Snowmass Technology SUMMARY Report

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Organizer: M. Abdou

Subgroup Leaders: M. Abdou, R. Callis, S. Milora, M. Ulrickson

Topics & Discussion Leaders:

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i. Overview Summary

i.1 Introduction

The Technology Working Group (WG) sessions at Snowmass focused on the key issues and opportunities for technology research in the next decade for both magnetic (MFE) and inertial (IFE) fusion energy. Technology was divided into two subgroups: i.) Chamber Technology, and ii.) Plasma Support Technology. Seven topics were addressed under Chamber Technology and three topics were the focus of Plasma Support Technology. These topics were pre-selected by the community, eight months prior to the meeting, as the most important to discuss at Snowmass. In addition to the ten topics in the two subgroups, the Technology Working Group addressed two broader topics that were common to both subgroups. These common topics included the contributions of technology research to improving the vision for an attractive fusion energy system and to advancing science. The Technology WG organizers gave structure to the process such that it would encourage full participation, encourage mutual understanding, and foster inclusive solutions. This was accomplished in several ways: 1) A web site was constructed to exchange information and comments. 2) Regular conference calls were held. 3) Core group opinion papers were submitted to stimulate pre-Snowmass discussion. 4) Hierarchy in decision-making was reduced and, 5) Flexibility was maintained in order to accommodate new or unexpected comments and differing views. Thus, the process of the Technology WG encouraged participants from the community to exchange a broad range of opinions and information before, during, and after the Snowmass meeting.

The Snowmass experience revealed amazing consensus on many important issues and opportunities for technology research in the next decade, not just for technologists, but also the physicists. While there were varied opinions among the participants on some complex technical issues, the process helped provide a better understanding of the issues and options to resolve them. And most importantly, there came a commitment across disciplines to work together for common goals. This paper exhibits the significant advances that can be made when the right teams are formed to tackle fusion technology issues at hand.

i.2 Exciting Opportunities and Partnership

It is the consensus of the Snowmass participants that the opportunities for fusion technology research in the next decade are exciting. These opportunities also call for fostering the “partnership” between technology and plasma physics and between IFE and MFE. These opportunities for technology research carried out in the “partnership” framework will provide essential contributions to the following key elements of fusion research:

I. Creating an improved vision for:
   a) an attractive and competitive fusion product
   b) a cost-effective path for fusion development

II. Enabling near-term fusion progress

III. Advancing Science

The ten topics discussed in the technology sessions at Snowmass identified many opportunities for contributions to the above three elements. These are summarized in the next several sections and detailed in the main report. Table 1 shows examples of opportunities for creating improved vision through performance enhancement and reduction of both cost and complexity. Table 2 shows other examples for creating improved vision through the development of innovative solutions. Table 3 provides examples of opportunities for enabling near-term fusion progress.

Technology research will continue to offer excellent opportunities for advancing science by a) enabling plasma science, and b) advancing the underlying engineering sciences. Many of the most significant advances in plasma research became possible as a direct consequence of improvements in heating, current drive, magnetics, plasma-interactive components and other technologies. Technology research involves a broad array of engineering sciences such as surface science, fluid dynamics and MHD, thermomechanics, material science, electrodynamics, and nuclear science. Improved understanding and advances in the underlying engineering sciences are prerequisites to technological innovation. Many of the topics discussed in the next several sections identify exciting opportunities for broadening the frontiers of science and enhancing the prospects for innovation.

Table 1

Examples of Opportunities for Creating Improved Vision through Performance Enhancement, Cost and Complexity Reduction

Magnets	**• Develop innovative conductors and design criteria to reduce cost (factor of 2) and reduce size**
Plasma Profile Control	**• Develop high power density, efficient heating and current drive systems**
PFC/Solid Walls	**• Extend capabilities of solid wall concepts**
Availability	**• Increase emphasis on maintainability and reliability in design studies and as a metric for confinement and technology concepts**

Table 2

Examples of Opportunities for Creating Improved Vision through Innovative Solutions

