Snowmass Technology Working Group Report

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

Topics & Discussion Leaders:

Liquid Walls (R. Moir / N. Morley)
Solid Walls (R. Mattas / M. Ulrickson)
Availability/Reliability/Maintainability (M. Tillack / B. Nelson)
Testing Conditions and Facilities (S. Zinkle / A. Ying)
Radioactive Waste Minimization (D. Petti / E. Cheng)
Tritium Self-Sufficiency (M Sawan / S. Willms)
Materials (S. Zinkle / M. Billone)

Heating/Current Drive/Fueling Technologies (D. Swain / R. Temkin)
Magnets (J. Schultz / R. Woolley)
IFE Targets (K. Schultz / W. Steckle))

i. Snowmass Technology Report

1. Introduction

This report summarizes the discussions and main findings and conclusions of the Technology Working Group sessions at the Fusion Summer Study held in Snowmass, Colorado, July 12-23, 1999.

The report is organized around the same subgroup and topic structure utilized for discussions at Snowmass. Technology is divided into two subgroups:

i. Chamber Science and Technology
ii. Plasma Support Technology

Seven topics are addressed under Chamber Science and Technology in Section 2 and three topics are discussed under Plasma Support Technology in Section 3. In Section 4, special topics of common interest to technology are discussed. First, an attempt is made to develop working definitions for the R&D stages. Second, examples of the contributions of technology research to science are given. It is important to note that most topics under Technology covered both IFE and MFE

The Snowmass meeting was a new experience for the fusion community, as to its breadth, format, and structure. Therefore, it is worthwhile to briefly summarize the process that was followed by the Technology Working Group (WG) in preparing for and conducting the meeting. This process led to the final concluding summary at the meeting and to this final report.

Twelve key members of the fusion technology community were selected in November 1998 to serve as “conveners”. In consultation with the community, the conveners selected ten topics as the most important to discuss at Snowmass. For each topic, two members of the community were selected to serve as leaders/co-leaders. The conveners and topic leaders (25 persons) served as the “organizing team” that guided the preparation prior to the meeting, facilitated the session discussions during the meeting, and led the writing of this report. The 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) web site, 2) conference calls, 3) core group opinion papers and prospectuses, 4) reduction of hierarchy in decision-making, and 5) maintenance of flexibility in order to accommodate new or unexpected comments and differing views.

For each topic, the leader/co-leader recruited a group of experts who were most interested in helping with the preparation for the meeting. This “core working group” for each topic, together with the topic leaders, prepared a 2-page prospectus describing the key elements of the paper topic. They also authored a draft opinion paper that was intended to stimulate discussions prior to the Snowmass meeting.

A well-maintained web site greatly facilitated communication and encouraged community participation. All information including opinion papers, comments and responses from community members, and session plans were all made available on the web as soon as they were received.

The schedule for the sessions at Snowmass is given in Appendix D. During the first week, the participants concentrated on detailed discussions of the topics. In general, two or three parallel sessions were held each afternoon. During the second week, the focus was on synthesis and summary, mostly in plenary sessions for the technology participants. The technology sessions were well attended with broad participation from both the physics and technology members of the fusion community.

A final summary of the Technology Working Group was presented on Friday, July 23 at the meeting concluding the plenary session. The drafts of the final report for each topic were prepared by the topic leaders, core working groups, and participant volunteers. The draft report was then circulated to all participants and made available on the web site. All comments were incorporated into the final report.

2. Chamber Science and Technology

Subgroup Leaders : *M. Abdou, M. Ulrickson*

2.0 Introduction

The "Chamber" is the region immediately surrounding the plasma in MFE or the IFE target in MFE. The main functions of the chamber include providing for: 1) particle handling, 2) heat deposition and extraction, 3) tritium breeding and processing, 4) vacuum and, 5) radiation shielding. Chamber R&D includes many scientific areas and technical disciplines, such as: 1) neutronics, 2) materials, 3) thermofluids, 4) thermomechanics, 5) chemistry, 6) remote maintenance, and 7) safety and environmental impact.

Prior to the Snowmass meeting, seven topics were selected as the most useful to discuss at Snowmass. Each topic was formulated as a key question and given a number (CQn: Chamber Question n). These topics were as follows:

CQ1: Liquid Walls
CQ2: Solid Walls
CQ3: Availability/Reliability/Maintainability
CQ4: Testing Conditions and Facilities

CQ5: Radioactive Waste Minimization

CQ6: Tritium Self-Sufficiency

CQ7: Materials

The Snowmass participants observed that the recent re-structuring of the fusion program has resulted in major changes in chamber technology. The new focus of chamber technology research aims to:

1) emphasize scientific exploration of innovative concepts with more promising potential
2) establish US leadership in Advanced Chamber Technology Concepts
3) enhance collaboration with international programs

The Snowmass discussions have reinforced this new focus.

Summaries of the Snowmass discussion sessions are given in Sections 2.1 through 2.7 for topics CQ1 through CQ7, respectively.

2.1 Liquid Walls

Key Questions

What are the merits and issues for liquid walls? What experiments, modeling, and analysis must be done to judge their potential for IFE and MFE? What are the key go/no go issues and how can they be explored quickly?

Topic Leaders

Ralph Moir (LLNL), Neil Morley (UCLA)

Core Working Group

Dick Majeski, Rich Mattas, Dale Meade, Craig Olsen, Steve Payne, Per Peterson, Tom Rognlien, Mohamed Sawan, Dai-Kai Sze, Mike Ulrickson

2.1.1 Motivation

For some time now people have thought of liquid walls as an attractive solution to the technology problems of high power-density plasma configurations for MFE, and as (nearly) essential for the pulsed wall-loading conditions in IFE. A flowing, renewable surface could be eroded, evaporated and even be broken apart with no permanent adverse effects on a structure that would require frequent maintenance and replacement. Alpha particle energy could be removed without conduction through a solid wall and the associated thermal stress and creep failure modes, and the energy could possibly be extracted at high temperatures for efficient energy conversion. If a liquid wall of sufficient depth could be formed, radiation damage and waste disposal issues for solid structures could be significantly ameliorated.

All these benefits are indeed possible, if only liquid walls could be made to work! As was seen during this session, there are many issues associated with the successful and attractive implementation of liquid walls. But a theme to keep in mind, advanced during the pre-Snowmass discussions via the web, is:

> Ask not 'will liquid walls work,' ask 'what can you do to make liquid walls work!' Rick Nebel

This type of theme could be equally applied to other nuclear technology concepts as well, and is in keeping with the spirit of innovation leading to better vision for fusion that is advanced by the technology community.

2.1.1.1 Session Organization

The discussions on this topic were spread over 3 afternoon sessions and were focused on 5 subtopics

1. Do liquid walls really have the potential to yield a more attractive fusion energy product?

2. What modeling and experiments are required to establish the hydrodynamics and surface heat transfer of various liquid wall configurations for MFE and IFE?

3. What plasma modeling and experiments are required to determine the criteria of compatibility of liquid walls with acceptable tokamak or emerging concepts plasma operation (*e.g.* allowable surface temperature?)

4. Will residual liquid vapor and droplets affect target and driver propagation in IFE?

5. Is there a clearly superior choice of working liquid?

Each subtopic was lead by different teams assembled in advance of the meeting. The teams made short introductory comments to begin the session and stimulate discussion. Interested community members requesting time were allowed to make presentations, although in some cases this took much time and significantly reduced the time left for meaningful discussion. Side discussion also took place that helped provide input into the main sessions.

The total number of participants was about 60, and the makeup was about 2/3 MFE to 1/3 IFE and 2/3 Engineering to 1/3 Physicists.

2.1.1.2 Attractiveness of Liquid Wall Systems

The question was put to the group on the first day whether or not liquid walls were worth pursuing. A presentation by R. Moir put forth the case that liquid walls do have the potential to dramatically improve the vision of a fusion reactor because of three technological points:

- Elimination of first wall (divertor, and blanket) structure resulting in reduced thermo-mechanical problems related to thermal stress, embrittlement, creep, *etc.*

- Neutron attenuation by liquid in front of most (possibly all) solid structures, reduced parasitic capture of neutrons in solid structure

The impact that these two points can manifest in a variety of possible reactor improvements such as:

- Higher power density capability – results in smaller and lower cost components (magnets, chambers, vacuum vessel)
- Elimination of erosion lifetime limitations at divertor and FW/Limiter surfaces
- Increased disruption survivability potential
- Reduced volume of radioactive waste (reduced size, increased lifetime)
- Reduced radioactive hazard from accidental releases
- Higher availability due to increased lifetime and reduced failure rates
- Lower capital cost by reduction in first wall and blanket replacement, number of hot and cold cells, amount of handling equipment, *etc*. (Highly design dependent)
- Makes difficult structural material problems more tractable

Not all these potentials have been fully demonstrated by calculation and analysis (although several have) but are based on the opinions of the participants. The group expressed caution about overstating the benefits of liquid walls, as comparatively little research has been done in this area. For instance, the point was made that LW may actually be required in order to achieve disruption survivability. This point was debated and not all participants felt this was true, but most agreed that the potential for survivability was there.

In addition to these technological features, potential benefits on plasma confinement and stability were brought up and discussed by physicists attending the session. These physics improvements are unique to liquid metals LMs because of the high electrical conductivity, and are only realized if the LM flow is electrically connected toroidally. They include:

- higher elongation (κ=3 possible) resulting in higher stable β (20%)
- stabilization of resistive wall modes (requires wall velocity ~>10 m/s)
- flat T profile very close to the wall via DT gettering from the edge

Only several centimeters of LM are required to realize this potential. Some debate ensued concerning the effect of hydrogen gettering on the plasma temperature profile, and the point was made that to maintain such a profile would require intense edge fueling to keep the density high. It is not yet known to what extent Flibe may also aid in pumping of impurities, although its hydrogen solubility is low. The ability of Flibe to getter hydrogen and helium ash needs to be resolved.

2.1.2 Top Level Issues

It was certainly recognized in the sessions that implementing a thick liquid wall would not be a trivial matter and would likely engender a variety of tradeoffs. Various design concepts were advanced and are briefly summarized in Table 1 and design trade-offs in Table 2. In addition, feasibility and attractiveness of any one is not assured and may come out negatively, different conclusions are possible for different plasma confinement schemes (MFE) and drivers (IFE). Rethinking of optimum plasma confinement and driver propagation should proceed hand-in-hand with liquid wall development to achieve both a feasible and attractive vision of a fusion reactor. The top level issues discussed in the sessions included:

- Evaporation and sputtering threatens to put out plasma burn in MFE systems limiting the blanket operating temperature

- Evaporation and liquid debris threatens pulse rate in IFE systems

- Nozzles and liquid supply systems must form the liquid wall and divertor flow pattern required and exit nozzles (or drains) must receive the flow without drips and other liquid debris

- MHD effects with liquid metals might preclude the desired open channel flows for MFE systems unless active electromagnetic pumping is used. Insulators are integral parts of the liquid metal concepts.

- MHD effects arising from plasma currents in the divertor and off-normal plasma events like disruptions

- Penetrations for MFE systems and beam port protection for IFE systems have serious design issues and can cause breaks in axi-symmetry assumptions needed for some LM concepts in MFE

- Low bulk temperature rise in thick liquid wall schemes threatens attractiveness via penalties in pumping power and efficient energy conversion

A significant effort is needed to determine if core plasma performance can be maintained in the presence of liquid walls. This effort includes theory and modeling with impurity transport codes, near-surface plasma codes and stability codes.

These issues (and their sub-issues) were discussed in detail and it was generally felt that no concepts should yet be ruled out. There were, however, very strong opinions concerning the feasibility of the thick wall flows for tokamaks. Some felt that only thin liquid layers (especially of cold Lithium) would be feasible, and that they would require in-situ EM pumping to overcome MHD interaction with the confining magnetic field. Such a thin liquid wall implementation would in principle allow the use of some other minimum structure, easily replaceable conventional liquid blanket outside of the first wall region. A similar feeling was observed for laser IFE. Because of direct drive illumination and final optics requirements, thick liquids are not considered feasible and thin liquids are only considered as a back-up for large dry-wall gas protected chambers due to fear of contamination of optics.

The group, however, felt that the possibility of implementing a thick liquid wall, with all the neutron attenuation benefits associated with it, should be pursued for MFE (as well as IFE) in tokamaks and other confinement schemes might be more amenable to thick liquid walls, like the FRC.

2.1.3 Opportunities for Fusion

The consensus of the participants is that research on liquid walls represents and exciting opportunity to advance fusion. Research and development for liquid walls include modeling, laboratory experiments and tests in present physics devices, as well as more integrated tests in future fusion devices. The near term R&D should focus on modeling and experiments for concept exploration and feasibility issues that are generic to a variety of concepts and working fluids.

Although there was agreement on the focus of the near-term R&D, there were varied views on the most effective R&D path for liquid walls. The general feeling was that the likely path for development of liquid walls in tokamaks would proceed via a liquid divertor experiment in a major tokamak like C-MOD or NSTX, and a suite of single effect and multiple effect experiments would support the design and integration of these divertor experiments. Such divertors may be needed for power handling in high power, long-pulse tokamaks, and some people believe this would be the ideal initial test bed for plasma-LW interaction data. Such a pathway would also help develop the integration of liquid divertors with all-liquid-wall reactors by exploring the formation of the divertor surface and the interaction with plasma currents. Both these issues have been raised as serious concerns.

Another opinion is that the same liquid will be needed in the divertor as in the thick liquid wall and that the liquid should be the molten salt Flibe. This selection is made in spite of known shortcomings of Flibe, such as low thermal conductivity and problematic chemistry, since its low electrical conductivity would lessen the MHD difficulties associated with liquid metals. Before thick free channel flow of liquid metals even with insulating surface coatings can be taken seriously, it must be shown that MHD drag forces can be handled.

Off-line discussions about the need for a dedicated liquid wall/tokamak experiment, where the beneficial physics implications of all liquid walls could be explored in a controlled way, was also reported as a possible alternate pathway. Such a dedicated device, following successful initial experiments in small plasma machines, would require long pulse or steady state toroidal fields so that liquid motion could be stabilized before plasma initiation, and a large volume flow loop to establish liquid wall configurations of interest. The details of such an experiment are still vague, but this suggestion represents an intriguing opportunity to push forward the development of liquid walls in a major way.

For IFE liquid walls, scaling suggests that liquid hydrodynamics can be studied with water facilities, and that chamber dynamics (x-ray ablation and debris venting) can be studied independently with z-pinches and NIF. This work would be in parallel with an Integrated Research Experiment IRE that will focus on driver propagation in artificially generated, but realistic liquid chamber environments. Integrated, geometrically scaled experiments with water would follow near term hydrodynamic feasibility experiments for both oscillating jets and thin film flows. Chamber scaling suggests that studies with ignited targets and prototypical coolants (i.e. Flibe) are not required until actual construction of an average power chamber for an IFE Engineering Test Facility

In either event, near term R&D should focus on concept exploration and feasibility modeling and experiments that are generic to a variety of concepts and working liquids. From the discussion, the following experimental and modeling efforts are needed.

Fundamental Design, Theory and Modeling: This is needed for all concepts and experiments and should include

- 3D Hydrodynamics/Free surface codes with appropriate turbulence, MHD, and shock loading models (MFE and IFE)
- Turbulent, wavy surface, and droplet heat and mass transport modeling (MFE and IFE)
- Plasma impurity transport modeling (MFE)
- Vapor transport and condensation modeling (IFE)
- System modeling and concept analysis and design (MFE and IFE)

Thermal-Fluid Flibe Free Surface Flow Experiments: A series of scaled hydrodynamic configuration and cavity clearing experiments is needed to simulate Flibe liquid wall dynamics and heat transfer. (MFE and IFE)

LM-MHD Free Surface Flow Experiments: A series of magneto-hydrodynamic configuration experiments in magnetic fields representative of the tokamak and other magnetic configurations is needed. These experiments should explore both passive free surface flow in relevant fields and field gradients, and the active electro-magnetically pumped concepts as well. Also, a series of experiments is needed in which heat transfer is studied in liquid metal free surface flows with relevant MHD effects. It is not yet clear whether such experiments can be effectively combined with the MHD free surface flow experiments. (MFE)

Flibe Chemistry and Handling Experiments: Experience in handling and utilizing high temperature Flibe is needed before it can be realistically developed for reactor applications. Such experiments will focus on safety, corrosion, TF control, target debris recovery, and tritium handling. (MFE and IFE)

Small and Large Impurity Influx Plasma Experiments: Data on the effect of liquid contact (evaporation and sputtering) plasma operation can be obtained from experiments on existing plasma devices, and is seriously needed in order to establish the feasibility of liquid wall concepts (MFE)

Driver Propagation Experiments: Experiments to explore the effects of residual vapor and droplet mists on the efficient propagation of driver beams. (IFE)

Vapor Condensation Experiments: The level of residual vapor in chambers following wall ablation is a crucial parameter for determining driver propagation, and needs to be better quantified by experiments and compared to numerical prediction.

Pursuing this research represents an opportunity to advance fusion in the near term and in the long term. It is significant that there is intense overlapping interest from both the physics and technology communities and IFE and MFE communities. These synergistic interests can be capitalized upon to reduce the development time and costs for fusion energy in a significant way via joint facilities and modeling tool development, improved prospects for materials development, and the reduction of the need for neutrons in large component feasibility tests.

Acknowledgments: We would like to take this opportunity to thank everyone who participated in the liquid walls pre-Snowmass discussions and the Snowmass sessions for providing their thoughtful input and genuine enthusiasm.

Table 1: Samples of Various Liquid Walls Concepts

MFE	IFE
TLW: Thick Liquid Walls. All solid surfaces covered by a thick (~40-50 cm) liquid layer flowing on curved back-wall using centrifugal forces	**Thick Liquid Pockets:** Arrays of oscillating and stationary jets enclosed target explosion and protect beam-lines. Vapor is condensed on cold droplet sprays
SWIRL: Swirling (Vortex) Thick Liquid (~40-50 cm). A subset of TLW developed for cylindrical vessels like in the FRC	**Wetted wall:** Thin liquid layer, fed from nozzles or introduced through porous back-wall, protects the solid surface from X-ray ablation and target debris
CLiFF: Convective Liquid Flow First-wall. Thin (~1-2 cm) liquid layer flowing on curved back-wall. Attempts to protect all solid surfaces from surface heat flux.	**SWIRL:** same as MFE applied to certain innovative IFE concepts
EMR, Magnetic Propulsion or Bubble: Electromagnetically Restrained or Pumped. Similar to TLW or CLIFF ideas with applied or induced current in LM to push flow against back-wall or pump the liquid along the flow direction.	**INPORT:** Array of easily replaceable porous tubes protected like wetted walls
ALPS: Liquid Divertor - Various film, jet and droplet designs for intercepting plasma heat flux in the divertor region only	**PERITs:** Interleaving thin flat liquid jets issuing from Perforated Rigid Tubes protect first solid wall like fish scales
	EMG: Electromagnetically guided thin liquid layer for X-ray and debris protection

Table 2: Liquid Wall Design Choices for Fusion

Thick Liquid Wall Attenuates neutrons and absorbs surface heat fluxes, maximizes the potential of liquid walls	Thin Liquid Layer Absorbs surface heat and protects underlying surfaces, but does little to attenuate neutrons
Free Jets Thick liquid jet or jet arrays make up plasma/target facing surface	Wall Flow Liquid layer flows attached to a wall or oozing through porous media
Passive Flow Control Using centrifugal force from back-wall contours, vanes or initial flow momentum to guide liquid flow	Active Flow Control Use applied electrical current or electrically controlled actuators to influence liquid flows, *e.g.* in-situ pumping
Toroidally Continuous Liquid flows that are completely toroidally continuous with no electrical breaks (axi-symmetric)	Toroidally Segmented Divided flow with sector channels or penetrations (or vanes) so that MHD boundary layers can form
Liquid Metal High electrical and thermal conductivity	Molten Salt Low electrical and thermal conductivity

2.2 Solid Walls

Key Questions

What advances may be possible for evolutionary concepts (e.g. solid plasma facing materials, traditional blanket concepts)? What are the potential near term applications for this area? What is the potential for achieving high temperature and high power density?

Topic Leaders

Mike Ulrickson (SNL) / Rich Mattas (ANL)

Core Working Group

Unidentified

2.2.1 Introduction

The successful development of high performance plasma facing components (PFCs) is central to the overall development of fusion, and has posed progressively more difficult challenges as the power of fusion devices has increased. Solid surface PFCs are needed to achieve the near term mission of fusion research. We must have robust PFCs in long pulse and DT devices. Helium ash must be removed, and with the necessary pumping and required particle flow to the first wall region (e.g., divertor) comes intense heat. PFCs are bombarded by energetic neutrals, ions, electrons and photons and must survive intense plasma-materials interactions, such as sputtering, without contaminating the plasma. In long pulse devices, PFCs must also continuously remove high heat fluxes while withstanding off-normal heating transients (such as disruptions). And in DT devices, remotely maintained PFCs must survive neutron radiation and simultaneous cyclic thermal heat loads while avoiding the build-up of unacceptable tritium inventories. Activation of materials and after heat are also important considerations.

The development of adequate power and particle control technology for fusion is a multi-disciplinary effort. It includes plasma science, through investigation of the plasma edge, surface science and plasma-materials interactions, and technology development in materials, joining, manufacturing, high heat flux and materials testing as well as advances in engineering such as thermohydraulics and heat transfer. There have to be and are strong direct links between particle and power handling technology and the Plasma Science Program.

Plasma current disruptions cause both very high heat flux on plasma facing components and large forces on the PFC and supporting structure. The high heat flux causes melting and/or spallation of the surface. Disruption induced erosion is the life limiting process for metallic PFC. It is a significant factor in the life of carbon based PFC. The forces can cause failure of the component. Currents induced by disruptions may weld parts together. If disruptions could be eliminated or controlled, the design of PFCs would be simplified and operation at higher heat flux in steady state would be possible.

2.2.2 Recent Progress

The PFC program is building on its recent science and technology successes. The basic materials processes and mechanisms that result in carbon/tritium redeposition have been determined. The viability of in-situ plasma spray repair of Be has been proven. The program has achieved long-term (>3000 cycles) reliability of beryllium and tungsten joints to copper for heat fluxes above 10 MW/m^2. The program is continuing its campaign to complete and model erosion/redeposition measurements of the graphite divertor plate in the DIII-D lower divertor to allow more accurate extrapolation of component lifetime. Joint experiments with MIT on Alcator C-Mod will provide data on lower-erosion-high-Z materials, which may be candidates as alternate concept plasma/facing materials. Innovative wall conditioning techniques such as electron cyclotron discharge cleaning (ECDC) will continue in Alcator C-Mod and in off-line facilities. Data already available from TFTR needs to be analyzed to better understand the effects of lithium wall conditioning on first wall materials and tritium retention in carbon based materials. High strength copper alloys have been identified for use as heat sinks for long pulse operation.

Recent plasma experiments on ASDEX-U, JT-60U and DIII-D have shown that detached plasma conditions are possible in relatively closed divertors. The detached divertor plasmas radiate most of the plasma thermal energy to the walls of the divertor channel. This reduces the heat load on the divertor surfaces a factor of 3-5 compared to attached divertor operation. Another method for reducing heat loads is to increase radiation in the outer region of the core plasma. Such operating modes have been demonstrated recently. In addition, the low temperature and high-density plasmas found in the divertor are suitable for the use of high Z refractory metals for the plasma-facing surface. With the exception of disruption heat load effects and ELM’s, a refractory metal detached plasma divertor has the potential of having no sputter erosion of the surfaces. The lifetime of such components will be determined by thermal fatigue and/or neutron irradiation. Higher power density devices will require even more care to make the heat loads on the plasma facing components tolerable.

Tritium retention in carbon based materials (either in the bulk or in eroded and redeposited layers) has been identified as a major issue. In both TFTR and JET the retention was found to be approximately 50% of the tritium injected into the torus. Methods for effectively removing the retained tritium without contaminating the plasma chamber are not adequately demonstrated. Irradiation of carbon based materials increases the tritium retention. Thermal conductivity decrease and swelling can also result from irradiation. These effects are worst below about 1200°C. Some of the newer carbon fiber composite materials are not well characterized under neutron irradiation. Alternatives such as SiC have low thermal conductivity. The majority of participants in the solid wall sessions felt that carbon based materials could not be used in a DT machine. A minority felt that new CFCs might be developed or that operation at very high temperatures would be beneficial.

The PFC program has successfully completed two important technology milestones for ITER in FY 1999 by 1) completing, in conjunction with industry, the ITER divertor cassette prototype fabrication and its verification testing, and 2) optimization of joining technology for ITER-relevant, plasma-facing materials and heat sinks with demonstration of component reliability.

Results from the ITER plasma facing component development program have shown that changes in the size of the pieces of refractory metals placed on a heat sink greatly improve the lifetime of such components. The use of tungsten rods (3 mm dia.) has successfully been tested in thermal steady-state for up to 1000 thermal cycles at 25-30 MW/m2. New methods for joining tungsten rods to a copper alloy substrate have also been developed. New joining methods will be needed for refractory metals to refractory heat sinks.

The PFC community has initiated two joint basic science working group activities with the plasma science community. The first will investigate the PMI needs of candidate alternate concepts such as NSTX, in order to begin development of new diagnostic techniques and impurity control, development of new plasma-facing components such as divertors (NSTX, LHD) and helicity injectors (NSTX). The second will investigate the basic science behind the processes responsible for thermal and plasma wall conditioning. In addition, the PFC element has reached two important milestones linking basic plasma science and technology, which have relevance for both alternate concepts and tokamaks. This activity includes completing the benchmarking of theoretical codes such as REDEP with tokamak data, and the application of this science to the design of candidate alternate concepts. Finally, PFC research is continuing its characterization of mixed wall materials (tokamakium) and their effects on the core plasma.

2.2.3 Opportunities for PFC Research in the next decade

Our greatest challenge is the combination of the competing requirements associated with minimizing contamination to the plasma through the selection of armor, maximizing heat removal through cleverly engineered heat sinks, and joining these two features. The step to long pulse operation even without D/T is a major step since it requires the development of robust actively-cooled PFCs and, for superconducting devices, wall conditioning techniques that can be done in the presence of the magnetic field

Working in close collaboration with the plasma physics program, the R&D goals of the PFC program are to:

- provide the plasma science and materials expertise necessary to understand, ameliorate and exploit the interactions between the fusion plasma and the wall and components that surround it,

- qualify the plasma facing materials, technology and components necessary to maintain continued progress in the tokamak program, and

- develop the PFC technology essential to support the maturation of early-concept alternate magnetic fusion confinement and inertial fusion energy reactor concepts.

The high edge plasma temperatures caused by the pumping of liquid walls may not be acceptable from an erosion standpoint. Low edge temperature (detached) plasma have been found in several devices. Such conditions favor high Z materials that have low or no sputter erosion. There are opportunities for development of robust PFCs that exploit new fabrication technologies for refractory metals. Industrial involvement in this area should continue. Existing experimental facilities should be fully exploited to address key issues. Fabrication techniques need to be developed for refractory metals. Potential joining techniques for refractory alloys include: brazing, friction stir welding, e-beam welding, and explosion bonding. The properties of some of the new refractory alloys are not well known.

Disruptions are a major near term issue. We must continue to live with disruptions until mitigation and control techniques are proven. Disruptions constrain design options and performance. They limit the lifetime of PFCs but are not a single shot failure mechanism with proper design.

The design of chambers for Inertial Fusion Energy devices presents new challenges for materials. The short pulse length indicates the development of a single particle model for material removal is important. Mechanical integrity may be more important that thermal conductivity and erosion. The majority felt that even though tritium inventory in hot irradiated graphite is uncertain it is likely to be a problem for an IFE reactor. Experiments on IFE relevant materials need to be started.

Major international experimental efforts are underway to understand and control plasma surface interactions. In JET the US is helping to characterize and model the complex phenomena associated with erosion/redeposition of their carbon divertor. New plasma edge diagnostic measurements are proposed for ASDEX-U. Wall conditioning studies are underway in TEXTOR and LHD. US plasma test facilities are being used to study the fundamental science of erosion/redeposition and tritium retention/removal with our Japanese and European colleagues. There are also extensive international collaborations in the areas of high heat flux testing and PFC engineering. The US is testing actively cooled divertor concepts from Russia, Japan, and the European Union. Innovative manufacturing techniques such as plasma-spray technology, advanced joining techniques, and new plasma-facing materials are being evaluated for JET, LHD, W7-X and KSTAR. Collaborative power deposition measurements and modeling for the Tore Supra CIEL components are also under discussion.

Plasma-Facing Components must be able to survive Plasma Materials Interactions (PMI) such as sputtering, without plasma contamination. The allowed plasma contamination for impurities depends strongly on the atomic number of the material. Either eroding materials that are low atomic number, or high atomic number, refractory materials that do not erode appreciably are favored. Since erosion of carbon-based materials leads to tritiated codeposited films, alternatives to graphite must be developed. Plasma facing components must also remove steady-state High Heat Flux up to 50 MW/m2 and withstand off-normal heating (e.g. disruptions) from the plasma at heat fluxes that are 100 – 1000 times the normal load. Finally, plasma-facing components must be able to survive neutron irradiation simultaneously with cyclic heating under high stress, and be compatible with remote handling and maintenance.

The test of the first theoretical code that couples core plasma to the first wall while integrating all of the known major plasma/first wall interactions will be attempted. This milestone will not only provide a more complete theoretical understanding of wall interactions with the core plasma, but will allow for significant improvements in the design of more accurate alternate concept proofs-of-principle and new machines.

By 2005, the program will provide the next generation of reliable solid state sensors ("smart tiles") required for the understanding of plasma/materials interactions in tokamak and alternate concept plasmas. A demonstration of new conditioning techniques compatible with high magnetic fields will also be performed.

The advent of smaller alternate concept devices means that development of more efficient and reliable heat and impurity removal techniques will be required. In the next 5 to 10 years, the program will complete the fabrication and demonstration of an innovative plasma/facing component which will have both a 50% increase in critical heat flux, as well as a 50% increase in erosion lifetime. Plans for deployment of such a component on a fusion device will be developed.

New molybdenum and tungsten alloys have been developed in the Japanese fusion program. The new alloys are formed by the addition of nano-particles of TiC to either Mo or W. The effect of the addition is to increase the recrystallization temperature and lower the ductile to brittle transition temperature. These alloys are only made in small quantities. Even though there are still many questions about how these materials will behave under irradiation, they have the potential of enabling advances in the capability of refractory metal PFCs. Joining of refractory metals is an area that remains to be investigated. The fracture toughness and power handling capability of these new materials will be tested soon.

Since refractory metals prefer to operate at high temperatures, the use of helium gas cooling is a logical choice. An important alternative to helium gas is the use of a liquid metal spray against the back face of the heat sink. The phase change of the liquid metal can remove large quantities of heat with a lower operating pressure that helium gas. Several Department of Energy funded Small Business Innovative Research Grants have focused on development of porous metal heat exchangers for gas cooling. The critical heat flux for gas cooling has improved significantly (up to about 30 MW/m2) as a result of these efforts. Demonstration of an efficient refractory porous-metal heat exchanger would make application of high-temperature refractory-metal components to a fusion device possible.

2.2.4 Key Issues

The issues are:

1. How effective are the improvements that have been made to refractory metals in increasing the useful fatigue life of such materials for plasma facing materials? Under low neutron fluence in near term devices? Eventually for reactors with high fluence?

2. What are the best joining materials and methods for attaching refractory rods to a refractory heat sink? How high can the operating temperature be? What are the irradiation effects on such materials? What are the failure modes of the joints?

3. What are the best techniques for fabrication of refractory porous metals for the heat sink? What are the operating temperature limits?

4. What purity must be achieved in the He gas to prevent damage to the refractory metal heat sink or the porous metal? What is the generation rate of impurities in the coolant? Is tritium an issue for the coolant?

5. What is the optimum physical arrangement for the coolant passages in a divertor application? How can flow instabilities be avoided under non-ideal conditions?

6. How can components like these be applied to existing tokamaks or innovative concepts?

7. What is the trade-off in safety between higher temperature operation versus decay heat of the refractory materials?

8. Can plasma disruptions be controlled or eliminated in magnetic fusion devices?

9. What are the best choices for IFE reactor chamber materials for each driver?

10. Can carbon based materials be used for IFE reactors?

2.2.6 First Wall/Blanket Area

2.2.6.1 Introduction

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. In order to meet these goals, it will be necessary to push the candidate design concepts and materials to the limits of performance in terms of temperature, mechanical properties, compatibility, and radiation damage. The overall objective of this topic is to address the key issues associated with improving the attractiveness of conventional first wall/blanket systems.

Possible ways of improving performance include reducing the mechanical constraints on the FW/Blanket to reduce thermal strains, simplifying the design to improve reliability and maintenance, moving radiation sensitive joints to the rear of the blanket, inserting insulator breaks to reduce EM forces during disruptions, enhancing the heat transfer coefficient to the coolant to achieve higher power densities.

Optimization of system performance requires compromises among the key areas. For example, higher operating temperatures are possible if the operating stresses of the structure can be reduced. High power density and thermal efficiency may be achievable with the use of refractory metal alloys, like tungsten, but these benefits must be traded-off against a greater difficulty of fabrication and joining for these alloys which can affect system reliability.

Factors to be considered for materials performance include temperature limits of structural materials, operating temperature windows for breeding materials and coolants, radiation lifetime limits, mechanical property limits. Possible ways of improving material performance include increasing thermal conductivity, improving the mechanical properties, improving the radiation damage resistance, improving joint properties, etc.

2.2.6.2 Key Issues

The issues are:

1. What are the limiting factors to achieving the desired performance goals?

2. Are there ways of improving performance through design modification?

3. Are there ways of improving performance through improved materials both structural and non-structural?
4. Are there ways of improving performance through less restrictive design criteria? Is a code based on existing codes like the ASME B&PV code too restrictive? Where can the code be relaxed? How can design margin be increased?

5. What R&D is required to achieve improved performance?

6. What improvements can be made in the power limits of solid breeder materials?

2.2.6.3 Structural Materials/Design Interface

Development of a viable structural material for the first wall/blanket system is one of the key feasibility issues for the successful deployment of fusion as an energy source. An important issue in the development and selection of structural materials for fusion system applications relates to the materials integration and design constraints. The structural materials must not only provide acceptable thermomechanical performance, be resistant to radiation damage, and be reliably fabricable; but must also be compatible with the coolant and tritium breeding materials, compatible with tritium and the plasma environment, exhibit acceptable neutronic characteristics, and be procured at an acceptable cost. The major difficulty is defining a materials system that will meet all of the requirements simultaneously. A structural material that offers high temperature capability but which cannot be readily fabricated or reliably jointed will not be acceptable. Likewise, a structural material must not only accommodate acceptable surface heat fluxes, but must also be compatible with the breeder and coolant.

Key Points
1. The international community has focussed on three candidate materials – Ferritic SS, vanadium alloys, SiC/SiC
2. Recent analysis predicts that V-Cr-Ti can reach neutron wall loads of 10 MW/m2, 2x the 3Sm limit
3. Ferritic SS/Flibe is not credible – temperature range is too narrow
4. Joining for W, Mo, SiC/SiC has been worked on a lot and is beyond what programs can do
5. Carbon is ruled out by tritium concerns
6. Materials focus – DEMO (Abroad), Reactor (US) gives different answers
7. There is a low level of effort at present on insulator coatings. Additional work is needed. Coatings are also needed for tritium barriers.

2.2.6.4 Power Density Capability for Solid Wall Systems

One of the desired features of first wall and blanket systems is the ability to operate at high power density. The power density of fusion systems is limited by several constraints including the temperature and stress limits of the structural material, breeder, and neutron multiplier; the compatibility limits between the breeder, multiplier, and structural materials (or coatings if required); and the limits on coolant pressure and size of the manifolds. 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. The blanket designs can be divided into three categories.

1. Conventional blanket systems with ferritic steels as typified by the European solid breeder and PbLi designs. The wall loading limits are 2-3 MW/m2 and the power conversion efficiency is 30-35%.
2. Blanket systems cooled by liquid metals and using advanced structural materials as typified by the ARIES-RS V/Li blanket. The wall-loading limit is 4-5 MW/m2, and the power conversion efficiency is 40-45%.
3. Higher power blankets using high temperature refractory alloys, like tungsten with either He or Li vapor coolants. Neutron wall loads of 10 MW/m2 appear possible with power conversion efficiencies of 55-60%.

Key Points

1. Solid wall blankets has the potential to achieve very high thermal efficiencies.

2. With advanced design and high performance structural material, solid wall blankets have the capability of handling very high thermal and neutron wall loadings.

3. Solid wall designs with liquid breeder/coolant require low pumping power.

4. The critical issues of solid wall blanket concepts are the reliability, and the replacement of the first wall.

5. Radiation damage is a critical issue on the material development.

6. Volume of the waste of solid wall blanket designs is a concern.

7. The structure in the solid wall design will have negative impact on tritium breeding. The effect on tritium breeding can be changed by including of Be in the design.

8. Design optimization where both primary and secondary stresses are minimized can increase the allowed power density. In the case of the Li/V system, an improvement of a factor of two may be possible.

2.2.6.5 Lifetime Considerations

Determination of the lifetime of fusion in-vessel components is important for assessing the attractiveness of advanced fusion power systems. There are no fixed lifetimes associated with each specific material. The desired lifetime of materials needs to be coupled with the desired power density, power conversion efficiency (closely tied to operating temperature), and other design objectives such as minimum required tritium breeding ratio. As the performance levels of in-vessel components are pushed, it is expected that the operating lifetime of the materials will be reduced. On the other hand, materials lifetime can potentially be very high if more modest levels of power density and power conversion efficiency are acceptable. A key need is to develop more sophisticated computer lifetime codes that couple material behavior models with component design and with fusion design code rules.

2.2.6.6 Design Criteria

All fusion power systems will be designed in accordance with a fusion design code which establishes the allowable operating conditions (e.g. stress, temperature, cyclic response, off-normal response) for components. In the US, the code most often used for the design of unirradiated structures is the ASME Boiler and Pressure Vessel Code, and it has been used as the basis for setting fusion design criteria. As part of the ITER process, significant progress has been made in establishing a specific fusion design code. The result of this work is the ITER Interim Structural Design Criteria which occupies several volumes. The code provides guidelines for assessing whether in-vessel components can successfully operate under a fusion environment. Since the ITER device was designed to operate at relatively low temperatures, design rules were not developed to address high temperature operation. High temperature design criteria are now being addressed within the US as part of the ongoing design work on advanced first wall blanket systems like the APEX and ARIES. Key data needed to validate high temperature design rules are:

1. Out-of-pile thermal creep deformation and rupture data at high temperatures,
2. In-pile thermal and irradiation-induced creep data,
3. Data on helium embrittlement, particularly with regard to its effect on creep rupture time and tensile and creep ductilities,
4. In addition to materials properties data, some more structures-type tests (e.g., three-point or four-point bending tests) should be conducted to help guide the development of high temperature design rules under neutron environment.

2.2.6.7 Solid Breeder Research

A key feasibility issue for solid breeders is the failure mode and lifetime limiting phenomena, which is largely determined by the thermomechanical interactions between the solid breeder and the surrounding environment. Solid breeders are typically low thermal conducting leading to a neutron wall load capability of only ~2-3 MW/m2. In order to improve the power density capability of solid breeders the following work is desirable.

1. Establishment of a thermal and mechanical property data base.
2. Conduct unit cell experiments for investigating thermomechanical interactions.
3. Participate in international efforts for testing solid breeders in unit cells in fission reactors.
4. Develop methods for enhancing thermal conductivity of Be/Solid Breeder pebbles.
5. Optimize Li-6 enrichment to reduce peak nuclear heating.

Discussion Points

1. Issue of design margin vs. data base, e.g. use minimum properties
2. Design codes were designed for high primary stress and low thermal stress conditions. Fusion needs to develop its own application
3. We are decades away from having a complete material specification and data base
4. Continue to develop design codes and use them to define materials property requirements for materials scientists to use as goals.
5. Improve CFC by replacing "weak" matrix with higher performance matrix, e.g. use higher k fiber plus SiC matrix. Lattice perfection improves (lowers) tritium permeation.
6. Reduction/mitigation of disruption frequency in tokamaks is an area of active investigation. We should not extrapolate current disruption frequencies to future devices. Need to design for only a few disruptions (1-?<5)
7. Develop metrics for blanket operation
8. How do we treat "unforeseen failure" modes and rates?
9. Ask physics to minimize peak to average wall loads.
10. It will be difficult to develop high temperature refractory alloys for fusion applications.

