools developed and used in stellarator research.

Key research steps detailed in *ReNeW* include:

- Conceptual design and modeling of quasi-axisymmetric stellarators with variable levels of 3-D shaping to perform as stellarator-tokamak hybrids.
- Test of variable 3-D shaping on stellarator-tokamak hybrids.
- Application of 3-D analysis techniques to reverse field pinch plasmas.
- Application of 3-D analysis and design approaches to ELM suppression on existing tokamaks and ITER.

Understanding the fundamental consequences of 3-D shaping could bring wide-ranging benefits to the science of toroidal magnetic confinement of plasma.

A.2.5.2 Readiness

The U.S. has the leading theoretical and experimental program in quasi-symmetric stellarators, and operates the only QS stellarator in the world (the HSX experiment at the University of Wisconsin.) A small-scale stellarator-tokamak hybrid (the CTH at Auburn University) investigates the suppression of current-driven instabilities with 3-D shaping. U.S. researchers also are leaders in the control of ELMs by application of 3-D fields. In concert with a broad international program in stellarators, the U.S. research program is scientifically and technically ready to proceed with diverted, hot-ion QS experiments at a fusion-relevant intermediate scale.

A.2.5.3 Priorities

Near-term (1-2 years)

- Advance the fundamental physics and broad understanding of 3-D magnetic fields through advancements in theory and simulation and by using existing university-scale experiments.
- Initiate theory and design efforts to explore stellarator configurations with simpler and maintainable magnet systems with practical 3-D divertors
- Partner with efforts to study and understand 3-D magnetic fields applied to tokamaks, especially to understand 3-D shaping applications to future ITER research scenarios.

Mid-term (3-5 years)

- Complete theory and design efforts to explore stellarator configurations with simpler and maintainable magnet systems with practical 3-D divertors
- Participate in experiments with the Wendelstein 7-X (W7-X) device in Germany and evaluate plasma confinement in quasi-isodynamic (*i.e.* non-quasisymmetric) stellarators

Long-Term (6-10 Years)

- Design and construct a new optimized stellarator research experiment to advance plasma confinement physics in an attractive 3-D magnetic configuration

Appendix B: Comparative Research Utilization of Facilities

The challenge that the fusion program faces can be understood in part by a comparison of the breakdown of its funding between Research, Facility Operations, and Construction, with other programs in the DOE Office of Science (DOE/SC). Table B.1 shows the data reported in the FY2013 Presidential Request (see http://science.energy.gov/~media/budget/pdf/sc-budget-request-to-congress/fy-2013/Cong_Budget_2013_Overview.pdf , p. 14). One must be aware of possible interpretive differences between the different programs. However, the contrasts are so stark as to overwhelm such effects. Facility operations in FES is just 10% of the total, by far the lowest fraction of any program. Facility operations being less than a quarter of the research budget is out of balance, and hampers research, because of low facility utilization. Moreover, FES construction is 45%, by far the highest fraction. This is predominantly ITER; and the fraction promises to rise even further if the FES total is fixed.

The problem is mostly that the U.S. ITER construction contribution is out of proportion to the FES program as a whole. By comparison, the U.S. DOE contribution to the LHC, a forefront high energy physics *international* construction project, at its peak never exceeded 10% of the whole HEP program (\$70M per year, out of a total program of \$683M in FY2000). Or again, in 2003, the peak year for the construction of SNS, a premier *domestic* facility, the BES program's total construction was 25% (\$256M of a total of \$1001M). Construction cost fractions exceeding 40% of a total research office budget are unprecedented in DOE/SC. That is why Fusion Energy Sciences is in such great difficulty. It was in anticipation of these problems of

	Research	Facility Operations	Future Facilities	Workforce
Advanced Scientific Computing Research	49%	50%	0%	1%
Basic Energy Sciences	46%	44%	10%	0%
Biological and Environmental Research	65%	35%	0%	0%
Fusion Energy Sciences	45%	10%	45%	0%
High Energy Physics	55%	30%	15%	0%
Nuclear Physics	32%	56%	12%	0%
Workforce Development for Teachers and Scientists	0%	0%	0%	100%
Science Laboratories Infrastructure	0%	0%	100%	0%
Total, Office of Science	47%	38%	14%	1%

Table B.1: Extract from the FY2013 Office of Science Presidential Request to Congress, showing fraction of program devoted to different categories.

balance that the fusion community insisted, before endorsing ITER participation, it could not to be at the expense of the domestic research program.