Safety and Environmental	**• Develop new rad-waste management strategy that minimizes both volume and hazard**
Liquid Walls	**• Develop liquid wall concepts for both MFE and IFE**
Materials	**• Increase performance limits for low activation materials** **• Expand scope to include high performance refractory alloys**
Tritium	**• Explore ways to ensure tritium supplies for future needs (Develop breeding technology on burning plasma devices?)**

Table 3

Examples of Opportunities for Enabling Near Term Fusion Progress

- ☐ **Develop tools to locally optimize and sustain plasma profiles**
- ☐ **Develop disruption avoidance and mitigation techniques**
- ☐ **For PFC: Apply new refractory alloys with improved thermal shock and radiation damage resistance**
- ☐ **Develop targets for IFE/IRE including fabrication, injection and tracking.**
- ☐ **Utilize existing facilities and construct new non-neutron test facilities for chamber technology exploration. Assess neutron source options and recommend strategy and priorities**

i.3 Liquid Walls

About 60 participants from MFE/IFE, Technology/Physics discussed the potential attractiveness, key issues, and current research related to liquid walls. Many configurations and concepts utilizing the liquid wall idea were presented. The area is very concept-rich. The following themes emerged from the discussion:

- Possibility of an improved vision of a fusion product
- Reduced development costs,
- Enabled attractive physics regimes, and
- Considerable physics/technology, MFE/IFE synergy

Liquid walls have the potential to lead to an improved fusion energy reactor product with competitive economics and attractive safety & environmental features. Examples of this potential include high power density, impulse loading, and disruption handling capability without failures in highly-stressed FW/D components, enabling high β, stable physics regimes that lead to smaller devices, reduction in volume and hazard of radioactive waste, *etc*. In addition liquid walls have the potential to reduce MFE/IFE development costs and affect the technology and physics programs in positive ways by making the material development and testing issues more tractable, allowing FW/D proof-of-principle demonstration without neutrons, providing near-term HHF technology for long-pulse physics experiments.

What are the Opportunities for Liquid Wall Development?

Liquid Walls are just now entering into serious concept exploration – it is clearly too early to identify the most promising configuration or working liquid. Significant design, analysis, modeling, and experiments are needed to quantify the potential benefits and identify attractiveness trade-offs, explore the many concept variations that utilize the LW idea, and explore generic critical issues in (magneto-) hydrodynamics, heat transfer, plasma edge transport, surface composition and sputtering, *etc*.

Facilities to address generic R&D needs have been identified and include:

- LM-MHD/Free Surface Flow in Tokamak-like Magnetic Fields
- Thermal-Fluid/Free Surface Flibe Simulant Flow and Flibe Handling
- Laser and Heavy Ion Beam Propagation in Vapor and Droplet Mists
- HHF, Sputtering, and Plasma Interaction Experiments

It was generally concluded that testing in tokamaks should initially focus on divertor, but may need a dedicated all-liquid wall experiment to fully test the physics potential.

i.4 Solid Walls

The Solid Walls topic was covered in three parts. The first session covered first wall and blanket applications of solid walls. Plasma facing component applications for solid walls were the topic of the second session. Opportunities for solid wall research were discussed in the final session. Approximately thirty people attended the sessions. A brief summary of the conclusions in the areas of first wall and blanket and plasma facing components follows.

First Wall and Blanket

An attractive fusion power system should incorporate high power conversion efficiency, the ability to accommodate high power densities, a low failure rate, the capability for faster maintenance and extended component lifetime, adequate tritium breeding, as well as exhibiting favorable safety and environmental features. Over the years, many design concepts have been studied, and they provide a starting point to estimate the power loading capability of solid first wall systems.

Key issues for first wall and blanket components include: a) high power density/power conversion capability, b) lifetime/reliability of solid wall/blanket systems, c) high temperature refractory metal alloy development, d) insulator coatings needed for liquid metal systems, and e) solid breeder thermomechanical interactions. The following opportunities were identified during the sessions: 1) investigate the scientific foundations for innovative, high-performance systems through integrated design studies, 2) development of high temperature design code rules, 3) leverage access to international research with work on conventional systems, and 4) develop solid breeder materials with enhanced thermal properties.