2.3 Availability/Reliability/Maintainability

Key Questions

What should the reliability (MTBF), maintainability (MTTR), and life time goals be to achieve the goal availability? What is the impact of the confinement configuration and the Chamber concept on MTTR and, hence, on MTBF requirements? What can we do today, given the lack of fusion testing data, to assess the prospects for various proposed technology concepts and designs? Given the difficult constraints on the program, how can we show that fusion chamber technologies will be able to meet the reliability, maintainability, and availability goals?

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2.3.1 Introduction

Availability is a primary metric for an attractive power plant, directly affecting the cost of electricity (COE). Beyond simple COE considerations, power plant operators will be averse to the uncertainties associated with unplanned outages. Regulators also may take a dim view of frequent failures in a system containing large inventories of radioactive materials.

Fusion system designs to date have incorporated availability projections in a relatively primitive way, without distinguishing between the unique attributes of various design concepts. In addition, availability is usually a minor consideration in the design of confinement and next-step experiments. Many factors contribute to system availability, including both planned and unplanned outages, failure rates of different power core technologies, component lifetime, and the time to perform regular maintenance and/or to diagnose and recover from failures with various levels of severity.

In an attempt to stimulate community interest in this topic and to gather information, a short questionnaire was distributed two weeks before the Snowmass Summer Study to all participants. The overwhelming majority of respondents thought fusion energy would be practical – most within 50 years. Most of the respondents also considered availability as a critical issue. For MFE concepts, the blanket, first wall and divertor systems clearly were believed to be the components most likely to cause availability problems. For IFE concepts, the laser driver, debris shields, and target chamber were thought to have the greatest availability problems, but there was less consensus. The comparison of concepts indicated that the stellarator was thought to have the best prospects for high availability, but there were strong opinions about other concepts as well. The key points from the survey are: 1) the community is optimistic about the prospects for practical fusion energy, and 2) availability is considered to be a critical problem.

This hot topic was discussed during the Summer Study in four major subtopics which help to characterize the nature of the problem and seek solutions:

1. What are the requirements for reliability, maintainability, and inspectability? What values of failure rate, repair time, and component lifetime are needed to meet reasonable availability goals?
2. How can we quantify fusion component reliability and maintenance time? Without test data, can we project reliability from other technologies?
3. How do other technologies achieve reliability and maintainability?
4. How can the fusion program achieve designs with high availability?

These subtopics are further discussed below.

2.3.2 Subtopic 1

What are the requirements for reliability, maintainability, and inspectability? What values of failure rate, repair time, and component lifetime are needed to meet reasonable availability goals?

Availability is governed by both planned and unplanned outages. The simplest formula that accounts for both planned (f_p) and forced (f_f) outage rates is:

$$A = (1 - f_p)\,(1 - f_f)$$

where the forced outage rate is governed by the mean time between failures (MTBF) and the mean time to repair (MTTR):

$$(1 - f_f) = MTBF/(MTBF+MTTR)$$

Planned outages allow for replacement of life-limited components (such as first wall and divertor elements) and other scheduled maintenance items. With the present U.S. fleet of fission power plants numbering ~100, the addition of one percentage point to the fleet-averaged plant factor is esentially equivalent to adding a new plant of average output.

In order to proceed beyond using the plant factor, p_f, as a free parameter in a 'screening-curve' comparison of Cost of Electricity [COE (mill/kWeh)], a strawman value of p_f typically is selected. Often, this value is the same for a number of fusion concepts, reflecting the absence of such discriminatory information as would allow distinctions between concepts or variants of concepts based upon differences in aggregate failure rates or differences in scheduled maintenance durations.

For example, $p_f = 0.75$ is representative of a scheduled 30-day outage and a 60-day allowance for forced (unscheduled) outages on an annual basis. This 75% availability is considered to be the lower bound for competitive economic performance. Higher values of p_f improve COE as seen in the usual definition:

$$\text{COE (mill/kWeh)} = \frac{C_{AC} + C_{O\&M} + C_{SCR} + C_F}{8760\ P_E\ p_f} + C_{D\&D}$$

where C_{AC} (\$/yr) is the annual capital cost, $C_{O\&M}$ (\$/yr) is the annual operation and maintenance cost, C_{SCR} (\$/yr) is the annual scheduled component replacement cost, and C_F (\$/yr) is the annual fuel (deuterium) cost. The net electrical output is P_E (MWe) = 1,000 (typical), leading to COE (\$/MWeh) = COE (mill/kWeh). A fund to cover decontamination and decommissioning is accumulated through $C_{D\&D}$ (mill/kWeh) = ~1.

Practically, improvements in p_f are obtained at the cost of increases in terms in the numerator of this COE expression (*e.g.,* redundant subsystems which add to the capital cost term or additional maintenance efforts which add to the O&M term).

The fleet average plant capacity factor for US fission reactors has seen steady growth during the past decades, currently exceeding 80% and expected to continue to grow. Lower values of p_f for fusion relative to its perceived competitors may be inevitable, reflecting complexity and other considerations. Preliminary system allocations of MTBF and MTTR values for fusion power plants are consistent with the above global use of p_f~0.75 and begin to highlight possible unavailability drivers for improvement.

2.3.3 Subtopic 2

How can we quantify fusion component reliability and maintenance time? Without test data, can we project reliability from other technologies?

Quantifying reliability and maintainability for a technology still in its early stages of development is difficult, to say the least. Substantial testing is needed to obtain confidence in reliability projections. In the absence of testing in a prototypical environment, estimates of availability have been made by deconstructing conceptual designs into sub-elements (such as welds, pipe bends, pumps, *etc.*) that resemble sub-elements from related technologies [1–4]. Since there is no database for fusion, the fission reactor database is most often used.

Often the results of these studies are disappointing; however, the accuracy of this method is limited due to the lack of design detail, the applicability of the database, and a plethora of other uncertainties (see Table 1). Nevertheless, the process may help identify problem areas in the design and suggest areas for further design improvement.

In the past 5-10 years, several advances have been made that are expected to improve availability (such as improved divertor heat sink designs, advanced configuration and remote maintenance techniques, liquid blanket concepts, and advanced fabrication techniques), although quantitative analysis of the improvements have not generally been made.

This sub-topic included discussion of the various methodologies for estimating failure rates and maintenance times and the uncertainties in those methodologies. Application of these methods to various confinement concepts and power core technologies were recommended to highlight the potential differences between design concepts.

Table 1
Uncertainties in projecting availability

- Inadequate test program for reliability growth
- Design and technology improvements can not be discounted
- Trade-offs between performance limits and failure rates must be included
- Proper incorporation of neutron damage in design rules
- Opportunities for preventative maintenance are unknown

- Lack of resources for RAM and logistics support
- Insufficient level of detail in conceptual designs
- No operating experience
- Inadequate level of effort on remote maintenance

Discussion

Data extracted from other technologies have many limitations restricting their validity under fusion-specific applications. For example, weld data often is obtained under different or undocumented conditions relating to design margins, operating environment (thermal, chemical, stress), and materials (*e.g.,* refractory alloys are very difficult to join). Great care must be used in applying such data.

Nevertheless, experience from related industries is one of the few sources of useful data. Often examples of poor operating experience are used to highlight the difficulty of obtaining high availability. Examples of success are at least as important as examples of failure in establishing strategies for design improvement and availability growth. For example, the EBR-II sodium-cooled fission test reactor had very few failures over time, and may be a better model than a light water reactor. This database may be more relevant for some of the fusion blanket designs under consideration; efforts to obtain the data are underway.

Since fusion power core component designs are still evolving, we have an opportunity to identify the most critical areas for improvement. This requires a concentrated effort including an assessment of the current status, examination of the availability of reactor elements system-by-system and determination of the biggest payoffs for improved reliability and maintenance.

As part of this learning process, the current status should be reviewed in the context of a continuing R&D program that started many years ago. In other words, past design improvements and their impact on availability should be assessed as a basis for moving forward. For example, advanced PFC technology developed during the ITER EDA has resulted in much more robust components that could credibly survive for the required lifetime in a power plant. Other examples include advanced maintenance schemes, liquid blankets (including liquid walls), advanced fabrication, inspection and testing (*e.g.,* reliability centered maintenance).

Finally, it was well recognized that both physics (especially confinement concept and plasma configuration) and technology choices determine the reliability and maintainability characteristics of a power plant. In order to provide both visibility of the problem and clear metrics for evaluating progress, more effort was recommended to develop a matrix of issues for different concepts to uncover both the weak and strong points of the various concepts.

2.3.4 Subtopic 3

How do other technologies achieve reliability and maintainability?

Reliability and maintainability (R&M) programs are well established in several existing industries. Automotive, chemical processing, fission power, military equipment, and aerospace industries all share these common features:

- Reliability is a goal or requirement in the design
- The design incorporates reliability and maintainability
- Designs are reviewed and equipment is tested during construction/assembly
- Quality assurance and estimation/prediction of reliability are performed
- Inservice inspection and maintenance are performed
- Field performance is monitored
- Performance is analyzed for feedback to operation and design

Early fission plants typically began life with low availability, typically under 33%. As operating time grew, the availability was seen to have step increases of double initial values. In the 1970's – 1980's, power plant availability was in the 70% range, but small increases in the 1990's have resulted in power plants that average in the 80% availability range. These increases came about from increased safety monitoring and performance, applying new techniques such as enhanced maintenance procedures, better personnel training, reliability centered maintenance, and other innovations to keep the plants operating efficiently. [5]

Reliability and maintainability can be expensive in capital cost and in annual operating costs. For example, the traditional means of preventing multiple equipment failures from one common cause is to use separate, redundant components or to use two or more diverse types of components that are not susceptible to the same failure causes. Adding equipment items increases purchase and installation costs, and adds more equipment items that require maintenance and perhaps spare parts inventories. Following the R&M steps can be costly; for example, annual maintenance costs at chemical plants can be up to 10% of plant investment cost. Collecting and analyzing data also will be costly; most industries only collect data if regulated to do so for safety reasons (*e.g.*, fission industry), or if there is a clear economic advantage to do so (*e.g.*, automotive industry). Therefore, the fusion community must decide how relevant each of the reliability program steps listed above are to fusion design processes. Early design studies, such as the International Tokamak Reactor (INTOR) [6] in the early 1980's, discussed the importance of setting an R&M goal and allocating reliability during the design process to meet operations and safety requirements.

The main observations from this session were:

1. There was general agreement with the summary of practices that incorporate R&M into the products of other industries. No additions were suggested.

2. Fusion devices do not always incorporate a formal R&M requirement in the design process. While this omission is acceptable for scientific experiments, a reliability goal should be included in the design process as fusion moves forward on the path toward commercialization.

3. Fusion device designs should incorporate R&M goals. The audience agreed that some treatment of reliability, either qualitative or quantitative, was an important part of the design process for existing and near-term experiments as well as future reactors. Based on the subtopic 1 discussion, the contributors to this topic chose a 75% availability goal. Availability requires that both the failure rate and repair time be managed.

4. Fusion does not have a formal process for collecting or analyzing operating experience feedback, just informal communication between machine operators at existing experiments. Increased communication about operating experiences was suggested as a positive step to address R&M issues. A suggestion was made that fusion could post weekly operating reports from existing experiments on the internet, similar to what is already done with graphics files of good plasma operation shots. This posting would make operating experience available to the design community.

2.3.5 Subtopic 4

How can the fusion program achieve designs with high availability?

There is concern that the availability of a fusion reactor may be poor, due to the complexity of present design concepts, the difficulty of repair, and the harsh operating environment of the internal components. These are only perceptions, however, and cannot be based on experience, since there are no operating fusion reactors. The first step for the fusion program to achieve reliable and maintainable systems is to include these attributes as top level requirements for both power plants and next step devices, on a par with initial capital cost. This was the case during many of the early fusion reactor design studies, and continued with the ITER design [7–10].

Once reliability and maintainability are recognized as a top level goals, the design of the power plant probably will be steered in obvious directions, including more modular designs with standardized components that operate with significant margin and, where possible, redundancy. The designs will be tolerant of a few minor failures before maintenance is required. The maintenance procedures themselves will be simplified to take advantage of the modular approach. Finally, the power plant concepts may have to be judged based on features that are inherently more reliable or maintainable. A steady state concept may be selected over a pulsed concept, or a concept that does not disrupt may be favored over one that must deal with plasma disruptions. Table 2 reiterates a few design features that may improve reliability and maintainability. [11–12]

Table 2
Design features to improve availability

Reliability	• Keep designs as simple as possible (*e.g.,* minimize number of parts)
	• Provide maximum margin (for transients, unforeseen modes)
	• Use proven designs where possible, extensive testing is preferred
	• Use modular designs, pre-tested, minimizing series assembly operations
	• Provide redundancy where possible, continue operation until next scheduled repair
Maintainability	• Minimize remote handling around machine (provide sufficient shielding for hands-on access to high maintenance items, perform complex operations in hot cell, not in-situ)
	• Configure high failure rate items for quick maintenance (sufficient access)
	• Optimize all remote handling operations (testing, mockups, etc)

Discussion

The various fusion concepts are often compared qualitatively as more or less “reliable”, “maintainable”, or “complex” as a means of ranking their relative attractiveness as power reactors. It would appear very useful, then, to perform more rigorous assessments of the availability of these concepts to obtain a quantitative comparison. However, it is very difficult, at this stage of the fusion program, to make accurate quantitative assessments. The uncertainty in the analysis could easily be much greater than the differences among the reactor concepts.

A more useful approach may be to assess generic classes of concepts and look for specific areas in the design or maintenance approach that could be changed to improve the overall availability. Four classes of concepts are suggested, including:

1. Superconducting, toroidal MFE concepts – Designs with non-demountable coils surrounding the plasma and vacuum thermal insulation (*e.g.*, ARIES-RS, ITER)

2. Non-superconducting toroidal MFE concepts – Designs with demountable coils around the plasma and no vacuum thermal insulation (ARIES-ST)

3. Linear MFE concepts – Designs with linear geometries (*e.g.,* FRC)

4. IFE concepts

The assessment of these generic concepts would involve examining the failure modes and maintenance approach for each of the primary systems, followed by a relative ranking of the problem areas within each of the concepts. It may be possible to identify "go, no-go" issues that must be resolved. This information could help guide the efforts of both the concept designers and the technology developers. For example, configuration changes could be made to simplify maintenance, or R&D activities could be initiated to solve specific reliability issues. It may be useful to summarize the findings in a simple table that can be updated as issues are addressed and concept improvements are made.

In addition to examining reactor concepts, assessments should be made of the next step devices. The penalties for low availability on an experimental device include delays in gathering needed data, loss of continuity in the program, the direct cost of failure recovery, and the indirect cost of idle time on an expensive facility.

The assessments of next step devices could use information from the generic studies, but should be much more detailed and design specific. We should start by gathering data from existing fusion experiments and technology programs. For example, the DIII-D program keeps data on each of their unplanned outages. This information, as well as information from other devices, should be assembled into a reliability database specific to fusion experiments. This could be done readily by asking the various programs to forward their existing data and all future data as it is collected. The assessment should proceed in the traditional way, using failure mode and effects analysis (FMEA), maintenance time estimates, and the assembled reliability data.

One of the outcomes of the availability assessment of the next step devices would be a balance between capital cost and operating cost. Traditional designs tend to minimize initial investment at the expense of maintainability. For example, the toroidal field coil configuration and number for a tokamak are often chosen to minimize coil size and weight, and thus initial cost. The result could be poor access for maintenance of internal components, which of necessity must be performed through ports between the TF coil outer legs.

Another example of a tradeoff that may be made is between demountable coils and monolithic coils. The demountable coil may greatly improve maintenance of other components, but could result in a much less reliable coil design. A demountable, single turn coil, as in the ST designs, may be both more maintainable and more reliable.

In addition to assessing design issues with reactors and near term experiments, some limited R&D could be useful. Many industries, including the power industry, are making use of reliability-centered maintenance (RCM) to continuously monitor process performance. predict when maintenance is needed, and greatly reduce forced outages. As part of an availability improvement initiative, it would be useful to demonstrate RCM on a limited number of components on one or more existing experiments.

Since speed of maintenance is half of the availability equation, the program should also try to stay connected with remote maintenance technology, both inside and outside fusion. This can be done as part of ongoing work, including international collaborations, in such areas as remote metrology, advanced motion control, and remote welding and cutting. The program should identify and develop a few common purpose remote maintenance tools and techniques and demonstrate these tools and techniques, in limited ways, on existing machines.

Finally, the fusion program cannot be passive in its quest for high availability. There must be a deliberate effort to identify availability issues and raise these issues to a high level of importance.

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2.4 Testing Conditions and Facilities

Key Questions

What environmental and testing conditions are most important in experiments needed to resolve the key Chamber issues? When are neutron sources and fusion technology testing facilities needed? What issues differentiate among the various options for testing facilities?

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2.4.0 Introduction

Two sessions were held at Snowmass to discuss what facilities are needed to address fusion nuclear technology issues. These sessions were co-chaired by S. Zinkle and A. Ying, with M. Billone serving as the recording secretary. Presentations were given by E.E. Bloom (IFMIF), L.J. Perkins (laser neutron source), B. Hooper (GDT), E. Cheng (LEDA and VNS-ST), and A. Ying (non-irradiation facilities). 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. 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 can 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.

2.4.1 Technical Issues and Types of Testing

Fusion nuclear technology feasibility and attractiveness issues have been identified and characterized in several studies (e.g. [1-5]). Feasibility issues are those whose negative resolution will [5] close the design window, have unacceptable safety risk, or have unacceptable reliability/availability. These technology issues are analogous to the Concept Exploration and Proof of Principle Stages in plasma physics R&D studies. Attractiveness issues are analogous to the Proof of Performance Stage in plasma physics R&D

studies, and their negative resolution would result in [5] reduced system performance/component lifetime, increased cost, and/or less desirable safety or environmental implications. The reactor components affected by the nuclear environment include blanket/first wall components (generally considered the critical path for fusion nuclear technology development), plasma interactive and high heat flux components (divertor, limiter, rf antennas, launchers, waveguides), shield components, tritium processing systems, heat transport and power conversion systems, and diagnostic and control systems. The environmental attractiveness of fusion is largely determined by the induced activation and lifetime of material components. A summary of the critical issues of fusion nuclear technology is given in Table 1. In many cases, these critical issues are linked to a particular design concept (e.g., MHD insulators for liquid metal coolant/breeder).

The testing needs for fusion nuclear technology have also been addressed in previous studies (e.g. references 1-5). In general, the R&D on candidate materials or chamber technology components may be envisioned to proceed in a series of steps. The level of integration provides a rough measure of test complexity and an approximate indication of the chronological order. First, screening experiments are performed in the laboratory to establish the baseline feasibility (Concept Exploration). If satisfactory results are obtained, more advanced small-scale experiments are performed in order to identify promising concepts that perform well in a "single effect" environment (in irradiation and/or non-irradiation test facilities). These experiments along with focussed multiple-variable tests provide the basis for establishing the Proof of Principle for a given component of technology design concept. Finally, experiments with a prototypical test section in an environment that combines the appropriate nuclear, chemical, thermomechanical and magnetic field effects is necessary to establish the Performance Extension metrics. An important consideration is that integrated and component (Performance Extension) tests typically can be performed only in fusion devices. On the other hand, Concept Exploration and Proof of Principle experiments are relatively low in cost (particularly in non-neutron test stands) and they are important and useful in reducing the large costs and risks associated with future Proof of Performance tests.

Table 1. Summary of Critical R&D Issues for Fusion Nuclear Technology (extended from ref. [5])

1. Tritium issues	
➢ D-T fuel cycle neutronics	Proof of Principle
➢ T_2 inventory and recovery in solid breeder under fusion conditions	Proof of Principle
➢ T_2 inventory and recovery in liquid breeder under fusion conditions	Proof of Principle
➢ Tritium permeation and inventory in the structure	Proof of Principle
2. First wall and blanket material science	
➢ Materials compatibility	Concept Exploration
➢ Development of self-healing MHD insulators for liquid metal blankets, including thermal/mechanical/electrical/nuclear loading	Concept Exploration
➢ Fabrication/joining	Concept Exploration
➢ Operating limits of first wall and blanket components	Concept Exploration / Proof of Principle
➢ Thermomechanical loadings and response of blanket components under normal and off-normal operation	Proof of Principle
➢ Lifetime of first wall and blanket components	Performance Extension
3. Divertor HHF component thermomechanical response & lifetime	
➢ Liquid Divertor	Concept Exploration
➢ Solid Divertor	Concept Exploration / Proof of Principle
4. RAM issues	
➢ Identification & characterization of failure modes, effects, and rates	Proof of Principle
➢ Remote maintenance with acceptable machine shutdown time	Performance Extension
5. Radiation Shielding: accuracy of prediction and quantification of radiation protection requirements	Performance Extension
6. MFE Liquid Walls	
➢ Plasma Liquid Interactions	Concept Exploration
➢ Free Surface Temperature and Choice of Liquid	Concept Exploration
➢ Hydrodynamics Feasibility Assessment (penetrations, etc.)	Concept Exploration
➢ Large scale thermal-hydrodynamics and engineering POP tests	Proof of Principle
➢ Materials for Thick Liquid Walls	Proof of Principle
7. IFE Liquid Walls Liquid Chamber Clearing (including vaporized materials and splashed liquid droplets)	
➢ Partial-pocket experiments (0.25 scale)	Concept Exploration
➢ Vapor condensation experiments	Concept Exploration
➢ Complete pocket experiments (0.42 scale)	Proof of Principle
8. IFE Solid Walls Chamber cavity clearing	Concept Exploration
9. Final focus design and survivability for laser-driven IFE	Concept Exploration
10. Safety (aerosol release, Flibe safety, etc.)	Concept Exploration
11. Prediction accuracy of nuclear parameters: tritium production, radioactivity build up, transmutation	Proof of Principle

2.4.2 Technology test facilities

Non-plasma neutron facilities play an important role in Concept Exploration and Proof of Principle stages of technology testing because of availability and low cost. These facilities can provide essential screening of proposed components and help reduce the risks and costs of the more complex, integrated tests in the fusion environment. However, Proof of Performance generally can not be verified in non-fusion facilities, except for liquid-wall facilities where major issues center on fluid mechanics rather than radiation effects. Technology test facilities can be classified into: a) non-irradiation test facilities, b) fission reactors, c) accelerator-based neutron sources, and d) fusion test facilities. Each of these is discussed briefly below. Two important considerations for neutron sources are a) capability for accelerated testing compared to a DEMO reactor, and b) sufficient irradiation volume with fusion-relevant neutron spectrum for testing of materials and/or components.

2.4.2.1 Non-irradiation Test Facilities

Non-irradiation test facilities can provide Concept Exploration and Proof of Principle test results on basic property data, single-effect experiments, and some of the multiple-effect/multiple interaction tests for which the neutron field is not important. Examples include scoping studies on MHD electrical insulators (including chemical compatibility, thermal cycling and self-healing studies), high heat flux testing of innovative materials/design concepts, investigation of thermohydraulic issues, plasma-material interaction studies (including liquid walls), safety studies involving aerosol formation, etc. Existing small-scale facilities can and should continue to play a role in addressing a broad spectrum of technology Concept Exploration issues.

Several large-scale non-irradiation test facilities are proposed for proof-of-principle experiments during the next decade. The objectives are to address multiple effect phenomena using scaled models, after the resolution of key feasibility issues. In particular, synergism between MFE and IFE liquid walls will allow experiments to be conducted at the same large-scale integrated thermofluid facility. Such a facility for Flibe liquid walls would require the following capabilities (for example):

- Liquid volumetric flow rate: 10 m^3/s
- Supply pressure: up to 2.5 atm
- Storage and vacuum receiver vessel volumes: 300 m^3 (transient flow option)

Of course, this facility would be built only if positive results are obtained in earlier concept exploration tests.

2.4.2.2 Fission Reactors

Fission reactors provide neutrons in a moderate volume and are thus suitable for many Proof of Principle experiments. Table 2 summarizes the capabilities of the two high-flux fission reactors available in the USA for blanket tests. Several additional reactors exist in Europe, Russia and Japan. Testing in fission reactors suffers from several limitations, the most important of which are lack of fusion-relevant neutron spectra and surface heat flux/magnetic field effects. Worldwide, there is no operating fission reactor that can provide a test volume with □ 15 cm equivalent circular diameter at a fast neutron flux equivalent to 1 MW/m^2 wall loading (> 1 x 10^{15} $n \bullet cm^{-2} \bullet s^{-1}$ E>0.1 MeV fission neutron spectrum). This size limitation makes it difficult to evaluate the synergistic effects present in multiple-variable tests (e.g., magnetic field and mechanical forces). The well-known difference between the fission and DT fusion reactor neutron spectra leads to difficulties in simulating processes such as helium and tritium production. Despite these limitations, fission reactor testing is suitable for some multiple effect tests that are needed to establish the Proof of Principle for certain designs. Examples include tests of a unit cell of a solid breeder blanket to investigate tritium release behavior, and coolant-structure compatibility in the presence of mechanical stress and radiation.

Table 2. Capabilities of Operating US Fission Reactors Available for Blanket Tests

Reactor	Loca-tion	Reactor Power (MW)	Fast Flux, E>0.1 MeV (n/cm^2s)	Irradiation Capsule Diameter (cm)	Nuclear heating (W/g)	Core Height (cm)	Comments
HFIR-RB " -target	ORNL	85 "	$4.4x10^{14}$ $1.1x10^{15}$	4.0 1.2	12 46	50 "	In-situ test cap.
ATR-ITV " –I holes	Idaho Falls	250 "	$4.5x10^{14}$ $3.0x10^{12}$	2.5 12.5	~20 1.5	122	In-situ test cap.

2.4.2.3 Accelerator-Based Neutron Sources

Accelerator based neutron sources produce neutrons in a volume that is smaller than typical fission reactors. Deuterium-Tritium accelerator sources produce 14 MeV neutrons, but are limited by target fabrication and cooling considerations to neutron fluxes that are orders of magnitude lower than that in a fusion reactor with 1 MW/m^2 wall load. However, the effects of neutrons/generated gammas can be detected at reasonable depth (~1.5 m) inside test assemblies. The neutron yield among existing DT neutron facilities is ~0.5-30 x 10^{12} n/s (Table 3), and can be tailored for specific neutronic tests (e.g., line source at FNS). These facilities can be used for verifying the prediction capability of present codes and databases for tritium production in solid breeder (or LM) to confirm tritium self-sufficiency issue and to generate safety factors for the design purposes. Additionally, irradiation of various samples for low-activation tests and updating/verification of our current dosimetry and activation/decay heat databases can be performed at these facilities along with studies on neutron and gamma ray shielding. Part of the D-T fuel cycle pertaining to tritium production in solid breeders (or LM) can be tested in these facilities and viewed as “Proof of Principle” tests. However, “Concept Exploration” to verify tritium self-sufficiency can not be undertaken in these facilities since it requires a full sector of the FW/blanket system to be placed in a plasma-based neutron source facility of ITER or VNS type with a closed cycle that includes plasma vacuum exhaust and tritium processing/fueling subsystems.

A higher-flux high energy neutron source that has been in operation for over 10 years is the Los Alamos Spallation Radiation Effects Facility, which utilizes spallation neutrons created from the interaction of 750 MeV protons (~1 mA beam current) with a high-Z target. The irradiation parameters for LASREF are summarized in Table 3. This facility is well-suited for targeted studies on several key materials issues (e.g., the effect of He generation on low-temperature embrittlement of structural alloys), but it is currently limited to cumulative damage levels below ~10 displacements per atom. Recent calculations indicate that the neutron flux could be increased by more than a factor of two by utilizing a W target in the facility (estimated capital cost for upgrade is ~15 M$, cf. Table 3).

A high-current (~100 mA) accelerator designed to accelerate protons to 8-20 MeV has recently been constructed at Los Alamos as part of the Accelerator Production of Tritium project. This facility (LEDA) could potentially be used for fusion materials irradiation studies. The capabilities of LEDA are summarized in Table 3. Installation of a Li target assembly and test chamber and an upgrade in beam energy to 20-25 MeV would be required prior to operation (estimated capital cost ~50 M$).

Recently, an international design activity under the auspices of the International Energy Agency (IEA) was completed [6-8] for a D-Li source called IFMIF (International Fusion Material Irradiation Facility). In this concept, neutrons are produced by bombarding a flowing lithium target with energetic (~35-40 MeV) deuterons. The irradiation parameters for the IFMIF design are summarized in Table 3. Accelerated testing of an adequate number of internationally-accepted miniaturized mechanical property specimens for evaluation of DEMO structural materials, etc. can be performed in this facility [6-8]. Since this facility is largely based on technology already in-hand, its cost and availability can be estimated with high reliability compared to plasma-based high-intensity neutron sources. There are several technical issues regarding the suitability of a D-Li source for fusion nuclear technology studies, including: 1) neutron spectrum, 2) steep flux gradient in the high-flux regions, and 3) the surface area and volume available for testing. Although there are uncertainties in the nuclear data above 14 MeV, the international consensus is that radiation effects observed with D-Li neutron spectra can be adequately correlated to those in a fusion reactor [9]. It is important to note that the medium-flux regions of IFMIF are designed for in-situ testing of blanket components (e.g., ceramic breeders), and the low-flux regions are intended for in-situ tests of insulators, optical components, magnet components, vacuum vessel materials, etc. The recent reduced-cost IFMIF design (with minimal hot cell equipment and no provision to expand beyond the 2 accelerator/250 mA base design) has an estimated capital cost of ~500 M$, including ~100 M$ in contingency costs).

2.4.2.4 Laser Point Neutron Source

A laser-based high-intensity point neutron source has recently been proposed by LLNL researchers [10]. Due to recent rapid development in the field of short-pulse, high-intensity lasers, intensities of 10^{18} W/cm^2 are currently available from table-top lasers and systems capable of ~10^{21} W/cm^2 are now starting to come on line. In the present target concept, the laser energy interacts with a central DT gas cluster and the escaping fast ions are absorbed in a surrounding DT “collar” to produce 14 MeV beam-target neutrons [11]. Preliminary predictions from a 1-D code for a full-scale D-T irradiation facility driven by a 100J, 100Hz (i.e., 10kW average power) diode-pumped solid state laser indicate that the steady-state, uncollided 14MeV neutron flux at a close-coupled material specimen is ~10^{15}cm^{-2}s^{-1} at a laser intensity of ~10^{17}W/cm^2. For comparison, the time-averaged neutron flux at the first wall of a typical fusion reactor is ~1-4x10^{15}cm^{-2}s^{-1}. The resulting small source volumes offer the potential for a high flux of 14MeV neutrons at close-coupled, micro (□1 mm) test specimens of fusion-relevant, single materials. This facility offers the potential for relatively low-cost construction (<~100 M$) and operation, but the high-flux volume is restricted to ~0.5 cm^3 with steep flux gradients. This facility may be useful for fundamental radiation effects studies on microstructure stability, tensile properties, etc. that are needed for Concept Exploration analyses.

Future planned work includes computational modeling and experimental investigations to predict neutron fluxes and fluences ultimately obtainable in a production level facility. The potential modest size/cost of such a facility (<$100M) and low overall power input (~10kW) means that high neutron fluxes are obtainable only over small specimen volumes. A complementary materials science and computational modeling program is therefore necessary to validate damage models and provide a multiscale predictive capability for the extrapolated behavior of engineering-scale components.

Table 3. WORKING DRAFT Comparison of the Irradiation Parameters for Several High Energy Neutron Sources

Facility	Location	**Power (MW_e)**	**Total neutron flux (n/cm^2s)**	**Irradiation Volume**	**Nuclear heating (W/g)**	**Capital/ Ann. Operating Cost**	Lead Time for Design/ R&D(Yrs)	**Concept Maturity**	**Comments**
D-T neutron RTNS-1 FNS FNG SNEG-13	 US JAERI Italy Russia		$1x10^{12}$ n/s $\sim 5x10^{12}$ n/s $\sim 0.5x10^{12}$ n/s $\sim 3x10^{13}$ n/s	~0.5 cm^3; low flux regions are available 1 m x 1m		5(?)/1(?) M$ 10/1 M$ 8(?)/1(?) M$ 3(?)/0.3(?) M$	NA	Operating	**Existing facilities** (~4 00 keV D) (~260 keV D) (100-300 keV D)
LASREF	**LANL**	~1?	$5.0x10^{13}$ $>1.0x10^{14}$	$12x25x50cm^3$ (~15 liters)	~0.4	15/20 M$	NA	Operating	**Existing facility**
LEDA (p-Li)	**LANL**	4	$4x10^{14}$ $4x10^{12}$	0.5 cm^3 0.5 liters	4 0.04	50/20 (?) M$	NA	Existing Technology	**Existing accelerator (8 MeV)**
IFMIF (40 MeV d-Li)	---	50	$>4.0x10^{14}$ (>20 dpa/FPY) 1-20 dpa/FPY 0.1-1 dpa/FPY <0.1 dpa/FPY	0.5 liters 6 liters ~10 liters >>10 liters	3.8 ~0.5-2	0.8 B/60 M$ (incl. 0.17B$ contingency) **reduced- 0.5 B/50 M $** (incl. 0.1B$ contingency)	3-5	Existing Technology	>70% design availability Lower-fluence regions (<20 dpa/FPY) intended for in-situ studies on blanket components, magnets, etc.
Laser (micro-fusion)	---	0.01	10^{14}-10^{15}	~0.5 cm^3		~100/<2(?) M$	?	Will it work?	High-fluence fundamental studies on miniature spec.
GDT	---	<50	1-4 MW/m^2	~100 liters (1 m^2x0.1m)	~1	0.25-0.5 B$	~5	5M $ investment for proof of extrapolation	<150 g T_2/year
VNS-ST	---	170-400	1-2 MW/m^2 (1-5 MW/m^2)	~1600 liters (16 m^2x0.1m)	~2	1.5 B/180 M $	?	Prototype = NSTX	Availability is part of the development

2.4.2.5 Plasma-based Facilities

Several different plasma-based neutron sources have been proposed for the testing of fusion technology components, including the gas dynamic trap and several variants of a "volumetric neutron source". Further developments in continuous-wave plasma heating components (neutral beams and/RF) are needed before these plasma-based sources are constructed. Although the likely construction time frame for these sources (~2010-2015) is outside the R&D time period specifically covered by Snowmass, the proposed roles for a plasma-based facility must be included for overall planning purposes for the coming ten years.

The Gas Dynamic Trap Neutron Source (GDTNS) is a volumetric plasma (14 MeV) source with a neutron spectrum and intensity very close to that predicted for ITER and fusion reactor designs. The neutron production is by a nonMaxwellian ion tail in a dense, nearly Maxwellian plasma. Energetic ions are produced by neutral beam injected at 30-45° to the magnetic field in an axisymmetric mirror machine, with most neutrons produced at the sloshing-ion turning points near the mirrors. MHD stability is produced by the out-flow of dense plasma through one of the mirrors into a magnetic cusp, with the flow momentum in the good-curvature part of the field dominating the plasma instability drive in the bad curvature part of the confined volume. Calculations and extrapolation of experiments indicate that 1-2 MW/m^2 of uncollided 14 MeV neutrons can be produced in a 100 liter volume with small tritium consumption (< 150 g/year at 100% availability) and intrinsically steady-state operation. Positive ion-based neutral beams (< 65 keV, 60 MW) can be used at mirror fields of 13 T (mirror ratio 10); upgrades to 20 T and 250 keV negative-ion based neutral beams could produce as much as 4 MW/m^2 of 14 MeV neutrons.

An experiment operating at the Budker Inst. Nuclear Physics, Novosibirsk, Russia, has demonstrated much of the GDTNS physics with a pulse-length of 3-5 ms: Stability against MHD modes, stability against ion velocity-space modes so that the ions decay and scatter classically, and T_e =130 eV (equal to classical predictions for the available neutral beam power - 3.5 MW at 15 keV). Experiments at higher power (costing ~5 M$) are required to verify the extrapolation of the electron temperature to the neutron source regime. The proposed neutron source can test materials, reactor components, and many of the blanket systems and subsystems needed for a reactor. An important point is that the neutrons are produced in two test zones (~1-2 m length), with low neutron flux in the remainder of the structure. Construction is relatively straightforward, tritium usage is low, and the estimated cost reasonable (~$500M) for a large 14 MeV neutron source.

A high volume plasma-based, DT-fueled, volumetric neutron source (VNS) has been considered as a valuable facility to support the development of a demonstration fusion power reactor (DEMO). It can be used to test and develop necessary fusion blanket and divertor components and provide sufficient database, particularly on the reliability of nuclear components necessary for DEMO. Two different concepts have been investigated for the VNS, based on advanced tokamak [5,12] and spherical torus [13,14] concepts. The VNS requirements are stated [5,12] as steady state plasma with Q~ 1, neutron wall load of ~ 1-2 MW/m^2, a total test area of 10 m^2, and "cumulative" neutron fluence of ~ 6 $MW.y/m^2$. Device availability testing and improvements is part of the VNS mission.

In a recent study [13], a minimum cost VNS-ST was identified that could satisfy the minimum nuclear technology development requirements identified by an IEA study. STVNS-I has a major radius of 0.8 m, aspect ratio of 1.33, and an on-axis magnetic field strength of 1.8 tesla. It would be operated with a single-turn (in the central post) normal copper magnet to produce 38 MW fusion power when driven and heated by 21 MW of neutral particles at 500 keV energy. The neutron wall loading at the 10 m^2 test section would reach 1 MW/m^2. Recent work [15] has focussed on the optimization and technological feasibility of the VNS-ST at the higher neutron wall loads (0.5 - 5 MW/m^2) anticipated in future power reactors. To satisfy these goals, a slightly larger device (STVNS-II) was proposed with a major radius of 1.07 m and an aspect ratio of 1.4. The design included physics and mechanical design analysis, materials selection, neutronics and activation analysis, availability assessment, and cost analysis [15]. During the initial operation, the expected availability will be very low due to lack of knowledge and experience in operating such a device. Feasible core component concepts (e.g., ferritic steel or vanadium alloy structures with appropriate breeders and coolants) can be tested and selected for higher neutron wall loading operation after about 0.5 $MW\text{-}y/m^2$ fluence. Tritium breeding in the shielding blanket region may not be needed at this stage because the needed tritium consumption is only 640 g/y (30% availability) and the total tritium consumption is <2.1 kg. Partial tritium breeding would be available in the power reactor blanket being developed in the test section.

During the higher wall loading stages, fusion power blankets could be fabricated as blanket segments rather than modules and tested in the entire outboard region. Tritium breeding will be available during these stages. Since the power blanket segment can be employed in the entire outboard region, an overall tritium breeding can be assessed, along with mean-time-to-failure data. Under this scenario, the staged high wall loading operation of an VNS-ST can provide an accumulated neutron fluence of more than 30 $MW\text{-}y/m^2$ in a power reactor relevant environment within about 20 years. If successful, the VNS-ST can provide a reactor relevant environment for the power core components to demonstrate all functions required for a power plant or DEMO through the start-up, transition, and steady-state operation at an estimated capital cost of ~1.5 B$.

2.4.3 Suggested Roles for the Various Technology Test Facilities

The environmental and economic attractiveness of fusion must be assessed using a range of experimental facilities, including existing and new facilities. Fission reactors and accelerator- or laser-based neutron sources can provide meaningful data needed for Concept Evaluation and Proof of Principle studies on fusion technology issues (for both structural and non-structural materials). However, it is generally agreed that a plasma-based source will be needed for the majority of the required Proof of Performance testing of fusion components. An optimized fusion technology testing strategy (roadmap) should include a mixture of non-irradiation, fission reactor, accelerator and/or laser-based neutron sources, and a plasma-based test facility. Key features for the irradiation facilities include available volume and the capability for performing accelerated testing (i.e., high neutron flux). Further discussion of the interactive roles of the various proposed fusion technology facilities is given in the following.

The minimum volume requirements for a systematic evaluation of the key irradiated mechanical properties of seven different structural materials (e.g., ferritic steel, V alloys, SiC/SiC composites, etc.) has been determined to be 0.5 liters (using realistic allowances for specimen packaging, coolant, etc.) [7,16]. Within this volume, the effects of irradiation on properties such as fracture toughness, fatigue, crack growth rate, irradiation creep, and tensile properties could be obtained for 5 different damage levels between 20 and 150 dpa (DEMO-relevant lifetime dose) and 6 irradiation temperatures within a time frame of 20 years, assuming a facility availability of 70% and an equivalent first wall neutron loading of ≥ 2 MW/m^2. Larger volumes (>5 liters) are typically needed for testing of miniaturized blanket components.