Appendix C: Acronyms and Abbreviations

2-D	Two-dimensional, sometimes used to describe axisymmetric systems
3-D	Three-dimensional, sometimes used to describe non-axisymmetric systems
APS	American Physical Society
AT	Advanced Tokamak has high beta, large self-driven plasma current, and long-pulse operation
Beta	β = the ratio of plasma pressure to magnetic field pressure
BLM	Barrier Localized Mode
Bootstrap	refers to Bootstrap current, a self generated current within a toroidal plasma
DEMO	Demonstration fusion power plant
DIII-D	A tokamak at General Atomics (GA)
Disruption	A rapid and catastrophic loss of plasma confinement in an MFE device
Divertor	A magnetic system to direct edge plasma in a tokamak to the scape-off layer (SOL) and a material target
DMS	Disruption Mitigation System
DOE	Department of Energy
ECCD	Electron Cyclotron Current Drive
ECH	Electron Cyclotron Heating
ECRF	Electron Cyclotron Range of Frequencies
ELM	Edge Localized Mode, periodic bursts of energy from the edge plasma of

a tokamak during high-confinement operation

FACA	Federal Advisory Committee Act
FES	Office of Fusion Energy Science in the Department of Energy
FESAC	Fusion Energy Sciences Advisory Committee
FNSF	Fusion Nuclear Science Facility for integrated tests of components and scenarios
FRC	Field-Reversed Configuration, a magnetic confinement system with no toroidal field and high β
FSP	Fusion Simulation Program
GA	General Atomics, California
H-Mode	High-confinement Mode, regime with edge transport barrier in a tokamak
HSX	Helically Symmetric Experiment, a stellarator at the University of Wisconsin-Madison
I-mode	An ELM-free H-mode discovered on Alcator C-mod
ICRF	Ion Cyclotron Range of Frequencies
IFMIF	International Fusion Materials Irradiation Facility
ITB	Internal Transport Barrier, typically associated with H-mode in a tokamak
ITER	International burning plasma experiment being built in Cadarache, France
JET	European tokamak sited in the UK
JRT	Joint Research Target, an annual joint facilities (C-Mod, DIII-D, NSTX) experimental milestone, defined annually
L-mode	Low-confinement Mode, regime without edge transport barrier in a tokamak

LHCD	Lower Hybrid Current Drive
LHRF	Lower Hybrid Radiofrequency
MFE	Magnetic Fusion Energy
MGI	Massive Gas Injection, a technique to mitigate disruptions
MHD	Magnetohydrodynamics
NSTX	National Spherical Torus Experiment at PPPL
NSTX-U	National Spherical Torus Experiment-Upgrade at PPPL
NTM	Neoclassical Tearing Mode, related to magnetic reconnection in an MFE device
Pedestal	A flat profile of density and/or temperature near the edge of a tokamak plasma, typically associated with H-mode operation
PFC	Plasma Facing Component
PMI	Plasma-material Interaction
PPPL	Princeton Plasma Physics Laboratory, Princeton University, New Jersey
PSI	Plasma-surface Interaction
PWI	Plasma-wall Interaction
Q	The ratio between fusion power produced and heating power supplied
q-profile	A measure of the local twist of magnetic field lines, q is the sometimes referred to as the “safety factor”
QH-mode	H-mode performance without ELMs, first obtained in DIII-D
QS	Quasi-symmetric
Quasi-isodynamic	A 3-D helical symmetry employed at the W7-X stellarator in Germany
Quasi-	A 3-D helical symmetry employed in some stellarators

symmetry

R&D **Research and Development**

RAMI **Reliability, Availability, Maintainability, and Inspectability**

RE **Runaway Electron**

ReNeW **Research Needs Workshop (Magnetic Fusion Energy Sciences) and associated report**

