TRANSPORT AND TURBULENCE

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INTRODUCTION

This report is a summary of discussions held in the transport and turbulence breakout subgroups of the Magnetic Confinement Concepts Working Group (which met in the mornings) and the Plasma Sciences Working Group (which met in the afternoons). It was the judgement of the convenors of these groups, that a combined report was appropriate considering the overlap in subject matter and membership of the two groups. The report is organized to cover the most general topics first, then to move on to more specific topics. IFE and basic processes were primarily the focus of the Plasma Science group, while discussion of specific MFE concepts was carried out in the Magnetic Concepts breakouts. The substantial topic of MFE overarching issues was covered extensively in both sets of sessions.

A strong consensus emerged in support of a physics program to understand and control transport.~~,~~ The priority would be a program to develop a science based predictive capability for transport, a program which would benefit from a strong computational initiative but which is **not** to be understood as simply a computational task. It was agreed that this goal could only be achieved by the close interaction of experiment, computation, and theory.

* This is the list of authors who participated more directly in writing assignments for this summary report. This summary is based on the presentations and discussions in two working groups at Snowmass involving a larger group of people. Most of the scheduled speakers and discussion leaders are listed at the end.

In the Transport and Turbulence Science Subgroup, there were 5 overview talks at the beginning to help generate discussion, two on MFE (Jim Drake and Ed Synakowski), and three on IFE (W. Kruer, J.P. Matte, S. Glenzer). There were 15-20 shorter contributed talks (listed at the end of this Transport and Turbulence summary) throughout the first week, with lots of discussion after each talk. The two transport groups independently prepared viewgraph summaries of their discussion, which were incorporated in the MFE Concepts and the Plasma Science summary talks (on record at www.ap.columbia.edu/fusion/snowmass/WG_summaries.html .)

The Turbulence and Transport Science Subgroup came to a general agreement on 3 main goals:

Goal 1: Comprehensive transport models

Goal 1a: Pursue the challenging, yet realistic goal of developing comprehensive predictive transport models, based on physically reasonable assumptions and well-tested against experiments.

Goal 1b: Detailed Experiment/Theory Comparisons

Goal 2. Develop tools and understanding for control of transport and transport barriers

Goal 3: Improved IFE heat transport models, and better understanding of Fast Ignition Physics.

The MFE Transport subgroup reviewed issues for each concept in discussions led by researchers in the field (Tokamaks-C. Greenfield, ST-P. Efthimion, Stellarators-A. Boozer, ET-M. Kissick, RFP-B. Chapman, FRC-L. Steinhauer, Spheromak-B. Hooper, Dipoles-J. Kesner). Common issues were identified and were summarized:

- Need for science-based predictive capability for transport including density limits.
- While empiricism has been useful, it can only take us so far
- Special requirements: particle and impurity transport, electron thermal transport, neoclassical transport, dynamics
- Need to control turbulence and transport barriers

The following sections describes these transport issues and goals in more detail.

UNDERSTANDING THE BASIC PHYSICS OF TRANSPORT

Understanding turbulence and turbulent transport is one of the great unsolved problems of physics, predating the revolutions in quantum mechanics, relativity, and other areas that ushered in 20th century physics. It is of fundamental importance in fluid mechanics and applications thereof, atmospheric and oceanic sciences, astrophysics, and plasma physics; and represents an important area within both statistical physics and the rapidly developing field of nonlinear dynamics. Turbulence is a natural and robust byproduct of the large gradients that are required to maintain fusion plasmas far from equilibrium. Because the transport driven by turbulence seeks to restore the plasma to equilibrium, it is generally deleterious to the maintenance of fusion conditions. An exception is turbulence in the scrape-off-layer that is useful in spreading out the power to the divertor plate. Intelligently dealing with the turbulence problem in fusion, under the numerous constraints imposed by heating, current drive, steady state operation, profile control, ash removal, burn control, economic feasibility etc., requires significant advances in the understanding and control of turbulence and will, because of common dynamics, benefit the other fields mentioned above. Furthermore, the potential impact is expanding because fusion turbulence studies are moving from analyses of linear instabilities and the saturation of simplified nonlinear systems to studies that treat plasma turbulence as a globally distributed, bounded flow, and probe for universal features. Under this trend, the problems of fusion turbulence are increasingly relevant to many classical fluid dynamics problems such as turbulent boundary flows and turbulent shear flows. Fusion turbulence studies are also beginning to consider the effects of magnetic as well as electrostatic turbulence, which is crucial at high plasma pressure (β) and important in self-organized plasmas such as the RFP and spheromak, making it more relevant to astrophysical and space plasmas.