The key experimental conditions for fusion nuclear component studies are: 1) neutron effects (radiation damage, tritium and helium production), 2) bulk heating (nuclear heating in a significant volume), 3) non-nuclear conditions (e.g. magnetic field, surface heat flux, particle flux, mechanical forces), 4) conditions for simulating thermal-mechanical-chemical-electrical interactions, and 5) conditions for integrated tests and synergistic effects. Non-plasma facilities are well-suited for single-variable tests and some multiple effect/multiple interaction experiments (Concept Exploration and Proof of Principle tests). In particular, fission reactors and large-scale accelerator-based irradiation facilities provide a good simulation of the fusion bulk heating environment (cf. Tables 2,3). In a few specific cases (e.g., in-situ measurement of tritium production and release in ceramic breeder mockup assemblies), accelerator-based irradiation facilities can investigate multiple-effects that can establish Proof of Performance for a given component. On the other hand, non-plasma facilities are not suitable for investigating the synergistic effects that occur in many other fusion components.

Table 4 shows the contribution of non-irradiation facilities, fission reactors and accelerator-based facilities as well as plasma-based neutron facilities to resolving the fusion nuclear technology critical issues. The following descriptive terms are used in Table 4 to classify the relative contribution of each facility toward resolving a given critical issue, in ascending order of their impact: none, limited, moderate, substantial. “Limited” denotes information that may provide some useful (but fragmented) underlying data, whereas “substantial” implies that the majority of the key phenomena are addressed (although not completely resolved). Valuable information needed to assess the Concept Exploration and Proof of Principle issues can be obtained using non-plasma-neutron facilities. However, there is no critical issue that can be fully resolved by testing in non-plasma-neutron facilities alone. On the basis of cost considerations, non-plasma-neutron facilities can and should be used to narrow materials and design concept options. This approach clearly reduces the costs and risks of the more costly and complex tests in any high-intensity plasma-based facility.

The key question that remains to be answered is what is the appropriate time frame for introduction of a high-intensity plasma-based test facility. Further development of an integrated fusion technology roadmap is needed before this question can be answered. Table 5 shows a possible flow chart for fusion chamber technology facilities. Existing and small-scale facilities could be utilized to address many of the technology Concept Exploration issues. Conversely, new non-irradiation and irradiation facilities would be needed to address many of the technology Proof of Principle and Performance Extension issues.

Table 4. WORKING DRAFT Contribution of Non-plasma Facilities to Resolving Critical Issues for Fusion Nuclear Technology Component Performance Demonstration (revised from Ref. [5])

Critical Issue	Non-neutron Test Stands	Fission Reactors	Laser-based Neutron Source	Accelerator Based Neutron Sources			Plasma-based neutron source	
				D-T	LASREF	IFMIF,LEDA	GDT	VNS
1. Tritium issues								
➢ **D-T fuel cycle neutronics**	None	Moderate	Moderate	Moderate	Moderate	Moderate	Substantial	Substantial
➢ **Tritium inventory** & recovery in solid breeder under actual operating conditions	Moderate	Substantial	Limited	Limited	Moderate	Moderate	Substantial	Substantial
➢ **Tritium inventory** & recovery in the liquid breeder under actual operating conditions	Moderate	Substantial	Limited	Limited	Moderate	Moderate	Substantial	Substantial
➢ **Tritium permeation** and inventory in the structure	Moderate	Moderate	Limited	Limited	Moderate	Moderate	Substantial	Substantial
2. **1**st wall & blanket materials science								
➢ Materials **compatibility**	Moderate	Moderate	Limited	Limited	Moderate	Substantial	Substantial	Substantial
➢ Development of self-healing MHD insulators, incl. thermal/ mechanical/ electrical/nuclear	Moderate	Limited	Limited	Limited	Limited	Limited	Substantial	Substantial
➢ Fabrication/joining	Moderate	Moderate	None	None	Moderate	Substantial	Substantial	Substantial
➢ Operating limits of first wall and blanket components	Moderate	Moderate	None	None	Moderate	Moderate	Substantial	Substantial
➢ **Thermomechanical** loadings and response of blanket components under normal and off-normal operation	Moderate	Limited	None	None	Limited	Limited	Substantial	Substantial
➢ Lifetime of first wall and blanket components	None	Moderate	Limited	None	Limited	Substantial	Moderate	Substantial
3. Divertor HHF component thermo-mechanical response and lifetime								
➢ Solid divertors	Moderate	Limited	None	None	Limited	Limited	Substantial	Substantial
➢ Liquid divertors	Moderate	Limited	None	None	Limited	Limited	Moderate	Substantial

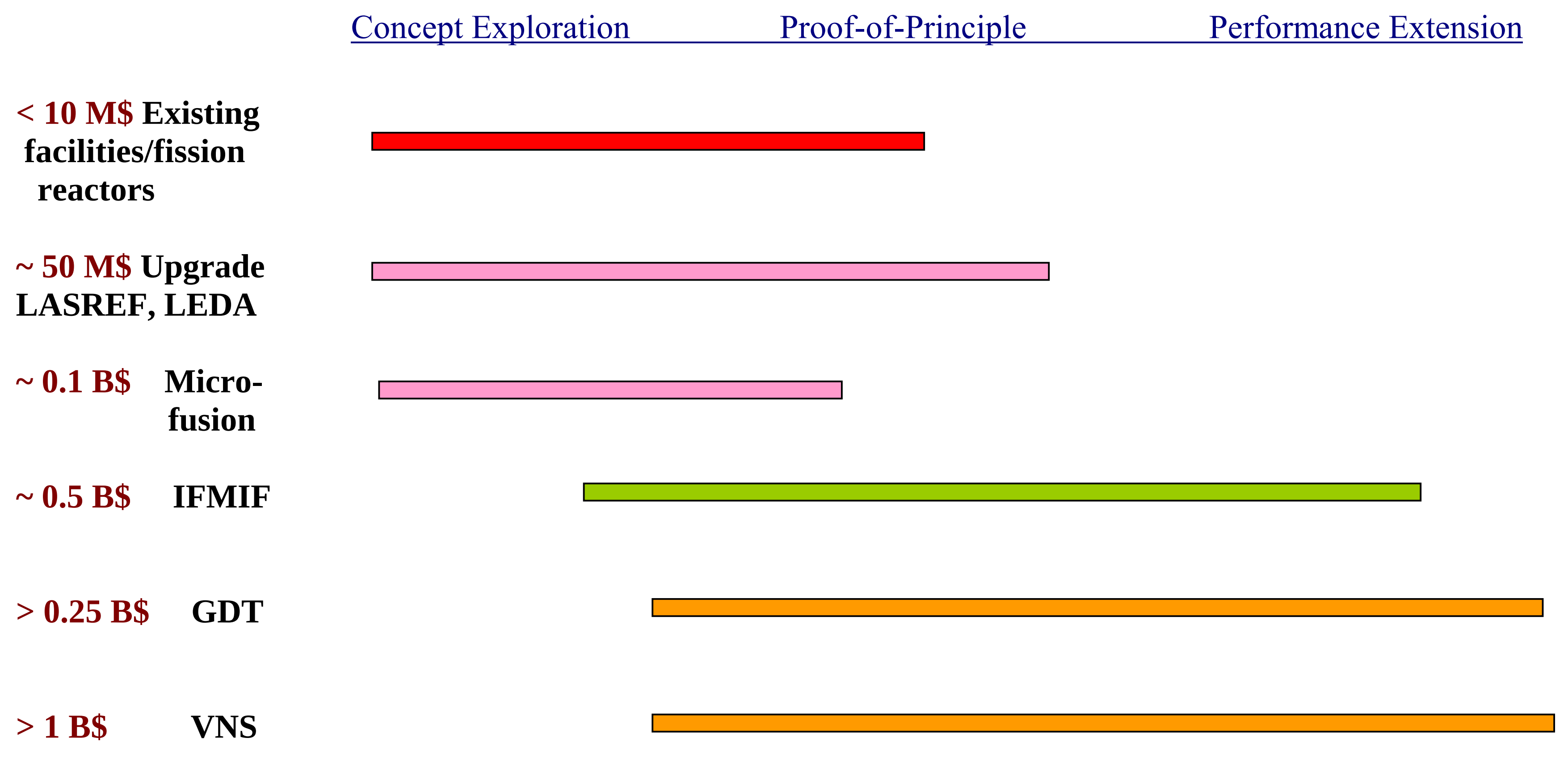
4. RAM issues								
➢ Identification and characterization of failure modes, effects, and rates	Limited	Limited	Limited	None	Moderate	Moderate	Moderate	Substantial
➢ Remote maintenance with acceptable machine shutdown time	Moderate	Limited	None	None	Limited	Limited	Moderate	Substantial
5. Radiation Shielding: accuracy of prediction and quantification of radiation protection requirements	None	Moderate	Substantial	Substantial	Substantial	Substantial	Substantial	Substantial
6. MFE Liquid Walls								
➢ Plasma Liquid Interactions	None	None	None	None	None	None	Moderate	Substantial
➢ Temperature Control: Free Surface Temperature and Choice of Liquid	Substantial	None	None	None	None	None	Moderate	Substantial
➢ Hydrodynamics Feasibility	Substantial	None	None	None	None	Limited	Moderate	Substantial
➢ Materials for Thick Liquid Walls	Moderate	Substantial	Limited	None	Substantial	Substantial	Substantial	Substantial
7. IFE Liquid Walls Liquid Chamber Clearing (including vaporized materials and splashed liquid droplets)		None	None	None	None	None	None	None
➢ Partial-pocket expts (0.25 scale)	Moderate							
➢ Vapor condensation experiments	Moderate							
➢ Complete pocket expts (0.42 scale)	Substantial							
8. IFE Solid Walls: Cavity Clearing	Moderate	None	None	None	None	None	None	None
9. Final focus design and survivability for laser-driven IFE	Moderate	None	None	None	None	None	None	None
10. Safety (aerosol release, Flibe safety, etc.)	Substantial	Limited	Limited	Limited	Limited	Limited	Substantial	Substantial
11. Prediction accuracy of nuclear parameters: T_2 production, radio-activity build up, transmutation	None	Limited	Limited	Moderate	Moderate	Moderate	Substantial	Substantial

Table 5. The Environmental & Economic Attractiveness of Fusion Must Be Assessed Using a Range of Experimental Facilities

Facility Opportunities for Chamber Technology Issues

- **Develop Non-Irradiation Test Facilities (e.g., Thermal-fluid facility for IFE/MFE Liquid Walls) < 10 M$**
- **Assess Neutron Source Options and Recommend Strategy and Priorities**

Neutron Source Options and Capabilities



2.4.3.1. Possible Roles for Neutron Irradiation Facilities Within the Next 10 Years

Existing 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 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. From the cost and performance capabilities of the proposed irradiation facilities summarized in Tables 3 and 4, the following conclusions may be drawn:

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 and PoP issues, along with limited Performance Extension issues, cf. Table 4) 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.

The existing US fusion energy development strategy assumes the deployment of a “point neutron source” (IFMIF-class) for materials development and a “volumetric neutron source” for component development (with undefined deployment dates). Due to a lack of time at Snowmass, discussions were not held on the value of the existing vs. alternative strategies for deployment of neutron sources. Two possible neutron source strategies are listed below for the purpose of stimulating post-Snowmass comments. It should be noted that the experimental studies in both strawman strategies should be supplemented by computational modeling in order to maximize the benefit to the fusion program.

Strategy A (highly constrained budget): Continue utilization of existing fission reactors and deploy either LASREF-upgrade, LEDA-upgrade, or the laser point neutron source to provide complementary information. In addition, perform the low-cost physics tests needed to establish the plasma physics viability of plasma-based sources. This development path necessarily restricts most of the fusion technology R&D to the Concept Exploration stage for the foreseeable future.

Strategy B (stable or slightly increased fusion technology budgets): Assuming the availability of a fusion chamber technology budget of ~20-25 M$/yr for 5 years to construct IFMIF (US share for international facility) and a similar annual operating budget to perform experiments in IFMIF and other fusion chamber technology facilities (irradiation and non-irradiation), then significant R&D on chamber technology Concept Exploration and PoP issues could be initiated (cf. Table 4). The necessary low-cost physics tests to assess plasma-based sources could be performed in parallel.

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2.5 Radioactive Waste Minimization

Key Questions

What is the better strategy for fusion waste minimization: hazard versus volume? What are the implications of each strategy for fusion potential and R&D? What is the potential for recycling.

Topic Leaders

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2.5.1 Background

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 active material. A tokamak power plant is large, and there is a potential to generate a correspondingly large volume of activated material. The adoption of low activation materials, while important to reduce the radiotoxicity of the most active components, should be done as part of a strategy that also minimizes the volume of waste material that might be categorized as radioactive, even if low level. Waste management strategies have typically concentrated on minimizing the activity of first wall and blanket components where the level of specific activity (Bq/kg) is highest [1].

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. (It should be noted that the criteria for clearance are more restrictive than those assumed for recycling.) 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.

In this hot topic, we propose to address the following questions:

- What is the volume of radioactive waste in a fusion plant and how does it compare to other technologies?
- What criteria should be considered to allow recycling of fusion materials? Contact dose for handling both hands on and remote operation? Very low activation for unlimited reuse or clearance?
- What would be the impact of changing the current constraints on fusion ex-vessel components from insulator dose on the magnet and re-weldability of the vacuum vessel to constraints that limit the ex-vessel activation to very low values? Very low activation might allow these large components to meet recycling/reuse criteria and thus would not have to be counted as waste. Would this new constraint require increasing the radial build of the machine significantly or would other materials need to be considered? If other material need to be considered, can we meet such new constraints with low activation materials or do we have to consider higher activation materials for shielding?
- Are current designs at an optimum in regard to the overall tradeoff between waste hazard and waste volume? How would a fusion tokamak reactor design be different if we changed the optimization and focused more on volume and a little less on hazard?
- Do the new high power density designs in APEX offer the potential to reduce the volume of waste?
- What is the impact on the overall COE of this new strategy? Is there a simple scaling relation between radial build and cost of energy that could be compared with current estimates of waste disposal cost so that we could examine the overall tradeoff?
- What would such a shift in design optimization mean for fusion technology development, the low activation materials program and our new advanced power extraction initiative?

To help answer these questions, we review scoping studies with neutronics and activation models to examine these issues, and identify the trends which allow improved in-vessel shielding to result in reduced ex-vessel activation. The performance of typical fusion power plant designs such as ARIES-RS with respect to recycling and clearance criteria are also assessed, to show the potential for improvement in waste volume reduction by careful selection of materials combinations. We also examine some of the very preliminary designs being considered in the APEX project. The implications of the results on the development path for fusion power are discussed.

2.5.2 WASTE HAZARD AND VOLUME

It is constructive to compare the hazard and volume of fusion waste to waste from fission and coal based on radioactive inventories in fusion, fission and coal (e.g., radon, uranium, thorium, and polonium).[i] There are many ways to try and categorize the hazard of fusion wastes. We show one comparison here.

Two metrics for the hazard comparison are the inhalation and ingestion dose of the radioactive inventories of the waste. Figure 1 compares these metrics for a fusion plant with conventional materials, a low-activation fusion plant, a PWR and coal plant after 25 years of operation at 1 gigawatt electric. The results suggest that the hazard as measured by inhalation and ingestion dose are two to three orders of magnitude lower for fusion than fission after 10 years of decay and four to five orders of magnitude lower after 100 years of decay. In fact, the waste hazard of fusion wastes is roughly the same order of magnitude as coal ash, which is not considered by the public to be a serious hazard.

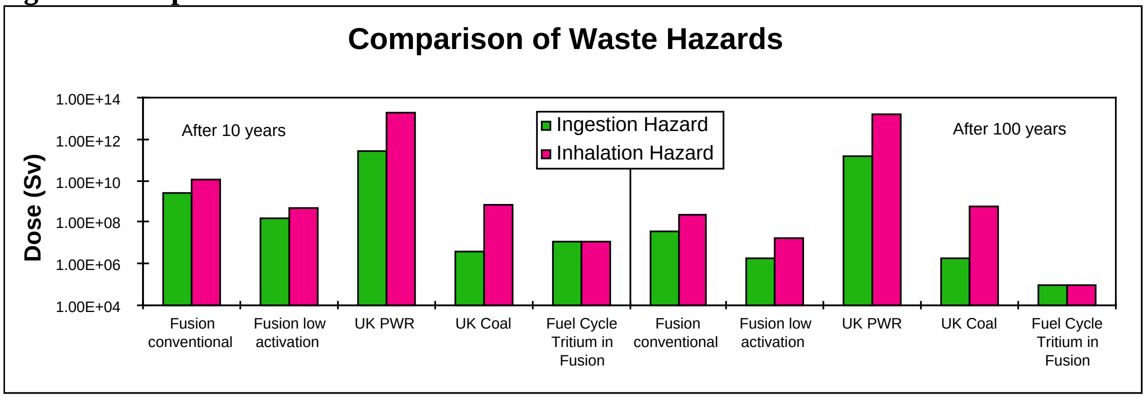
In reference i, there is also an estimate of the solid waste mass and volume for the fusion designs and compared them to an UK PWR (very similar to US PWRs). The results are shown in Figure 1. These results indicate that for a 5.3 meter machine, fusion wastes are a factor of 2 to 3 greater in volume than fission wastes but are 320 times less volume than coal wastes. (The coal wastes include ash, gypsum and sludge. It does not include 1.57E+08 of gaseous waste (CO_2, SO_2, and NO_x).

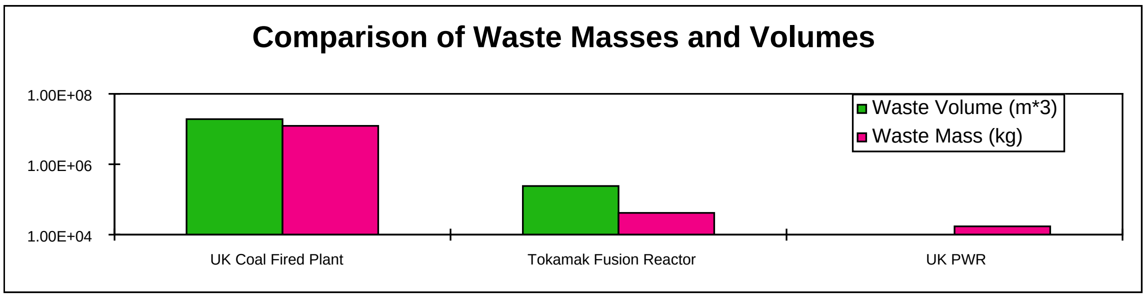
In Table 1, we show the major waste streams for ARIES-RS. All materials from the first wall to the vacuum vessel classify for shallow land burial and represent about 9800 tons of material. Most of the waste mass in ARIES-RS is associated with the shield (~ 55% of the total). The replaceable components (the first wall and blanket) represent 31% of the total and the remaining 14% is the vacuum vessel.

Table 1. Waste Streams in ARIES-RS

Replacement Frequency (FPY)	Component/Mass (tons)		Waste Classification	Number of Replacements in 40 FPYs	Total Waste Mass (tons) and % of Class C Waste	
2.5	FW and BKT1	59	Class C	16	938	10%
7.5	BKT2, Reflector and Replaceable Shield	420	Class C	5	2100	21%
40	Shield	5402	Class C	1	5400	55%
40	VV	1357	Class C	1	1357	14%
40	Magnets, structure and cryostat	4874	cleared	1	4874	---
	TOTAL	12112			14671	

Figure 1 Comparison of Waste Hazard and Volume





Another comparison is shown in Table 2, where we compare the waste volume due to ARIES-RS with that from an LWR. The results show that the total mass of waste is about seven times more for ARIES-RS than for an LWR of the same power output. The volume of waste from ARIES-RS is about a factor of 10 greater than from an LWR core. (The comparison here for the LWR does not include non-fuel waste, which would increase the LWR volume. Also, the ARIES-RS value is uncompacted waste volume). (These results would change somewhat if recycling were considered for both fission and fusion wastes. See section on recycling.) There was disagreement at Snowmass about whether the factor of 10 was a fair comparison. A review of other work after Snowmass indicated a value on the order of 2 between fission and fusion, similar to the factor of 2 to 3 found in the UK study. Nevertheless, it is clear by these comparisons that fusion waste masses and volumes are greater than a fission reactor of the same thermal power.

Table 2. Comparison of Waste from ARIES-RS and fission

Plant	Waste Classification	Reactor Components in Plant		Total Waste upon Decommissioning		Waste due to component replacement			
		Mass (tons)	Volume (m*3)	Mass (tons)	Volume (m*3)	Mass (tons)	% of total	Vol. (m*3)	% of total
ARIES-RS	Class C	7240	1030	9800	1400	2560	26	366	26
LWR	Deep Geological Disposal	100	10	1320	132	1220	92	122	92

2.5.3 CLASS C WASTE AND RECYLING

The U.S. 10CFR61 regulations define the following disposal categories for the radioactive waste: geologic disposal, near-surface disposal (Class A, B and C, shallow-land burial low-level waste), and below regulatory concern. In terms of gamma-equivalent dose rate, the waste classifications are determined as shown in Figure 2:

Clearance or Below regulatory concern – less than 10 mrem/y, or 1 microrem/h,
Near-surface waste – above 1 microrem/h and less than 60 mrem/h,
Deep geologic waste – above 60 mrem/h.

Due to the high neutron flux in the fusion reactor first wall and blanket components, some of the fusion materials and impurities will be activated and produce long-lived radionuclides. These radionuclides have half-lives longer than 500 y (e.g., the dominating radionuclides are Nb94 (half-life 20,000 y), Ag108m (418 y), Ir192m (241 y), and Ho166m (1,200 y)).   The near-surface disposal has an intruder barrier designed to be monitored for 500 y, generally dealing with shorter-lived fission radionuclides with half-lives generally lower than 100 y (e.g., the dominating radionuclides are Ni63 (100 y), Sr90 (29 y), and Cs137 (30 y)). The practicality of near-surface burial disposal appears to be questionable when large quantities of fusion waste are to be managed in the next century.

Recycle and reuse of fusion materials is an attractive option to reduce the waste quantity. Remote-handling recycling (RHR) is necessary to handle the modestly activated materials. The current dose rate limit (which needs to be reexamined based on current remote handling technology) is assumed to be about 1 rem/h.

The present fusion waste management strategy in the US is to minimize the waste hazard below the level of near-surface burial. As shown in Table 2, the waste volume generated in a fusion power plant such as ARIES-RS (1 GWe) is about 1,400 m*3 during the 40 full power year lifetime. This waste volume is primarily from the in-vessel and vacuum vessel components. About 25% of it is due to the replacement components. After 50 years of cooling, this discharged waste can be managed as near-surface burial waste based on the present regulations. There is additional 600 m*3 of ex-vessel components and magnets to be decommissioned from the power plant. However, after 50 years of cooling, the activation level of these components drops below the regulatory concern and these components can be cleared from the regulatory control.

The recycling option will allow the in-vessel components and vacuum vessel to be reused after re-fabrication. To first order, recycling of in-vessel components can be performed until the activation exceeds the assumed remote-handling limit of 1 rem/h. This corresponds to 16 recycles before disposal for ARIES-RS. The volume upon decommissioning in this case is only 88 m*3 which is 20 times less waste than in the case of no recycling.

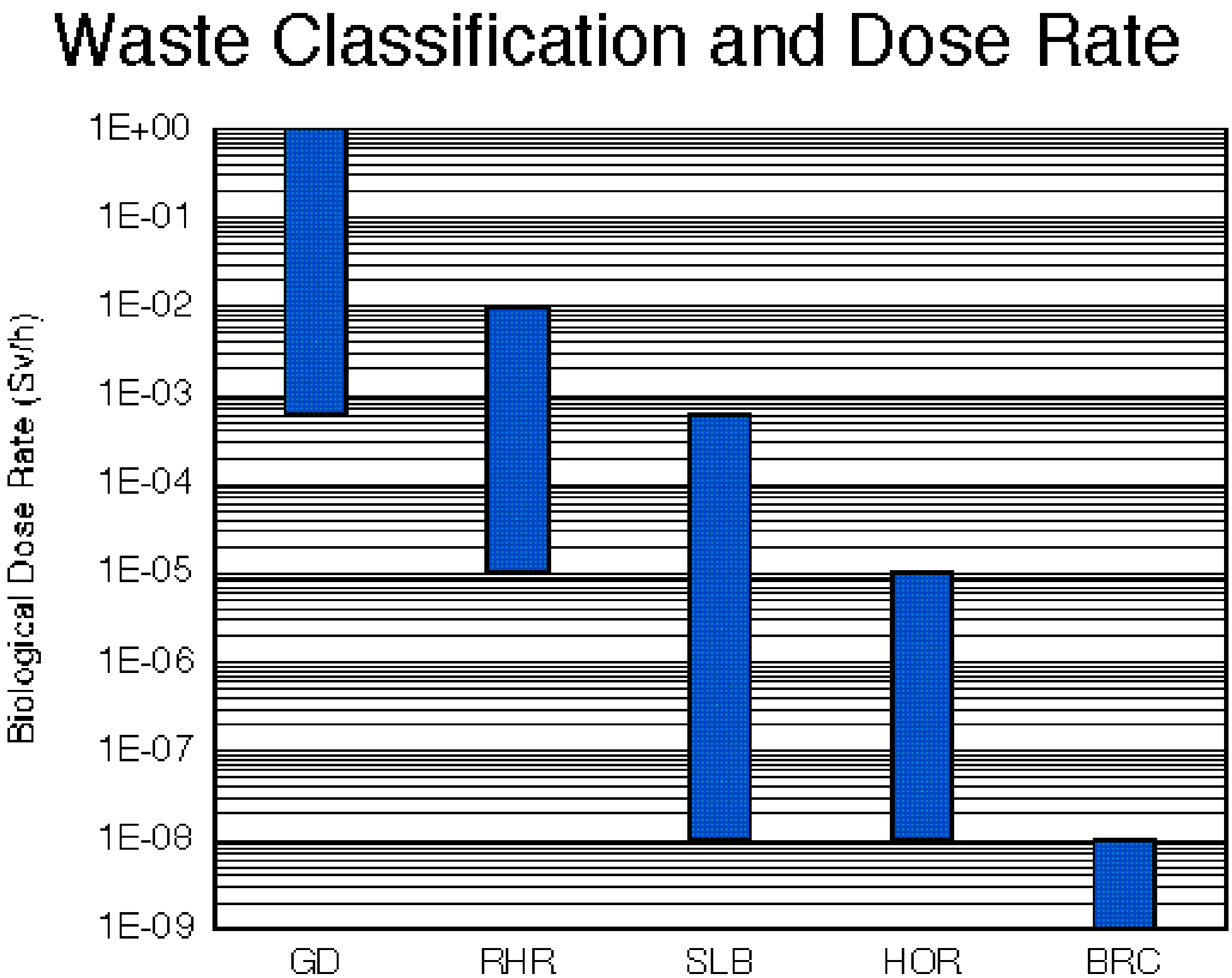


Figure 2. (GD - Geological Disposal, RHR - Remote Handling Recycling, SLB - Shallow Land Burial, HOR - Hands-on Recycling, BRC - Below Regulatory Concern)

Recycling will reduce the overall waste volume but will also increase the hazard of the recycled material because of buildup of certain impurities via reuse in a fusion machine (e.g., Nb-94). The 88 m*3 of waste following recycling would have to currently be disposed of via deep geological burial (e.g., Yucca Mountain). Thus, a strategy to dispose of this material once recycling is no longer feasible has to be established.

2.5.4 EX-VESSEL ACTIVATION AND CLEARANCE

Scoping study: Influence of blanket concept on ex-vessel activation. A scoping study has been performed to examine a broad range of blanket design concepts in Europe and the US with fixed plasma and ex-vessel components and determine their ability to minimize ex-vessel activation. [3] Most of these blanket options are based on the tokamak power plant designs studied in the European Safety and Environmental Assessments of Fusion Power (SEAFP), in particular the three models adopted in the second phase of that study. These were augmented by a lithium metal/vanadium concept based on work in the US and a silicon carbide variant of one of the SEAFP blankets.

The five different design options are:

1. Lithium oxide ceramic breeder/vanadium alloy structure/helium coolant
2. Liquid LiPb breeder/low activation martensitic (LAM) steel structure/water coolant
3. Lithium silicate ceramic breeder/ LAM steel structure/helium coolant
4. Lithium silicate ceramic breeder/silicon carbide composite structure/helium coolant
5. Self-cooled lithium breeder/vanadium alloy structure

In all cases except the lithium/V design, water-cooled austenitic steel (containing a full set of elements and impurities) is used for the shield and vacuum vessel. For the lithium/V design, because of the safety concern related to lithium/water interaction, the shield and vacuum vessel are helium-cooled austenitic steel. Beyond the vacuum vessel is the superconducting magnet winding pack with the associated insulation enclosed in its austenitic steel coil case.

Although such design options may not be fully optimized, the use of constant ex-vessel components allows us to understand the degree of ex-vessel activation as a function of the low activation materials combinations in the FW/blanket and further helps us understand the underlying trends. (In fact, in an optimized design, different shields might be used to compensate for the shielding effectiveness of different blankets.) To compare the ex-vessel activation of the different options, we use a clearance index based on IAEA recommendations [4] regarding levels of activation below which a material is no longer classified as radioactive waste.[1] (Note that the US effective clearance level termed "below regulatory concern" discussed earlier is about a factor of 10 more stringent than the IAEA value used here. The US value corresponds to a value about one-tenth the radiation dose from natural background).

Figure 2 plots the clearance index in each component after 50 years of decay. The activation response of the different plant models in Figure 1 shows that the use of different low activation material/coolant combinations in the blanket produces very different levels of activation in the shield, vacuum vessel and magnets. It is clear that optimizing the breeder, structure and coolant materials within the first wall and blanket to reduce the activation response in these regions, results in a higher activation in the shield, vacuum vessel and magnets. The converse is also true, as seen by comparing the behavior of ceramic/SiC/He with that of LiPb/LAM/water.

Further, the level of induced activation of the vacuum vessel as given by the clearance index varies by a factor of 500 among the different design options. The combination of water, LiPb and low activation martensitic steel in the blanket provides superior shielding of all the blankets considered from an ex-vessel neutron activation standpoint. The shielding offered by the Pb and the moderation of the water results in the lowest clearance index (and although not shown, the lowest specific activity and dose rate) in the shield and vacuum vessel of all the designs considered.

Vacuum vessel/ex-vessel activation is primarily a function of the choice of low activation structural material in first wall and blanket and the neutron moderating effectiveness of blanket coolant fluid, as well as the choice of materials for the ex-vessel components themselves. These factors directly influence the overall ex-vessel activation and the ability to clear ex-vessel components and furthermore indicate that a single material/design for the shield is not optimal for all blankets. The differences in the neutron spectra produced by different material combinations must be considered when neutronically optimizing a shield to reduce the overall activation of the bulky ex-vessel components in the plant. The neutron flux should be moderated as close to the plasma as possible. While the outboard magnets can be cleared in all cases, only in the LiPb/LAM/water design does the vacuum vessel meet the IAEA clearance limits. (It should be noted that more could be cleared if the 316 stainless steel were replaced by a reduced-activation steel (e.g. OPSTAB or low activation ferritic) for all ex-blanket components, and the time were extended by a few decades.)

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[1] Clearance index, defined in the same manner as the US waste disposal rating, is the ratio of the specific activity of a nuclide divided by the clearance limit summed over all isotopes

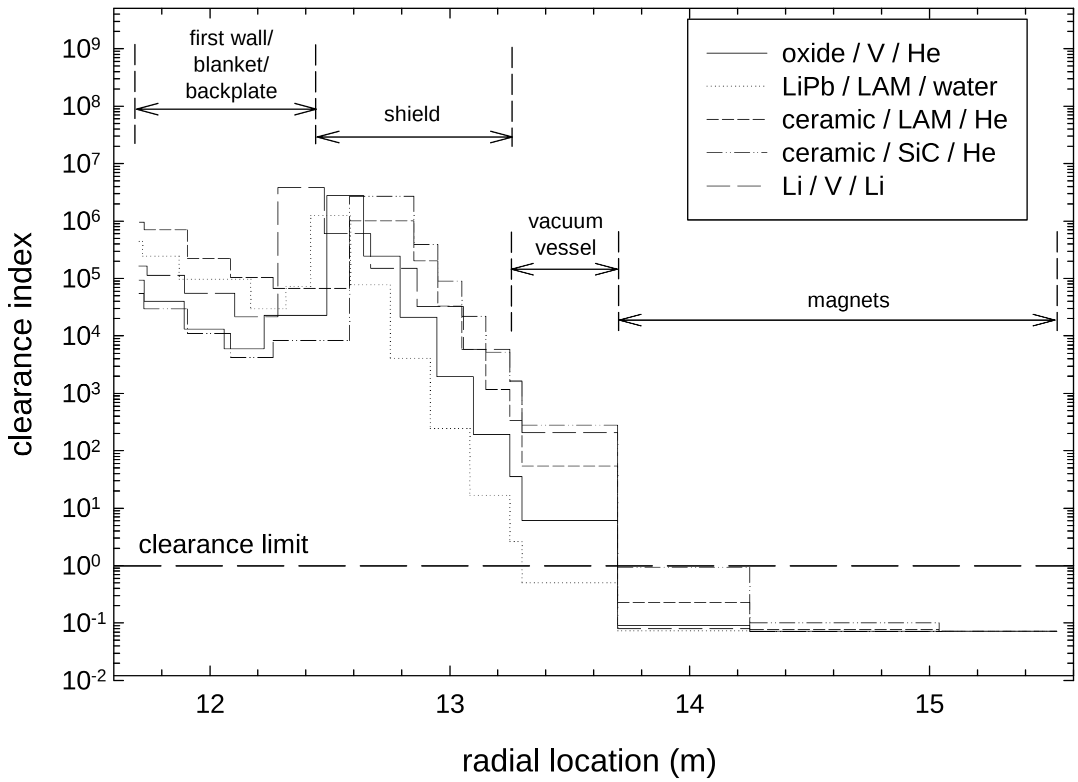

Figure 2 Clearance Index for Different Design Options

ARIES RS Design. The scoping study was useful to understand the trends offered by different low activation material combinations. It is however useful to examine the more optimized ARIES-RS design to determine the degree to which ex-vessel activation can be reduced by more careful design of the shield. The ARIES-RS design has a Li/V blanket and specially optimized shields for both the inboard and outboard portions of the machine. Both inboard and outboard regions contain a vanadium alloy/tenelon high-temperature shield. The inboard low temperature shield is WC and Tenelon. (Tenelon is a low activation austenitic steel). The outboard low temperature shield is a mixture of borated Tenelon and Tenelon.

In Table 3, we compare the clearance index for the vacuum vessel for the Li/V design in the scoping study, the ARIES-RS design, and a modified ARIES-RS design in which the vacuum vessel is changed to a low activation ferritic steel (LAFS). (It should be noted that changing to LAFS may result in not meeting the magnet insulator dose limit. Thus, the design would have to be optimized. The change in materials here is just to illustrate the effect on clearance.) Changing the VV material from 316 SS to Tenelon, a reduced activation austenitic steel reduced the clearance index (and hence ex-vessel activation) of the vacuum vessel significantly (from a clearance index of 1600 to 10), largely because of the reduction in the Co concentration of the two steels. Going from Tenelon to LAFS reduces the clearance level even more on the outboard side. The differences in clearance are associated with the differences in the assumed level of Ir impurities for Tenelon and LAFS. Significant effort has gone into establishing the impurities for LAFS whereas the Tenelon impurities are based on BCSS results and thus may be overestimated based on current steel manufacturing techniques. The magnets in ARIES-RS, which are ~ 1350 tonnes, are low enough in activation to be cleared using IAEA rules and not considered waste.

Table 3. Effect of shielding and VV material on the IAEA Clearance Index for different Li/V options.

	Li/V design option from scoping study with 316 SS VV	ARIES-RS with Tenelon VV	ARIES-RS with low activation ferritic steel VV
Inboard VV	---	5.7	5.6
Outboard VV	1600	10	1.5

2.5.5 HIGH POWER DENSITY/HIGH WALL LOAD DESIGNS

As part of examining this overall question on reduced ex-vessel activation, it is useful to also consider what role the new high power density/high wall load designs being considered in the Advanced Power Extraction (APEX) study may have on reducing ex-vessel activation. The higher wall loads accessible by liquid walls, and the use of refractory alloys (e.g. tungsten) with superior shielding capabilities relative to steel could lessen the ex-vessel activation. We examine these two issues below.

Influence of Higher Wall Load. For a given power output, the higher wall load afforded by a liquid first wall and blanket can reduce the volume of waste from the first wall and blanket because of the reduction in size of the FW/blanket, the lower structural content of liquid wall designs, and the potential for increased lifetime of a liquid blanket. Based on ARIES-RS, the FW/blanket usually represents about ~ 20-25% of the total waste volume from the plant after 40 full power years of operation (based on the 2.5 year lifetime of the FW and 7.5 year lifetime of the blanket). The degree to which liquid walls can reduce the volume of FW/blanket waste depends on the amount of structural material in the FW/blanket system, the wall loading, the thickness of the liquid, and the lifetime of the structure in the FW/blanket. To first order,,

$$V \propto A\,\delta\,\chi\,N,$$

where V is the total waste volume, A is the surface area of the first wall, δ is the thickness, χ is the structure fraction in the FW/blanket and N is the number of planned replacements of the structure for the 40 years of plant operation. The surface area is related to the power, P, and wall load, Γ, via the equation $A = P/\Gamma$. Therfore, a higher wall load would allow for a reduced FW surface area, A, which would translate into a smaller volume of the FW/blanket for a given power. However, because of radiation damage to structure in the FW/blanket, such components are not lifetime and must be replaced periodically. The number of replacements is given by $N = 40/(L/\Gamma)$ where L is the lifetime assumed to be ~ 15 $MWyr/m^2$ (~

150 dpa). In this case, higher wall load leads to more frequent replacement because of the greater rate of fluence accumulation. This would increase the volume. As the thickness of the liquid blanket increases and the structural content of the blanket, x, decreases, there is less activated metal and thus some reduction in waste volume is possible. The exact value of volume reduction depends on design details. Combining all of these factors yields the following scaling relationship for the volume of waste for replaceable components like the FW and blanket:

$$V \propto \chi/L$$

This relationship is true when comparing two conventional blankets and it says that for such a blanket, wall load does not matter since the benefit of reduced surface area is offset by the increased replacement frequency. Thus, all that matters is the structural content of the blanket. Thus, as shown in the Table 4, for the case of a complete thick liquid blanket with no structure in the first wall/blanket, the waste volume reduction was considered by some to be infinite. Furthermore, if structures such as nozzles, flow baffles or guides are needed to maintain the flow pattern, then the reduction will be much less. For example if a mere 1% structure is needed, its limited lifetime will require replacement and then the reduction in FW/blanket waste volume is a factor of 10 relative to a conventional design such as ARIES-RS. Others pointed out that in the case of a thick liquid Flibe blanket (~ 50 cm thick) that the scaling relationship above is not exactly correct. If one can develop a design in which all of the structure is behind the liquid then there is additional gain in waste volume reduction. For the case of 4% structure behind the liquid, the thick liquid reduces the radiation damage so that this structure is lifetime. Thus, the waste volume reduction relative to ARIES-RS is about a factor of 70. Some felt this comparison was not logical since material behind the blanket could just as well be considered the shield and thus was not a fair comparison.

There was still more discussion at the meeting about these calculations. Some argued at Snowmass for higher values based on more pessimistic estimates of lifetimes of conventional solid walls. Still others noted that if solid first walls and blankets can be recycled and reused then that material is no longer waste and thus the waste volume from the different concepts would be very similar. (It is important to note that we have not examined the activation and resulting waste streams from the breeding material in this comparison. For the liquids, we would have to examine the waste stream from Li, Flibe, LiPb, or LiSn and for solids it is important to examine the waste streams from the solid breeder (e.g., lithium zirconate, silicate, titanate or aluminate) and the Be neutron multiplier.)

In addition to a reduction in the FW/blanket waste volume, higher power density designs result in a more compact machine which reduces the volume of the shield, vacuum vessel and magnet components. Figure 3 plots the volume of these components as a function of wall load. Comparison of the volume of these components at the 10 MW/m^2 value with the volume at the ARIES-RS value of 5 MW/m^2 shows a reduction in overall volume of these components by about 50%. This value assumes that all of these components are permanent lifetime components. However, in ARIES-RS, part of the shield is not a lifetime component. In this case, the overall volume reduction afforded by high wall load might be reduced to about 30%.