Plasma Facing Components

In the last five years the capability of plasma facing components has increased ten-fold and reliability has increased. This effort will continue to enable high-power-density long-pulse-length fusion devices for the near term. The addition of refractory metals and helium cooling capabilities will continue the usefulness of solid walls in future devices. Solid surfaces are a necessary alternative to liquid walls until those systems are proven.

Several key issues were identified in the plasma facing component session. Carbon based materials are unlikely to extrapolate to fusion reactors or burning plasma devices because of tritium retention. Water cooling presents safety concerns for fusion energy (e.g., hydrogen generation, mobilization of activated materials due to interaction of water with hot surfaces,…). Heat removal capabilities need to improve to keep pace with fusion device performance advances. New refractory alloys (e.g., Mo and W with nano-particles of TiC added) that have improved thermal shock and radiation damage resistance can be applied to plasma facing components. We still have the opportunity to improve the heat removal capability of water (near term) and He gas cooled PFCs (long term) through the use of porous metals. The use of low-pressure liquid-metal cooling needs to be investigated. The joining and fabrication methods for refractory metals based PFCs needs to be developed (candidates include e-beam welding, friction stir welding and brazing). We need to take advantage of existing disruption facility to test new materials for both MFE and IFE.

i.5 Availability/Reliability/Maintainability

Availability is a primary metric for an attractive power plant, directly affecting the cost of electricity. Reliability (represented here by the mean time between failures, MTBF) and maintainability (represented here by the mean time to repair, MTTR) are two of the most important quantities that influence the availability of a fusion device. The availability resulting solely from random failures is:

$$A = MTBF/(MTBF+MTTR)$$

A goal availability of ~75% typically is assumed for power plant studies, but confidence in our ability to achieve this is low due to many uncertainties associated with a technology still in its infancy. Nevertheless, the importance of availability to an attractive fusion energy source strongly suggests that it should be raised to a higher level of importance in next step options, reactor studies, and emerging concepts development.

Availability improvement depends on both physics and technology. For the technology program, the most important near-term need is to vigorously pursue innovation, taking advantage of advances both inside and outside the fusion program. For the physics and confinement side of our program, we need to develop availability metrics and apply them as a discriminator in confinement concept development.

Reliability and maintainability programs are well established in many existing industries, such as the automotive, chemical processing, fission power, military equipment, and aerospace industries. Their long history of successes and failures provides a wealth of information for us to draw upon. Expanded efforts to “mine” and utilize existing databases should be supported, in addition to more formal compilation of experience on existing fusion research devices.

The current program of fusion-specific work on “maintenance technologies” in the US is modest or non-existent; much larger program exist in Europe and Japan. Due to the importance of this topic, advanced maintenance techniques (such as “reliability-centered maintenance”) should be applied to operating machines and a few common purpose remote maintenance tools and techniques (e.g., remote metrology, remote welding/cutting, advanced motion control) should be identified and developed. Remote inspection, handling and/or repair of selected internal components and/or diagnostics can be demonstrated on existing machines. With such a limited number of sources of fusion-specific data on reliability and maintainability, every opportunity needs to be fully exploited.

i.6 Testing Conditions and Facilities

Two sessions were held at Snowmass to discuss what facilities are needed to address fusion nuclear technology issues. About 30 scientists participated in these sessions. It is generally agreed that existing non-irradiation facilities, fission reactors and 14 MeV neutron sources can continue to provide information useful for fusion technology Concept Exploration (and in limited cases, Proof of Principle) issues for the next 10 years. However, construction of new non-irradiation and irradiation facilities are needed during this time frame to enable R&D on numerous technology Proof of Principle (PoP) issues in the years beyond 2005-2010. Any effort for a new proposed facility should consider opportunities for synergistic and multiple application researches and strive for reduction of costs. Potential new non-irradiation facilities include a large-scale thermofluid facility for IFE/MFE liquid walls. The wrap-up discussion focused on the evaluation of performance capabilities of the presented irradiation facilities. However, due to a lack of time at Snowmass, no conclusion could be drawn on the relative merits of the various irradiation facilities and possible strategies for the deployment of neutron sources. Nevertheless, it is believed that the output from the Snowmass’ discussions provide a stepping-stone to further stimulate and focus community input and debate. Specifically, with regard to the irradiation facilities, the following observations can be made:

The LASREF (spallation neutron) upgrade facility could provide useful information that is complementary to existing facility capabilities at a relatively modest cost, but it cannot address many of the technology PoP issues (cf. Table 4). The LEDA (p-Li) upgrade facility has capabilities that are comparable to the LASREF upgrade facility. The laser neutron source provides an interesting scientific tool -- a 14MeV neutron point source in space and time with a narrow energy spectrum -- at modest cost. It can provide data on both pulsed and steady-state damage accrual by adjustment of the pulse repetition rate and can investigate the microstructural stability of small (~1mm) samples up to lifetime doses (a key PoP/Performance Extension issue). None of these facilities (LASREF, LEDA, laser neutron) are suitable for a comprehensive fusion materials (structural and nonstructural) materials development program, due to limitations in flux and/or volume. It is unclear at the present time whether any of these facilities would be of interest for international collaborations.

The proposed IFMIF (d-Li) would enable fusion materials development (technology Concept Exploration, PoP, and performance extension [lifetime] issues) and some material interaction issues related to FW/blanket/magnet development. The construction and operating costs of IFMIF are significantly higher than LASREF, LEDA or the laser neutron source. An increased commitment to fusion technology development would be needed in order for the US to fully participate in this proposed international facility.

The plasma neutron sources (GDT, VNS-ST, VNS-AT) offer the potential for investigating most of the key technology issues up through the Performance Extension stage, but at a higher risk (due to unresolved physics or technology issues) and cost compared to the other neutron sources. More detailed estimates of the availability and facility cost of the various plasma neutron sources are needed in order to judge which facility is most attractive for fusion technology testing.

i.7 Waste Minimization

Materials choice has long been recognized as a key factor in realizing the full safety and environmental potential of fusion power. Because the materials are de-coupled from the fusion energy source (the plasma), the long-term neutron-induced activation of components can be tailored by proper selection of materials to avoid generation of waste that would require deep geological disposal. Thus, the idea of “low activation” materials was conceived for the US fusion program with the hope that such material could be disposed of as low level waste (e.g., shallow land burial) and would not pose a burden to future generations.

The environmental impact of waste material is, however, determined not only by the level of activation, but also the total volume of activated materials. A tokamak power plant is large, and there is a potential to generate a correspondingly large volume of activated material. Fusion's waste volume is a direct environmental concern. Fusion's waste volume and mass is ~ 3 times that of fission wastes for similar net electric power and fusion average neutron walloading of ~ 4 MW/m^2. Furthermore, the public believes low level waste is bad and high level waste is even worse. The radiotoxicity hazard of the waste depends on the release risk, which is understood only by technical experts.

Some materials may become candidates for recycling, and others may be cleared from regulatory control by meeting prescribed criteria that have yet to be agreed upon internationally. Recently these concepts of recycling or clearance have been recognized as options for reducing the volume of radioactive waste from a fusion power plant. Determining if a material can be recycled or cleared from regulatory control depends largely on our ability to limit the induced activation of the component. Thus, there is a need to explore new and innovative concepts that can substantially reduce the activation of the large ex-vessel components that contribute significantly to the overall volume of activated material and to extend the capability of conventional conceptual fusion designs with proper optimization to achieve the same goal. The impact of these parameters on other aspects of plant performance must also be considered.

The results of our discussion suggest that the waste management strategy for fusion needs to be modified. While low activation materials do reduce the long-term activation hazard of the waste, their use in and of itself does not necessarily reduce the volume of activated material and the subsequent amount of radioactive waste arising from the plant. Furthermore, the current waste management strategy of ensuring that all material can be disposed of via shallow land burial may not be practical when large quantities of fusion waste are to be managed in the next century. Thus, a waste management strategy focused solely on low activation materials does not address the entirety of the radioactive waste picture for fusion. We recommend a strategy that is balanced with respect to minimizing both the hazard (via low activation materials) and the volume (including ex-vessel). As such we propose the following minimum design goals:

- To reduce the overall radioactive waste volume by limiting vessel/ex-vessel activation so that the bulkier large volume components be cleared or recycled for re-use.
- To minimize activated material in a fusion plant that cannot be cleared or recycled

There are many opportunities to address the implications of this strategy in the next decade. There is a need to better understand the tradeoffs associated with this dual strategy of minimizing both hazard and volume. Systems and power plant studies should examine in a systematic manner the tradeoffs associated with changing blanket and shield materials to meet these new design goals relative to changes in the radial build of the machine, cost of energy, performance impacts and reduction of radioactive waste volume.