RF **Radiofrequency**

RFP **Reversed Field Pinch, a magnetic confinement system with low toroidal field**

RMP **Resonant Magnetic Perturbation, method of ELM control**

RWM **Resistive Wall Mode, plasma instability allowed by a resistive first wall**

SC **DOE Office of Science**

SciDAC **Scientific Discovery through Advanced Computing**

Snowflake **A particular magnetic divertor geometry**

SOL **Scrape-off Layer, boundary layer between hot fusion plasma and first wall**

ST **Spherical Torus, a low aspect ratio tokamak confinement system**

Stellarator **A magnetic confinement system using strong 3-D magnetic fields**

Super-X **A particular magnetic divertor geometry**

TBM **Test Blanket Module**

TF **Toroidal Magnetic Field**

Tokamak **A magnetic confinement system with 2-D magnetic fields and a plasma current**

Torus **A 3-D surface topologically similar to the surface of a donut**

VDE **Vertical Displacement Event, violent movement of plasma following a disruption**

W7-X **Wendelstein-7X, an optimized stellarator being built in Germany**

Appendix D: Statement of Charge



Department of Energy
Office of Science
Washington, DC 20585

Office of the Director

April 13, 2012

Dr Martin Greenwald
Plasma Science and Fusion Center
Massachusetts Institute of Technology
77 Massachusetts Avenue
Cambridge, MA 02139

Dear Dr. Greenwald:

The U.S. fusion program has demonstrated leadership capability in plasma dynamics and plasma control science. It also has made major contributions to fusion materials science, but our effort there has been relatively modest. Progress on both fronts is required to establish the scientific basis for fusion.

A leading concern is how best to use the resources available. This includes positioning the U.S. to capture the science of ITER and, reciprocally, how to position the U.S. to be as influential as possible in the ITER era. Such influence can come both through the science directed to ITER, and that science which complements it. It also includes consideration of opportunities that will complement ITER and burning plasma science research. Indeed, in both the plasma and fusion materials sciences, gaps have been identified that, if filled, will lead to U.S. leadership and the chance to accelerate the time line to establish the scientific basis for fusion energy.

For planning purposes, it will be of value to the Office of Science and the Office of Fusion Energy Sciences to have an assessment by FESAC of priorities among and within the elements of the magnetic fusion energy science program. Note that General Plasma Science and High-Energy-Density Laboratory Physics programs, while recognized by the Office as important to the broader plasma sciences, are not part of this charge. I therefore ask FESAC to consider the following charge related to scientific priorities for magnetic fusion. Please assume that the ITER project is ongoing, will be until the end of this decade, and is supported separately from the rest of the program:

1. With the focus on research that supports burning plasma science and that addresses critical challenges for long-pulse/steady-state operation including plasma-wall interactions and materials, prioritize among and within the FY2013 elements of the non-ITER magnetic fusion portion of the Fusion Energy Sciences program. Assume funding at the FY2013 Presidential budget request level of effort, and that a sustained investment in the US ITER project will extend over much of this decade. New elements may be inserted into the prioritization after FY2013, with an accompanying adjustment in priorities.

2. Considering the same focus as in (1), again prioritize the elements of the non-ITER part of the magnetic fusion portion of the FES program, but assume a restoration of the budget to the 2012 level for that part of the program. New elements may be inserted in the prioritization after FY2013.
3. Prioritize the elements of a U.S. program that has a substantially enhanced emphasis on fusion materials science. Consider the five year period following the roll-off in ITER project construction funding. Assume that the roll-off allows a 50 percent increase in the non-ITER magnetic fusion level of effort during that 5-year period over that in the FY2013 budget, and that research on fusion materials science and harnessing fusion power will capture much of this increase.

In assessing an element's value, consider the role of cross-links between program elements. Consider research opportunities overseas to be part of a U.S. research program. Your assessment should be informed by the recent FESAC reports in international collaboration opportunities and materials science, the 2009 Research Needs for Magnetic Energy Sciences Workshop (ReNeW) report, the 2007 FESAC report on Priorities, Gaps, and Opportunities, and earlier input by the National Academies and FESAC.

Yours Sincerely,



W. F. Brinkman
Director, Office of Science, DOE