Table 4 Rough order of magnitude comparison of FW/blanket waste volumes for different concepts

Concept type	Peak Wall Load MW/m^2	FW/Blanket Structure Fraction	Approximate Structure Replacement Time	Reduction in waste volume of FW and blanket components for liquid wall high power density designs relative to ARIES-RS
ARIES-RS	5.5	10%	2.5 FPY	
Thick Liquid walls with no structure	10	0%	n/a	infinite
Thick Liquid Flibe walls with 4% structure behind the liquid	10	4%	40 FPY	70
Thick liquid walls with structure to guide the flow	10	1%	~ 1.5 FPY	10

Influence of refractory alloys. Finally, we examine the effect of using refractory alloys in the first wall and blanket with their superior shielding capabilities relative to steel on the ex-vessel activation characteristics in some of the APEX designs. All of the APEX designs use a LAFS vacuum vessel. Thus, Figure 4 compares the clearance indices of different APEX designs to the baseline ARIES-RS design with a LAFS VV. The results show the higher activation afforded by the TZM alloy in the FW and blanket. Comparison of the clearance index in the VV of the different designs show that the clearance index of the VV in all of the APEX designs is higher than in ARIES-RS. Clearly, the APEX designs are at a very conceptual stage and have not had the degree of optimization as had ARIES-RS. Thus, because of the small structural fraction of tungsten in these blankets (~ 5-10%), the tungsten does not appear to help ex-vessel activation significantly compared to the ARIES-RS design. (It does help in that it affords higher wall loads but that effect was discussed earlier).

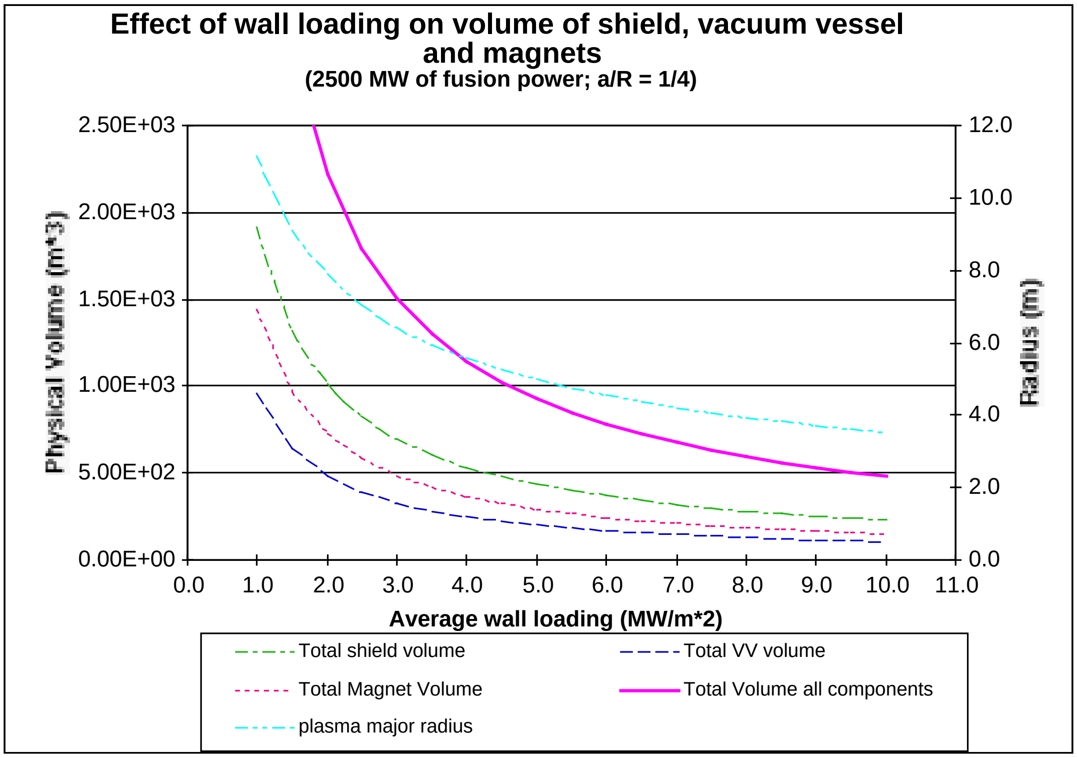

Figure 3

Tungsten may also play a role in limiting ex-vessel activation if it were to be considered as shielding material. Table 5 shows the impact of using tungsten carbide filler on the volume mass and cost of the outboard shield and VV in the advanced ARIES-RS design currently being worked on. (The advanced ARIES-RS blanket is based on lithium lead coolant and SiC structure. The use of WC does reduce the overall thickness and hence volume of the outboard shield and vacuum vessel by about 40%, however the overall mass of the structure is increased by ~ 22% and the cost is almost double. By comparison, the use of borated ferritic steel offers a better tradeoff. In this case, the volume and mass are reduced by about 10% for about the same cost as ferritic steel. Thus, it appears that tungsten may not be optimal to reduce activation on the outboard side of the machine; borated ferritic steel is better. ARIES-RS does use tungsten carbide as a filler in the inboard shield to reduce the overall volume there.

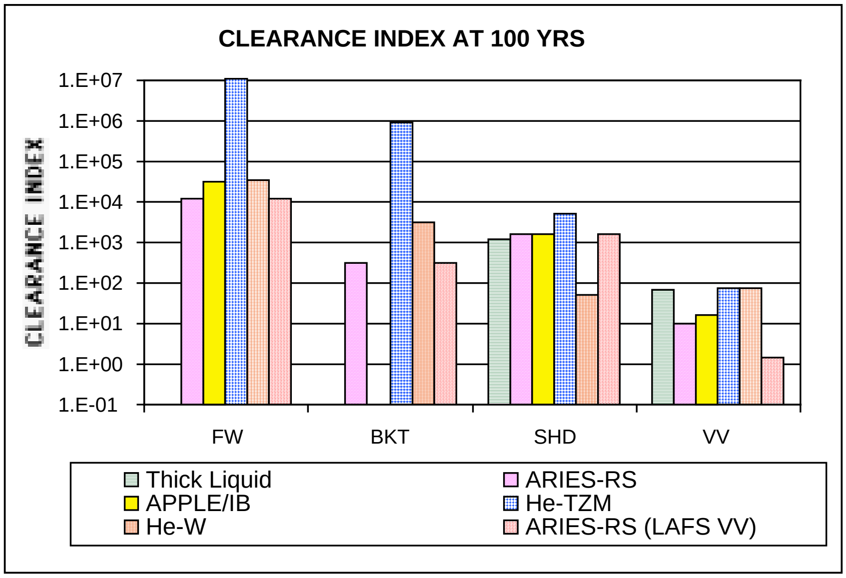

Figure 4. Clearance index for different APEX design concepts

Table 5. Influence of shield material filler on waste volume, mass and cost

Filler Material	**Ferritic Steel**	**Borated-Ferritic Steel**	**Tungsten carbide**
Thickness			
HT Shield	15	15	15
LT Shield	30	20	10
VV	30	30	30
Total	**75**	**65**	**55**
Volume (m*3)			
HT Shield	50	50	50
LT Shield	105	70	35
VV	110	108	107
Total	**265**	**228**	**192**
Mass (tonnes)			
HT Shield	320	320	580
LT Shield	700	470	400
VV	730	710	1160
Total	**1750**	**1500**	**2140**
Cost ($M)			
HT Shield	16	16	45
LT Shield	23	16	26
VV	28	28	77
Total	**67**	**60**	**148**

2.5.6 SUMMARY

The environmental impact of waste material is, however, determined not only by the level of activation, but also the total volume of active material. 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. Current estimates are that fusion's waste volume and mass is ~ 3 times that of fission wastes for similar net electric power. 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 generally understood by technical experts but may not be as well understood by the public.

Our results suggest that the waste management strategy for fusion needs to be modified slightly. While low activation materials do reduce the long-term activation hazard of the waste, for some of low activation materials 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. As has been shown previously, [5, 6, 7, 8] the development of low activation materials to reduce the in-vessel radiotoxicity hazard requires careful attention to impurities in the materials used in the near plasma components. While impurities are still important, the reduction of vacuum vessel/ex-vessel activation to levels that would allow the vacuum vessel and magnets to be cleared requires a combination of optimized bulk shielding by the blanket and shield and the use of reduced-activation steel in the vacuum vessel and ex-vessel components.

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 (via reduction of ex-vessel activation). 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 this 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. For example, some results suggest that some choices of low activation materials for near plasma components suffer the penalty of enhanced neutron penetration, giving rise to higher overall levels of activation, contact dose rate, and clearance index in the shield and ex-vessel components. Materials are considered "low activation" either because their neutron cross sections are low or because their activation products are stable or benign (short-lived), or some combination of these two properties. Those which are merely low absorption may result in higher overall plant activated material 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.

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 impurity buildup 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 offer important advantages relative to the overall volume of activated waste in a fusion machine. Liquid wall concepts with higher wall loads can reduce the volume of FW and blanket structural material wastes somewhat relative to conventional solid wall concepts. Furthermore, 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%.

This new strategy may also have implications for fusion development more broadly. Currently the materials program limits its development to materials and alloy systems that meet current low activation criteria. Thus, the fusion program focuses on vanadium alloys, low activation ferritic steel and SiC. Could a material (e.g., a new material or an alloying addition to our current materials) be developed that would substantially improve the extremely constrained design window at the first wall (e.g., higher wall load, higher temperature capability, improved strength) over the current state of the art? Such a material would allow a reduction in the size of the machine and hence a reduction in overall waste volume relative to current designs. Would the tradeoff in using this material at the first wall be worthwhile if it could reduce the overall waste volume and improve the performance (and hence economics) but because of its induced activation would generate some amount of waste requiring deep geological burial?

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2.6 Tritium Self-Sufficiency

Key Questions

What is the potential of current plasma confinement and chamber technology concepts for attaining tritium self-sufficiency and what are the implications for requirements on plasma and technology R&D? (include issues of tritium fractional burnup in the plasma, and tritium permeation, inventory, and processing). Is there a time window for the availability of tritium startup inventory? What are the implications of such time window on the schedule for tritium-producing Chamber technology?

Topic Leaders

Mohamed Sawan (UWIS), Scott Willms (LANL)

Core Working Group

Mohamed Sawan, Scott Willms, Laila El-Guebaly, Mahmoud Youssef , Edward Cheng, Dai-Kai Sze, W. Nevins, Karani Gulec

2.6.1 Introduction

This key question was discussed in two sessions at Snowmass. Fifteen people participated in the discussion. Several subtopics and questions were identified and discussed. Members of the core working group gave brief presentations to stimulate the discussion for each of the subtopics considered. Consensus was achieved on many of the technical points discussed. This report summarizes the technical findings and identifies the critical issues and the opportunities that exist to resolve them.

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. The tritium fuel cycle involves many subsystems whose physical and operational characteristics will dictate the success in achieving tritium self-sufficiency. In order to insure tritium self-sufficiency, the calculated achievable TBR should be larger than the minimum required TBR. In addition, there are uncertainties associated with the calculation of the achievable TBR and the prediction of the minimum required TBR. These uncertainties should be included when assessing the potential for achieving tritium self-sufficiency.

The calculated achievable TBR should account for the 3-D geometrical configuration of the chamber including penetrations. Hence, in addition to the dependence on the blanket type, the achievable TBR could depend on the plasma confinement concept considered. The required TBR in a fusion system must exceed unity by a margin that accounts for tritium losses and radioactive decay, tritium inventory in the plant components, and supplying inventory for startup of other fusion plants. To accurately determine the required TBR, one has to consider the entire fuel cycle for the D-T plant. The tritium fuel cycle involves many subsystems. Simulation of this cycle, including the dynamic behavior is required for accurate evaluation of tritium build-up and consumption/losses in this closed cycle [1, 2].

The first generation of fusion ignition machines are designed without tritium breeding blankets and rely on the existing tritium resources for fueling. These resources are decreasing due to radioactive decay and reduced production rate. An important issue, to be addressed in this report, is whether there is a time window for the availability of tritium to supply the tritium requirements for the ignition machines. This time window will impact the schedule for developing tritium producing chamber technologies.

2.6.2 Achievable TBR

2.6.2.1 Impact of FW/blanket concept

The breeders could be divided into three groups according to their breeding potential as shown in Fig. 1. The first group includes liquid Li and LiPb with the largest breeding potential. The second group contains Li_2O, Flibe, and LiSn with medium breeding potential. To achieve tritium self-sufficiency with these breeders, the structure content needs to be minimized in the FW and the front of the blanket and/or moderate amount of neutron multiplier should be added. The third group includes several ceramic solid breeders, such as Li_2ZrO_3, Li_2TiO_3, Li_4SiO_4, and $LiAlO_2$, which have poor breeding potential and need substantial amount of neutron multiplier to achieve adequate breeding. Enriching the lithium in the isotope ^6Li does not always help the breeding. Breeders with natural Li provide the highest TBR except for LiPb and LiSn. The TBR could optimize at higher enrichment when structural materials and multipliers are included in the blanket.

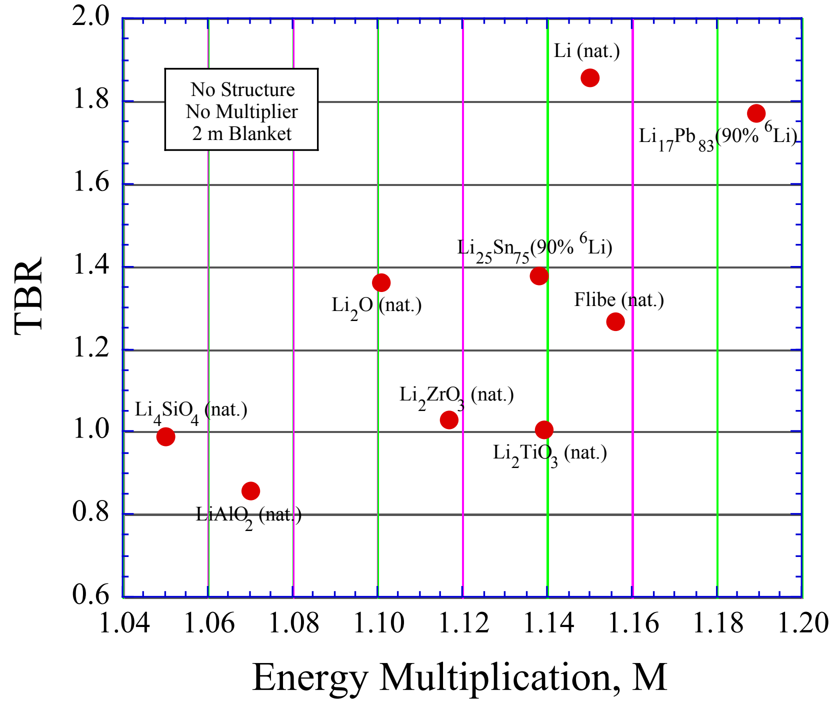

Fig. 1. Tritium Breeding Potential of Candidate Breeding Materials

Using structural material in the FW/blanket results in degrading the achievable TBR. The amount of structure in the FW and the front layer of the blanket has a much more severe impact on tritium breeding relative to the structural content in the bulk of the blanket. Depending on the breeding material and structure type and content, up to 20% degradation in TBR might result. Except for LiPb and LiSn, which use highly enriched Li, some of the TBR degradation resulting from the structure can be recovered by enriching the Li in ^{6}Li. Eliminating the structure completely as in IFE chambers [3] and MFE liquid wall concepts [4] improves the tritium breeding capability. Separate coolants could be utilized with solid and liquid breeders. Due to its low density, He gas has negligible impact on tritium breeding. Using 20% water coolant in the FW/blanket system reduces the TBR by up to 7%. Different neutron multipliers can be used to enhance the achievable TBR. The enhancement depends on the breeding and structural materials used. Beryllium is the best neutron multiplier followed by Pb, Be_2C, and BeO. However, Be has problems associated with toxicity, compatibility, resources, and cost.

2.6.2.2 Impact of confinement concept

There are several geometrical, spectral, and temporal differences between the IFE and MFE systems that could impact the achievable TBR [5]. While a cylindrical or toroidal chamber surrounds a volumetric distributed source in MFE systems, a nearly spherical chamber in IFE plants surrounds a point neutron source. As a result, source neutrons in IFE chambers impinge on the FW/blanket in a more perpendicular direction than in MFE chambers. This leads to lower tritium production rate at the front with lower radial gradient. The local achievable TBR will be similar in the two systems if the blanket is quite thick. Fusion neutron interactions in the highly compressed target results in considerable softening of the neutron spectrum incident on the FW/blanket in IFE chambers. These neutrons can have average energies as low as 10 MeV. The combined geometrical and spectral effects result in local TBR values in IFE chambers that are slightly lower than those in MFE chambers depending on the type and thickness of the FW/blanket. The difference in the temporal characteristics of the source neutrons in IFE and MFE systems does not affect the time integrated local TBR.

The calculated achievable TBR should account for the 3-D geometrical effects. The achievable overall TBR depends on the confinement concept due to differences in breeding blanket coverage and possible limitation on blanket thickness. Confinement schemes, such as tokamaks, STs, and stellarators require a divertor (or limiter) system with tritium breeding being compromised or absent in the divertor region. FRCs and spheromaks, do not require divertors. No divertors or limiters are needed in an IFE system.

A tokamak with a single null divertor will have about 5% higher breeding capability than a double null design. Due to space limitation in the inboard side of a tokamak, a thinner blanket is usually used in the inboard region with a negative impact on the achievable overall TBR. STs depend entirely on the outboard blanket for tritium breeding. However, this is not expected to drastically affect the overall TBR since the low aspect ratio results in less than 10% inboard coverage. The coverage fraction of the divertor in stellarators is relatively large. On the other hand, absence of disruptions allows using FWs that are thinner than in tokamaks. However, the breeding blanket in stellarators need to be kept as thin as possible to insure that the external coils producing the rotational transform of the magnetic field are as close to the plasma as practicable.

In linear confinement concepts such as the FRCs, elongating the cylindrical chamber can reduce the end losses. These concepts also allow for using uniformly thick blankets. In IFE plants, the chamber geometrical configuration results in nearly full coverage with blankets that could be as thick as needed at all locations in the chamber resulting in the achievable overall TBR being very close to the local value.

Penetrations are required in MFE chambers to accommodate heating and current drive systems, and diagnostic systems. Given our present limited knowledge regarding the heating and current drive requirements for the various alternate confinement schemes, it is not possible to reach any conclusion regarding the relative advantages in tritium breeding of any one magnetic confinement scheme over the others. In recent conceptual MFE power plant designs [6], the area taken by the heating and current drive penetrations amounts to 1-2% of the FW area and the net effect on the overall TBR is about 2-3% reduction. The diagnostic penetrations are usually much smaller than those required for heating and current drive. Therefore, they are not expected to have a dominant impact on the TBR.

No heating or current drive systems are needed in IFE power plants. The penetrations in an IFE chamber provided for the laser or ion beam fusion driver represent less than 0.5% of the FW area for direct drive concepts with up to ~100 beam ports [7]. For indirect drive concepts, the fraction taken by the beam ports is much lower [3].

It is clear from the above discussion that the IFE systems have a clear advantage since no divertors, limiters, or heating and current drive systems are employed. In addition, IFE chambers have near full blanket coverage. Blankets can be made as thick as needed in IFE chambers without impacting the high cost driver. In addition, absence of magnetic fields makes it easier to employ flowing liquid breeders. There is no clear advantage for any of the MFE confinement concepts with respect to the potential for achieving tritium self-sufficiency. It should be emphasized that, even though some MFE confinement concepts suffer from reduced blanket coverage and limited blanket thickness, tritium self-sufficiency can still be achieved in such concepts with carefully designed blanket concepts that have high breeding potential.

2.6.2.3 Uncertainties in predicting the achievable TBR

The uncertainties in the achievable TBR are due to uncertainties in nuclear data, calculational method, and modeling [8]. Cross section sensitivity/uncertainty analyses couples the sensitivity of the TBR to changes in cross sections of material present in the blanket with the actual uncertainty in these cross sections. These are quantified in terms of covariance and correlation data that reflect the size of uncertainties and the correlation that may exist among various cross section errors over specific energy ranges. These uncertainties have been quantified for several blanket concepts that have solid as well liquid breeder/coolants and were found to be in the range of ~2-6%. In these solid wall blanket concepts, most of the uncertainty is attributed to the uncertainties in the partial cross sections of the structural materials (iron, chromium, nickel, etc.) or in the partial cross sections of the elements in the breeder compound materials (e.g. oxygen in Li_2O). For liquid FW/blanket concepts without structure, the uncertainty in the TBR is anticipated to be smaller since it is mainly driven by the uncertainties in the ^{6}Li and ^{7}Li cross sections whose size is generally small, particularly in liquid lithium. However, uncertainties in the cross sections of the other elements of the breeder compound (e.g. Be in Flibe, Sn in Li-Sn) are dominant and could lead to ~3-8% uncertainties in the TBR.

Data processing and representation in the data bases include tabulating data at a finite number of energy points or representation as analytic function. Processing these data to produce point-wise or multi-group data libraries involves interpolation and use of approximate weighting functions. Studies showed that ~4% uncertainty in the TBR could be attributed to processing of various multi-group data whereas the corresponding uncertainty arising from processing point-wise data for Monte Carlo calculation is less (~2%).

The uncertainties in the TBR resulting from possible modeling approximations such as zone homogenization are estimated to be ~1-2% with the state-of-the art Monte Carlo transport codes. The uncertainty in the TBR calculation due to the transport calculation itself is small, depending on the number of histories involved. Uncertainty of ~1% results from the statistical nature of the Monte Carlo method. Uncertainties in discrete ordinates calculations, arising from limitations on modeling the geometrical configuration and due to using multi-group data, are larger and could be in the range of 2-3%.

Assuming that the above listed uncertainties are uncorrelated, the uncertainty in the TBR due to data, modeling, and calculational method uncertainties is ~5-9% for multi-group discrete ordinates calculations and ~3-6% for 3-D continuous energy Monte Carlo calculations.

2.6.3 Required TBR

2.6.3.1 Parameters affecting the required TBR

The pathways for tritium to be added to the system, F_i, are:
Importation from external sources (F_{im})
Tritium breeding (F_{br})

The pathways for tritium to leave the system, F_j, are:
Decay (F_{de})
Burnup (F_{bu})
Loss to the environment (F_{en})
Loss to materials/waste (F_{mw})
Export of tritium to other sites (F_{ex})

These pathways are summarized schematically with figure 2. A more detailed analysis for this topic can be found in references 1 and 2.

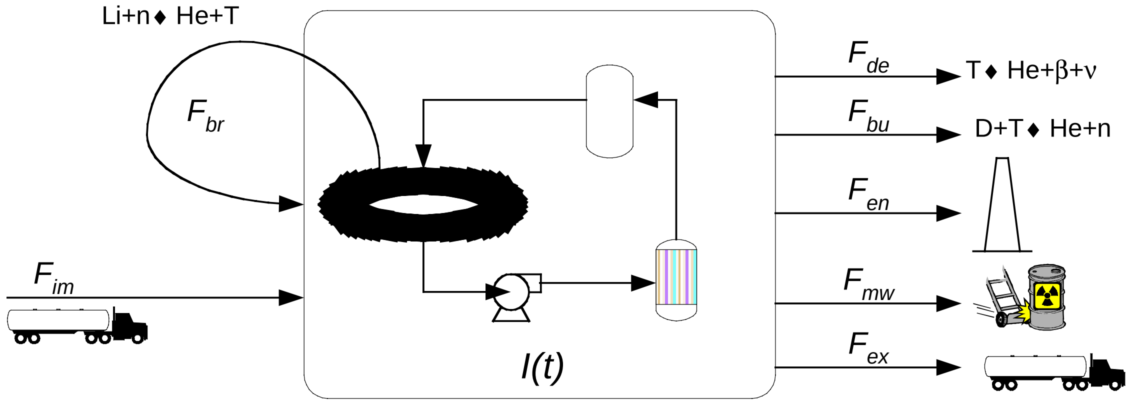

Figure 2 Tritium Pathways in a Fusion Plant

Following a startup period the external source term will become and remain zero. Then the system mass balance is:

$$F_{br} = F_{de} + F_{bu} + F_{en} + F_{mw} + F_{ex}\,. \qquad (1)$$

This equation can be expressed in terms of a tritium breeding ratio (TBR) as:

$$TBR = \frac{F_{br}}{F_{bu}} = 1 + \frac{F_{de} + F_{en} + F_{mw} + F_{ex}}{F_{bu}}\,. \qquad (2)$$

This expression emphasizes the fact that each fusion reaction which produces one neutron must in turn breed one tritium to account for the "1" on the right-hand-side of equation 2. Beyond that, further tritium must be bred to make up for the remaining terms in equation 2. The sink terms in equations 1 and 2 have the following general dependencies and ranges for a 1 GW(fusion) plant.

Parameter	**Primary Dependence**	**Range for ~1 GW_f average plant**	**Contribution to TBR**
F_{de}	Tritium inventory	For 2,000 g inventory, 100 g T/yr	0.002
F_{bu}	Fusion power	56,000 g T/yr	1.0
F_{en}	Tritium systems design, coolant system design, maintenance philosophy	Typically <0.5 g T/yr	Nill
F_{mw}	Materials of construction, component replacement rate, waste handling design	For carbon, could be 100's of g T/yr, other materials will likely be much lower	0.0005 - 0.005
F_{ex}	Overall fusion strategy, world tritium supplies	100-2,000 g T/yr	0.002 – 0.03
Total		56,200 – 58,300	1.004 – 1.04

From the numbers in this table it is observed that most of the tritium breeding is used to make up for the tritium that is burned. In other terms, the TBR must be at least unity. Then, more tritium must be bred to make up for the other smaller tritium losses. Of these smaller terms, the three largest factors are the rate at which tritium needs to be generated for export to other plants, the tritium that is lost to material/waste and the tritium lost to radioactive decay. Which will be largest will depend on what materials are used for the reactor components (codeposition on carbon components can make this term large), how aggressively tritium is bred for other sources (e.g., required doubling time), and the overall tritium inventory. In the context of the TBR, the tritium lost to the environment is negligible. This results from the high safety requirements that limit the tritium loss rate to the environment to less than 10 Ci/day.

For the range of parameters listed in the table above, the required TBR will range from 1.004 to 1.04. It should be noted that the total tritium inventory in the plant greatly influences the required breeding margin. The amount of online reserve tritium inventory required is uncertain and need to be assessed. Up to a 2 day reserve was assumed in several studies (see references 1 and 2). It should be emphasized that the high safety and environmental requirements imply that an effort should be made to reduce the total tritium inventory on site. Required TBR values that exceed the values given in the above table (~1.1 or more) were predicted in these studies with different assumptions for the system parameters. One of the participants (Dai-Kai Sze) requested that his opinion regarding the required TBR be recorded. He stated that under the conservative assumptions of 20 kg total tritium inventory and 5 year doubling time, the required TBR will be only 1.04.

2.6.3.2 Tritium fractional burn-up in plasma

The tritium fractional burn-up in the plasma impacts the tritium inventory in some of the components, such as the plasma fueling system, and hence affects the required TBR. Increasing tritium burn-up reduces the required TBR and, hence, improves the potential for achieving tritium self-sufficiency. The tritium burn-up fractions projected for MFE systems are high enough (>10%) that they should not greatly impact the required TBR. In IFE targets, the tritium burn-up fraction is expected to be about 30%.

2.6.3.3 Tritium inventory

There are three key components within the chamber technology area that have potentially high tritium inventory. These three components with their estimated tritium inventory are:

a. Blanket; The estimated tritium inventory in the blanket varies from few g for Flibe and Pb-Li blankets, up to about 100 g for the lithium blanket.

b. The cryogenic pump: The typical regeneration time for the cryopump is about 30 minutes. The tritium flow rate to the cryopump is less than 600 g/hr (3000 MW fusion power, with plasma burn-up fraction of 3%.). Therefore, the tritium inventory on the cryopump is less than 300 g.

c. The divertor: Based on the selection of the divertor material, the tritium inventory in the divertor can be as high as 5 to 10 kg, especially if graphite is used for the divertor material.

Therefore, the tritium inventory is dominated by the divertor. The selection of the divertor material, will have dominant effect on tritium inventory.

For IFE systems, there is no divertor and no need for cryopump. Therefore, the tritium inventory in the chamber is dominated by the blanket, which is a small fraction of the total on site tritium inventory.

The tritium recovery system from the breeding material includes the following sub-systems:
The component to remove tritium from the breeding material.
The components to remove all impurities, including He carrier gas, impurities, and activation products to form a stream of only pure hydrogen isotopes.
A safety device, most likely a Pa diffuser, to assure no harmful impurities will pass to the isotope separation system (ISS).
An ISS, to separate the hydrogen isotopes into the required isotopic compositions to be either reused as the fuel to the plasma, or to be released as waste.

The tritium inventories in the breeding material in the blanket range from few grams for Pb-Li, to about 100 g for a lithium blanket. The tritium residence time in the purification system is about 3 hours. The tritium throughput from the blanket is about 500 g/FPD. Therefore, the tritium inventory in the purification system is about 60 g. There is essentially no tritium inventory in the diffuser. The total tritium inventory in the ISS can be limited to about 100 g, based on ITER study. The additional tritium inventory due to the blanket stream is on the order of 10 g, based on the ITER calculation with a Li blanket.

The total tritium inventory in the tritium extraction system, including the total tritium in the isotope separation system (ISS), is about 500 g. This tritium inventory is much smaller than the expected tritium inventory in the PMI system. Therefore, it is expected that the tritium extraction system will have only minor impact on the required TBR. In IFE systems, the tritium inventory in the fueling system depends on the filling method. It ranges from ~300 g if pellet injection with fill tubes is used to ~2 kg if the diffusion method is used.

2.6.3.4 Uncertainties in determining the required TBR

Uncertainties in defining the parameters affecting the required TBR (such as, doubling time, burn-up fraction, inventory, etc.) lead to uncertainty in the required TBR. Based on assuming a lognormal distribution for the key plasma and engineering system parameters that impact the required TBR, the probable uncertainty in the required TBR is ~5% [1].

2.6.4 R&D needs to increase the potential for tritium self-sufficiency

2.6.4.1 Plasma R&D

The plasma R&D needs driven mainly by tritium self-sufficiency would be efforts to increase the tritium burn-up fraction by increasing the helium pumping efficiency (minimizing τ_{He}^*/τ_E), and enriching the helium in the particle exhaust stream (minimizing τ_{He}^*/τ_{DT}^*).

Efforts to minimize the size of heating and current drive systems are already strongly driven by efforts to reduce the recirculating power. Some advantage for tritium self-sufficiency might be gained by efforts to integrate a tritium breeding capability into RF antenna systems.

2.6.4.2 Technology R&D

To increase the prospect of achieving tritium self-sufficiency, the needed R&D program should aim at reducing the uncertainties in the achievable and required TBR. It is hoped that the uncertainty in the achievable TBR be < 3%. To reach this goal, we need to have better evaluation of the magnitude of the uncertainties in estimating the achievable TBR and attempt to reduce them. More reliable estimates to the uncertainties involved can be quantified through the use of integral experiments using the existing 14 MeV neutron sources. Data improvement through integral experiments and performing extensive cross section sensitivity/uncertainty analyses is viewed as an essential mean to minimize the uncertainties involved. Full covariance data for smooth and differential cross sections should be implemented in the current data libraries and for materials whose cross sections impact the estimated TBR (e.g. neutron multipliers, compounds of breeding materials, etc.). Code development is also needed to produce adequate computational tools for sensitivity/uncertainty analysis based on Monte Carlo techniques.

The 14 MeV neutron source facilities that exist around the world are the FNS facility (JAERI, Japan), the FNG facility (Frascati, Italy), the SNEG-13 facility (Near Moscow, RF), and the RTNS-I facility (UC-Berkley, US). The neutron yield among these facilities varies (~5×10^{12}, n/s, ~5×10^{11} n/s, ~3×10^{13} n/s, and 1×10^{12} n/s, respectively) and can be tailored for specific neutrons tests. However, it should be noted that direct demonstration of tritium self-sufficiency requires a fully integrated reactor system, including the plasma and all reactor prototypical nuclear components [9]. For at least one blanket type, testing an entire sector, fully integrated with tritium processing system, is required. Such testing could be planned in a fusion test facility such as ITER, or in smaller test stand, such as a compact volumetric neutron source (VNS) facility. A sector test, rather than only a module test, is necessary because:

(1) there are strong poloidal variations in tritium production rates due to variations in wall load, geometry, penetrations, etc. Thus, the extrapolation of results from each module to reactor conditions will be different, and

(2) the uncertainties arising from specifying the boundary conditions for modules are large and make extrapolation difficult.

2.6.5 Tritium availability for DT fusion development

The following analysis is based on a paper by P. H. Rutherford [10] which addresses this topic in detail. Separate assessment by Sze and Anderson for ITER [11] and internal CFFTP assessment [12] agreed with the conclusions. The world tritium available for a D-T fusion reactor is expected to only reasonably be available from the Ontario Power Generation (formerly Ontario Hydro) company. This tritium is bred in their CANDU reactors. Presently OPG includes twenty such reactors, but seven have been idled due to maintenance problems. All of OPG's reactors are licensed for 40 years. The tritium is removed from the heavy water moderator at the Darlington Tritium Removal Facility (DTRF). Presently DTRF has accumulated a tritium inventory of about 15 kg. The present expectation is that this tritium will be collected at a rate of 2.1 kg/yr, decreasing to 1.7 kg/yr in 2005. Thereafter, the rate will remain constant until 2025 when the rate will decrease rapidly due to reactors reaching their end-of-life. This tritium is presently sold to various interests at a rate of about 0.1 Kg/yr. And, of course, tritium is lost due to decay at a rate of 5.47%/yr. Based on these values the upper line on figure 3 was prepared to show the expected yearly inventory of tritium which may be available for D-T fusion development.

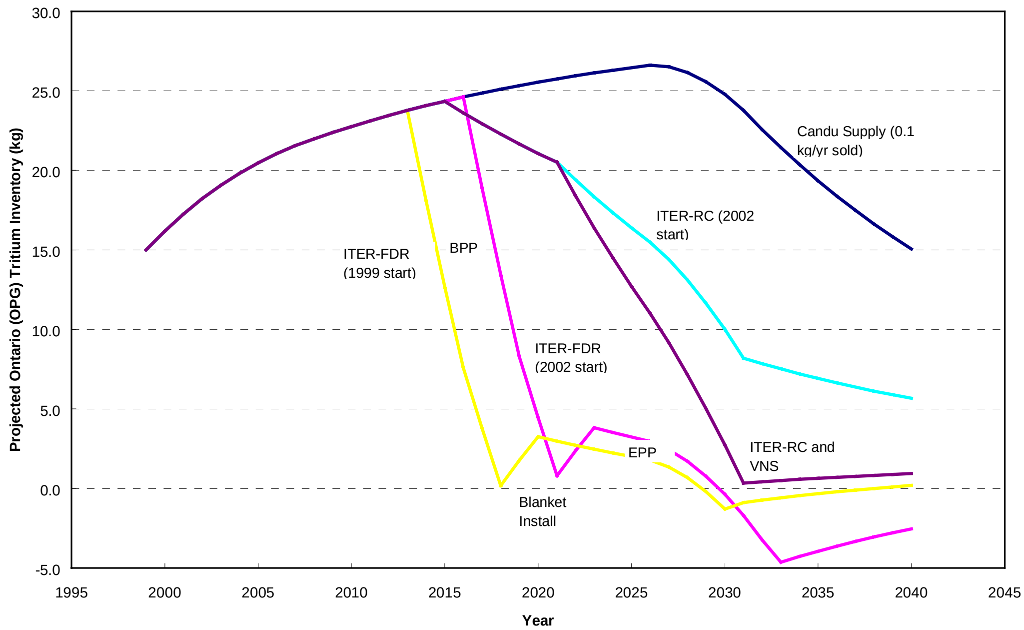

Figure 3. Expected Tritium Available for D-T Fusion Development for Various Scenarios

There are various means by which this supply might be increased including, 1) extending the lifetime of the OPG CANDU reactors, 2) restarting some of the CANDU reactors, 3) processing moderator from other CANDU's such as the ones in Quebec and New Brunswick, and elsewhere in the world, 4) building more CANDU reactors, 5) purposely generate T for fusion by irradiating Li targets in commercial reactors, and 6) obtaining tritium from "nuclear superpowers". These options however are not likely to be practical due to economic, political and societal considerations. Also, on the downside, it is credible that presently expected CANDU performance will be less than anticipated. All-in-all the upper line on figure 3 appears to be the most credible scenario which should be used for planning purposes. This shows a peak of about 26 kg of tritium in the late 2020's time frame with a rapid decrease in the available tritium thereafter.

The ITER-FDR (Final Design Report) machine has been designed to burn 5 kg T/yr for five years during the BPP phase. This would be followed by a two-year installation of a breeding blanket. Then there would be a ten-year EPP phase during which a breeding ratio of 0.8 would leave a requirement of 1.7 kg T/yr from external sources. Beyond this tritium which is burned, it is expected that about three kg T will be required to operate the machine (fill materials and processing systems, lost in waste, etc.). This tritium was assumed to be drawn from the world inventory at 1 kg/yr in the first three years of the BPP phase. Using this analysis the lower two curves on figure 3 were prepared. They show the available tritium inventory each year for if the ITER-FDR construction is started in 1999 and if it is started in 2002. As can be seen, the available tritium decreases to almost zero at the end of the BPP. The two-year blanket installation allows the inventory to rebound a bit. Then the EPP depletes the available tritium to below zero in the

2030 time frame. The 1999 ITER-FDR start only takes the inventory about one kg below zero, but the later start goes substantially below zero. This is because the OPG supply will begin to drop rapidly at that time. A later start would make this situation even worse. It should be pointed out that there is no available tritium for any other significant work coincident with the ITER-FDR and there is no tritium remaining at the end for the startup of another reactor.

The previous analysis was instructive as a case study, but at present, it is not credible that ITER-FDR will be built. However, there is some likelihood that ITER-RC (reduced cost) will be built. This machine is expected to require 1 kg T/yr for six years followed by 1.6 kg T/yr for ten years. In this case, this is the total tritium required (no additional amount is taken for materials, processing systems, etc.). The result of this analysis is labeled on figure 3. At the end of these operations about 1 kg T remains, so this is an achievable scenario. However, ITER-RC will not provide for a blanket test facility. Thus, another tritium-burning facility such as the "Volumetric Neutron Source" may be required which could require 1 kg T/yr coincident with the 1.6 kg T/yr years for ITER-RC. This scenario, labeled on figure 3, takes the available tritium to just above zero at the end of these programs in 2030. Thus, while this is achievable, there is insufficient tritium remaining for a follow-on D-T machine.

This analysis shows that the presently expected world supplies of tritium are marginal for the development of D-T fusion. This factor must be considered seriously when planning the world's D-T fusion development program. Considering that the available tritium will most likely diminish rapidly after 2025 emphasizes the urgency of starting the next D-T fusion machine in the very near future. Waiting longer will place ever-greater pressure on the quick success of future machines to breed their own tritium. Waiting even longer may result in a situation where D-T fusion development becomes excessively expensive and possibly impractical.

2.6.6 Major issues and opportunities

Two major critical issues were identified for this topic. 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.

2.6.7. Summary

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. 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, Li_2O 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.

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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.

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2.7 Materials

Key Questions

What advances may be possible in materials over the next ten years that can contribute to: 1) improving the vision for an attractive and competitive fusion energy system, and 2) lowering the cost and time for fusion R&D?

Topic Leaders

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Core Working Group

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2.7.1 INTRODUCTION

Two Materials Advances sessions were held to discuss the potential advances in structural and non-structural 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 materials R&D. These sessions were co-chaired by M. Billone and S. Zinkle, with A. Rowcliffe serving as the recording secretary. Brief presentations, organized to stimulate discussion, were made by members of the Materials Advances' Core Working Group and representatives from other topic areas on: structural materials for first-wall blanket applications (Bloom); non-structural materials for plasma-facing components (Billone/Nygren), tritium solid breeders (Billone), Be multiplier (Billone), liquid metal/salt coolants and breeders (Smith/Sze), shielding material (Billone/Youssef), and special material needs for IFE reactor design concepts (Snead). These presentations were closely coordinated with materials issues identified in topical sessions on design concepts utilizing liquid walls and solid walls for MFE and IFE. Summary presentations were also made on large-scale experimental facilities (Zinkle), waste management strategies (Petti) and high-performance vs. low-activation issues addressed in Energy Issues Subgroup A2 (Steiner). The wrap-up session focused on key materials needs to support the feasibility and proof-of-principle of high-performance reactor design concepts, as well as cost-effective and timely R&D plans to address the material needs over the next decade (Zinkle).

Several common themes were emphasized by the presenters/discussion-leaders: performance goals for materials are established in system design studies; the challenge for materials R&D is to develop materials that meet or approach these performance goals; materials R&D must take into account the behavior of combinations of materials (e.g., structure/breeder/coolant) identified by the systems design studies; and the use of "roadmaps" is essential in planning relevant and cost-effective R&D.