Recycling can clearly reduce the volume of radioactive waste needing disposal. However, a serious study of the economics and technical tradeoffs and the environmental impact associated with recycle is needed to determine the efficacy of this approach and the impact on the environmental picture for fusion. Such a study should examine the economics of recycling and the criteria used for recycling. It is also important to understand the tradeoffs associated with volume reduction via recycling versus increasing the hazard of the waste because of buildup of certain impurities via reuse in a fusion machine.

In addition to their improved performance potential via high wall load and high efficiency, high power density/high wall load concepts (e.g. liquid walls, refractory alloys)offer important advantages relative to the overall volume of activated waste in a fusion machine. The higher wall load produces a more compact machine, which in turn reduces the volume of the bulkier activated components (e.g., shield, VV, and magnets) by 30 to 50%. Finally, other fusion technology innovations that may reduce the volume of activated materials should be explored.

i.8 Tritium Self-Sufficiency

Attaining tritium self-sufficiency is necessary for self-sustaining fusion plants operating on the D-T fuel cycle. Tritium is bred in a lithium-containing blanket surrounding the plasma or the IFE target. The tritium fuel cycle involves many subsystems whose physical and operational characteristics impact the success in achieving tritium self-sufficiency. To insure tritium self-sufficiency, the calculated achievable tritium breeding ratio (TBR) should be larger than the required TBR.

The minimum required TBR should exceed unity by a margin that accounts for tritium losses, radioactive decay, tritium inventory in plant components, and supplying inventory for startup of other plants. The latter two have the largest impact. The required TBR is >1.01 depending on the tritium inventory, required doubling time and other system parameters. The amount of online reserve tritium inventory required is uncertain and need to be assessed. Tritium fractional burn-up impacts tritium inventory. High burn-up fractions are desirable to reduce the required TBR.

Chamber technology concepts utilizing liquid Li or LiPb have the largest potential for achieving tritium self-sufficiency followed by LiSn and Flibe. Among solid breeder candidates, Li2O has the best chance for achieving tritium self-sufficiency. An effort should be made to reduce the amount of structural material particularly in the FW and front 10 cm of blanket. Uncertainties in calculating the achievable TBR could be as high as ~10% due to uncertainties in nuclear data, calculation method, and modeling. An aggressive effort is required to reduce the uncertainty to <3%.

The achievable overall TBR depends on the confinement scheme due to the impact on breeding blanket coverage and possible limitation on blanket thickness. The IFE system has a clear advantage since no divertor, limiter, heating and current drive systems are employed. Furthermore, IFE systems have nearly full blanket coverage and the blankets can be made as thick as needed without impacting the high cost driver. Absence of magnetic fields in IFE chambers makes it easier to employ flowing liquid breeders. There is no clear advantage for any of the MFE confinement concepts. Better definition of penetration requirements is needed. Even though some MFE confinement concepts suffer from reduced blanket coverage and limited blanket thickness, tritium self-sufficiency can still be achieved with carefully designed blanket concepts.

Two major critical issues were identified. These are:

- Tritium supply is currently marginal and diminishes rapidly after 2025.
- Tritium self-sufficiency in DT fusion power plants can not be assured unless specific plasma and technology conditions are met.