2.7.2. Summary of Major Opportunities

Three general areas of major opportunities were identified at the Materials Advances sessions. These cannot be considered as "consensus" items because no formal votes were taken. However, they were highlighted and discussed during the sessions.

2.7.2.1 Opportunities to consider additional structural materials

The current Advanced Materials Program has focused its R&D efforts on three low-activation structural materials for first-wall/blanket application: low-activation ferritic steels, vanadium alloys and SiC/SiC composites. There is also a small effort on copper alloys, based on past ITER-related work, that could be useful in next-step device applications. Potential performance benefits from high-power-density designs and liquid-wall designs, along with re-evaluation of the waste management strategy to include waste volume minimization (along with activation), open the door for consideration of additional structural alloys and alloying elements. Higher power density and longer lifetime could result in lower waste volume and counteract small quantities of higher activation elements. Refractory W and Mo alloys, which have been rejected in the past due to stringent low activation criteria, fabrication and joining difficulties, and embrittlement under neutron radiation, may now be reconsidered. The performance of vanadium alloys may be improved with the addition of certain higher-activation elements (e.g., Al), which may make the alloys more oxygen- and creep-resistant. There was general agreement that the materials community, in conjunction with system design groups, should undertake a re-evaluation of the current low activation philosophy in light of these new ideas. Tradeoffs among high power density, longer lifetime, activation and waste volume could allow some relaxation of the severe restrictions on alloy composition currently in place. This would provide the much-needed flexibility in developing high performance materials for the highest neutron and thermal flux regions of the first-wall component.

2.7.2.2 Near-term, cost-effective R&D

Reactor design concepts range from low-performance, next-step devices (with and without tritium breeding and heat utilization) to intermediate-to-high-performance reactor concepts (e.g., solid first walls with He or liquid metal coolants) to high-performance reactor concepts (e.g., liquid walls, He-cooled refractory metals, etc.). There is a wide range of feasibility and proof-of-principle issues that could be addressed in the next 5-10 years by the fusion program using existing non-nuclear experimental facilities, relatively low-cost new non-nuclear facilities, and existing fission reactors for low-damage-level experiments. Several examples cited during the sessions are: chemical compatibility between candidate structural materials and liquid metal/salt coolant/breeders, development of electrical insulator ceramic coatings for Pb-Li and Sn-Li liquid metal systems (along with the current work being done on the V/Li system), development of tritium barriers for designs employing Pb-Li, Sn-Li or Flibe coolant/breeder, improved thermomechanical performance of solid breeder and Be pebble beds, neutron radiation stability of improved W (with dispersed TiC nano-particles), etc. While considerable cost-effective R&D is being performed currently in the Advanced Materials Program (AMP) for three structural materials, there is much more that could be done during the next decade for a wider range of structural materials, for non-structural PFC materials, and for non-structural ceramic breeders and coatings – all in support of addressing key feasibility and proof-of-principle issues. The current AMP roadmap could serve as a model for performing R&D on other materials.

2.7.2.3 Integration of materials R&D

Concerns were raised that certain materials (e.g., solid breeders and Be multiplier) could "fall into a crack" in our current R&D plan and that there was insufficient communication among the different areas of chamber technology regarding materials development and selection. The Advanced Materials Program has a well-developed R&D plan for structural materials only. Other programs within Advanced-, Enabling- and IFE-Chamber-Technologies deal with a wide range of structural materials and non-structural metals and ceramics. Integration of the materials R&D (or at least improved communication) within the chamber technology areas offers the potential for utilizing the best materials expertise to address materials issues. While the idea of having a single fusion materials program was not recommended at this time, there was general agreement that the formation of a materials' coordinating body could facilitate interaction and communication among the various groups concerned with materials-related issues.

2.7.2.3 Advances in Materials R&D

With the U.S. shift in emphasis from mission-dominated R&D (e.g., ITER, DEMO, commercial power plants) to the advancement of plasma science, fusion science and fusion technology, there has been a parallel shift in materials science and technology R&D direction and strategy. Rather than narrowing the R&D to characterize existing materials limitations and developing a materials database for qualification of materials for design and construction, the field has been broadened to emphasize improvement in materials performance through optimization of alloy composition and microstructure by means of a comprehensive modeling and experimental program. In the following, examples of advances in materials are presented for structural and nonstructural materials.

2.7.3 Structural Materials

The Advanced Materials Program (AMP) has developed a comprehensive R&D plan that highlights technical challenges and key issues for structural materials, as well as deliverables through 2005 (see VLT web site). In the case of structural materials, it is recognized that high temperature capability greatly enhances the attractiveness of fusion energy. Concurrently, radiation resistance at lower temperatures increases the design temperature window for structural materials that operate at temperatures as low as the coolant inlet temperature.

2.7.3.1 Low-activation ferritic steels

Low-activation ferritic steels have been developed to improve low-temperature radiation resistance, while at the same time exhibiting sufficiently low activation to qualify as Class C waste for shallow land burial. State-of-the-art alloy development efforts resulted in the development of ferritic-martensitic Fe-8Cr-TaWV alloy, which has superior low-temperature ductility (as compared to most other ferritic steels) after neutron irradiation (without He production), but is still limited by thermal creep to a maximum temperature of ≈550˚C, depending on the design operating stresses and lifetime. Planned future work will focus on developing creep-resistant, oxide-dispersion-strengthened (ODS) ferritic steels to increase the high temperature operating limit by at least 100˚C (from 550˚C to 650˚C). ODS ferritic steel alloys developed 15-20 years ago as part of the fast breeder program produced significant increases in yield strength and creep resistance compared to non-ODS ferritic steels. However, the mechanical properties were highly anisotropic (high creep strength was only achievable parallel to the extrusion direction).

A long-term R&D effort in Japan has recently achieved successful fabrication of laboratory-scale heats of fusion-relevant, low-activation ODS ferritic steels [Fe-(11-14)Cr-(2-3)W-(0.4-0.5)Ti-(0.2-0.7)Y_2O_3] with isotropic mechanical properties. Further effort needs to be directed at optimizing these steels for fusion applications, demonstrating that ODS ferritic steel can be fabricated and joined to form complex component shapes envisioned for first wall/blanket structures, and quantifying lower temperature limits based on radiation-induced embrittlement and higher temperature limits based on radiation-induced softening. This will require an iterative process involving irradiation experiments and modification in alloy composition and heat treatment in combination with an integrated modeling/experimental program to understand the relationships between processing, microstructure and properties.

2.7.3.2 Vanadium alloys

A long-term vanadium-alloy (V-Ti and V-Cr-Ti) development effort within the Advanced Materials Program has resulted in the scale-up to commercial size production of a V-4Cr-4Ti alloy that has enabled the concept of a low activation, Li-cooled blanket to operate at temperatures up to ≈700°C (plasma-side). Low temperature operation may be limited by irradiation embrittlement to ≈400°C (coolant side). These numbers are not absolute limits as the temperature limits depend on design stresses and lifetime. Several critical issues, as defined in the AMP roadmap, are being vigorously addressed. These include the high temperature (650-800°C) thermal creep resistance of this alloy, the impact of fusion neutron-generated He on creep behavior, and the development of joining methods. One of the highest R&D priorities for the near term is the development of self-healing electrical insulator coatings for vanadium alloys cooled by liquid Li, as such coatings are essential for reducing the MHD pressure drop. Concurrent with these efforts, other compositions of the V-Cr-Ti alloy system are being considered for even higher temperature operation. The creep resistance of these alloys can be increased significantly by increasing the Cr content to 10-15%. Optimization is required to maintain reasonable ductility at the lower operating temperatures (≥400°C) while increasing the upper temperature limit to T > 700°C by increasing the thermal creep resistance.

2.7.3.3 SiC/SiC composites

Unlike ferritic steels and vanadium alloys, SiC/SiC composites are at a relatively early stage of development. They offer the attractive possibility of high temperature operation (≈1000˚C), but also offer the highest development risk due to uncertainties in fabrication methods and cost, joining, thermomechanical and radiation stability, etc. Significant progress in the broader materials science field during the past 10 years has led to fibers and composites with improved thermal conductivity and strength both before and after irradiation. The highest priority R&D issues identified in the AMP roadmap include quantification of the radiation-induced decrease in thermal conductivity, development of joining techniques, and determination of the maximum operating temperature due to thermal creep and void swelling considerations.

2.7.3.4 Copper alloys

Copper alloys are included in the AMP roadmap due to their importance for several near-term fusion applications, as well as the proposed center-post magnet for spherical torus concepts and as a structural material for high heat flux components. A limited amount of work is planned to determine the key mechanical properties (including fracture toughness) of Cu alloys at temperatures up to ≈400˚C before and after irradiation. Possibilities for developing ODS copper alloys, with improved strength and fracture toughness properties in the range of 250-500°C, will also be examined to a limited extent.

2.7.3.5 Refractory alloys

High temperature refractory metals such as W and Mo alloys were rejected as structural materials by the fusion program more than a decade ago due to fabrication and joining difficulties and radiation-induced embrittlement issues. Because of concerns regarding activation (after-heat and long-term waste disposal), there has been no incentive to re-evaluate these alloys as structural materials. However, recent innovative modifications in the microstructure of refractory alloys (e.g., TiC precipitates), along with an on-going re-evaluation of the waste disposal strategy, suggest that these alloys may offer some promise as high-performance structural materials. Limited efforts are planned to determine the mechanical properties of W+TiC before and after irradiation. Some consideration should be given to expanding the current efforts to include other W alloys, as well as other refractory alloys. The priority of these activities would depend on the economic and environmental tradeoffs (as established by reactor and power plant systems design studies) and benefits resulting from the use of these alloys.

2.7.3.6 Fabrication and joining

Advances in stir friction welding (a solid-state joining technique) by the broader materials science community offer promise for the joining of ODS ferritic steels and other structural alloys. Since stir friction welding does not produce melting, it is an appealing process for joining ODS alloys (which cannot be welded using conventional techniques such as gas tungsten arc or electron beam welding). This solid state joining technique may also allow a step-change increase in the allowable He content in structural alloys for the vacuum vessel, etc. (the He content is currently limited to ~1 appm for conventional welding processes due to He bubble-induced cracking). The impact of the advances in stir friction welding may be a reduction in shielding needed and, hence, a reduction in radial build and capital cost of a fusion reactor (assuming that improved superconducting magnet insulators can also be developed). In addition, it may be a useful technique for field welding of V and other refractory alloys (which cannot be field-welded using conventional techniques due to impurity pick-up). Other advances in materials processing may have a significant impact on fusion, including rapid prototype forming and near-net-shape fabrication of complex structures.

2.7.4 Non-structural materials

Tungsten and W alloys are leading candidates for plasma-facing components due to their low plasma sputtering rates and high thermal conductivity. Near-term R&D in Japan is focused currently on W alloys containing nano-particles of TiC, which appear to have improved ductility before and after irradiation. A more extensive fabrication and test program is required to verify improved performance for these alloys.

Carbon-fiber reinforced composites may also be suitable as a plasma-facing component for some IFE concepts (CFCs are generally not suitable for MFE applications due to their high tritium retention at temperatures <900˚C). Advances in this area need to be evaluated in the context of IFE design concepts.

There are several other examples where advances in materials science have led to improved materials which are of potentially high benefit to the fusion program. For example, KU-1 quartz (developed by Russian researchers) appears to have significantly improved radiation resistance to optical degradation compared to all existing grades of quartz. This material has potential applications in both IFE and MFE (plasma diagnostics) systems. Free standing chemical-vapor-deposited diamond wafers suitable for high-power ECH windows have been commercially developed within the last six years which have significantly lower cost (two orders of magnitude) and improved performance (factor of 15 lower loss tangent) compared to earlier CVD diamond films. The general field of high temperature superconductors is of obvious interest for fusion energy applications, and recent advances in ceramic processing (e.g. tape casting) may enable radiation-resistant ceramic insulators to replace the radiation dose-limiting polymer insulators in magnets.

The issue of improving the thermal performance of solid breeder ceramics was discussed. Current EU and JPN designs utilize solid breeders (as well as the Be multiplier) in pebble-bed form to minimize uncertainties in thermal performance due to thermal-stress-induced cracking and interface heat conduction between the breeder ceramic and metal cladding. Skepticism was voiced as to whether any dramatic improvements in ceramic breeder performance could be achieved by materials advances alone. Ceramic breeder issues concern thermomechanical performance, control and predictability, as well as engineering design simplicity. These issues require non-nuclear engineering tests on the sub-component and component level, along with modeling, for resolution and possible performance enhancement. These same activities need to be performed for Be-multiplier pebble beds. Both the EU and Japan have active solid-breeder R&D programs in these areas.

2.7.5 Modeling and Computational Methods

In parallel to the advances in high performance materials, there have been pronounced advances in materials science modeling and computational analysis. These advances, when coupled with appropriate experiments, can be used to develop materials based on fundamental understanding of phenomena from the atomic scale to the macroscopic scale. For example, “materials by design” is now being applied in the materials science community to guide the development of improved alloys that are tailored for specific applications. In addition, recent advances in computational materials science allow for multi-scale modeling of the overarching considerations of the effects of irradiation-induced microstructural changes on the constitutive deformation equations and fracture properties of materials.

2.7.6 Suggestions for Key Materials Issues and R&D Strategies

The presentations and discussions during the Materials Advances sessions produced a wide variety of suggestions regarding material issues and R&D strategies. These are highlighted in the following. The participants in the sessions represented experts and interested parties in most areas of Advanced Technologies, Materials, and Design and IFE Chamber Technology, as well as some of the areas of Enabling Technologies (plasma facing materials and safety). Unfortunately, no presentations on magnet materials were during the Materials sessions.

2.7.6.1 Overall approach to materials issues and strategies

To the greatest extent possible, advanced materials R&D should address key feasibility issues associated with both MFE and IFE design concepts. Examples include chemical compatibility of structural materials and liquid metal/salt breeders and coolants, development of ceramic coatings for tritium barriers, and radiation damage to structural materials from low to intermediate damage levels. The approach should not limit itself to existing structural alloys. Rather, it should be expansive enough to include new alloy modifications to enhance the feasibility of high power density, as well as intermediate-to-high power density, concepts in the areas of fabrication and joining, chemical compatibility, and mechanical performance at low-to-intermediate damage levels. Cost-effective strategies should be developed for concept feasibility issue resolution ranging from bench-top experiments, to flowing liquid metal/salt loops, to low-damage level (<10 dpa) fission reactor experiments. The experimental efforts and alloy development should be strongly coupled to a materials modeling effort. Such a strategy focusing on key feasibility issues for the near term (with complementary experiments and computer modeling) has been outlined in the Advanced Materials Program plan for several structural materials, and should be developed for materials R&D programs in the other parts of the fusion program.

One suggestion raised during the Materials Advances sessions was the possibility of forming a “fusion materials committee” composed of fusion materials experts involved in the various sectors of the fusion program (structural materials, high-heat flux materials, confinement, diagnostics, plasma heating, etc.). This committee could serve as the point of contact for addressing a broad range of materials questions. The goal of such a committee would be to utilize the experience and expertise both within the fusion community and within the broader range of materials departments and divisions/sections within the universities and the national laboratories. Although there were no presentations given in the area of magnet materials, a couple of examples were given as to how the development of magnet materials might be enhanced by broader involvement of the materials community. Copper alloys are candidate magnet materials for non-superconducting magnets. There should be closer interaction between the copper-alloy development program within the AMP and the magnet materials program. In the area of superconducting magnets, the progress made by non-fusion groups (e.g., the ceramics processing groups at the various national laboratories) should be made readily available to the fusion magnet materials effort.

Several important points were raised regarding the role and image of the materials community in the overall field of fusion technology. It was emphasized that the materials community should be more active in communicating how specific advances in materials translate into improving the vision for a more attractive fusion energy system. This raised an interesting question as to how this could be accomplished. The current way that the materials community communicates its advances to the broader fusion community is through interaction with, and support work for, advanced reactor design studies. Within these studies, design concepts are proposed to enhance the performance of chamber systems and overall power plant economics and safety. The limitation of this current approach is that materials advances are somewhat "buried" in the context of the larger systems studies. The challenge for the materials community is to be both more proactive in its interaction with the other branches of fusion technology and more "self-promoting" so that its contributions to fusion technology development are well communicated and understood by the broader fusion community.

2.7.6.2 Specific recommendations

Specific recommendations were made by presenters and discussion participants. These are highlighted below.

2.7.6.2.1 Solid-Breeder/Be-Multiplier Designs

The EU and Japan have included solid-breeder concepts in their DEMO reactor designs. Experiments are being conducted on a sub-component engineering scale, particularly in the areas of the effective heat transfer of breeder and Be pebble beds and of tritium retention and release. The current EU solid-breeder blanket design is a He-cooled and -purged Li_4SiO_4/Be breeder/multiplier with ferritic steel structural material. The backup solid breeder ceramic is Li_2TiO_3. In Japan, solid-breeder blanket designs have been proposed using either Li_2O or Li_2TiO_3 as the breeding ceramic. Also, some past design studies in the U.S. have considered He-cooled SiC/SiC structural material for the solid breeder designs. Because solid breeder designs have performance limitations, rather than major feasibility issues, the U.S. should, at a minimum, keep abreast of the progress made by the EU and Japan in the area of solid breeder performance and design. This would provide the U.S. with a low-performance, low-development-risk back-up design that may be useful in next-step machines. Low-cost areas in which the U.S. could participate (in order to have access to the EU and Japanese data) are: independent database assessment, thermomechanical and tritium-release modeling, and pebble-bed testing on a sub-module scale.

Although solid breeder designs do not have major feasibility issues, there are a number of performance issues that are being considered within the EU and Japanese R&D programs. These include optimization of breeder/Be/structure/coolant volume fractions to achieve breeding ratios > 1, optimization of protium additions to the He purge to both minimize tritium inventory and facilitate tritium recovery from the purge, development of tritium barriers to minimize permeation losses to the coolant, improving the predictability and controllability of the thermomechanical performance of pebble beds, and determining the life-limiting mechanisms (e.g., swelling, Li-burnup, compatibility, etc.) for solid breeder blankets.

2.7.6.2.2 Intermediate-to-high power density solid-wall designs

Ideas for materials advancement and testing were proposed for these design concepts beyond what is in the current AMP roadmap.

The ARIES-ST design calls for low-performance SiC or SiC/SiC thermal-insulator material in contact with Pb-Li liquid breeder at temperatures up 700°C. The Advanced ARIES-RS design calls for high-performance SiC/SiC structural material in contact with liquid Pb-Li at temperatures as high as 1000°C. There are no reliable data on the compatibility of these materials in either stagnant or flowing Pb-Li. Low-cost R&D could be performed in this area.

The current V-4Cr-4Ti alloy that is included in the AMP roadmap is compatible with liquid Li coolant. Because of the affinity of vanadium alloys for oxygen and the embrittling effects of excess interstitial oxygen, no serious consideration has been given to He-cooled/vanadium-alloy designs unless the He coolant could be purified to << wppm levels of oxygen and moisture. However, relaxation of the current activation guidelines, which may allow for the use of Al as an alloying element, would open up the possibility of developing creep-resistant V-Ti-Al alloys with sufficient oxidation-resistance to allow for non-Li-cooled/vanadium-alloy first-wall/blanket systems to be considered.

He-cooled tungsten alloys have been proposed for high heat flux (divertor) plasma facing components in the ARIES-ST design, as well as other design concepts. Along with the development of better tungsten and tungsten alloys, chemical compatibility issues should be considered to determine acceptable oxygen and moisture impurity levels in the He coolant. Similarly, for He-cooled W and Mo alloys proposed by APEX for high power density designs, the same compatibility issues should be considered. If there are any “holes” in the current database, then low-cost, near-term R&D could be performed in this area that would help to determine if such compatibility was a performance issue or a feasibility issue.

Regarding the potential activation of W and Mo alloys, pure W could qualify as Class C waste, but alloying and impurity elements may not allow it to be qualified as Class C waste. Mo alloys do not qualify as Class C waste material. Significant improvements in performance would have to be realized to offset activation and waste disposal concerns.

A question was raised regarding the status of using low-activation ferritic steels with liquid Li coolant. Although advanced ferritic-steel/liquid-metal designs have been proposed using Pb-Li as the breeder and/or coolant, no current designs are proposing the ferritic-steel/Li system. Some consideration should be given to the allowable temperature window for such a materials combination based on low-temperature embrittlement, compatibility, and thermal creep. As the data for such an evaluation are available, the use of ferritic-steel/Li is more of a systems design issue than an R&D issue.

2.7.6.2.3 High power density liquid-wall designs and new IFE designs

While improvements have been made in the area of carbon fiber composites (CFCs) as candidate PFC materials and IFE first wall material, the issue of high tritium retention, particularly in redeposited regions, remains for MFE systems. However, if CFCs could be operated at $T \geq 1000°C$ in IFE systems, then tritium retention may be less of a concern. This may, however, transfer the problem of tritium retention further into the blanket. Another issue discussed for IFE systems was the basis for the choice of existing heats of 304L austenitic stainless steel as a low-cost, low-activation structural material in thick-liquid-wall concepts. With current impurity levels of Nb, extra processing at increased cost would be needed to qualify this material as “low-activation.” Closer interaction between the materials community and the IFE community would be beneficial to the development of IFE reactor design concepts.

Liquid wall designs have many feasibility issues beyond materials issues. However, based on information presented in the plenary sessions and the Liquid Wall topical sessions, there appears to be considerable enthusiasm regarding the potential of liquid walls. During the Materials Advances sessions, serious consideration was given to feasibility issues that could be addressed within the materials program. These areas (compatibility, coating development, etc.) have been highlighted in earlier parts of this report. If the feasibility issues are resolved favorably, then liquid-wall designs could have a significant impact on long-term R&D for structural materials. Thick liquid walls would result in decreases in both neutron damage rate and helium transmutation rate. Significant reduction in the He production rate may render current fission reactors more relevant for near-term testing. However, the question of the eventual need for a high-intensity 14-MeV neutron source to resolve life-limiting issues related to simultaneous neutron damage and helium transmutation remains an open one.

2.7.7 Summary and Recommendations

The Materials Advances sessions offered a valuable opportunity for open discussion on a wide range of materials issues and ideas for R&D to resolve these issues. However, time did not permit a full resolution of the two main questions addressed in these sessions. The first question was: "What advances may be possible over the next ten years that can contribute to improving the vision for an attractive and competitive fusion energy system?" It became clear through the discussions during the Materials Advances sessions, as well as in other topical sessions, that materials performance goals which lead to "an attractive and competitive fusion energy system" must come from systems design studies such as those performed currently and in the past by the ARIES Team. In these studies, tradeoffs are considered among power density, component lifetime, capital and operating costs, safety, and waste disposal. The ARIES-RS systems study established materials performance goals and issues for a vanadium-alloy/liquid-Li first-wall/blanket and divertor system, along with the shielding requirements to protect the vacuum vessel and magnets. The ARIES-ST systems study established performance goals for a He-cooled, ferritic steel first-wall and blanket structure with Pb-17Li breeder, for a He-cooled tungsten divertor, for the associated shielding and for the copper-alloy center post magnet. The current advanced ARIES-RS study is considering a high-temperature He-cooled, SiC/SiC first-wall/blanket structure with Pb-17Li breeder to optimize thermal efficiency. Even more advanced first-wall/blanket systems and divertors are being studied within the context of the APEX and ALPS programs, respectively. These studies, along with parallel IFE systems studies, should determine which materials and materials combinations are to be included in the fusion materials programs and what performance goals lead to attractive and competitive fusion energy systems. In turn, the fusion materials program should determine the issues associated with these materials combinations and goal operating conditions, as well as to develop advanced materials whose capabilities approach these performance goals. With the exception of SiC and SiC/SiC compatibility with Pb-17Li and refractory W and Mo alloys, many of the materials and materials issues are being addressed by the current Advanced Materials Program R&D plan.

The second question addressed in the Materials Advances sessions was: "What advances may be possible in materials over the next ten years that can contribute to lowering the cost and time for fusion R&D?" Although anticipated advances in materials were discussed, along with feasibility and proof-of-principle issues that could be addressed during the next decade, time did not permit either the prioritization of R&D issues or specific time tables for addressing these issues. The Advanced Materials Program R&D roadmap was mentioned several times during the course of the discussions for the development of low-activation ferritic steels, vanadium alloys, SiC/SiC composites and copper alloys. It was also suggested that this roadmap could serve as a model for establishing the development path of non-structural materials. The AMP R&D plan does an excellent job of identifying key materials issues, pathways for resolving these issues, and deliverables through 2005. However, this plan would have to be extended through 2010 to address the time frame identified for the Snowmass meeting goals. It would also have to include additional prioritization of R&D activities “across” the different structural materials, along with dates for decision points, to include the level of detail required of a model roadmap. Once this is completed, the approach could be used for non-structural materials. In principle, these roadmaps for individual materials and materials combinations could be used to create an overall roadmap for fusion materials development (which could then be integrated into a master fusion technology roadmap). In practice, this last step could prove to be very challenging.

3. Plasma Support Technology

Subgroup Leaders : *R. Callis, S. Milora*

3.0 Introduction

Plasma Support Technology covers plasma heating, current drive, fueling, IFE target fabrication and injection, and magnets.

Prior to the Snowmass meeting, three topics were selected as the most useful to discuss at Snowmass. Each topic was formulated as a key question and given a number (PQn: Plasma Technology Question n). These topics were as follows:

PQ1: Heating/Current Drive/Fueling
PQ2: Magnets
PQ3: IFE Targets

Summaries of the Snowmass discussion sessions are given in Sections 3.1 through 3.3.

3.1 Heating/Current Drive/Fueling Technologies

Key Questions

What is the potential for and what advances will be required in profile control technologies (plasma heating, current drive and fueling) to enable present, near term, and next step devices to meet their performance goals and ultimate research potential?

Topic Leaders
David Swain (ORNL), Rick Temkin (MIT)

Core Working Group
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3.1.1 Heating, Current Drive, and Fueling: Overview

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. The table below shows specific needs for operation of next-generation experiments identified by different working groups.

Needs for improved profile control identified by Working Groups at Snowmass

Group	**Needs**
MFE Transport Physics	Control turbulence and transport/optimize confinement – Sharpen up current drive, heating, flow drive, and fueling tools – Tokamak, ST, RFP, Spheromak
MHD Physics	Avoid/mitigate disruptions and control tearing modes – Profile control, current drive, RF stabilization
Steady-state	Continuously sustainable high performance fusion plasma

Physics	– Equilibrium, MHD, profile control, using IBW/ EBW(ITB), HHFW/OFCD/LH/EC(CD), SC magnets
Burning Plasma Physics	Fueling (for burn control), current drive, disruption mitigation
Wave Particle Physics	Develop reliable rf plasma control techniques for j(r) control P(r) control by localized heating and fueling IBW for shear flow (has great potential for ITB if developed)

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 β 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.

3.1.2 Heating and Current Drive

- **Heating and non-inductive current drive of plasmas can be supported using several different technologies, sometimes with several different systems being used on the same machine, depending on the plasma parameters and performance objectives.**

3.1.2.1 Neutral Beam Injection Technology

Issue	**Opportunity**
Ion Source performance (sensitivity to O_2 leaks)	**Participate in the negative ion source development at JT-60U and LHD**

Negative Ion Neutral Beam Systems

Neutral beam systems based upon positive ion sources have been the workhorse technology for most of the heating on large fusion devices. Their appeal stems from two fundamental characteristics: the efficiency with which they couple their power to the plasma is only weakly sensitive to details of the plasma edge condition and shape, and the energy transfer is almost entirely through simple two-body collisions. Their performance is boringly predictable. JT-60U and LHD are bringing into operation the first generation of negative ion based neutral beams, which will allow access to the high energies which are needed for larger and denser plasmas. A number of physical processes which were initially limiting the performance of these sources are now becoming understood and ameliorated. However, the most intractable remaining problem seems to be that these sources use cesium, which is readily oxidized by even tiny air or water leaks, resulting in substantial degradation in negative ion current density.

In order for negative ion based beams to eventually become the robustly reliable heating systems which their positive ion predecessors were, it may become necessary to either develop a new generation of sources which avoid the use of alkali metals, or alternatively to develop the technology to preclude the possibility of air or water leaks. The cesium-exclusion approach would involve using a small test stand to explore the basic ion source physics to look for better ways to supply low energy electrons to the extraction plasma to form negative ions. The water-and-air-exclusion approach would be an engineering effort devoted to adapting heat pipe technology to replace water in beam systems, and developing techniques for jacketing all vacuum seals in nitrogen gas. Participating in the development of a new generation of robust negative ion neutral beams could be a low cost, high leverage opportunity for the U.S. to expand its now small niche in negative ion development. This could most probably be implemented by modestly expanding the scope of the present U.S. collaboration with the Japanese negative ion groups.

Neutral Beam Source Based on an RF Quadrupole Accelerator

Energetic neutral beams are useful for magnetic fusion or ICF where the minimization of space charge effects is desirable. However neutral beam sources are often expensive and large in size. A new approach is presented here for the production of neutralized beams, which are defined as beams containing positive and negative ions at overlapping locations, or beams of fast neutrals formed from the recombination of positive and negative ions. This method uses oscillating electric fields to confine and accelerate ions in a wave structure. Positive and negative ions are trapped in adjacent potential wells of this propagating wave and gain energy as the wave accelerates axially. At the end of acceleration positive and negative ions are induced to overlap axially and to recombine. The recombination rate depends on the density of ions and their relative velocities. Recombination rates can be enhanced by using additional agitating RF fields to induce overlapping encounters between ions. The high efficiency results from the use of low voltage AC drive, the high confinement rate (>90%) of ions, and the low loss in the AC circuit. An rf quadrupole (RFQ) of small radial dimension (mm size) can be used to impose this wave structure. The small dimension allows a high ion density to be attained for a given externally imposed potential. These small modules can be combined to yield the desired total beam current.

3.1.2.2 Electron Cyclotron Heating Technology

Issues	Opportunities
Reliability **(increase power, location, pulse length)**	**Develop higher power, longer pulse length, more robust (higher safety margin) gyrotrons and launchers for DIII-D**
System Cost **(reduce cost by a factor of 2 to 3)**	**Develop higher efficiency gyrotrons at higher unit power level, participate in international collaborations (JT-60U, LHD, W7X, etc.)**

The physics of EC heating (ECH) and current drive (ECCD) is well developed in both experiment and theory. Experiments have been performed over a period of more than two decades in mirrors, tokamaks, and stellarators. These experiments have demonstrated effective plasma heating, with reliable access to the H-mode. Experiments have also demonstrated current drive, with recent work on off-axis current drive demonstrating higher efficiency than expected. In other experimental work, plasma startup has been extensively studied, as well as control of sawteeth, ELMs, locked modes, and control of m = 2 modes and some types of disruptions. The fundamental properties of EC wave absorption and propagation lead directly to its application as an auxiliary heating and current drive mechanism in fusion plasmas. High frequency, multi-megawatt systems are needed for the proposed applications, particularly off-axis current drive. Over the past decade, steady progress in the technology has been made and recently, sources and systems of the required power and pulse length are beginning to come on line. Several experiments have or plan multi-megawatt ECH installations (DIII-D, LHD, JT-60U, TCV, W-7X, and ASDEX).

R&D needs include: Reliable and robust gyrotrons in the range of 110 – 170 GHz; minimum source output power in excess of 1 MW, with a goal of 1.5 to 2.0 MW; system cost reduction through the development of higher efficiency gyrotrons (single and multi-stage depressed collectors) and higher unit power (1.5 – 2.0 MW) sources; development of steerable injection systems (antennas and launchers) and development of efficient gyrotron internal mode converters producing Gaussian output beams. Longer-term priorities concern themselves largely with improving the efficiency, economics, and performance of an ECH system for next stage and ignition devices. The goals here include development of very long-pulse / CW tubes and hardening of transmission and injection components under long pulse operation; development of frequency tunable sources such as step tunable gyrotrons together with broadband transmission systems to deliver the power to the plasma; and development of higher frequency sources. It is also highly desirable to complete the test stand for megawatt gyrotrons which was installed at CPI but never successfully brought on line.

3.1.2.3 Lower Hybrid Range of Frequency (LHRF) Technology

Issue	**Opportunity**
Reactor relevant launchers (steady state, high power, high wall loading, close to plasma edge)	**Research steady state issues, possibly through international collaboration with KSTAR**

One of the most important needs of both present-day and future tokamak devices is a technique for controlling the off-axis current density, especially in the presence of an internal transport barrier (ITB). Ideally this technique must maintain $q_{min} > 2$ and $(r/a)_{qmin} \geq 2/3$ in order to insure MHD stable operation at the β-limit and beyond. The current profile control technique must also be shown to be feasible at reactor-relevant densities and for pulse lengths exceeding the current relaxation time. The scientific basis of LH current profile control is well established. Recent experiments in JT60-U have demonstrated maintenance of a low density reversed shear configuration for 6 s at $I_p = 0.85$ MA and $B_0 = 2.0$ T with $(r/a)_{qmin} \approx 0.6$ using 2.3 MW of off-axis LH current drive power. In JET optimized-shear plasmas, a broad q-profile with negative central shear was formed with moderate LH power (2 MW at 3.7 GHz) during the initial low density current ramp-up phase ($n_e(0) \leq 1.5\text{x}10^{20}$ m^{-3}).

In next step devices, an adequate cooling design will be required at the window location of the grill. The use of a quasi-optical grill design or hyperguide (fixed $n_{\|}$) may provide promising alternatives in a reactor environment. However further technological development is needed in this area. For LHRF applications in next-step devices, CW source development at 5.5 GHz and the 1 MW level will also be required. Based on the present state of source development this goal can definitely be achieved.

3.1.2.4 Ion Cyclotron Technology

Issues	**Opportunities**
Reliability and Flexibility **(plasma power coupling, dynamic load variation)**	**Enhance existing ICH systems (DIII-D, CMOD, NSTX, etc.). Participate in international collaborations (JT-60U, LHD, JFT-2M, KSTAR, JET, ASDEX, etc.).**
Launcher Power Density **(more power, smaller port)**	**Improve the understanding of voltage limitations. Apply to present machines (DIII-D, CMOD, NSTX, etc.) and through international collaborations (JT-60U, LHD, JFT-2M, KSTAR, JET, ASDEX, etc.). Develop and test new antenna concepts.**

The long-term goal of the ion cyclotron program R&D is to develop reliable, advanced ion cyclotron heating and current drive systems. Ion cyclotron heating and current drive have been demonstrated on a number of tokamak and stellarator experiments. However, in order to meet future needs the present technology must be improved. *Incremental* development can provide sufficient capability/reliability, but significant improvements in power-handling capability would make IC much more attractive for designers of future devices, since more power could be delivered through fewer/smaller ports.
The needed R&D identified by the Snowmass participants includes:

Higher-power density launchers: There are two routes that can be taken: first, to improve voltage and power-handling capability of present-day launchers and second, to develop new launcher technology concepts. Both should be pursued.

Long-pulse operation, reactor survivability: R&D is needed to sustain long-pulse, fully noninductive discharges using fast-wave and/or mode-conversion current drive to maintain current and/or current profile control. Testing of IC launchers in increasingly long-pulse and high-power conditions with increasingly robust plasmas will be needed to convincingly demonstrate this capability.

Improved Control: In order to work with rapidly varying loads, control current-drive efficiency, and improve reliability, fast automated control systems must be developed that: detect arcs in the rf components and protect the system from their effects, deliver a large fraction of available rf power to the plasma, even during rapid changes in plasma loading, and allow real-time control of current drive directivity while maintaining high heating power to the plasma.

<u>Improved understanding of Ion Bernstein Wave (IBW) physics and technology:</u> One of the major outcomes of Snowmass was the need for improved control of the internal transport barriers (ITB) in tokamaks (and other concepts). One of the promising avenues for ITB control and modification identified at the meeting is the use of IBW to control shear flow, thereby affecting ITB formation. However, experiments to date using IBW have had mixed results Some experiments (notably the recent results on FTU) have shown unequivocal evidence of heating of the plasma by IBW, while in other experiments the power "launched" by an IBW antenna did not appear to cause any experimentally measurable heating.
The potential of IBW for profile control is such that a significant effort is needed to understand the physics and technology of IBW launchers and the resulting interaction of the launched waves with the plasma. This will require coordination among theory, experiment, and technology R&D.

<u>Increased system reliability:</u> Some experiments (e.g., TFTR) have developed IC systems that have been very reliable, consistently delivering a large fraction of the installed IC power to the plasma with high reproducibility and reliability; in other experiments this is not the case. Often the components that cause low reliability are outside the tokamak; common "weak links" are transmission line insulators, tuning and matching components, and rf sources. Two improvements are needed: first, identifying ways to operate the components more reliably (e.g., operating them at lower fractions of their ultimate failure parameters); second, developing techniques to detect and isolate problems quickly when failures occur (e.g., being able to detect and determine the location of arcs when they occur in transmission lines and matching systems).

3.1.2.5 Helicity Injection for Current Profile Control

Issue	**Opportunity**
Efficiency & effectiveness (demonstrated for startup, may not be compatible for well-confined plasmas, impurity concerns from electrodes)	**Basic physics is more urgent than technology development. Test start-up concepts on larger devices, participate in international collaborations (JFT-2M, other machines)**

In a system of nested closed magnetic surfaces, the magnetic helicity H is the linkage of toroidal flux ψ_T by poloidal flux ψ_P, with $H(\psi_P) = 2\int \psi_T \, d\psi_P$. Since the safety factor q is $d\psi_T/d\psi_P$, specification of $H(\psi_P)$ and $q(\psi_P)$ are equivalent, and either is equivalent to specification of the current profile in a given axisymmetric toroidal equilibrium. The available theory for transport of helicity requires breaking the magnetic surfaces if helicity is to be transported radially appreciably faster than the Ohmic diffusion rate.

Therefore, it is very important to identify helicity injection techniques that minimally open magnetic surfaces.

Helicity injection current drive has been demonstrated in some toroidal systems. Oscillating fluxes current drive (OFCD) has been applied experimentally to RFPs and tokamaks, but it was difficult to discern the current drive effect with the available diagnostics. OFCD will be tested again on the MST RFP. Classically, the oscillating fluxes do not penetrate deeply into hot plasmas, but in RFPs helicity can be transported farther inward by surface-breaking terms, due to instabilities generated as the plasma stays near its relaxed state. DC helicity injection (DCHI), which typically uses magnetized electrodes in the plasma scrape-off layer, has been applied to spheromaks, RFPs and tokamaks. The CTX and SPHEX spheromaks were fully driven by DCHI, as were spherical tokamak plasmas in the HIT machines. The edge injection produced current drive throughout the plasma, but it was accompanied by strong MHD activity, as expected theoretically. The injection of helicity-containing spheromak plasmoids into a larger plasma also drives current, as was demonstrated on the tokamaks Encore, STOR-M and Tokamak de Varennes. The plasmoid injection speed must be fast enough to penetrate the main plasma magnetic field to a desired depth, where the plasmoid reconnects with the main plasma and deposits its helicity. Spheromak injection is the only technique to date that offers the conceptual possibility of control of the internal current profile. DCHI is useful to start up toroidal plasmas and ramp their current, while both OFCD and DCHI might be useful to control edge current. Basic physics is more important at present than technology development.

3.1.3 Fueling

- **A critical issue for fueling in next-step device plasma regimes is the degree to which density profile peaking is needed (peaking of the density profile for higher fusion reactivity and reducing transport via turbulence suppression) and the technological requirements to meet that need (pellet speed and injection location, CT density and the physics of CT deposition).**

Fueling system functions for reactors and similar scale fusion devices are: to provide hydrogenic fuel to maintain the plasma density level and profile shape for a specified fusion power, to replace the deuterium-tritium (D-T) ions consumed in the fusion reaction, to establish a density gradient for plasma particle (especially helium ash) flow to the edge, and, to supply hydrogenic edge fueling for strong scrape off layer flow for optimum divertor operation. Additional functions related to fueling are: the injection of impurity gases at lower flow rates for divertor plasma radiative cooling and wall conditioning, and plasma discharge termination on demand by either injection of massive gas puffs or liquid jets/solid pellets.

3.1.4.1 Pellet Injection

Issue	Opportunity
Deep pellet penetration (increase velocity by a factor of 2)	**Develop and test high-speed injectors on existing machines (DIII-D, C-MOD, etc.). Combine the attractive physics feature of high field side (HFS) launch with high pellet velocity. HFS pellet injection deposition mechanism is not well understood, needs additional testing and theory development.**

Pellet injection is an established plasma refueling method, and its development has spanned the last 20 years or so. This technology has enabled important new results in experimental plasma physics with studies of internal transport barrier formation (PEP mode and pellet induced H-mode) and operation at high density above the empirical density limit. Recent results with the direction of pellet launch along the magnetic field gradient have led to a deeper understanding of the ablation and relaxation physics associated with the penetration of pellets. Deeper mass deposition of the pellet and higher fueling efficiency is found with high field side (HFS) injection than from the traditional outside midplane injection. This yields stronger peaking of the density profile and the possibility to more easily overcome the empirical density limits, which are both desirable features for future fusion reactor scale devices.

For pellet injection the major research and development issues and opportunities are as follows: Density profile control is needed that can produce peaked density profiles that are more favorable for high fusion yield. This requires development of pellet fueling technology that can meet the needs for density profile control, which includes injection from high-field locations and high-speed pellets to maximize penetration of the pellet. The development of high speed injection technology that can take advantage of the magnetic field gradient (high-field-side launch) is needed for density control in future fusion devices. High fueling efficiency is needed to provide core fuel that leads to a high burn-up fraction and minimization of tritium inventories. A better understanding of the mass deposition process and penetration depth scaling for pellets injected from the high-field side is needed to project the fueling needs in future next step and reactor devices such as FIRE and ITER-RC.

3.1.4.2 Deep Fueling of a Tokamak with Accelerated FRC's

Issue	Opportunity
Efficiency and effectiveness (penetration depth, density, impurities, rep-rate)	**Could be tested on an existing magnetic device (DIII-D, C-MOD, etc.). A good candidate for international collaboration (JFT-2M, LHD, etc.)**

Deep fueling with an accelerated Compact Toroid (CT) such as the Field Reversed Configuration (FRC) not only provides for the replenishing of burned fuel, but allows for the possibility of density profile control by varying the depth of penetration. There are other CT plasmoids (such as the spheromak) that have been used initially for fueling, but for the following several reasons, the FRC is found to be the best candidate for deep fueling. The FRC has the highest β (~1) of any magnetized plasma due in large part to the simplicity of the magnetic configuration, which is comprised of only a poloidal field that becomes vanishingly small near the field null. The lack of significant configurational magnetic energy minimizes the formation energy requirements as well as maximizes the CT mass load. Since the toroidal current necessary for confinement is produced inductively, the FRC is formed without electrodes. After formation and during acceleration, the FRC is kept magnetically isolated from the chamber by an axial field. Due to the prolate FRC shape, only a small port of ~ 10 cm radius will be required for FRC entry. The much longer Alfven time for the high β, prolate FRC also allows for the FRC to penetrate much deeper before disruption and disassembly. Experiments on the TRAP device demonstrated that FRC's of 0.6 milligram mass (D_2 fill) could be accelerated to velocities > $2\text{x}10^5$ m/s with peak densities of $1.5\text{x}10^{22}$ m^{-3}, and then translated over several meters without disruption, wall contact or significant inventory loss. These FRC's were then observed to penetrate a transverse field gradient with a peak field ~ 1 T. Given the parameters achieved on TRAP, the next step developments needed for an FRC fueler are: first, a single pulse FRC injection experiment on a device such as DIII-D or JET to quantify and understand the fuel deposition and the effect on the tokamak plasma confinement and second, continued development of the pulse power technology that will be required for a repetitive fueler.

3.1.4.3 Disruption Mitigation

- Reducing the severity of disruptions could allow the advanced tokamak to operate nearer its ultimate potential. Pellet fueling and massive gas-puffing technology has 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.

Issues	Opportunities
Disruption detection (reaction time, reliability)	**Use existing machines (DIII-D, C-MOD, etc.) to develop concepts and test robustness of a detection system. International collaboration (JET, ASDEX, JT-60U, etc.).**
Runaway generation (need low Z)	**Develop low-Z mitigation techniques (i.e. massive gas-puff, liquid jet injector, etc.).**
Integrated reactor relevant mitigation **(detection, energy removal, recovery, reliability)**	Integrate detection and mitigation into the control system of an existing tokamak to test the full system, and demonstrate reliability.

Mitigation techniques such as the massive gas puff have proven successful in staged mitigation tests where more than 90% of the thermal energy of the plasma is radiated away and the halo currents are reduced by more than a factor of 2. The scaling of this technique to a reactor size device needs to be determined. The further development of disruption mitigation methods needs a comprehensive, broad-based effort among multiple tokamaks. Techniques and procedures for detection of disruptions can proceed in parallel with existing tokamak experiments with virtually no interference between the two. The development of low-Z mitigation hardware such as the liquid jet injector is another parallel development that does not interfere with existing tokamak experimental programs. In the longer term the final area requiring increased effort is the integration and testing of the mitigation techniques into the tokamak control system. Of course, the actual testing of the mitigation will require either additional tokamak experimental time or the displacement of other experiments. Finally, innovative ideas in this area need to be encouraged.

3.1.5 International Collaboration Opportunities in Plasma Profile Control Technology

The US technology community has long enjoyed a rich tradition of pioneering international collaborations that have advanced the science and technology objectives of the US fusion program. In many instances, the development of unique hardware and subsequent application of that hardware on foreign facilities has provided the broader scientific community with access to unique facilities that complement US capabilities. For example, science collaborations on Tore Supra and JET followed the deployment on those devices of advanced heating, current drive, fueling and power handling technologies. These interactions have resulted in a significant leveraging of program resources with the following benefits to the scientific and technology community:

Science Program

- Collaborations provide access to facilities that complement US capabilities.
- The combination of unique technology and unique facility capabilities often leads to science advances (e.g., the PEP mode or enhanced shear reversal on JET).
- Advanced technologies are “exported” to the US program (e.g., the DIII-D pellet injector).

Technology Program

- Costs and knowledge are shared through joint systems development thereby leveraging technology resources.
- Developers are provided with access to unique or advanced physical plants (e.g., rf and tritium plants on JET).

- Collaboration partners provide dedicated operations for testing and validation of US developed hardware and concepts (e.g., combline antenna testing on JFT-2M).
- Involvement in collaborations sharpens critical expertise and maintains core competencies.

The involvement of the technology community in these types of collaborations is widespread and spans the breadth and depth of the confinement device portfolio from concept exploration to performance extension and burning plasma phases of tokamaks and alternate confinement concepts. Recent technology contributions include R&D on a wide spectrum of devices including JET (tritium and rf technology), LHD (high power ICRF antenna, ECH technology and PFCs), Tore Supra (pellet fueling, ICRF heating), START/MAST/TJ-II (neutral beam technology), JFT2-M (advanced ICRF antenna development), KSTAR (design of magnets, heating and current drive systems and PFCs), and GAMMA 10 (pellet fueling). Many of the US institutions involved in these collaborations have received their funding from the foreign facility. The importance and future contribution of technology in the MFE area is recognized in the OFES Strategic Plan for International Collaborations in Fusion Science and Technology Research. The stated focus of the Department's strategy with regard to enabling technology is to "deploy US technologies to access test conditions unavailable domestically, particularly on scientific issues related to long pulse/steady-state operation, high-power densities, and reliability." Of particular importance to the US plasma science program are collaborations in the areas of (1) burning plasma physics and the achievement and sustainment (long pulse) of high performance plasmas. The technology community is well positioned to exploit opportunities in all of the following major experimental programs
:

- Performance Extension Tokamaks
  - JET (burning plasma/ITER advanced operating scenarios –application of Fast Wave heating technology and inside pellet launch technology/physics)
  - Tore Supra (long-pulse advanced operating scenarios – long pulse Fast Wave current drive launcher testing)
  - JT-60U (advanced tokamak, - negative ion beam technology)
  - ASDEX U (divertor R&D – advanced Fast Wave launcher development)
- Proof-of-Principle Tokamaks
  - JFT-2M (testing of advanced rf launcher)
  - KSTAR (long pulse advanced tokamak – ICRF/Lower Hybrid launchers)
- Performance Extension Stellarators

  - LHD (Folded Waveguide launcher tests, pellet injection technology, ECH technology, CT fueling physics)
  - W-7X (ECH technology)
- Proof-of-Principle Alternates
  - MAST (spherical torus – long pulse NBI technology)

3.1.6 MFE Plasma Support Technology Opportunities for Heating, Current Drive, and Fueling

	ICH	**ECH**	**Helicity & CT Inject.**	**Pellet Injection**	**NBI**	**Lower Hybrid**
C-Mod 3-5s	ICRH FWCD IBW upgrade	170 GHz ECCD upgrade				LHCD
NSTX 5s	HHFW,H&CD,IBW, wavegd.	EBW ECH Upgrade	Coaxial HI	Upgrade		
DIII-D 5-10 s 20s	Faraday shield upgrade	ECCD 10 MW upgrade		Inside Launch		
NCSX 1 s	HHFW, IBW Electron heating preferred	ECH Startup EC Heating Upgrade		?	Long pulse upgrade	Upgrade?
KSTAR* 20 – 300 s	FWCD	ECCD Upgrade		?	8 – 20 MW	LHCD
JET* 20 s	Reliability, Antenna upgrade			Inside Launch Upgrade		
LHD* CW	IBW, Steady – state	CW gyrotrons	CT	Repeating		?
Tore Supra* 100 s	FWCD			Repeating		LHCD
W7-X* CW	ICRH	CW gyrotrons		Repeating		
MST			Oscill. field CD Rotomak			
CDX	IBW, 0.5 MHz HHFW,10 MHz	ECH startup 2.45 GHz, EBW 10 GHz	Rotomak edge CD			1 MHz Alfven
FIRE	ICRH			High v; inside launch		
LDX		Multi-frequency				
LAPD	α channeling, IBW	ECH Startup EC Heating				
Pegasus		EBW Steady-state				
JFT-2M*			Rotomak?			

*** US Technology International Collaboration Opportunities**

3.2 Magnets

Key Questions

What can be done to lower the cost, improve cost/performance ratio, and improve the reliability and maintainability of magnets (rings within rings) in MFE systems? What are the most promising new magnet technologies (e.g. high T superconductors and high field capability)? What is the role of evolutionary concepts (e.g. Copper magnets) in burning plasma devices and in innovative confinement concepts?

Topic Leaders

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3.2.1 Summary of Opportunities, Goals & Strategy for Magnet Systems

Introduction

MFE configurations rely on dc and/or pulsed magnetic fields for plasma initiation, confinement, ohmic heating, inductive current drive, plasma shaping, equilibrium and stability control. Inertial Fusion Energy (IFE) may also utilize magnetic fields, for example, in beam focusing quadrupole fields in the Heavy Ion Driver (HID) concept or Helmholtz coils in KrF lasers. These magnets may use either resistive conductors or superconductors. The majority of past and present magnetic fusion devices use normal resistive magnets, but almost all of the recent large fusion machines built or being built outside the United States (e.g. LHD, Wendelstein VII-X, KSTAR, SST, and HT7-U) use superconducting magnets.

There are 3 ways in which advances in magnet technology can lower the cost of fusion experiments and power production:

- reduce the cost of the conductor and magnet components and/or assembly processes,
- optimize the magnet systems, so that the cost of other fusion subsystems may be reduced, and
- provide conductor and magnet performance which substantially increases or optimizes the physics performance, e.g. increased magnetic field or some special magnetic field configuration.

These were addressed at Snowmass by defining metrics and using these metrics to set performance goals for the major components. The opportunities, goals & strategy are summarized in the following sections &

tables. In many cases, tentative numerical target goals have been identified for a multi-year research program.

Goal

The general goal for MFE magnet research is to reduce the 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 factor for the HIF is related to the system because of the anticipated direct impact of the length and overall envelope of quadrupole arrays on the system costs.

Strategy

The method for significantly reducing the cost & size of all fusion magnets over the next 10 years is to increase the performance of the individual materials & components. The specific targets are to increase the allowable current density, the working stress and the electric field. The components in which the greatest improvements can be made are the superconducting strand and cable, where a cost reduction factor of 3-10 is possible. For magnet structure, improvements in design and manufacturing techniques should be as significant as improvements in material properties.

Introduction of High Temperature Superconductors

Magnets require high levels of electrical, mechanical, thermal and structural engineering design and technology and advanced materials. If they are superconducting or cooled with a cryogen (e.g.- liquid nitrogen), they also require cryogenic technology. At present, only the Low Temperature Superconductors with critical temperatures of ~10K for the ductile alloy NbTi or ~18K for the brittle compound Nb_3Sn are in use or planned for future devices, with the significant exception of the recently approved Levitation Coil for LDX, which will use a High Temperature Superconductor, BSSCO-2223, in tape form. . 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.

Need for Innovative Concept Exploration Facility

A review of the metrics and targets for superconductors indicates that near-term research can reduce the cost of conductors by a factor of 3-10. This will involve Concept Exploration to develop innovative conductors, quench detectors, and protection mechanisms not previously used in large scale fusion magnets. The combined result will be embodied in a Superconductor Laced Copper Cable conductor

(SLCC), which will be composed of mostly Cu strands and a small number of SC strands to reduce dramatically the total amount of superconductor and copper required for a given magnet. This research is applicable to Nb3Sn, & NbTi superconductors. It will require an innovative Concept Exploration facility (<M$1) with a 250 mm bore, 10T, 2.5 T/s field capability. Consideration should also be given to using superconducting magnets for all or part of near term fusion magnet requirements. The coils could demonstrate the expected performance improvements on a larger scale & develop manufacturing technology using more significant amounts of conductor than in the Concept Exploration tests. Depending on the specific case, the superconducting coils may also be more cost effective for the lifetime of the device or possibly, be used for energy storage during operation.

World Program

Significant success in development of magnet technology for fusion applications has been achieved over the past two decades, but no large fusion programs using superconductors or resistive conductors are now underway in the U.S. Countries that have been more aggressive in introducing superconducting technology with working tokamaks and stellarators include France, Russia, China and Japan. Germany (Wendelstein VII-X), Korea (KSTAR), India (SST-1) and China (HT-7U) all have significant programs underway for introducing new superconducting steady state stellarators and tokamaks in the next few years. The last large operating fusion experiments with resistive conductors were produced in Europe and Japan whereas smaller scale devices were built in the US (i.e.- D-IIID, PDX, Alcator C-MOD, & NSTX).

The most promising configuration of the conductor is the cable-in-conduit (CIC). This concept is still relatively new and requires extensive analyses and testing to arrive at a better understanding of the CIC, reducing the operating margins and therefore cost. Due to the high cost of fabrication of the test samples and testing itself, an efficient way to conduct the R&D is an international collaboration with the active facilities – CS Model Coil Test Facility, TOSKA and SULTAN. Especially important are the test of the CSMC and the insert coils. These were built by the US, Japan and Russia, with participation by the EU, and will be tested in Japan. Another opportunity for international collaboration is the TF Model Coil which is being built and will be tested in the EU.

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. to design development & demonstration of cost reduction research. 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 an array.

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 Hi Tc superconductors. A prototype program of 1-2 coils should be adequate.

Resistive Magnets

R&D for resistive magnets will also result in improved performance and reduced magnet cost for selected missions, either pulsed or steady state. Some of the key technical issues for resistive magnets are similar to and simpler than those for superconducting magnets: 1) intensive active cooling optimization within mechanical stress limits, 2) adaptation of simplified 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. The relative importance of these issues depends on the concepts, such as plasma beta, size and field strength. It would also be beneficial to qualify & develop materials with improved radiation, electrical and mechanical properties.

Generic Development

Areas of generic interest for magnets 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 improve integrated CAD, CAE & CAM. Major benefits could also accrue from advanced code development for conductor & structural analyses.

Conclusion

For the U.S. fusion program, the main issues for magnet research revolve around cost, performance, and reliability. The United States should be developing the elements of magnet technology that are specifically focused on the experimental needs of the magnetic and inertial fusion physics programs and that will substantially lower the projected cost of the experiment or of fusion power.

3.2.2 Strategy for Resistive Magnet Technology

Both short-pulse and steady-state resistive magnets are expected to be important parts of the future fusion program's portfolio of magnet technologies. The majority of the US experimental devices past and present use short-pulse resistive magnets. As fusion research advances towards the goal of steady state reactors, the requirements for the magnetic field duration will increase to "quasi steady state" pulses of tens to hundreds of seconds, and then towards true steady state. Resistive magnets can fulfill all these needs, but ultimately the choice of magnet technology (resistive, low temperature superconductor, or high temperature superconductor) will depend on performance and economics. Resistive magnets are best suited to some applications because of their high tolerance to heat loads, radiation, and transient field conditions. For example, STs require a low aspect ratio and therefore the central bundles of their toroidal field systems are, of necessity, made as compact as possible; with limited space for thermal and nuclear shielding, resistive magnets are necessary. For pulsed devices like spheromaks and reversed field pinches (RFP's) resistive magnets may be necessary. Similarly, in-vessel control coils in tokamaks require compact sizes, rapid changes of field, and are also likely to remain as resistive coils.

Resistive electromagnets have been used industrially for well over a century and have frequently been designed for steady-state operations. Resistive magnets can be simple, robust, and cheap. For steady operation, it is only necessary to provide an electrical power source sufficient to overcome steady resistive losses in the magnets, and a cooling system sufficient to remove the dissipated power from the magnets. In forced convection designs operating steadily at room temperature, pressurized coolant (frequently deionized water) is pumped through passages in the magnet's copper conductors, then through a heat exchanger to reject heat into the environment before the coolant returns to the pump. Steady cooling at high power densities requires that coolant flow paths in the conductor must be kept short and coolant flow rates kept high.

Resistive magnets used in "quasi steady state" applications may have significant cost advantage. By actively cooling with liquid nitrogen during the pulse, the pulse duration can be lengthened arbitrarily by simply increasing the size of the external liquid nitrogen reservoir.

Since resistive magnets have the handicap compared to superconducting magnets of consuming significant electrical power during operation, they are only considered for fusion applications because of their offsetting advantages. One such advantage is the possibility of reducing shielding and operating with significant neutron and/or gamma radiation heating of the magnets. To exploit this advantage requires special attention to high fluence insulation and to activation issues, and may also require operation at room temperature. A second advantage is that higher magnetic field operation becomes possible because resistive conductors have no limiting modes corresponding to superconductors' quench phenomena. To exploit this advantage requires that high stresses be accommodated by some means. A third advantage is the possibility of low magnet or fusion device fabrication cost. Historically magnets cost 20 to 90 times the "bare" conductor cost. With simplified conductor and coil designs, a reduction of at least 2 can be achieved. This low cost imperative might favor designs with a small number of very high current turns constructed from plates, e.g. from thick, rolled copper stock machined with a pattern of surface grooves, which are then covered to form a cooling channel system. For low cost assembly and maintenance, reliable joints between the plates should be demountable (like PBX-M or DIII-D) but not necessarily sliding (like

C-MOD); demountable joints are feasible in resistive magnets but not in low temperature superconducting magnets. Demountable joints also facilitate an internal PF coil set in tokamaks, which can provide advantages in plasma shaping and control. Developing low-cost magnet design concepts is an important opportunity to advance fusion in the next decade.

Key issues and opportunities are to reduce the cost of resistive magnets, to increase their maximum field capability, to optimize intense active cooling for steady-state magnets, to optimize demountable joint designs, and to minimize power consumption. They include:

1. Development of cheap strong, castable conductor alloys with good fatigue properties.
2. Improved high field designs (improved structure, conductors, configuration, active presses, etc.)
3. Improved compact high current demountable joints.
4. Development of reliable compact sliding joints.
5. Development of optimized designs for intense active cooling
6. Scale model of high performance tokamak magnet systems.

The last 2 are generic opportunities which are applicable to all magnet technologies:

7. Improved insulation
8. Development of advanced manufacturing techniques.

The table which follows on the next page, provides details of the opportunities, goals and strategy suggested for resistive magnets and developed during the meeting. Similar tables were constructed for LTS and HTS and are in the corresponding sections.

Details of Opportunities and Goals for Resistive Magnet Technology

Note: items in blue are applicable to all magnet technologies.

I. Materials & Components	***Opportunity***	***Goal***	***Strategy***
1a. Copper & Aluminum	**Fusion specific copper and aluminum conductor data base**	**Develop for typical sizes and alloys used in fusion**	**National lab / industrial development of improved conductors.**
1b. Copper & Aluminum	**Advanced manufacturing techniques**	**Develop large size conductor mfg.**	**Work with industrial developers.**
1c. Copper & Aluminum	**Improved ST center posts**	**Investigate better alloys, manufacturing techniques**	**National lab / industry feasibility study**
2a. Insulation	**Extended operating life, performance of magnets**	**Radiation resistance > 5 x 10^{10} rads and mechanical / electrical / processing properties $\geq$ best present epoxy formulations.**	**We should support work already underway by industrial developers.**
2b. Insulation	**Develop coils compatible with high temperature & radiation .**	**Develop non-organic insulation systems.**	**Develop a national lab / industrial program.**
3a. Sliding Demountable joints with fixed supports	**Will permit demountable TF coil machine configurations with reduced TF conductor stress.**	**Develop robust high current joints. These joints can also be used in magnets and leads.**	**Develop a national lab / industrial program in demountable joints.**
3b. "fixed" demountable joints	**Will permit demountable TF coil configurations. (will not reduce conductor stress).**	**A possibly simpler alternative to 3a. (e.g., structurally simpler). Can also be used in PF magnets and leads.**	**Develop a national lab / industrial program to develop demountable joints.**
3d. Improved conductor joining processes	**Develop joining process(es) which avoids thermal softening of conductors.**	**Electroform joining has been very successfully used on several coil repairs. It avoids annealing and results in joints with excellent mech/elec. properties.**	**National lab / industry development. This task would determine the possible range of applicability of this process.**
4. Cryo line electrical isolators	**Permit use of reliable bellows/braid cryo feed lines.**	**Develop designs and source(s) of reliable electrical isolation of cryogenic feed lines.**	**Collaborate with European laboratories which have developed similar isolators.**
II. Manufacturing			
1. CAD / CAM / CAE	**Reduce development / fabrication time and costs**	**Bring the US fusion community to the SOA in CAD/CAM/CAE**	**Funded intra-laboratory program of modernization in these areas.**
2. Advanced manufacturing methods	**Reduce fabrication costs.**	**Investigate latest manufacturing techniques such as laser casting which may be applicable to magnets.**	**National laboratory / industry effort.**

Details of Opportunities and Goals for Resistive Magnet Technology(cont'd.)

Note: items in blue are applicable to all magnet technologies.

III. Design, Analyses & Criteria	***Opportunity***	***Goal***	***Strategy***
1. Design optimization	**Develop optimized designs which permit higher performance levels**	**Refined analyses and design approaches.**	**Coordinated intra-laboratory effort.**
2. Refined criteria	**Permit higher performance magnet designs**	**Reduce margins where justified.**	**Coordinated intra-laboratory effort similar to that used for current criteria.**
3. Advanced analytical techniques	**Reduce design development time and costs.**	**Develop FEA techniques which includes plasma.**	**Intra-laborator effort supplemented by commercial FEA code developers.**
4. Reduce cost of large high field steady-state resistive magnet designs	**Develop unconventional designs using large plate/ turn sizes and demountable joints to save manufacturing costs.**	**Reduce the ratio of magnet cost/conductor cost from present range (20-90) by factor of at least 2.**	**Optimize design concepts by using large conductors directly with little additional processing.**
5. Increase maximum design field capability	**Develop TF designs for conductor and support systems permitting higher field operation**	**Design to provide 10 Tesla or greater at the plasma. This might also be applicable to high temperature superconductors.**	**Optimize design concepts with external structural support**
6. Optimize intense active cooling designs	**Permits higher power density steady-state operation (e.g., for a more economical ST)**	**Provide steady conductor cooling at power densities up to 100 MW/m^3**	**Optimize design concepts via design studies and analyses**
7. Higher performance machine configurations	**Develop machine configs. which might have higher perf. and lower costs.**	**Determine if "in-bore" PF is superior to external PF (requires demountable joints detailed in 3a and 3b.**	**Intra-laboratory study which would do a side by side comparison.**

3.2.3 Strategy for Low Temperature Super Conductors

Most concepts for power producing fusion power plants depend on superconducting magnets for efficient production of magnetic fields. The attraction of superconductivity is the ability to use very high current density with zero dc power dissipation. Superconductors will dissipate energy in a changing magnetic field, but overall power losses, including refrigeration power required to maintain the magnets, typically at 4K-8K, are extremely small compared with resistive magnets. This advantage grows with increasing magnetic fields and magnetic field volume, or where relatively long pulse or steady state operation is required. The upper limit for near term applications is about 13 T, but could be as high as 16+ T for fusion systems in the future, depending on the mission.

The overall strategy for LTS research with the goal of cost reduction is as follows:

A. Test strand, cables, and modify design criteria with the goal of cost reduction. Develop innovative conductor designs (ie-SLCC), quench detection & protection mechanisms to reduce the superconductor and copper required and reduce cost. This will require a 250 mm bore, 10 T, 2.5 T/s magnet facility to be built.

B. Participate effectively in International Programs, eg-ITER CS Model Coil Testing in Japan, ITER TF Model Coil Testing & Case Development Program in Europe, & Joint and Conductor Testing at SULTAN (EU).
   A&B require a core group of staff & students to analyze data, review theories & criteria, write new codes, perform component development & testing, & ultimately save $ by reducing margins.

C. Consider Superconductors for all magnet systems in near-term fusion experiments. Select specific cases as an opportunity to update & demonstrate new design criteria aimed toward cost reduction for the conductor and coils for the experiment and the future. eg:

   1. F & C coils for LDX
   2. Prototype quads and arrays for HIF experiments.
   3. Quad focussing system for IRE
   4. The saddle-coils or the discrete-coil options for NCSX using SSC cable.
   5. The outer PF coils for FIRE or IGNITOR.

Background

Only the Low Temperature Superconductors (LTS) with critical temperatures of ~10K for the ductile alloy NbTi or ~18K for the brittle compound Nb_3Sn are in use or planned for future devices. Another A-15 compound, Nb_3Al, is often considered for high field application because of it's superior critical field and lower sensitivity to strain degradation, but there is very little commercial production because of fabrication difficulties. Table 1 summarizes typical properties.

Table 1. Typical critical properties of useful low temperature superconductors.

Superconductor	**T_c (K) at B=0**	**B_{c2} (Tesla) at T=0K**
NbTi	9.0-9.7	14-15
Nb_3Sn	18.3	24-30
Nb_3Al	19.1	36-41

The working ranges of magnetic field and temperature are much smaller than the critical values to allow margins for operation. This provides an opportunity for cost reduction by exploring concepts to reduce quantities of conductor for a specified operating point.

NbTi alloys are a fairly mature technology, available commercially in large quantities with consistent quality. It was used in the TF coils of Tore Supra (France), and in the TF coils of the former T-7 (Russia), presently operated as HT-7 (PRC). MFTF-B coils were mostly NbTi, except for the choke coils, which used Nb_3Sn inserts. The Large Coil Program demonstrated moderate-scale TF coil technology with five NbTi coils and one Nb_3Sn coil. The Large Helical Device (LHD) has just begun operation in Japan using all NbTi superconducting magnets. The stellarator Wendelstein 7-X is presently under construction using NbTi CICC with a co-extruded aluminum conduit. India is building SST with NbTi TF and PF magnets, and China is designing the HT-7U tokamak with all NbTi superconducting windings. K-STAR in Korea is beginning construction using Nb_3Sn CICC.

A major milestone in large-scale superconducting magnet design for MCF was the delivery of the US Inner Module and the JA Outer Module for the ITER Central Solenoid Model Coil. These coils and their structure form the world's most powerful pulsed superconducting magnet and the largest Nb_3Sn magnet (640 MJ). The program required development of many new fabrication methods for constructing massive coils with CICC and an aggressive Nb_3Sn superconducting strand development program to achieve fusion program specifications. The largest Nb_3Sn production in the world (~30 tons) was done for the ITER CS and TF model coils & about 60 tons of the high strength superalloy Incoloy Alloy 908 was produced in the form of extruded conduit for the conductor jackets. The coils will be tested in 1999/2000 & will provide a wealth of important performance data. The US should provide a core team to review this data and take part in other international programs (eg- TF Model Coil Test and testing at SULTAN) to push forward the theory and recommend small scale experiments to allow cost reduction improvements in design criteria.

A superconducting floating ring coil for the Levitated Dipole Experiment (LDX) is under construction. It uses Nb3Sn in the floating coil and NbTi in the charging coil. The Nb_3Sn strand used for LDX was developed as a modification of the ITER strand.

Issues

1. Reduce cost – The high cost of LTS magnets is partly due to high costs of the superconducting raw materials and strand processing costs which are labor intensive. Coil fabrication costs are also high because the superconductor properties are more sensitive to handling and fabrication stresses then, for example, copper or copper alloys. In addition, Nb_3Sn must undergo a high temperature reaction heat treatment (about 650 C for 200 hours) which complicates the fabrication process. There are substantial other costs associated with low temperature operation and their need for cryogenic refrigeration.

2. Improve performance - A strong benefit results from higher magnetic field since fusion power is proportional to B^4. In addition, higher operating current density could reduce the size of the winding pack, as would better quench protection systems, thus reducing overall system cost. Improvements in the ability to absorb higher nuclear flux and fluence could reduce the machine size and cost as a whole if better insulation systems could be developed or if superconductor stability could be increased in order to reduce the size of the radiation shield protecting the magnet.

Opportunities, Goals and Strategy

The three ways in which advances in LTS magnet technology can lower the cost of experiments and fusion power production are listed in the introduction and are common for all conductors types. These can be addressed for LTS by defining metrics and using these metrics to set performance goals for the following major components: Superconductor, Stabilizer, Quench Detection and Instrumentation, Structure, Joints, & Leads. These components, and other items are outlined in the attached table which summarizes the opportunity, goal and strategy for each case.

Summary of Opportunities, Goals & Strategies for Low Temperature Superconductors to Reduce Cost or Improve Performance in Fusion

	Opportunity	**Goal**	**Strategy**
Materials & Components			
Superconducting strand	**Improved manfg techniques and innovative concepts for protection**	**-improve Jeff -x 1.5 for NbTi -x 2 for Nb3Sn -increase Jstab -x1.5 for CICC -x1.5 for quads**	**Procure sequential billets with clear specs Evaluate for DC & AC performance Coordinate MFE strand development with HEP Develop innovative quench protection & detection Apply & evaluate at small scale & in fusion experimental coils**
Conductor & cable	**Concept exploration: SLCC (superconducter laced copper cable)**	**Combine output from above and from quench protection to achieve cost reduction of ~3 for NbTi cable & ~10 for Nb3Sn cable**	**Develop theory and modify codes Fabricate SLCC subcable & test -need 250 mm bore, 10 T, 2.5 T/s test facility Refine design criteria, full scale test**
Quench protection	**-innovative quench detection & protection will increase Jeff by large factor, decrease cost, & improve reliability**	**-develop fast quench propagation in CICC using embedded heaters; -complete development of internal voltage sensor termination -develop & reduce cost of embedded fiber optic quench detector using interferometry -develop other innovative concepts based on helium diagnostics**	**-develop cable with heater and leak tight termination; use cowound sensor for quench detection; qualify electrical integrity; develop codes, demonstrate capability & correlate with codes; repeat for fiber optic detector; demonstrate very high reliability; refine design criteria & evaluate impact on cost and safety**

Conductor conduit	**-Incoloy 908 data base developed for ITER; additional trials indicate minor mods can improve SAGBO resistance** **-evaluations indicate that mods to 300 ss chemistry would allow tolerance to Nb3Sn heat treatment requirements**	**-increase SAGBO resistance by improving tolerance to O2 by x 100 while maintaining or improving mech props at low temp; complete for ASTM qualification & improve weld processes to reduce specific cost x 0.5** **-develop modified 316LN to achieve K1c> 250 Mpa m^0.5 after a heat treat of 300 hr at 700 C; target cost similar to 316 LN**	**-continue testing on small samples with altered chemistry & evaluate; select composition and procure in plate form for test and validation** **--continue testing on small samples with altered chemistry & evaluate; select composition and procure in plate form for test and validation of plate & weld properties**
Joints	**-test data will become available from ITER CSMC & TFMC** **-testing facilities exist in EU & US; test programs underway in EU**	**-develop 50 kA joints for improved performance & decreased manfg time** **-for NbTi: decrease joint volume by ~10** **-for Nb3Sn: improve DC loss, volume & stability by factor of ~10 (simultaneously)**	**-design & test improved subscale & full scale joints in US & EU** **-design for reduced coil fabrication time** **-develop analytical codes, correlate with data and improve theory and predictive capability**
Cooling line electrical isolators	**-innovative design for higher voltage can allow decrease of coil current & lower coil fabrication cost**	**-improve voltage capability with glass beads to raise Paschen minimum** **-develop isolators for reduced size & use up to 40-50 kV to allow decrease of coil currents and/or amount of stabilizer**	**-review existing designs on large scale magnets** **-design & test samples at low temperature for voltage withstand** **-recommend changes to design criteria**
Thermal isolators	**Collaborations among universities & Labs, esp HEP & LHC; note esp important for HIFD quads**	**-Improve strength/thermal conductivity ratio of gravity supports to 10^4 t/w-m & bumpers to 2.5x10^4 t/w-m**	**-screen new advanced composite materials** **-design more effective column/spider supports**
Manufacturing			
-Magnets required for fusion experiments	**-F & C coil for LDX** **-magnets for HIF** **-quad focussing system for IRE** **-NCSX coils with SSC cable** **-Outer PF coils for FIRE or IGNITOR**	**-advance the demonstration of design criteria & develop manfg technology**	**-Consider mission, cost & benefits of LTS** **-apply latest criteria conservatively** **-operate to determine margins** **-refine criteria**

Design, Analyses & Criteria			
-Existing international test programs	**International Test Programs:** **-CSMC at JAERI** **-TFMC at FzK** **-SULTAN**	**Reduce design margins on conductor to reduce size and $; collaborate on innovative design concepts**	**Core group of staff & students refine design criteria, recommend new concepts & design improvements**

3.2.4. Strategy for HTS conductor and magnet technology in Magnetic and Inertial Fusion Experiments

Introduction

High temperature superconductors have a high promise for improving the cost/performance of magnetic and inertial fusion. The ability to operate without Joule heating at higher temperatures dramatically lowers the cost of cryogenic refrigeration and also simplifies current leads and thermal isolation. Available high temperature superconducting tapes of BSSCO-2212 and -2223 have excellent properties at low temperatures, surpassing Nb_3Sn at very high fields. Advanced high temperature superconductors, such as YBCO, have extraordinary properties at high fields and relatively high temperatures, maintaining usable current densities out to 10 T at 77 K or 30 T at 4 K in epitaxial films. High temperature superconductors also have the benefit that a great deal of development is being supported outside the fusion and high energy physics programs. They are expected to fulfill the promise for commercialization of superconductivity in motors, generators, transformers, and power lines that has remained tantalizingly beyond the reach of low temperature superconductors. Success in commercialization should lead to large reductions in the cost of this technology to stated goals of < $10/kA-m.

Issues

Despite their great promise, high-temperature superconductors are still a young technology. The limits on high-temperature superconductors that best reflect the early stage of their development are fivefold, including:

1) Cost
2) Performance
3) Piece size
4) Strength, and
5) Production Capacity

Typical goals in fusion magnets are compared
with the state-of-the-art in high temperature superconductors:

Cost/Performance Measure	Fusion Magnet Goals (near-term)	High Temperature Superconductor State-of-the-Art
$/kA-m	$10/kA-m	$160/kA-m
J_{eff} (4.2 K, 12 T)	700-1000 A/mm^2	250 A/mm^2
Piece size	50 MA-m	50 kA-m
Design Stress	250 MPa (NbTi), 100 MPa (Nb_3Sn)	40, 80, 150 MPa
Annual Production	20 tonnes/year	1 tonne/year

While cost and performance will probably continue to improve because of commercialization, the key to widespread use in the fusion program is the need to raise current and piece size. It is almost certain that commercial applications will stop at 2.5 k A and most high-temperature superconductor vendors have no development plans beyond 500 A. The highest current commercial tapes today are 200 A at 0 T and 77 K. The increase in piece size of three orders of magnitude is the greatest challenge of the fusion program. The other challenge, the scaleup of four orders of magnitude in production capacity should be handled by a measured strategy of increasing the scale of high temperature superconductor magnets in the fusion program over the next 10 years, as the industry grows in its capacity to produce tens, then hundreds, of tonnes of conductor.

Conductor performance is already reaching the level at which high-temperature superconductors can be introduced competitively in the lower field fusion applications. The "vision" that high-temperature superconductors are primarily to be used in ultrahigh field magnets is very long-term. In the immediate future, the best use of these conductors is where the conductor cost is relatively low in comparison with the rest of the system, so that an increase in temperature to 20-30 K would make a coil-refrigerator-power supply system competitive with either low temperature superconductors or normal magnets. In the short term, this will apply mostly to magnets in the 0.5-2 T design field range.

Application Strategy

All five of the above listed limits point toward a strategy of beginning with the simplest magnets: those that are small and have low field and current density. This buys time and gains fabrication and operating experience at the smallest cost to the fusion program. The strategy described here proposes three stages, progressing to medium performance/size magnets, and ending with the high performance/large size magnets needed for a successful fusion program.

The magnet programs that fit this strategy the best are listed in Table II:

Table II: Potential Candidates for Introduction of High-Temperature Superconductor

Category	Project/Magnet	kA-m	Scaleup
Small	LDX L-Coil	643	
Small-Medium	NCSX	14,600	22.7
Medium-Large	FIRE PF	348,000	23.8
Large	ITER-II	6,800,000	19.5

More steps can be contemplated, such as the use of high-temperature superconductor in Electra (0.3 T, 1800 kA-m). However, if one of the steps is omitted, a scaleup of several orders of magnitude would be needed to reach a predemo experiment.

Recommendations

1. The introduction of high-temperature superconductors to the physics program should be done with simple, low-field, near-term coils, including LDX-L and/or the Electra Helmholtz pair.
2. Today's high-temperature superconductors are marginal for the NCSX saddle coils. If they are improved sufficiently in cost and performance in the next two years to be used there, this experiment would be nearly ideal for a second-phase, medium-scale, medium-performance application in the introduction of high-temperature superconductors. The LDX F-coil might also be a good choice, because it would include the ability of dramatically increasing the coil float time.

If FIRE is built, it may be advisable to introduce high-temperature superconductors into the least demanding PF coils. This will decrease the jump to an ITER or ETR by another order of magnitude and also decrease the cost of nitrogen consumption, during operation.

Research and Theory

The most important goal of a high-temperature superconductor research program is the development of high-current superconductors. This includes the production of high effective-current density strands in long lengths, the cabling of ever larger numbers of strands until the 30-70 kA levels needed by magnetic fusion are attained, and the development of low-cost, low-loss, high-current joints. The fabrication of high-current samples should be accompanied by their test as hairpins and insert coils in order to establish overall properties, losses, and ramp-rate limitations (if any).

A unique issue concerning high-temperature superconductors is the possibility that they may not require dump circuits. In today's high-temperature superconductor magnets, it has been found that resistive voltages will develop in a high-temperature superconducting tape and the current will decay gradually without a quench/dump thermal instability. The obvious causes for this benign behavior are the low exponent "n" (i.e. high temperature superconductors are more resistive, close to their critical temperatures) and the higher heat capacity at higher temperatures. It seems probable that there are regimes within which this behavior will always occur and regimes (lower temperature, higher current density), where quench becomes possible again. Since the elimination of quench/dump will allow significant savings in magnet power supplies and improvements in magnet system reliability, the development of theory as to the bounds of "unquenchable" operation and confirmation by experiment should be done.

Pulsed losses have been an active area of research in high temperature superconductors. Separate loss terms in three field orientations do have to be calculated for the types of pulses contemplated by most fusion concepts. Field-tape orientation is particularly critical in high-temperature superconductor design. Coupling losses may be significant for large cable conductors, so losses and loss theory should be also be reconfirmed as the conductors scale up in current.

YBCO

YBCO is a material of enormous promise for high-temperature superconductors, but it is far less mature than BSSCO superconductors. In particular, they can only be made in tape form and cannot be made in 100 m lengths today at any current. Thus, the strategy that should be taken by the fusion community towards YBCO conductors is not as clear as that for BSSCO. Basic philosophies could include these three options: 1) The fusion program should wait until YBCO conductors in long-lengths are developed commercially by private industry. This has the obvious logic that it costs nothing up front and is exactly what the fusion program elected to do with BSSCO. 2) The program could begin to deliberately introduce YBCO early in the applications for which small lengths of conductor are best suited, as soon as their engineering current densities are competitive. These include current leads and thick-film quadrupoles for small aperture heavy ion fusion driver array prototypes. 3) We could actually develop new and superior materials, as was done with Incoloy 908 and Nb_3Sn.