The opportunities that exist for resolving these issues are:

- Aggressive tritium breeding technology should start without delay.
- Near-term DT burning devices (e.g. ITER-like) should provide for testing breeding technology and have their own breeding capability.
- Definitive demonstration of tritium self-sufficiency can be performed only in a DT fusion facility. These tests do not require long operating time.
- Use existing 14 MeV neutron source facilities and code development to improve the ability to predict tritium breeding.

i.9 Materials

Materials sessions were conducted at Snowmass to discuss potential advances in materials (over the next decade) that would: improve the vision for an attractive and competitive fusion energy system and contribute to the lowering of the cost and time for R&D. Presentations were made in the areas of: structural materials for first-wall/blanket applications; non-structural materials for plasma-facing components, tritium solid breeders, Be multiplier, liquid metal/salt coolants and breeders, shielding material, waste management strategies, large-scale experimental facilities, material needs for IFE reactor concepts, and energy issues. Common themes were: the importance of studying materials in combination (e.g., structural-materials/coating/coolant), the necessity of design optimization studies for evaluating materials performance and prioritizing materials R&D, and the desirability of conducting R&D that would serve both MFE and IFE needs. Key materials needs to support feasibility and proof-of-principle issues for reactor design concepts and R&D plans to address these needs were discussed. Thirty-five people participated in the sessions. Major opportunities highlighted during the sessions are summarized in the following.

Consideration of New and Improved Alloys: The Advanced Materials Program (AMP) R&D efforts are focused on three low-activation structural materials (vanadium alloys, ferritic steels and SiC/SiC), along with a small effort on copper alloys for next-step devices. Performance tradeoffs among high power density, longer lifetime, and activation and waste volume minimization could allow some relaxation of the severe restrictions on alloy composition currently in place. This would provide the much-needed flexibility for developing higher performance structural materials. Examples include consideration of improved W and Mo alloys and evaluation of more oxygen- and creep-resistant vanadium alloys (e.g., V-Ti-Al).

Near-term, Cost-effective R&D: Reactor designs range from low-performance, next-step devices to intermediate-to-high-performance concepts (e.g., solid first walls with He or liquid metal coolants) to very high-power-density concepts (e.g., liquid walls, He-cooled refractory metals). A wide range of materials feasibility and proof-of-principle issues could be addressed in the next decade using existing facilities and new low-cost, non-nuclear facilities. Examples of these issues are: chemical compatibility between structural materials and liquid metals/salts, reliability of electrical-insulator and tritium-barrier ceramic coatings, and neutron radiation stability of improved W and Mo alloys. The current AMP roadmap is based on the highest-priority issues for three low-activation structural materials. An expanded “fusion technology” materials roadmap may be needed to include the broader range of coolants and solid materials.

Integration of Materials R&D: The Advanced Materials Program focuses on structural materials, while other programs within Advanced-, Enabling- and IFE-Chamber-Technologies deal with a wide range of structural and non-structural materials. Integration of the materials R&D (or at least improved communication) within the chamber technology areas offers the potential of utilizing the best materials expertise to address materials issues. A coordination group would be useful to facilitate interaction and communication among the various technologies concerned with materials-related issues.

i.10 Heating, Current Drive, and Fueling

Heating and current drive technologies are essential for heating plasma to fusion-relevant betas and temperatures, and for manipulating plasma properties to access advanced operating scenarios (reversed shear, MHD stabilization, turbulence suppression). Physics and technology working groups at the Snowmass meeting emphasized the need for improved control of pressure and current profiles and transport barriers in order to access long-pulse advanced-tokamak scenarios. We need improvements in system reliability and flexibility, and a change from the present “blunt tool” capability for controlling profiles to a more refined ability to tailor profiles to access the high-beta, high-bootstrap-fraction plasmas needed for future research.

Significant progress has been made in developing and deploying high-power gyrotrons at the ~1-MW level at 110 GHz, and the development of 170-GHz prototype units for electron cyclotron heating/current drive (ECH/ECCD). Fast-wave (FW) antenna arrays in the >1-MW unit size for Ion Cyclotron Heating (ICH) and current drive (via direct electron heating) have been developed and tested. Progress is also being made in other countries on the development of negative-ion based, high power neutral beam injectors (NBI) (0.5–1.0 MeV). In line with the needs of the physics groups, the emphasis of the development of these heating and current drive technologies will concentrate on improving RF system reliability, robustness and performance; increasing power density (higher voltage limits for ICRF launchers), higher gyrotron unit power (1.5 to 2 MW), increased efficiency gyrotrons featuring depressed collectors, ICRF tuning and matching systems that are tolerant to rapid load changes, steady-state gyrotrons and actively cooled ICRF launchers for long-pulse/burning-plasma, next-step options, improved NBI sources, long pulse / CW lower hybrid (LH) antenna systems and basic research on helicity injection.