Bromberg has proposed that the use of reinforced YBCO radial plates, along with demountable high temperature superconducting toroidal field coils, promises unique advantages for tokamak reactors[2]. These advantages include a combination of higher operating temperature, high superconducting current density, high strength, structural strength approaching today's Bitter magnets, cost savings in stacked vs. wound magnet systems, and maintainability through demountability. Another potentially important use is that of "printed-circuit" thick film quadrupoles for Heavy Ion Fusion Drivers. The achievement of thin-film performances in thicker layers, such as 1,000,000 A/cm^2 at 64 K and 1 T would allow the 2 cm radius

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[1] L. Bromberg and M. Tekula, "Options for the Use of High Temperature Superconductor in Tokamak Fusion Reactor Designs,"

[2] R.O. Bangerter, "Update on Superconducting Magnets for Heavy Ion Fusion," a preSnowmass White Paper, April 19, 1999

quadrupoles advocated by Bangerter[2] to be built with approximately an 80 µm build. Operation at lower temperatures would lower the thickness further.

YBCO is not likely to be usable in any large fusion magnets in the next 10 years. However, unlike BSSCO, which may never surpass Nb_3Sn in performance at low temperatures and fields up to 16 T, YBCO is a revolutionary material with the potential for raising field, current density, and temperature simultaneously, while lowering refrigeration requirements. Strategy 2 of introducing YBCO in the smallest possible fusion experiments has the advantages of accelerating the development process that may be necessary to the ultimate success of fusion magnets. As experience is gained, decisions can be made about further acceleration through material development or introduction in large-scale experiments.

Conclusions

We believe that there should be a balance between applications research and introduction of high-temperature superconductors into significant fusion experiments. Without research, the conductors won't advance beyond whatever is required by low current commercialization, but without near-term fusion applications, there won't be enough focus or community interest to sustain development. We recommend what might be called an "affirmative-action" program for high-temperature superconductors in which they are introduced in applications where they are feasible and nearly competitive, in order to gain fabrication and operating experience, while lowering the operating costs of fusion physics experiments.

The key problem of high-temperature superconductors for the fusion community is the development of high current, high piece-length conductors and joints with acceptable costs.

Theory should concentrate on the transient phenomena of losses, ramp-rate limitations, and quench propagation. Theory should also be focussed on the design possibility of a radical simplification of understanding superconductor transients: no ramp-rate limitations, no quenches, no frequency dependence of loss/cycle.

YBCO should be introduced in small component development programs and reevaluated annually for possible acceleration. Attractive candidates include reinforced plates, demountable joints, and small, thick film quadropoles.

3.2.5 Magnets for Inertial Fusion Experiments

Introduction

Magnet system requirements for inertial fusion reactor systems are comparable in importance to those for magnetic fusion systems. KrF laser systems require a pair of Helmholtz coils for each laser diode box in order to prevent self-pinch of the electron beam. This has been accomplished with normal coils, so far; but it is recognized that these must be replaced by superconducting magnets for the reactor application, in order to reduce the recirculating power[3]. The replacement of normal coils by high-temperature superconductor coils in next-generation experiments, such as Electra, appears to be straightforward. The need for high performance superconducting magnets in Heavy Ion Fusion drivers is more critical. HIF drivers require quadrupole focusing magnets, induction linear accelerator cores, dipole bending magnets in the approach to the target and final focusing of the arrayed multiple beamlets. The most critical magnet components are the superconducting quadrupole magnets, not only because the cost of the arrays is a significant fraction of total accelerator cost, but also because the radius of quadrupole arrays controls the cost of the linacs, while the total length of the arrays affects the cost of the buildings and cryogenic plant.

A unique aspect of the quadrupole design for HIFD reactors is that the fundamental beam physics strongly favors compact, high current density, advanced quadrupole design. The achievable beam current density is inversely proportional to the beam radius, thus favoring large numbers of small beams. This conclusion is reinforced by the beneficial impact of small quadrupole arrays on the size of the induction cores, cryogenic refrigeration, and balance of plant costs. For any given technological approach, there is an obvious tradeoff and optimum size, due to constraints on thermal isolation, critical current density, protection, leads and coolant lines, and assembly constraints. Separate studies by Faltens at the Lawrence Berkeley National Laboratory (LBNL) and Meinke at the Advanced Magnet Laboratory indicate that present-day technology optimizes at a magnet bore diameter of approximately 120 mm. However, theoretical considerations by Bangerter[4] (LBNL) and systems studies by Meier calcuate systems costs that indicate first-order benefits in reducing magnet bores to as little as 40 mm. The focus of the magnet program is to take every possible step to reduce magnet system size without overly expensive tolerances, materials, manufacturing techniques, magnet heating, or beam control and field quality requirements.

<u>Krf Lasers</u>

The introduction of superconducting Helmholtz coils into the KrF laser program appears to be straightforward. A prototype coil pair can be built and inserted into the Electra experiment. Since the

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[3] M.W. McGeoch et al, "Conceptual design of a 2 MJ KrF laser fusion facility," Fusion Technology, Vol 32, Dec. 1997

[4] R.O. Bangerter, "Update on Superconducting Magnets for Heavy Ion Fusion," a preSnowmass White Paper, April 19, 1999

experiment itself has little interaction with the magnet, it can be fully qualified on a separate test stand, before inclusion in the experiment. One or two iterations should be adequate to demonstrate a magnet system that would be a small part of system cost and recirculating power in a commercial reactor. It seems likely that commercial high-temperature superconducting tape will be the long-term solution for this application.

<u>Heavy-Ion Fusion Drivers</u>

Heavy-Ion Fusion Drivers benefit the most from an aggressive magnet development and design effort. Work should continue on self-consistent magnet system sizing in HIFAN, the Mathcad-based system sizing code for Heavy Ion Fusion. An advantage of the Heavy-Ion Fusion Driver concept over any magnetic fusion concept is that the magnet system consists of many small magnets, rather than a few large, high cost ones. This should allow rapid turnaround times in magnet prototyping, meaningful performance metrics, and high impact of the improvements in each magnet generation. The size and number of quadrupole arrays is comparable to that of a dipole magnet in High Energy Physics accelerators. The individual quadrupole is 10-100 times smaller with proportionally larger numbers in the system. The development program might adopt a "rhythm" of, say, three generations of individual quadrupoles, followed by a quadrupole array prototype. This can be determined empirically, as the program matures. Higher risk or revolutionary technologies, such as YBCO thick film quadrupoles, should begin as individual quadrupole magnets.

Concepts that reduce the build further of unit quadrupole cells include the following:

1) "Stuffing" conductor into the corners of square unit cells, instead of cylindrically. This can be used to eliminate or reduce the impact of coil radial build on the unit cell size in an array. Issues to be addressed include a strategy for achieving adequate field errors and the structural support of conductors that can't be contained by shrink-fit tubes.
2) The use of higher current density superconductors. Artificial Pinning Center (APC) superconductors promise low cost and higher performance than NbTi at fields below 5 T. Nb3Sn and Nb3Al A15 superconductors already have superior performance. They also have high critical temperatures, possibly allowing higher heat leaks or dry heat removal. However, they are also expensive and brittle. Currently, they would not be competitive with NbTi on a systems cost basis, both because of the high strand cost and because of the incompatibility of brittle conductor with inexpensive, automated winding technology. A long term possibility would be the use of

"printed circuits" of thick-film YBCO conductors. Here all components would be stacked with a lead-cap soldered to the coil at one end.

3) Higher copper current density. Current operation of HEP physics magnets is limited to 1,000-1,400 A/mm2 in the copper, largely because of stability problems. There also has to be an internal heater, since dump between 1,000-2,000 A/mm2 is too rapid for external dump resistors at reasonable voltages. It is probable that design with A15's will allow a higher copper density, because of the greater energy margin of higher critical temperature superconductors. A design concept that is becoming popular for high temperature superconductors is to neglect protection on the grounds that quench is impossible. This is a long-term option, when the theory is adequately understood, and other events that could create quench, such as loss of coolant, are considered.
4) Higher specific thermal isolation. Recognizing the desirability of low heat leak in low maintenance cryogenic applications, commercial companies are gradually introducing insulating composites with higher strength/conductivity ratios than G10 or G11CR. Foster at Fermilab has proposed multiply-slotted spiders as a method of creating a long thermal path in a short length.
5) Efficient boundary conditions: The Edge Termination coils proposed by Meinke appear to eliminate all of the dummy quadrupoles needed to "complete" a circular array. The approximation to a circle with square-celled quadrupole tubes can be improved if larger arrays of smaller tubes are feasible.
6) Efficient layouts of coil terminations, leads, joints, coolant lines, thermal isolation, and cryostat supports. The concepts mentioned above for thermal isolation in the radial direction apply also to the axial direction. Meinke's circular cable conductor allows a tighter bend radius than a Rutherford cable. Fine-stranded cable improves the bending radius for an A15 option. Multiple arrays/cryostats can save vacuum space and perhaps create "natural" gaps for central exit of leads and/or coolant lines.

A second, well-defined issue for Heavy Ion Fusion Driver magnets is the cost/performance improvement of normal magnet Linear Induction Accelerator. Here the geometry is largely fixed by the quadrupoles and insulators. However, materials development of less expensive metglass or some other high-frequency ferromagnetic induction core has also been identified as an important cost-reduction program.

The third key magnet subsystem are the final-focus magnets. These not only require high accuracy in the formation of the final beam striking the target, but have critical setback requirements in their distance from the DT explosion.

Conclusions

Heavy-ion fusion drivers benefit the most strongly from improvements in magnet technology, because of the large variety and number of magnets and the direct effect of decreasing plant size from magnet performance improvements.

Because of the extremely large number of quadrupole magnets in an HIF reactor and their small size, quadrupole prototyping should be a cost effective method of improving the IFE concept with good metrics for the improvements in each generation.

The Heavy Ion Fusion Reactor system code has highly physical beam scaling in the accelerator section, but only generic magnet scaling. Improvement of the magnet scaling equations to distinguish credibly between design options should greatly clarify the economic potential for HIF reactors.

Size-reduction in HIF quadrupoles can be achieved by a combination of more advanced superconductors, higher-current density matrix operation, improved thermal isolation systems, better cell layouts, and improved array layouts and designs.

Manufacturing cost reductions can be achieved by automated and parameterized winding machines, lower-cost superconducting strand, and automated array stacking, assembly or joining techniques. Automated winding is available and can be demonstrated in the short term.

Helmholtz coils for KrF lasers can be prototyped to demonstrate costs that comparable to those of normal coils and circulating power that is adequate for reactors. Within a couple of years, if current trends continue, it should be possible to build them with high-temperature superconductors at a reasonable cost.

Acknowledgements

The authors would like to recognize the comments & participation by H. Schneider-Muntau, M. Tupper, D. Rasmussen, C. Baker, A. Molvick, S. Milora, R. Callis, R. Temkin, W. Marton, B. Hooper, J. Latkowski, S. Zinkle F. Dahlgren, & J. Minervini.

3.3 IFE Targets

Key Questions

Can the technologies needed for low cost, cryogenic targets and a high rep-rate injection system be developed?

Topic Leaders

Ken Schultz (GA), Warren Steckle (LANL)

Core Working Group

Ken Schultz, Warren Steckle, Gottfried Besenbruch, Paul Fisher, Dan Goodin, Wayne Miller, Art Nobile, Ron Petzoldt, Rich Stephens, Debbie Callahan Miller

This topic discussion group addressed the key question:

"Can the technologies needed for low cost, cryogenic targets and a high rep-rate injection system for inertial fusion energy (IFE) power plants be developed?"
We pursued 3 elements of this key question:

1. What are the key technologies needed to provide low cost, cryogenic targets at a high rep rate and with high accuracy? The goal of these discussions is to identify these key technologies for both direct and indirect drive IFE targets.

2. What are the critical scientific and technological issues that face us while we develop these technologies? The goal of these discussions is to identify the roadblocks, both scientific and technological, to developing these technologies.

3. What are the approaches to resolving these critical issues and what opportunities exist for pursuing these approaches? The goal of these discussions is to identify possible approaches and opportunities for resolving key issues and to provide a roadmap for the development of the required technologies.

The overall goal of these discussions was to consider as many target fabrication and injection technologies as possible for both direct and indirect drive IFE targets and to identify the pros, cons, issues and opportunities associated with each. We hope that the results of these discussions will be useful to the subsequent selection and development of a limited subset of these technologies for application to IFE.

This topic had strong interaction at SnowMass with the Target topic of the Inertial Fusion Concepts Working Group. The morning IFE Target Concepts topic group focused on what to do for IFE target fabrication and injection, and the companion afternoon IFE Target Technologies topic group focused its discussions on how to do IFE target fabrication and injection.

Topic participants: Dave Bailey (LLNL), Larry Baylor (ORNL), Gottfried Besenbruch (GA), Lee Cadwallader (INEEL), Rich Callis (GA), Koichi Kasuya (TIT), Stan Milora (ORNL), Art Molvik (LLNL), Bob Peterson (UWisc), Ron Petzoldt (UCB), Ken Schultz* (GA), Warren Steckle* (LANL), Rich Stephens (GA), and Scott Willms (LANL) (*: topic leaders)

3.3.1 Description

At the heart of an inertial fusion explosion is a target that has been compressed and heated to fusion conditions by the incident driver energy beams. For direct drive, the target consists of a spherical capsule that contains the DT fuel. For indirect drive, the capsule is contained within a cylindrical or spherical metal container or "hohlraum" which converts the incident driver energy into x-rays to drive the capsule. The "Target Factory" at an inertial fusion power plant must produce about 1-2 x 10^8 targets each year, fill them with deuterium-tritium fuel, layer the fuel into a symmetric and smooth shell inside the capsule, and deliver the completed target to the target chamber at a rate of about 5 Hz. The output of this factory must be a stream of targets with a minimum (<1%) of duds. These fragile targets must be injected to the center of the target chamber, operating at a temperature of 500 - 1500°C and possibly with liquid walls, without mechanical damage from handling and acceleration, or thermal damage from the hot target chamber environment. Target fabrication must be done with extreme precision of manufacture, extreme reliability of delivery and at a manufacturing cost four orders of magnitude lower than current inertial confinement fusion (ICF) target fabrication experience. Target filling and layering must be done with high precision, extreme uniformity of temperature, and with the minimum possible tritium inventory. Target injection must be done with precision and reliability of delivery, and without damaging the mechanically and thermally fragile targets. Prediction of target location at shot time must be very accurate. This challenge does appear to be achievable, but will require a serious — and successful — development program.

3.3.2 Subtopics:

3.3.2.1 Target Fabrication

Targets currently fabricated for ICF experiments have many of the characteristics that will be needed for IFE, although the size is smaller (capsule diameter ~ 0.5 mm for Nova, ~ 1 mm for Omega and ~ 2-3 mm for the NIF vs. ~4-5 mm for IFE). The fabrication techniques used for ICF targets were developed to meet exacting product specifications, to have maximum flexibility to accommodate changes in target designs and specifications, and to provide diagnostic features and a thorough characterization "pedigree" for each target. The current ICF target fabrication techniques may not be — and were not intended to be — particularly well-suited to economical mass production of IFE targets. Because of constant development required by the small number of any one design that are made, and because of the thorough characterization and labor-intensive assembly required for each target, a completed target can cost about $2000. For a power plant to be economically competitive, the target cost for a ~500 MJ yield target must be reduced to about $0.30. Higher yield targets could cost proportionally more.

Capsule fabrication.

One technique currently used to fabricate ICF capsules is the poly(alpha-methyl styrene)/glow discharge polymer (PAMS/GDP) process which may not extrapolate well to IFE. Alternate capsule fabrication techniques must be considered. The microencapsulation process previously used for ICF appears well-suited to IFE target production if sphericity and uniformity can be improved and capsule size increased. Microencapsulation is also well-suited to production of foam shells which may be needed for several IFE target designs. We pursued the following questions:

- What technologies appear promising for fabrication of low cost capsules?
- How can foam capsules or foam-covered capsules be fabricated, and which polymers are candidate materials?
- What are the key scientific and technological issues and the approach for resolving them?

Technologies/Issues

1. Indirect Drive: Current designs use a thick-walled doped plastic capsule or a thick walled beryllium capsule. Approaches to fabricating these capsules include the following technologies and face the following issues.
   - Direct microencapsulation of polymers. Issues include the uniformity of the wall thickness for thick walls (~100μm) and ability to add the desired concentration of dopants.
   - Microencapsulation of a thin mandrel followed by overcoating by GDP or other polymer. Issues: Cost. Use of a fluidized bed for coating deposition may solve this concern if adequate smoothness and uniformity can be obtained.

- PVD or CVD of Be on a CH mandrel (possible PVD to get a robust mandrel followed by CVD to get a thick coating). Issues: Cost, uniformity and smoothness. Filling of Be capsules is also a concern discussed in section 2.1 below.
- Direct machining of Be hemispheres and braze joining. This is being developed for NIF. If successful, this process might work for IFE if fully automated. Issues: Cost, fill and surface smoothness, especially with practical automated machining and handling constraints.
- Beryllium droplet formation of shells has the potential for producing 10-100 shells/sec and reduces the exposure to hazardous materials.

2. Direct Drive. Current designs include a simple thin-walled CH capsule, a thin-walled CH capsule with low density foam outside, and a CH capsule with low density foam inside and a thin high-Z coating outside.

- Direct microencapsulation. Issues: Can the capsule be made thin enough to satisfy target design requirements?
- Foam coating by microencapsulation around a CH mandrel. Issues: Uniformity, density, cell size and handling of the fragile foam-covered capsules.
- Microencapsulated foam capsules over-coated by interfacial polycondensation and topped by a thin sputtered layer of high Z metal. Issues: Finding a foam process that is capable of proper foam density and cell size and also compatible with interfacial polycondensation processes. Wall thickness uniformity.

Opportunities/Approach

This activity is part of the IFE Technologies program and will study the various manufacturing processes and develop the conceptual design of a target factory, including cost estimates, using these processes. Bench top development of promising processes will be done, followed by refinement of the design studies to select a limited number of concepts for prototyping in the IFE Integrated Research Experiment (IRE). It is of utmost importance that there is ongoing dialogue between the target fabrication effort and other efforts within the IFE program, i.e. – target design, chamber design, driver design, etc. Changes at this level can have a significant impact on material selection criteria and the fabrication technique.

Hohlraum fabrication.

For indirect drive targets, the capsules are mounted inside a hohlraum. For current experiments, the hohlraums are thin metal cylinders or spheres a few millimeters in diameter and length. For the NIF these dimensions will be just under a centimeter. For IFE they will be just over a centimeter and the distributed radiator concept for heavy ion fusion may require new hohlraum materials. ICF hohlraums are currently made by electroplating the hohlraum material, generally gold, onto a diamond-turned mandrel which is dissolved, leaving the empty hohlraum shell. This technique does not extrapolate to mass production. Stamping, die-casting and injection molding, however, may hold promise for IFE hohlraum production but have not been seriously considered. We addressed the following questions:

- What are the material requirements for the hohlraums?

- What are the critical technological issues to mass production of hohlraums?

- What techniques hold promise for hohlraum production?

Technologies/Issues

1. Metal hohlraums with doped Be radiation converters. The current ICF fabrication technique of machining a mandrel, plating the mandrel and then dissolving the mandrel will almost certainly not extrapolate to a low cost process for IFE. Stamping or die casting of hohlraum parts, possibly with a dip-coated plastic outer layer appears practical. Issues: accuracy of manufacture and automated assembly.
2. Distributed radiator hohlraums. These might be made by die casting the many foam components, followed by automated assembly and gluing. Issues: Materials selection and properties (strength, fabricability, etc.), accuracy, practical automated handling techniques, and cost.

Opportunities/Approach

We will use the same approach as described above for capsules.

Target characterization.

Precise target characterization of every target is needed to prepare the complete “pedigree” demanded by the ICF experimentalists. Characterization is largely done manually and is laborious. For IFE the target production processes must be sufficiently repeatable and accurate that characterization can be fully automated and used only with statistical sampling of key parameters for process control. We addressed the following question:

- What parameters must be monitored and how may they be controlled?

Technologies/Issues

The characterization process must not slow down production or add cost. That puts demands on every stage of the target production process for uniform product with relatively few outside acceptable limits for a successful target. Given that, one 1) can cull the product stream for gross defects which can be recognized on the fly and 2) characterize a small fraction of the product stream off-line to establish the average target characteristics for process control. There may well be target parameters (the inside ice roughness, or the centering of a shell in a hohlraum, in some target designs) which cannot be measured in a real target and whose value will have to be established during process development by surrogates.

The characterization techniques used for culling and process control will depend on the details of the target design; the target design, production process, and characterization techniques will have to be developed together for each target design.

Opportunities/Approach

Characterization will be addressed by design studies of target factories, study of sample rate statistics for required target yields (>99%) with reasonable production quality specifications. These studies will be refined as manufacturing process quality characteristics become known from capsule and hohlraum fabrication development.

Characterization for culling will be limited to attributes which can be rapidly recognized on the fly. Mass check by looking at the deflection caused by a gas puff, size and crude shape verification by shadowgraphy, and concentricity by rolling uniformity are three examples. Fast image analysis may be able to identify more subtle errors, but they should be limited to flaws previously established as common to the production technique and with the objective to reduce the number of "duds" in the output to the order of <1% of production.

Characterization for process control will be done on a small number of shells (<~1%) and done off-line so that measurement times up to a minute is feasible. For transparent shells, one could use interference microscopy on two orthogonal axes, along with pattern recognition to enable analyses for layer symmetry, thickness, crude surface roughness. This information would be fed back to the previous production step for iterative optimization of production parameters. Opaque shells might be tested through resonant ultrasound using laser driver/detection to reduce shell handling and increase throughput.

3.3.2.2 Target Fill and Layering

Targets for ICF experiments are filled by permeation and a uniform DT ice layer is formed by a process known as "beta layering". By use of very precise temperature control, excellent layer thickness uniformity and surface smoothness of about 1 μm RMS can be achieved. These processes are suited to IFE although the long fill and layering times needed may result in large (up to ~10 kg) tritium inventories. If IFE targets need DT ice smoothness better than ~1μm to achieve high gain, new layering techniques will be needed, such as the infra-red and microwave heating techniques that have shown about a factor of 2 improvement in DT ice surface smoothness.

Target filling.

Currently ICF capsules are filled by permeation. Filling of thick walled capsules can take as long as 40 hours and very precise pressure and temperature control is needed. Permeation filling of IFE polymer capsules may be practical, although for thin-walled direct drive capsules with high fill pressure to achieve thick DT layers, the pressure control precision requirements may be a significant challenge and the resulting tritium inventory may be large. For beryllium capsules permeation fill may not be practical. We addressed the following questions:

- What are the key technologies needed to fill of polymer capsules and what are the critical technological issues and implications for tritium inventory?
- What techniques might be used to fill beryllium capsules?

Technologies/Issues

1. CH Capsules.
    - Diffusion fill. Issues: Tritium inventory
    - Injection fill. Issues: Practicality, impact on capsule surface finish.
2. Be Capsules
    - Diffusion fill at high temperature. Issues: Feasibility of diffusion fill for Be. Tritium inventory.
    - Join hemispheres in high pressure DT atmosphere. Issues: Practicality, tritium inventory, cost

Opportunities/Approach

Development of ignition capsules for the NIF must address many of the issues facing IFE target fill. The results of this development will benefit IFE. We must do design studies to evaluate the attractiveness of various options.

DT Layering

DT layer smoothness is a potential performance limitation for IFE. The smoothness needed for indirect drive plastic targets appears to be very close to the limits of smoothness that can be achieved by very carefully controlled beta layering. Use of infra-red or microwave energy for enhanced layering may achieve still smoother DT ice surfaces. Use of polyimide or beryllium capsules may relax the surface smoothness required by factors of several. Since the gain curve is very sensitive to smoothness, a small variation in surface roughness might make a large difference in target gain. If the target gain is significantly reduced or if it is highly variable from shot to shot, this would be a performance limitation for IFE. For direct drive, the surface smoothness may need to be almost an order of magnitude better than has been achieved to date. The technology associated with layering is also a challenge. The individual target must be placed inside a uniform temperature "layering sphere" for several hours to achieve proper layer thickness uniformity and the temperature must be near the DT triple point (~19.5 K) to achieve adequate smoothness, while the desired DT gas pressure may require a temperature of around 18K. Innovative solutions may be possible. Techniques have been proposed for achieving a liquid surface on the inside DT surface that would be very smooth. These techniques would require development and would entail significant technical risk. We addressed the following questions:

- What are the DT ice smoothness characteristics and can they be improved in the future or can the smoothness requirements be relaxed in the future?

- What techniques can be used to observe the DT ice layer inside opaque Be capsules?

- What concepts appear practical for IFE target beta layering in large quantities?

- What techniques are potentially available for enhanced layering and reduced gas pressure, and what are their implications for tritium inventory?

Technologies/Issues

• Beta Layering. Issues: Beta layering appears to be limited to a ~1μm surface finish. Is this adequate for IFE target designs? Beta layering takes about ten 30 minute e-folds or 5 hours, resulting in a larger tritium inventory.

• IR and μW enhanced layering. Issues: Can adequate uniformity be achieved?

• Thermal profile. A major issue for all solid layering techniques is the requirement to establish very precise spherical temperature isotherms and to keep the capsule in this environment until a few seconds before shot time. Use of a layering sphere is planned for ICF. This would be impractical for IFE and lead to very high costs. A possible approach is to use a fluidized bed, which can provide a very uniform temperature environment. For indirect drive, it would be strongly preferable to insert the target in the hohlraum after layering. Issues: Practicality, handling after layering.

• Resonant ultrasound spectroscopy is currently being investigated to observe the DT ice layer inside opaque capsules.

Opportunities/Approach

We will let the NIF program resolve the issues of surface smoothness, and practicality of IR and μW techniques. IFE must do design studies of IFE layering systems and characteristics. Bench scale experiments of fluid bed layering may be desirable.

3.3.2.3 Target Injection and Tracking

Preliminary design studies of target injection for both direct drive and indirect drive IFE power plants were done as part of the SOMBRERO and OSIRIS studies completed in early 1992. The direct drive SOMBRERO design proposed a light gas gun to accelerate the cryogenic target capsules enclosed in a protective sabot. After separation of the sabot by centrifugal force, the capsule would be tracked using cross-axis light sources and detectors, and the laser beams were steered by movable mirrors to hit the target when it reached chamber center. Target steering after injection was not proposed. The indirect drive OSIRIS design proposed a similar gas gun system without a sabot for injection and crossed dipole steering magnets to direct the beams.

Target injection.

A gas gun indirect drive target injection experiment was done at LBNL. The results showed that relatively simple gas gun technology could repeatable inject a non-cryogenic simulated indirect drive target to within about 5 mm of the driver focus point, easily within the range of laser or beam steering mechanisms to hit, but not sufficient to avoid the need for beam steering. Recent results of DT ice layer tolerance of temperature changes indicate that much higher injection speed may be needed for direct drive targets. We addressed the following questions:

- What are the key technologies that are needed to inject targets at a high rep-rate and what speed is required? Is the gas gun system adequate for indirect drive and will it function in the hostile environment of a liquid wall chamber?
- What approaches should be pursued for high speed direct drive target injection?
- Are there thermal protection schemes for targets that could allow lower injection speed?
- What are the possibilities and trade-offs for reducing the chamber gas pressure on target heating and chamber life?

Technologies/Issues

1. Indirect Drive

We believe that a gas gun will probably be adequate for indirect drive target injection. Experiments at LBNL indicated adequate placement of room temperature target sized projectiles. The hohlraum with an external case, possibly made of Flibe, will provide adequate thermal insulation for the capsule. Shutters and shields will be needed to protect the barrel from Flibe droplets and neutron material damage. The propellant gas purge will help keep Flibe vapor out of the barrel, although pumping of the propellant gas to keep it out of the target chamber is a concern. Barrel wear and jamming during acceleration are concerns for any injector that has physical contact with the target.

2. Direct Drive

Electrostatic levitation might be possible for direct drive target injection. Due to vacuum breakdown, electrostatic acceleration is limited to order 1,000 m/s^2. It has the advantages (no wear or jamming and less sensitive to neutron swelling) and disadvantages (more difficult position control) of a non-contacting accelerator.

The maximum acceleration of a target is also limited by induced stress in the DT. The combination of low acceleration and gas friction with the barrel wall in a gas gun will limit target speed to about 400 m/s. If a gas gun is used for a direct drive target, we believe that an axially spring loaded two piece sabot will provide better thermal protection and cause less transverse capsule momentum than a centrifugal force separated sabot.

Magnetic acceleration schemes such as iron insert and high Tc superconductor are also viable target injection options which avoid the high temperature operation and propellant gas associated with a gas gun.

Reflective coatings will reduce the radiation heating of a target and are actually part of the recent NRL direct drive target design. A frozen gas layer on the target has been proposed but is not well understood.

The SOMBRERO chamber used 0.5 Torr of Xenon gas to reduce the rate at which x-ray and plasma energy would be deposited on the chamber wall surface. This kept the chamber surface temperature excursion below the value for which significant ablation would occur. This pressure is sufficient to change the position of a direct drive target by order of 20 cm and require in-chamber target tracking. With high reflectivity targets and gas filled chambers, the majority of the target heating will be from the gas rather than from radiation. We do not have a reliable model for the gas heating of a target in the pressure, temperature, and target speed regimes we expect to operate in. The reduced x-ray emissions from current low-z target designs may reduce the requirement for a protective gas in the chamber and should be evaluated. We also do not know with certainty what temperature profile will be required to avoid changes in the target that would reduce target gain. These are two important areas for near term study. A wetted chamber wall design such as was used in OSIRIS, Prometheus and Koyo could eliminate the need for a chamber gas. The use of magnetic fields to divert the charged particle debris from the wall should also be investigated.

Due to greater mass and thermal insulation, indirect drive targets are much less susceptible to target heating and velocity change due to gas in the chamber than are direct drive targets. In addition, the gas density in an indirect drive chamber is much less than in a gas-protected direct drive chamber ($\sim 5 \times 10^{13}$ vs. 2×10^{16} atoms/cm^3).

There was interested expressed at SnowMass in improving the target injection accuracy over the results achieved at LBNL. One possibility that will be seriously considered is target steering. Additional suggestions included:

- Look into e-beam or laser “rocket” target acceleration
- Check on possible inadvertent build-up of electric charge on direct drive targets which could affect trajectory.
- Evaluate the effect of a target hitting the opposite wall in the case of a driver mis-fire, or an injector or driver beam steering error.

Opportunities/Approach

We must carry out design studies of various injection options. The gas gun appears adequate for indirect drive, but the issue of gas load to the target chamber must be studied. Indirect drive injection experiments with beam and/or target steering are needed. The accelerator to be used for direct drive must be chosen and experiments done similar to those that have been done for indirect drive. An injection system for the IRE must be built. Cryogenic target injection system experiments must be done.

Target tracking.

The LBNL experiments showed that for low speed (~100 m/s) indirect drive target injection photodiode detector technology was adequate to detect the target position with sufficient accuracy that the driver beams should be able to achieve the ~±200 μm accuracy needed. The case of higher speed direct drive targets must now be considered. Direct drive targets are expected to require greater beam-on-target accuracy of ~±20 μm. We addressed the following questions:

- What detector technology will be adequate for direct drive target tracking?

- Can target tracking signals, when coupled with HIB steering magnet controls and laser steering mirror controls provide adequate time and accuracy to allow the target to be hit on the fly?

Technologies/Issues

There is no known fundamental limitation to prevent optical tracking from providing adequate position prediction capability. It does require an order of magnitude better tracking accuracy than has been achieved thus far. A laser beam combined with several smaller photodiodes should be able to provide the required position measurement precision. Care (such as supporting structure temperature control and vibration isolation) must be exercised to ensure that the light source and detectors do not move more than a few microns. A demonstration is required to answer this question in the affirmative.

We expect that ion beams can be steered to hit targets with the required accuracy for indirect drive. Tracking signals have already demonstrated the required accuracy. The required steering field is modest (of order 1 Tesla). Colliding beams have been directed with much greater accuracy at existing high-energy accelerators. Feed back of how accurately the beams are hitting the targets will probably be necessary for occasional steering calibration.

Steering and timing laser beams with sufficient accuracy for a direct drive target will be much more challenging, but is probably also achievable. Measuring the position that beams hit a target with sufficient accuracy for feed back to recalibrate steering mirrors will also be challenging. This feedback measurement would presumably be done with unfilled targets and could require large lenses due to the diffraction limit on resolution.

In-chamber target tracking may be needed and could be facilitated by the use of the laser optics and beam paths. If there is a chamber gas, it is likely that in-chamber tracking of direct drive targets will be needed.

Opportunities/Approach

We must continue the laboratory development of tracking as part of the injection technology development. These systems will then be demonstrated in the lab and later on the IRE.

3.3.3 IFE Roadmap Plans

IFE Target Fabrication and Injection are part of the IFE Roadmap. Activities are divided into Phase I, planned over the next 4 years (FY00-03), and Phase II, planned for the following 9 years (FY04-12) when the IFE Integrated Research Experiment(s) will be built and operated.

During Phase I, we will carry out the following tasks to support the decision as to whether to proceed with the IFE IRE, and if so, what target technologies to use:

Target Fabrication

- Work with target designers and chamber developers to select promising target designs that optimize target gain, robustness and cost.
- Develop materials for IFE target requirements, such as robust foams, doped ablators and distributed converter hohlraums for HIF.
- Develop mass production techniques by reviewing and identifying suitable industrial technologies (such as microencapsulation, fluidized bed coaters, injection molding of hohlraum parts, etc.), demonstrating on the lab bench that they can achieve the accuracy needed, and projecting that they can meet IFE coat goals.
- Develop statistical quality control characterization concepts.

Target Injection

- Work with target designers and chamber developers to select promising target and chamber designs and to define their injection requirements
- Select, design and develop the target injection systems best suited for direct drive and indirect drive targets
- Demonstrate injection and tracking of simulated targets at room temperature
- Measure the thermal response of cryogenic targets and demonstrate methods for thermal protection in the laboratory

During Phase II of the IFE Roadmap, we will carry out the following tasks:

Target Fabrication

- Carry out bench scale tests of IFE target production processes
- Evaluate proposed and alternative processes for accuracy, reliability/repeatability and cost
- Provide prototype targets for experiments on the IRE.

Target Injection

- Add cryogenic target capability and a high temperature surrogate target chamber to the Phase I injector-tracking system for injection experiments
- Provide a target injection-tracking system to the IRE for integrated system experiments

The cost of this IFE Target Fabrication and Injection development activity, which is part of the IFE Chamber and Target Element of the OFES Virtual Laboratory for Technology, is estimated to be approximately $3M/yr for 4 years (FY 2000 - FY 2003) to complete Phase I. The costs for Phase II can be estimated when the driver choice(s) for the IRE have been made.

3.3.4 Conclusions

IFE target fabrication, fill, layering, injection and tracking are key features of an IFE power plant. Numerous issues exist that must be resolved for IFE to be a practical energy alternative. However, numerous possible solutions also exist that, at least on paper and in design studies, appear attractive. The basic approach that is recommended is to take maximum advantage of the target fill and layering work that is being done by the ICF Program for the NIF. In parallel, we must carry out IFE design studies and modest scale laboratory development activities to demonstrate and select the appropriate options. This effort is expected to result in the demonstration that a credible pathway exists to practical IFE target fabrication and injection with a high probability of success. This information will contribute to the decision in about 2003 of whether or not to proceed with an IFE IRE and if so, what technologies to use. These will then be applied to the IRE, demonstrating many of the target fabrication and injection technologies needed for IFE.

4. Common Question C

4.0 Introduction

This question includes two parts of common interest to all areas of technology. The first (C1) is a working definition for R&D stages from a technology viewpoint. The second (C2) is the contribution of technology research to science. Both parts of the question were addressed mostly prior to Snowmass via contributions from many scientists and engineers and in "off-line" discussions during the Snowmass meeting. A brief session was held at Snowmass to review the output for Common Question C. The two parts of the questions, C1 and C2, are summarized in sections 4.1 and 4.2 respectively.

4.1 Research and Development Stages, Working Definitions (Common C_1)

Key Questions

What constitutes concept exploration, engineering proof of principle and engineering performance extension for fusion energy systems?

Authors

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Introduction

The restructured fusion energy sciences program has adopted a strategy in which exploration of alternate confinement schemes has been embraced as a means to seek potentially more attractive concepts for fusion energy applications as well as to increase our overall understanding of plasma confinement. Analogous changes have been implemented in the fusion technology program, where new ideas for both plasma enabling and fusion energy technologies have been encouraged. In order to provide a framework for evaluating and comparing progress for alternate technologies, and to establish guidelines for major programmatic decisions, clearly-defined stages of development are needed.

Technology stages of development can be defined in terms analogous to stages of development for confinement concepts that previously have been articulated by FESAC [1,2], including:

1) Concept exploration,
2) Proof-of-principle,
3) Performance extension, and
4) Fusion energy development.

Technology research encompasses a wide range of components and issues; the interpretation of "concept" here is broadened to include not only specific design concepts, but also general design strategies or resolution of issues that span a range of design concepts.

The stages of development of a concept actually represent points on a continuous scale. However, pragmatically, the boundaries between various stages usually represent quantum changes in the cost of program, in the level of commitment to that concept, and in the focus of the program. In each stage, the research program contains experiments, theory & modeling, and design studies elements, as well as supporting plasma physics that may be needed for a concept. The mix of these elements varies in each stage. Table 1 summarizes the distinguishing characteristics of the first three stages that are considered here.

Neutrons are the largest source of energy in DT fusion, and drive some of the most important and complicated phenomena. They are also by far the most expensive environmental condition to provide in experiments. Therefore, a key question for the fusion program is how to proceed through the development stages with a minimum requirement on neutron facilities. The need for neutrons and the extent to which fusion nuclear components can be developed without neutron sources remains an open question for the field. In the development stages described below, only the non-neutron experiments are considered; neutron facilities are considered large, line-item requirements for the program as a whole.

Table 1.
Development Stages and Distinguishing Characteristics

Development Stage	**Focus of Research**	**Level of Integration**	**Objectives**
Concept (Idea) Exploration	Studies of underlying phenomena	Single effects	Establish basic feasibility
Proof-of-Principle	Resolution of critical issues	Multiple effects, range of parameters	Establish power plant feasibility
Performance Extension	Establish performance Limits	Partially integrated systems	Establish power plant attractiveness

For each development stage, we describe the technical and programmatic features and key metrics for defining success and deciding on the readiness for a concept to proceed to a given step. The decision to proceed from one stage to the next should be based on the maturity of the concept in order to be reasonably confident that: 1) the next stage of the program will be successful; and 2) the anticipated benefits of the next stage of the research justifies the increased level of effort.

The FESAC program metrics and criteria are summarized in Table 2. These metrics are relevant for all stages of development, and are recommended to help guide and assess individual program elements.

Table 2.
Program Evaluation Criteria

1. Quality of Research	Is the proposed research program of very high quality; does it have scientific and technical credibility; is it based on an understanding appropriate for its stage of the program?
2. Confidence for Next Step	Will the proposed program provide reasonable expectation for a knowledge base to proceed to the next stage?
3. Engineering Science and Technology Benefit	What is the benefit to advancement in general engineering science issues?
4. Issue Resolution	Is the research likely to resolve key issues and provide the basis for a decision to advance to the next stage, to re-direct within a stage, or to terminate the concept?
5. Leading Edge	Is the research at the leading edge in the context of the national and international fusion programs?
6. Energy Vision.	What is the overall attractiveness of the energy vision for this concept?
7. Program Issues	*E.g.,* construction and operating costs, adequacy of resources, opportunities to be a national research facility, opportunities to leverage existing facilities.
8. Portfolio Balance.	Does the proposed program maintain a balanced portfolio of research opportunities?
9. Science/Technology Goals.	How does the proposed program contribute to broad based national science and technology goals and educational opportunities?
10. Milestones.	What are the key milestones to accomplish the proposed program?

Concept exploration

Concept exploration programs are aimed at innovation and basic understanding of relevant scientific and engineering phenomena. They consist of experiments costing typically less than $1M/year and/or theory and modeling, and strive at establishing: 1) the basic feasibility of a concept (*e.g.,* for a first wall/blanket system, these issues include establishing underlying thermal hydraulic and thermomechanical characteristics, tritium breeding potential, safety and environmental features, power conversion and power density limits, and compatibility with plasma confinement concepts); and/or 2) exploring certain phenomena of interest and benefit to multiple concepts. Evaluating the concept for power plant applications should be limited to studying feasibility and identification of potential advantages and disadvantages, since reliable scaling information for extrapolation to fusion conditions might not be available.