Fueling is another technology that is essential for achieving fusion-relevant plasma parameters and manipulating plasma parameters to achieve improved performance (peaking of the density profile for higher reactivity and reducing transport via turbulence suppression). Recent successes include sustained operation above the Greenwald density limit on DIII-D, high-field side launch with improved density profile peaking, internal transport barrier generation, the development of steady-state pellet injectors operating in the 1.5-km/s speed range, and the demonstration of core fueling in experiments using accelerated compact toroids (CTs). Pellet fueling technology has also been used recently to ameliorate the effects of major disruptions in tokamaks by delivering massive amounts of low- and high-Z material that rapidly quench the current in vertically unstable plasmas. It has been estimated that eliminating disruptions in tokamaks in the fusion energy development class would increase the lifetime of divertor plasma facing components by a factor of two. Reducing the severity of disruptions could allow the advanced tokamak to operate nearer its ultimate potential. A critical issue for fueling in next-step device plasma regimes is the degree to which profile peaking is needed (for higher density operation and improved reactivity and confinement) and the technological requirements to meet that need (pellet speed, CT density and the physics of CT deposition). Advances are also needed in disruption detection, in developing low-Z mitigation techniques (i.e. massive gas-puff, liquid jet injector, etc.) and integrating detection and mitigation into the control system of an existing tokamak to test the full system and demonstrate reliability.

i.11 Magnets

The Snowmass discussions on goals and strategy identified 3 ways for magnet technology to lower the cost of fusion experiments and reactors: 1) reduce the cost of the conductor, magnet components, and/or assembly processes; 2) optimize the magnet systems, so the cost of other subsystems are reduced; and 3) improve conductor and magnet performance to increase the physics performance, e.g. increase magnetic field. The general goal for MFE magnet research in the next 10 years is to reduce the size & cost of magnets by a factor of ~2. For IFE magnets the goal is also a factor of ~2 for KrF magnets and ~2 for HIF system costs. The specific targets are to increase the allowable current density, the working stress and the electric field.

The greatest improvements can be made in the superconducting strand and cable, where a cost reduction factor of 3-10 is possible. This will involve Concept Exploration for innovative conductors, quench detectors, and protection mechanisms not previously used in large scale fusion magnets. It will require an innovative Concept Exploration facility (<M$1). Consideration should also be given to using

superconductors for near term fusion magnets to demonstrate the performance improvements on a larger scale & develop manufacturing technology. For specific cases, the superconducting coils may also be more cost effective for the lifetime of the device.

Design and manufacturing technique improvements for magnet structure should be as significant as improvements in material properties for cost reduction of structure.

Introduction of High Temperature Superconductors (HTS) - Only the Low Temperature Superconductors, NbTi or Nb3Sn, are in use or planned for fusion devices, with the exception of the Levitation Coil for LDX, which will use HTS, BSSCO-2223 tape. In the near and midterm, other HTS magnets must be introduced to prepare for use in Performance Extension experiments. More advanced materials, such as YBCO, depend on development of large quantity fabrication of long conductor lengths. Magnetic fields of up to 20 Tesla and higher operating temperatures than LTS, are possible.

World Program - Significant developments in magnet technology for fusion have been achieved in world programs, but no large fusion programs exist in the U.S to drive magnet R&D. Due to the high cost of the test samples and testing, an efficient way to conduct R&D is through international collaborations, eg-CS Model Coil (JA), TOSKA (EU), SULTAN (EU, & PTF (US).

IFE Magnets - In HIF, because of the large number of small quads in multiple-quad arrays, the stored energy and size is such that a prototype activity is a cost effective approach. The winding distribution, stresses and strains are not the same for an array as for a single quad, so the effort should include an iterative approach with single quads followed by arrays. The low field requirements and simple geometry of coils for KrF lasers indicates that they are natural choices for near term application & demonstration of performance for HTS superconductors.

Resistive Magnets - Resistive magnet R&D can improve performance and reduce magnet cost for selected missions, either pulsed or steady state. Key issues are: 1) intensive active cooling optimization within mechanical stress limits, 2) simplification of coil & conductor designs, 3) improvement of the radiation compatibility of insulators & conductor materials, & 4) development of demountable TF coils which may significantly improve the tokamak or ST as reactor candidates.

Generic Development - Generic items that would extend machine lifetime and/or reduce costs are: 1) develop insulators with improved radiation resistance, mechanical, electrical properties and processing; 2) use advanced manufacturing techniques (eg- laser forming) and simplified designs; and 3) improve integrated CAD, CAE & CAM. Major benefits could also accrue from advanced code development for conductor & structural analyses.

i.12 IFE Target Fabrication and Injection

This group addressed the key question: “Can the technologies needed for low cost, cryogenic targets and a high rep-rate injection system be developed?” We discussed the target fabrication, DT filling and layering, and target injection and tracking technologies that will be needed for practical Inertial Fusion Energy (IFE). Our conclusion is that although target fabrication and injection issues exist that must be resolved for IFE to be a practical energy alternative, potential solutions also exist that appear attractive. The basic approach we recommend is to take maximum advantage of the target development work being done by the Inertial Confinement Fusion (ICF) Program for the National Ignition Facility (NIF). In parallel, we must carry out modest scale laboratory development activities as part of Phase I of the IFE Program Roadmap to demonstrate that a credible pathway exists to practical IFE target fabrication and injection. This information will contribute to the decision of whether or not to proceed with an IFE Integrated Research Experiment (IRE) and if so, what technologies to use. These developments will then be applied to the IRE, demonstrating many of the technologies needed for IFE.

<u>Target Fabrication</u>. We need high quality, mass produced targets at low cost. Many of the materials and fabrication processes needed for IFE targets have been or are being developed by the ICF program. However, some are not. High Z foams for Heavy Ion Fusion distributed radiator hohlraums have no ICF analog and will require development. Plastic foam capsules needed for direct drive targets have been developed but IFE specifications have not yet been met; more development is needed. Even where ICF fabrication techniques meet IFE specs, the processes need to be adapted to mass production at low cost. This includes selection of the candidate processes such as microencapsulation and fluid bed coating of capsules and injection molding of hohlraum parts, demonstration on the lab bench that the processes can meet accuracy specs, and projection they can meet cost goals. Every IFE target could be inspected for gross defects, but detailed characterization to adjust fabrication processes must be done on a statistical sampling basis. Stable and repeatable fabrication processes are essential.

<u>Filling and Layering</u>. We must fill ~500,000 targets per day and precisely layer the DT ice. Most of the basic DT fill and layer issues will be investigated and resolved on the NIF. Permeation fill can have several kilograms of tritium in process. Current layering techniques are equipment-intensive, using an isothermal layering sphere or a hohlraum with precisely tailored temperature distribution to let beta layering make a smooth uniform DT layer. We must investigate alternate fill and layer techniques, such as fluidized bed technology.

Injection and Tracking. We must accurately inject cryogenic targets into the high temperature target chamber at about 5 Hz without thermal or mechanical damage and track them with high precision so they may be hit by the driver beams. Gas gun experiments at LBNL have demonstrated injection and tracking for indirect drive surrogate targets at room temperature. These must be extended to the higher precision needed for direct drive and cryogenic targets. Dry wall laser chambers are proposed to have ~1 Torr of Xe gas to absorb x-rays. It would also heat and slow the injected direct drive target. The thermal and mechanical properties of cryogenic targets must be measured and the target/chamber trade-offs must be studied.

IFE Roadmap Plans. IFE Target Fabrication and Injection are part of the IFE Roadmap. During Phase I, we will carry out tasks to support the decision as to whether or not to proceed with the IFE IRE, and if so, what target technologies to use. During Phase II of the IFE Roadmap, we will continue target fabrication development and will provide a target injection-tracking system to the IRE for integrated system experiments.