Many independent experiments and modeling activities are preferred at this level and can be attempted in parallel, each focusing on a small set of issues. High risk, large payoff research is desirable and should be encouraged. Activities should be of short duration (less than 3 years, requiring renewal after a 3 year period) in order to allow for a high turnover rate.

The major benefits of these programs are in encouraging innovation and advancing general and fusion engineering science.

Engineering proof of principle

The proof-of-principle (PoP) stage is the lowest cost program aimed at developing an integrated and broad understanding of the basic scientific and engineering aspects of the concept which can be scaled with confidence to provide a basis for evaluating the potential of the concept for fusion energy applications. The basic prerequisite for embarking on an engineering PoP stage is that (1) its scientific and engineering basis looks promising and that (2) it will lead to an attractive energy utilization embodiment. As with the concept exploration stage, the PoP stage is a combined effort involving experiments, modeling and theory.

PoP experiments generally involve operation over a range of parameters. The parameters might not be entirely fusion-relevant in absolute terms, but should be sufficient to provide scaling relationships useful to establish a predictive capability for evaluating the concept. The development of modeling tools and careful diagnosis of experiments is a key element of the PoP stage. Experimental facilities usually can be expected to be in the range of $1M to $10M.

There are a number of criteria that should be applied to the engineering PoP stage:

- Scientific and engineering credibility and degree of engineering understanding available to launch the PoP program.
- Benefit to advancement of general engineering science and technology.
- Opportunity to address most key technology issues of concept.
- Represents leading edge research in context of national and international fusion programs.
- An attractive energy systems vision.
- Clearly defined milestones and deliverables.
- Impact on overall fusion program development cost and schedules.
- Other opportunities for collaborative research and education of students.

The construction, operation, and analysis of a Proof-of-Principle-class experiment takes roughly five to ten years, which sets the lower bound on the duration of a Proof-of-Principle program. Furthermore, substantial resources are necessary to operate a Proof-of-Principle-class experiment. These programs, therefore, should be national endeavors, drawing expertise from many institutions. Sufficient resources should be committed both to the Proof-of-Principle-class device as well as the supporting smaller experiments, theory and modeling, and power-plant studies in order to ensure an acceptable return on the investment of the talent as well as resources in such an activity.

It is beneficial for the Proof-of-Principle program to include Concept-Exploration-class experiments which focus on resolving certain key issues of the concept and help promote further innovations. Theory, modeling, and benchmarking with experiments should be vigorously pursued in order to provide a theoretical basis for scaling phenomena and evaluating the potential of the concept. Power-plant studies, including in-depth physics and engineering analysis, should be carried out to identify key physics and technological issues and help define the research program. Any technological issue specific to the concept should also be addressed during the Proof-of-Principle stage.

The major benefits at this stage are advancement of fusion energy science with some contribution to fusion energy development and power plants.

Engineering performance extension

Performance extension programs explore the engineering behavior of the concept at or near the fusion-relevant regime in absolute parameters, albeit without a burning plasma.

This stage aims at generating sufficient confidence so that absolute parameters needed for a fusion development device can be achieved and a fusion development program with a reasonable cost can be attempted. At this stage, the predictive capability and scaling information is refined further, new phenomena in fusion-relevant regimes are examined, and the performance of the concept is optimized. Because of the demand on absolute performance, a large single facility ($5-10M per year) may be needed and equipped with a variety of auxiliary systems for control and operational flexibility as well as extensive diagnostics providing complete coverage in space and time. This program should contain Concept-Exploration-class and possibly Proof-of-Principle-class experiments to help in optimization of the concept. Extensive theory and modeling activities should exist to analyze the experimental results on all issues and start providing a predictive capability for the concept. Both design studies and assessments of power plant applications, including in-depth physics and engineering analyses, should be carried out to focus on critical issues, help in optimizing the physics regimes, and evaluate the potential of the concept for fusion development and power plants.

Key questions for this stage include the importance of neutrons and the need for coupling to a plasma confinement device.

As with the Proof-of-Principle program, this must be a national endeavor, which should include expertise from many institutions and sufficient resources allocated for supporting activities. The major benefits at this stage are contributions to fusion energy development and power plants, and advancement of fusion energy science and technology.

References

[1] Report of the FESAC Scicom Alternate Concepts Panel, July 1996. (http://aries.ucsd.edu/SCICOM/AC-PANEL)

[2] FESAC Panel on Criteria, Goals and Metrics, July 1999. (http://wwwofe.er.doe.gov/More_HTML/FESAC_Charges_Reports.html)

Contributions to Science (Common C_2)

Key Questions

What contributions will technology make to advancing science? What research areas will be pushing the frontiers of science?

Topic Leaders

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4.2.1 Introduction

Technology research contributes to both the energy and science goals of the US Fusion Energy Sciences program. Contributions to the energy goal are generally well-appreciated: "plasma enabling technologies" are needed to support the creation and control of reactor grade plasmas, whereas "fusion energy technologies" are needed to create a useful product that meets economic, safety and environmental requirements. Less appreciated is the fact that the technology program also makes important contributions to the science mission of the program. In the same way that improved fundamental understanding of plasma physics is expected to help create more desirable characteristics of burning plasmas, improved fundamental understanding of the relevant engineering sciences also is expected to help create a more desirable end-product. A broad array of engineering sciences is involved in fusion technology, including solid state materials science, fluid mechanics, chemistry and nuclear science, to name only a few. In this document, we enumerate several of the more important disciplines and explain how advances can serve not only to accelerate the development of an attractive fusion energy product, but also to push the frontiers of basic engineering sciences.

The fusion technology program also plays an important role as "enabler of science". This has long been appreciated within the fusion program itself, as many of the most significant advances in confinement became possible as a direct consequence of improvements in heating and current drive, plasma feedback control, magnetics, plasma-interactive components and other plasma enabling technologies. In this document we summarize plasma science enabling technologies, with emphasis on the far-reaching benefits of enabling technologies for plasma research beyond the boundaries of the fusion energy sciences program.

4.2.2 Fusion Technology Contributions to the Engineering Sciences

Because the fusion technology program contains science, engineering science and applied engineering facets, we begin by defining what we mean by the advancement of science for fusion. Contributions that the technology program can make to advancing science include:

(1) Experiments, theory and/or modeling that improve the fundamental understanding of the underlying phenomena and/or processes needed to make fusion a viable energy source, and which may contribute to other broad fields of science and applied engineering.

(2) Applied engineering problems that represent computational challenges and whose solution may have applicability to other fields of science and engineering.

While relevance to fusion energy may be an important criterion for making programmatic decisions, it is not itself a measure of scientific merit. Several metrics are useful in determining the quality of science, including impact on the broader scientific community (*e.g.,* as measured by the frequency of citations in the literature) and the extent to which the research pushes the frontiers of existing knowledge. Below (and summarized in Table 1) are selected activities in the technology program that fit this definition. This list is not intended to be comprehensive, but rather representative of the numerous opportunities for leading-edge engineering science.

Table 1
Examples of Engineering Science Opportunities

1	**Materials science**
2	**Surface science and atomic physics**
3	**Fluid dynamics and magnetohydrodynamics**
4	**Heat and mass transfer**
5	**Chemistry**
6	**Aerosol science**
7	**Magnet science and superconductivity**
8	**Beams, Accelerators and Coherent Radiation Generation**
9	**Nuclear science**

4.2.2.1 Materials Science

Understanding the underlying physical behavior of materials is key to improving and optimizing the individual systems in which they are used. Many types of materials are expected to be used in a fusion power plant, including:

- Metals
- Ceramics and composites
- High-strength superalloys
- Superconductors
- Polymers and foams
- Hydrogenic solids
- Insulators

There are numerous examples where fusion materials science research is having a positive impact on the broader engineering/materials science fields. The following examples focus primarily on ceramics, metals and polymer foams, which are the subject of some of the largest and most coordinated materials science activities in fusion.

<u>Ceramics and composites</u>. Improvements in the basic understanding of phonon scattering and transport in materials (including development of superposition relationships for different types of defects) can be obtained from experimental studies and associated modeling of the dose-dependent thermal conductivity of irradiated ceramics containing controlled amounts of impurities. In the area of composite materials, fusion research is enabling the development of improved oxidation-resistant (multilayer SiC) interphases in SiC/SiC composites which may be used in aggressive chemical environments. Fundamental research on ceramics by fusion researchers has led to the first known experimental measurements of point defect (interstitial) migration energies in SiC, alumina and spinel. Computer modeling of radiation effects in materials (*e.g.,* V, SiC) is stimulating the first-principles development of improved interatomic potentials for these materials.

<u>Metals</u>. Experimental and theoretical analysis of neutron-irradiated metals is providing an improved understanding of the fundamentals of mechanical deformation, which has a far-reaching impact on numerous engineering disciplines. For example, it appears possible to finally obtain the constitutive equations for twinning (which is one of the 6 possible deformation mechanisms in solids) from an analysis of neutron-irradiated metals. Most of the present-day understanding of fracture mechanics (essential for all advanced engineering structural applications) is derived from early studies on neutron irradiated metals. Multi-scale computer modeling of displacement cascade damage and dislocation/defect cluster interactions, coupled with analysis of neutron-irradiated fusion materials experiments, can provide fundamental

understanding related to the physical mechanisms of flow and fracture of deformed metals. Significant advances in the science of mechanical deformation of refractory metals are being provided by fusion research on vanadium alloys and other refractory metals. Finally, the need for improved creep-resistant structural alloys for fusion is stimulating fundamental studies on the microchemistry of oxide dispersions in copper alloys and ferritic steel. Research using high-resolution analytical electron microscopy and the 3-D atom probe is providing the scientific basis for developing new classes of creep-resistant oxide dispersion strengthened materials.

Foams and polymers for IFE targets. Currently available foams do not meet IFE requirements on composition and vigorous density, pore size, and surface finish requirements. Not only do these materials need to be assessed cryogenically under static conditions, but their performance under acceleration comparable to those conditions in a gas gun will need to be evaluated. Advances in materials science, including the area of nanotechnology, are expected to provide more desirable properties. Potential synthetic techniques include nanotemplates, self-assembled materials, dendrimers, and phase separated materials. Porous materials have a vast potential outside of the field of fusion. Low density porous materials are of interest to the microelectronics industry, are useful in numerous membrane applications, and can be used for the controlled release of drugs.

Hydrogenic solids. The DT fuel in an inertial fusion energy target must be in the form of a thin (~100 - 300 μm) layer on the inside of the spherical ~5 mm diameter capsule at the center of the target. The inside surface smoothness of this DT ice layer is critical to avoiding Rayleigh-Taylor instabilities as the target is compressed to ignition conditions. Surface smoothness of ~1 μm RMS or even less may be required. To create a smooth, symmetric layer of DT ice inside an inertial fusion capsule will require significant advances in the understanding of the fundamental processes of hydrogen ice formation, crystal growth and structure, strength and vapor phase transport. These processes dominate the formation and smoothing of the DT ice layer. Understanding these processes is important to determining how to control the morphology of the DT ice layer and its surface finish.

4.2.2.2 Surface Science and Atomic Physics

The scientific basis needed to engineer the plasma edge requires understanding the underlying chemistry, solid state physics, and fluid mechanics that controls the transport behavior of plasma particles back to the plasma, in the edge itself and in plasma facing materials. Fusion experimental studies in the laboratory and on existing fusion devices and theoretical analysis on impurity transport in the plasma contribute to the fields of plasma physics and atomic physics by providing fundamental data to describe transport in the edge at fusion relevant temperatures and densities. Extensive surface science experiments are being conducted to understand the mechanisms controlling the release of particles from surfaces, such as sputtering and evaporation chemical erosion as a function of ion energy, angle of incidence and level of impurities for both liquid and solid materials. Surface science measurement of uptake and subsequent behavior of deuterium in plasma facing components contributes to the field of mass transport in solid materials. Fusion measurements add to the database on the diffusivity and solubility of hydrogen in solid materials and the influence of radiation damage on the overall permeability of the material.

4.2.2.3 Fluid Dynamics and Magnetohydrodynamics

The severe environment of a burning plasma, including very high heat flux, has led to the need cooling schemes that push the frontiers of knowledge in several sub-disciplines. This has become especially true with the recent consideration of free-surface design concepts. The presence of a strong magnetic field causes complex interactions linking the fluid dynamics of conducting liquids with the electromagnetic environment, leading to a wealth of new phenomena. Even molten salts possess sufficient electrical conductivity to exhibit MHD interactions. Below we treat the topics of free-surface turbulence, granular fluid dynamics, and MHD.

<u>Turbulence interaction with free surfaces</u>. In the general realm of (non-MHD) turbulent fluid mechanics, turbulence interaction with free surfaces is still not well-understood due to its complicated nature and challenging measurement and modeling requirements. The near-surface turbulent characteristics vary greatly depending upon the character of the free surface, *e.g.* wind shear, temperature gradients, large waves, *etc*. In any case, the surface deformations, even if very small, are extremely important in determining the production and/or redistribution of turbulent motion in the near surface regime and represent a very difficult computational challenge for existing computational fluid dynamics techniques. Current state-of-the-art calculations treat the free surface as non-deformable free-slip (equivalent to assuming infinite surface tension), which may fundamentally alter the heat and mass transfer predictions. Fusion researchers in the US and Japan are leading the way to coupling of surface tracking techniques and developing models for both Direct Numerical Simulation and Large Eddy Simulation techniques that will allow coupled calculation of turbulence dynamics and free surface location, as well as heat and passive scalar mass transfer. A new generation of turbulence experiments that introduce a measurable heat flux to a shear-free surface will be required to demonstrate the accuracy of new computational techniques.

Granular flows. A large assemblage of moving particles behaves in many ways like a fluid, even in cases absent conventional "fluidization" by a background carrier gas. Both closed channel and free surface (open channel) flows have applications in certain blanket cooling and advanced plasma-facing design concepts. The absence of cohesive forces leads to aspects of shear flow, turbulence and vorticity very different from ordinary liquids. New phenomena not observed in liquid flows include plug formation due to bridging of contact forces and scattering. Recent advances in computer capabilities have enabled direct modeling of individual particle interactions over very large assemblages, opening up new avenues of investigation. Related fields of study include soil mechanics (*e.g.,* ground flows during seismic events) and granular materials processing.

MHD. Conducting Liquid MHD is a large and growing scientific discipline with applications in a diverse set of fields including electromagnetic processing of metals, geophysics (*e.g.,* geodynamo studies), boundary layer control and turbulence studies. Fusion researchers developed the first general 3D numerical method for the solution of the inviscid core flow equations (including vorticity generation and decay) and provided some of the most detailed data on MHD fluctuations at high field strength (well beyond the transition to bulk laminar flow).

The latest efforts are providing improved understanding of MHD flows and turbulence at walls and free surfaces. For example, heat transfer at the free surface of a poorly conducting liquid is dominated by phenomena of rapid surface renewal by turbulent eddy structures generated either near the free surface due to temperature gradient driven viscosity and surface tension variations, or near the back wall or nozzle surfaces by frictional shear stresses. The intensity of these turbulent structures and their effectiveness in cycling energy from the free surface into the bulk flow of the liquid wall depends heavily on the velocity of the main flow, the deformation of the free surface, the distance from back wall and nozzle surfaces, the degree of damping by the magnetic field and even the magnitude and distribution of the surface heat flux itself. Fusion researchers are developing new models for Reynolds-Averaged Navier-Stokes (RANS) type turbulent formulations that take into account damping by magnetic fields and modification of near wall boundary layers in turbulence producing regions. Direct Numerical Simulations, solving simultaneously for the induced electrical currents and fine structure of turbulent flows are under-development and represent the state-of-the-art in turbulent-MHD computation. The optical transparence of several common low conductivity liquids makes the use of optical techniques in MHD experiments possible and promise to deliver new data for understanding MHD turbulence interactions.

The picture is different for a liquid metal, which may be *laminarized* by the magnetic field, but is still likely to be highly wavy or possess two-dimensional turbulence-like structures with vorticity oriented along the field lines. Surface waves and 2D turbulence increase the area for heat transfer and have motion that helps to convect heat into the bulk flow. Understanding the relative importance of these terms to the dominant conduction and radiation transport effects, and judging the effectiveness of using turbulence promoters such as coarse screens, is required to assess the feasibility of liquid metal walls from the heat transfer point of view. The complicated hydrodynamics is now heavily coupled to the applied magnetic fields and the motion of the plasma through Ohm's law and Maxwell's Equations. The solution to these systems is of similar complexity to the MHD fluid motions in the plasma.

4.2.2.4 Heat and mass transfer

The fusion environment provides unique challenges related to the thermal and thermomechanical behavior of fusion in-vessel components. This has led to significant contributions to thermal sciences, thermomechanics and transport phenomena. Examples include critical heat flux (CHF) in water-cooled systems, convective heat transfer in He-cooled porous media, thermomechanics of pebble beds, and tritium transport in solids.

<u>Critical heat flux</u>. Recent fusion research has led to improved understanding of CHF behavior and local flow interactions under spatially-varying one-sided heating in complex geometries such as the hypervapotron, porous coatings, screw tubes and swirl tape configurations. These results have been valuable in guiding fusion design choices but also serve as a pool of basic information for researchers working on CHF-relevant topics outside of fusion.

<u>Thermal-hydraulics of porous media</u>. A number of innovative techniques to increase the heat transfer capability of advanced He-cooled PFC concepts have been proposed, such as various porous heat exchangers, normal-flow and impingement jet configurations. Since heat transfer enhancement is always accompanied by a pressure drop penalty, optimization requires understanding of the underlying mechanisms. Examples of topics requiring fundamental investigation are the effect of flow dispersion and heat transfer at solid/fluid interfaces.

<u>Pebble bed thermomechanics</u>. Pebble beds are often used in ceramic breeder blankets. Thermomechanics issues are linked to particle-to-particle interactions for both stagnant and flowing bed versions. Fundamental aspects of heat transfer between particles include the combination of Knudsen regime heat transfer through the fluid phase, surface roughness and contact area. The importance of contact forces leads to intimate connections between the thermal and stress state, which in turn leads to a wide variety of

new phenomena.

Coupled heat and mass transfer of hydrogen isotopes. The coupling of heat and mass transfer processes is a key issue related to tritium inventory and recovery in blankets. One of the important successes of the fusion technology program over the last 15 years is the remarkable progress in the understanding of tritium transport mechanisms in ceramic breeders through a complementary program of experiments and model development. Advances have enabled development of models that have allowed the evaluation of fundamental property data and, through calibration and validation with experimental data, in more accurate extrapolations to reactor conditions. Initial estimates in the absence of adequate modeling tools and database were overly conservative and ranged as high as 380 kg. In contrast, recent estimates based on the more sophisticated and accurate tritium release and inventory models over the last few years range from about 1 to 100 g, thereby making ceramic breeder blankets much more attractive on this basis.

4.2.2.5 Chemistry

High Temperature Chemical Reactions and Vaporization. The mobilization of induced activation in fusion materials under air and steam ingress conditions is an important safety issue in all fusion designs. Chemical reactions between fusion materials and air or steam change the chemical form of the structure and, in the case of steam, can lead to the production of combustible quantities of hydrogen. In addition, the presence of air or steam changes the oxygen potential of the system resulting in vaporization of radioactivity from the structure. Experiments and extensive materials characterization are performed to measure the chemical reactivity of fusion materials and the concurrent vaporization. Basic thermodynamic information on the reactions of interest and gas phase and solid state mass transport modeling is used to understand the results of experiments. The data and associated modeling contributes to fundamental understanding on the behavior of fusion materials in high temperature air or steam environments and adds to the database on chemical thermodynamic behavior of materials that has applicability for high temperature and chemical processes in the aerospace, defense, chemical, nuclear and environmental remediation industries.

Polymer and interfacial chemistry. Production of shells for IFE requires an integration of chemistry, particularly polymer and interfacial chemistry, to produce the material, and a combination of rheology, and solution physics, to put it into the correct shape. The various disciplines are intertwined, so the chemistry determines many of the interfacial forces, while the physics of the solutions partially determines the chemical mixtures. And all of them change during the production process. Optical systems will be needed to validate the result. To understand all these forces, their time dependence during production of a shell, and to validate the resulting shell is a significant scientific challenge with both analytic and experimental components. The work required for success in NIF and IFE will require advances in the science of this field.

Surface coatings. Fundamental research on high-integrity thin coatings on structural components offers a potential for substantially increasing the operating space for many high performance structural materials. Coatings may be used to improve corrosion/compatibility limits, serve as hydrogen or tritium barriers, or change the electrical properties (insulator coatings). Research conducted under the Fusion Energy Sciences program is focused on development of a scientific basis for the formation of coatings, characterization of the thermodynamic stability and mechanical integrity of these coatings, and evaluation of the electrical properties of coatings as a function of temperature and chemistry of the environment. The scientific basis developed in this fusion program contributes not only to the fusion applications, but provides a basis for applications in the chemical industry, *e.g.,* as hydrogen barriers or to improve corrosion resistance, and the mechanical industry to improve tribology performance.

The scientific basis for the formation of high integrity coatings with specific properties involves a fundamental understanding of fluid chemistry, thermodynamics of numerous chemical reactions, and atomic bonding of the coating and the substrate. Development of in-situ coating processes by controlling the chemical thermodynamics and kinetics of fluid/substrate reactions is a key issue in the coating of large, complex systems and in the formation of self-healing coatings that can be readily repaired in case any defects occur during operation. As an example, the formation of electrically insulating CaO coatings on vanadium alloy channels to mitigate magnetohydrodynamic (MHD) pressure drop effects in lithium-cooled systems is achieved by controlling the reaction of calcium dissolved in the lithium with oxygen dissolved in the vanadium alloy structure. By controlling the compositions of the solvents, the reaction product consisting of a few microns of CaO formed at the structure/fluid interface provides electrical resistance sufficient to reduce the MHD pressure drop to acceptable levels. By proper control of system parameters the coating composition can be graded for improved adhesion, and self-healing of any defects can be achieved. The scientific basis for this research is being extended to applications of hydrogen permeation barriers to improve performance of advanced systems for the chemical industry.

4.2.2.6 Aerosol Science

Plasma-wall interactions in magnetic fusion (*e.g.,* erosion, disruptions) and target implosion in inertial fusion result in the production of particulate. The presence of particulate could impact operation of the facility. Particulate deposited on mirrors and diagnostics could impair these systems. In addition, the particulate has important safety implications; it could be radioactive, toxic and chemically reactive. Thus, characterization of the particulate in fusion systems is needed to better understand the safety hazard and operational limits that such material could impose on the system. Particulate production is a broad area of aerosol science that has important industrial applications including for example cloud formation, pollution control, precipitation technologies, and production of nanophase material. State of the art aerosol characterization technologies are used to measure the quantity, size and chemical form of the particulate in both magnetic and inertial fusion facilities. In addition, theoretical modeling of the plasma and the relevant vapor condensation, nucleation and growth phenomena are used to understand the basic mechanisms responsible for particulate production in magnetic and inertial fusion systems. This work is expected to add significantly to the understanding of aerosol nucleation phenomena and the transition from plasma to supersaturated gas to particulate.

4.2.2.7 Magnet Science and Superconductivity

The ability to achieve predictable magnet performance for a fusion science device at reasonable cost requires advances in materials science (*e.g.,* better superconductor critical properties: higher field, current density and operating temperature; better structural materials for low temperature; materials with lower resistivity), in manufacturing technology to achieve the sizes required (*e.g.,* long lengths for superconductor and large plates for resistive materials) and in the basic scientific understanding of the design criteria. Advances in each of these areas requires an extensive conductor development effort since the material must be available in the form required for fusion, which typically means large cross-section and high current. An exception is the quadrupoles for Heavy Ion Fusion where high current is not the issue, but material properties are very important and the fundamental scientific understanding and control of the destabilizing influences is the primary problem.

The particular fusion device mission and time for construction determines whether the coils should be resistive, low temperature or high temperature superconductors. Eventually, fusion systems are likely to converge on High T_c materials to reduce both overall device size and operating cost (*e.g.,* cryoplant). High T_c materials offer the promise of very high field operation, but are challenging from the standpoint of the materials science required to produce them in sufficient size while maintaining properites.

The theory of operation for superconducting cables and twisted-filament strands has similarities to that of toroidal plasmas, including the helical current paths, a multiplicity of circulating current modes, coupled thermal-magnetic-mechanical-fluid behavior, and the existence of disruptive modes (*i.e.,* quench). A thorough scientific understanding of the operation and design criteria cannot be done without a continuing small scale and large scale experimental effort with correlation to existing theory. The underlying goal should be to reduce design margins and cost for fusion science devices.

The complexity of the stability/design criteria problem has defied brute-force physical simulation; but, since the physical mechanisms are known, many approximations exist. Computer power is increasing and persistence in generating experimental data should ultimately permit the design of superconducting magnets on a fully scientific basis with reduced margins **and** acceptable risk. The acceptable risk can differ significantly for different applications (*e.g.,* quads for HIF are small enough to allow several full scale prototypes of individual coils to be made as part of design development, whereas this cannot be economically done for many other MFE coil types).

Success in material property enhancement and in scientific modeling of superconducting cable transients will also improve the performance of all pulsed applications, including Superconducting Magnetic Energy Storage (SMES), transformers, generators, and motors, including magnetic levitation.

4.2.2.8 Beams, Accelerators and Coherent Radiation Generation

<u>High-voltage rf breakdown</u>. The study of breakdown of gases exposed to ac fields has been carried out extensively at 60-Hz, and also at microwave frequencies. Less information is available concerning rf breakdown for frequencies in the ion cyclotron range (~10 – 200 MHz), especially in conditions corresponding to those at the edge of a fusion experiment. One of the factors limiting the power that an ion cyclotron launcher can deliver to the plasma is the maximum electric field that the launcher can withstand. Various parameters influence the electric field at which breakdown occurs; a partial list includes: electrode material, surface finish and contamination; background gas pressure and species; exposure to UV or x-ray radiation; bombardment by neutral or charged particles.

In an effort to increase the electric field that ion cyclotron launchers can withstand, a fundamental investigation of rf breakdown is planned. This will use a well-defined, simple geometry and will vary several of the parameters mentioned above. Initial tests will be carried out in vacuum. Subsequently, the same tests will be carried out while the sample is exposed near the edge of a tokamak plasma (ASDEX-U), to determine how *and why* the presence of a plasma nearby affects the breakdown. This work will add to the existing database on breakdown in ac fields, with the addition of the effects of plasma-like radiation.

Gyrotrons. Technology research within the research program of magnetic fusion has led to new coherent sources of radiation at frequencies from the UHF range to the microwave and the millimeter wave region. The development of the gyrotron is a prominent example of such an advance. Gyrotrons have progressed over the past two decades from a 200 kW source at 28 GHz to achieve megawatt power levels at frequencies from 8 to 200 GHz. These are now the most powerful coherent sources of radiation in this entire frequency band. Although developed for plasma heating, these sources have many other applications including materials treatment, advanced radar and spectroscopy. This technology advance has been truly enabling for other branches of science. The physics of coherent radiation generation is based on an understanding of the interaction of electromagnetic radiation with beams of electrons. This interaction is, from a formal point of view, the interaction of waves with a non-neutral plasma, an important branch of plasma physics. Progress in research in this area depends on the development of the understanding of plasma instabilities. Progress in our understanding of plasma physics helps our understanding of radiation generation from electron beams and vice versa.

Gyrotron development has also contributed to the field of accelerator physics. The gyroklystron is a major candidate for use in the propose future electron accelerator, the Next Linear Collider. The gyroklystron is able to generate power levels of over 100 MW at frequencies as high as 35 GHz. This would allow an electron accelerator to be built with much higher gradient than is possible with conventional klystrons, thus reducing the size and cost of future accelerators.

Ion beams. Research on heavy ion beams is addressing the problem of transport of high average power beams at high energy. Research in this field will advance our understanding of the physics of future machines such as the Spallation Neutron Source (SNS) and the Accelerator for Production of Tritium (APT).

4.2.2.9 Nuclear Science

Nuclear data and neutron and photon transport theory are derived from physical laws governing the interaction of neutrons and their induced gamma rays with materials. Fusion neutronics is an engineering practice applying the particle transport principle and fundamental nuclear data. The scientific aspects of neutronics reside in the development of a comprehensive (*i.e.*, complete and accurate) nuclear data base and computer codes solving for particle transport and interactions with blanket and shield materials.

4.2.3 Fusion Technology Contributions to Enabling Plasma Science

4.2.3.1 Overview

Many technologies developed for fusion experiments apply usefully to related plasma-science disciplines. Examples include the symbiosis of:

- inertial-confinement fusion with astrophysics,
- radio-frequency heating and discharge plasmas with applications such as plasma processing and lighting,
- diagnostics with applications such as plasma processing and thrusters,
- dipole experiments with magnetospheric plasma science, and
- ICF beam technology with non-neutral plasmas in accelerators.

These relationships represent important opportunities for fusion research to broaden its knowledge base and each will be discussed briefly below. Technologies developed primarily for fusion research also contribute to *non-plasma* sciences, including:

- fusion device production of neutrons for scattering applications such as tomography,
- fusion production of synchrotron radiation or bremsstrahlung for integrated circuit manufacture and other industrial processes,
- fusion products reacting with higher-Z elements to produce radioisotopes for medical science, and
- large-volume high-field magnets for imaging science.

Although these non-plasma science applications will not be discussed here, they represent a potentially important market for driven fusion devices, some even at small scale.

4.2.3.2 Inertial-confinement fusion (ICF)

The study of ICF plasmas requires experimental measurements and theoretical knowledge of many topics relevant to astrophysical plasmas. The plasma parameter regimes of novas, supernovas, and the interiors of stars are accessible only to ICF plasmas. The National Ignition Facility (NIF), in particular, has embraced plasma science as one important objective of its operation. Details of how NIF, which presently is under construction, and ICF experiments in general relate to astrophysical plasma science are contained in excellent worldwide web pages at:

http://www.llnl.gov/science_on_lasers/ucrl119170.html

The web pages include links to astrophysics and space physics, hydrodynamics, material properties, plasma physics, radiation sources, and radiative properties. ICF experiments relate not only to the obvious area of hot, high-density plasmas, but also to solar-wind shock waves and many atomic physics topics.

4.2.3.3 Radio-Frequency (RF) Heating

The use of plasmas for processing and lighting has historically progressed in large part empirically. As these techniques have improved, market incentives have driven an increased effort to understand their properties. These endeavors have led to fruitful communication and cross-fertilization of ideas with fusion scientists and engineers. This applies especially to the areas of low-temperature plasmas, impurity effects, plasma sheaths, and plasma-surface interactions. As the demand increases for more densely packed integrated circuits at smaller line scales, plasma processing will come even more into demand. Likewise, the immense lighting market gives strong leverage to even modest efficiency improvements. Both areas are opportunity areas for the application of technologies and techniques developed for fusion research.

4.2.3.4 Diagnostics

The impact of diagnostics developed for fusion experiments has been immense. New diagnostics often enable breakthroughs in understanding or lead to observations that unearth theoretical enigmas. The area of plasma processing, in particular, already benefits from collaboration with fusion researchers, and the benefits go both ways in a manner similar to that of the RF-heating field. Many diagnostics developed through the fusion research program have recently been reviewed [K.W. Gentle, "Diagnostics for Magnetically Confined High-Temperature Plasmas," *Rev. Mod. Phys.* **67**, (1995)]. Improvements over the past two decades have allowed fusion researchers to move from measuring globally averaged parameters to more localized measurements that have greatly expanded the understanding of small-scale plasma phenomena. Many of these techniques would help researchers interpret the science and increase the performance of the low-temperature plasmas of industrial applications. Significant growth in non-fusion plasma areas, such as processing and thrusters, can reasonably be expected, so this topic also represents an excellent collaborative opportunity.

4.2.3.5 Dipole

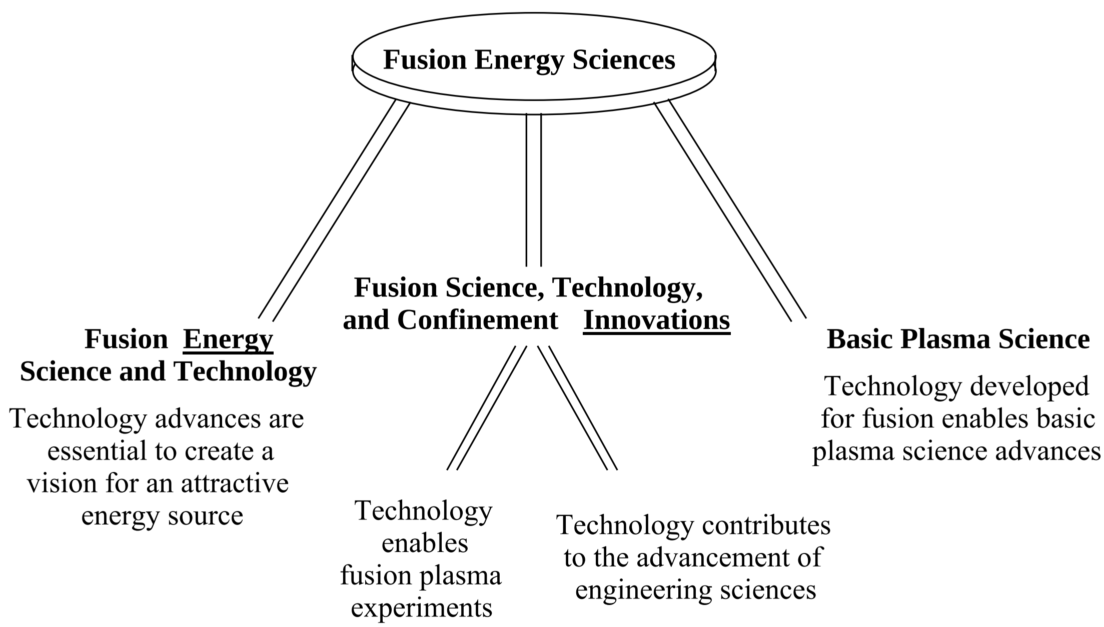
The recently initiated levitated dipole experiment, LDX, at Columbia University represents an excellent example of symbiotic fusion and astrophysical plasma research. Hasegawa invented the dipole fusion reactor concept over a decade ago by analogy with the high-β Jovian radiation belts. Many years later, LDX, although motivated by the fusion quest, may serve as a relatively easily diagnosed test bed for understanding the plasma science of radiation belts.

4.2.3.6 Light-Ion and Heavy-Ion Beams

A close relationship exists between non-neutral accelerator plasmas and the fusion-related development of heavy-ion and light-ion beams. Many prominent fusion pioneers worked on both accelerator and fusion-concept physics. Although there is a strong and interesting relationship between accelerator and ICF fusion beams, the relatively limited market makes this area less important as a fusion opportunity than some of the strongly expanding market mentioned earlier.

4.2.3.7 Summary

Fusion technology impacts both plasma and non-plasma science. Several of the plasma science areas are growing fields ripe for collaboration with fusion researchers. The ICF technology of NIF, in particular, should enable important advances in the science of astrophysical plasmas. Other areas, especially diagnostics and RF heating, could assist the developing science of industrial plasmas for processing and thrusters.



The technology program contributes to both the energy and science goals of the US fusion program

Appendix A – List of Conveners and Topic Leaders for Technology

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Zinkle, S. (ORNL)
Zweben, S. (PPPL)

Appendix C – List of Key Questions

CQ1. What are the merits and issues for liquid walls? What experiments, modeling, and analysis must be done to judge their potential for IFE and MFE? What are the key go/no go issues and how can they be explored quickly?

CQ2. What advances may be possible for evolutionary concepts (e.g. solid plasma facing materials, traditional blanket concepts)? What are the potential near term applications for this area? What is the potential for achieving high temperature and high power density?

CQ3. What should the reliability (MTBF), maintainability (MTTR), and life time goals be to achieve the goal availability? What is the impact of the confinement configuration and the Chamber concept on MTTR and, hence, on MTBF requirements? What can we do today, given the lack of fusion testing data, to assess the prospects for various proposed technology concepts and designs? Given the difficult constraints on the program, how can we show that fusion chamber technologies will be able to meet the reliability, maintainability, and availability goals?

CQ4. What environmental and testing conditions are most important in experiments needed to resolve the key Chamber issues? When are neutron sources and fusion technology testing facilities needed? What issues differentiate among the various options for testing facilities?

CQ5. What is the better strategy for fusion waste minimization: hazard versus volume? What are the implications of each strategy for fusion potential and R&D? What is the potential for recycling?

CQ6. What is the potential of current plasma confinement and chamber technology concepts for attaining tritium self-sufficiency and what are the implications for requirements on plasma and technology R&D? (include issues of tritium fractional burnup in the plasma, and tritium permeation, inventory, and processing). Is there a time window for the availability of tritium startup inventory? What are the implications of such time window on the schedule for tritium-producing Chamber technology?

CQ7. What advances may be possible in materials over the next ten years that can contribute to: 1) improving the vision for an attractive and competitive fusion energy system, and 2) lowering the cost and time for fusion R&D?

PQ1. What is the potential for and what advances will be required in profile control technologies (plasma heating, current drive and fueling) to enable present, near term, and next step devices to meet their performance goals and ultimate research potential?

PQ2. What can be done to lower the cost, improve cost/performance ratio, and improve the reliability and maintainability of magnets (rings within rings) in MFE systems? What are the most promising new magnet technologies (e.g. high T superconductors and high field capability)? What is the role of evolutionary concepts (e.g. Copper magnets) in burning plasma devices and in innovative confinement concepts?

PQ3. Can the technologies needed for low cost, cryogenic targets and a high rep-rate injection system be developed?

C_1. What constitutes concept exploration, engineering proof of principle and engineering performance extension for fusion energy systems?

C_2. What contributions will technology make to advancing science? What research areas will be pushing the frontiers of science?

Appendix D – Technology Sessions Schedule at Snowmass

Schedule Attached Below

	Tuesday				Wednesday				Thursday				Friday			
Room	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D
Cap.	100	55	15	15	100	55	15	15	100	55	15	15	100	55	15	15
1:30-3:15 PM	CQ1 (31)	PQ1 (24)	CQ3 (12)		CQ1 (31)		CQ4 (17)	PQ3 (17)	CQ1 (31)	CQ7 (24)	PQ2 (14)	PQ3 (17)	CQ7 (24)		CQ3 (12)	PQ3 (17)
3:45-5:30 PM		PQ2 (14)	CQ2 (7)	CQ5 (15)	PQ1 (24)		CQ2 (7)	CQ5 (10)		PQ1 (24)	CQ6 (12)	CQ4 (17)	PQ1 (24)	PQ2 (14)	CQ2 (7)	CQ6 (12)

	Tuesday				Wednesday			
Room	A	B	C	D	A	B	C	D
Cap.	100	55	15	15	100	55	15	15
1:30-3:15 PM	CA (86)	CC (32)			CQS (86)	PQSS (55)		
3:45-5:30 PM	CQS (86)	PQM (38)	PQI (17)		Plenary Summary (98)			

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	KEY	Capacity
A	Top of Village Tent	100
B	Top of Village Meeting Room	55
C	Top of Village Condo #1	15
D	Top of Village Condo #2	15

KEY

					Co-Chairpersons	
CQ1	Chamber Technology	Key question	1	Liquid Walls	R. Moir	N. Morley
CQ2	Chamber Technology	Key question	2	Solid Walls	M. Ulrickson	R. Mattas
CQ3	Chamber Technology	Key question	3	Reliability	M. Tillack	B. Nelson
CQ4	Chamber Technology	Key question	4	Testing Facilities	S. Zinkle	A. Ying
CQ5	Chamber Technology	Key question	5	Waste Minimization	D. Petti	E. Cheng
CQ6	Chamber Technology	Key question	6	Tritium	M. Sawan	S. Willms
CQ7	Chamber Technology	Key question	7	Materials Advances	S. Zinkle	M. Billone
PQ1	Plasma Technology	Key question	1	Profile Control	D. Swain	R. Temkin
PQ2	Plasma Technology	Key question	2	Magnets	J. Schultz	R. Wolley
PQ3	Plasma Technology	Key question	3	IFE Targets	K. Schultz	W. Steckle
CA	Common	Question	A	Contributions	S. Milora	M. Ulrickson
CC	Common	Question	C	Science Advances	D. Petti, M. Tillack	J. Santarius
CQS	Chamber Technology	Summary Session	Plenary		M. Abdou	M. Ulrickson
PQM	Plasma Technology	Summary Session	Magnetic		D. Swain	R. Wooley
PQI	Plasma Technology	Summary Session	Inertial		R. Callis	W. Steckle
PQSS	Plasma Technology	Summary Session	Joint		S. Milora	