Examples of fusion plasma turbulence problems with these connections are so numerous that only a partial listing can be attempted here. They include issues that deal with nonlocality, such as the relationship between global scales and local instability, what governs whether transport reflects global or local scaling, the effect of the plasma boundary on edge and core turbulence and its impact on mean spatial profiles, and the effect of small scale turbulence on large scales and the mean state through turbulent dynamo and flow drive processes. They include issues that deal with flows, such as the relationship of flow shear-induced transport barriers in fusion plasmas to atmospheric transport barriers, the relationship of Reynolds stress driven flows in the atmosphere to the shear flows of fusion transport barriers, the role of shear suppression in wall flows and turbulent boundary layers, and the physics of zonal flows. They include issues that deal with closure in turbulent systems and intermittent behavior, such as the role of subgrid scales on turbulence and their appropriate treatment, the role of spatial and temporal intermittency in turbulence and transport and its origin and proper treatment, and what types of fluctuations (collective resonances, clumps, holes, vortices, convective cells, zonal flows, eigenmodes of nonlinear instabilities, etc.) lead to the most advantageous decomposition of the turbulent spectrum. They also include issues that deal with dynamical processes, such as the role of nonlinear instability, the origin of residual fluctuations and transport in transport barriers, and the physics of coupled fluctuations and free energy sources.

A successful attack on the difficult problem of turbulence in fusion requires a recognition of its relationship to the general fundamental problem of turbulence, both to exploit advances made in related fields, and to foster support for fusion because of the contributions it has made and will make to the understanding of turbulence. Successes within the general field of turbulence suggest an approach to solving the fusion problem that incorporates the following elements.

1. In experiment

a. Carry out experiments on existing devices that are designed to optimize comparison with appropriate theory.

b. Develop new diagnostics to provide a better picture of turbulence in fusion devices and to address issues such as fluctuation structure and intermittency

c. Develop new analysis techniques for existing and new diagnostics that relate more directly to basic processes in turbulence, and therefore to basic elements of theory.

d. Develop specialized plasma devices to answer specific questions in turbulence that are not answered with existing devices.

A plan that establishes an appropriate balance between these elements should be developed.

2. In theory / simulation

a. Understand the basic dynamics of the complex turbulent systems of fusion experiments. This includes analysis of pieces of complex simulation codes, e.g., individual nonlinearities, studies of scaling properties, and analytic and computational studies of simplified models.

b. Understand how the basic elements integrate in the full system, and therefore what processes govern behavior of the full system under a range of relevant conditions.

c. Develop a range of appropriate models that synthesize behavior of the full system. Models that range from simplified paradigms with correct qualitative behavior to realistic simulations with predictive capability should be developed and employed. For example, simplified models that capture key physics of nonlinear instabilities could be fit to direct nonlinear simulations, and then used as reduced models in transport codes. Effective subgrid turbulence models could help increase speed of nonlinear simulations.

d. Execution of these steps should employ a synergistic combination of analytic theory, simulation, and experiments, utilizing meaningful comparisons and correlated development.

INERTIAL FUSION ENERGY ISSUES AND OPPORTUNITIES

Introduction

There are currently two main approaches for inertial fusion energy (IFE) target designs: direct and indirect drive. A comprehensive review of IFE target physics issues can be found in References 1 and 2. In the indirect-drive approach, energy from lasers, or heavy-ion beams, is converted in a *hohlraum* into x rays. These x rays are transported from the *hohlraum* walls to the implosion capsule, where they are absorbed by the *ablator*. The ablator heats and expands, driving the remainder of the shell inward thereby compressing the DT fuel. In the direct-drive approach the laser is incident directly on the imploding capsule. In both approaches the final DT-fuel configuration consists of a high-temperature *hot spot* that ignites and starts a propagating burn wave through the high-density *main fuel layer*, which surrounds the hot spot. For robust high-gain designs, it is vitally important to assemble the high-temperature hot spot and the cold, dense main fuel layer accurately.

The fast-ignitor approach (3) relies on a somewhat different ignition scheme than conventional direct or indirect drive. First, lasers, heavy-ion beams, or a Z-pinch is used to assemble a high-density DT fuel without the central hot-spot region. Then a high-intensity short-pulse laser impinges on the stagnated core to generate a hot spot and thus initiates the burn wave through the fuel.

Thermal Transport in IFE

Preheat of the fuel will increase the pressure of the fuel and thus make the target harder to compress. The amount of preheat can be quantified in terms of the *adiabat* α of the implosion. The adiabat is defined as the ratio of the fuel specific energy to the Fermi degenerate specific energy. It can be shown that the gain G of the target scales as $\alpha^{-3/5}$. Having a detailed model of thermal transport is therefore important in IFE target designs. J-P Matte presented an overview of the status of IFE transport simulation and theory. The majority of the predictive simulation codes used in IFE target design work rely on flux-limited Spitzer thermal transport. In this approach the classical Spitzer heat flow is limited to some fraction f of the free-streaming heat flow (the maximum possible heat flow.) The value of the flux limiter has been established by comparing experiments to hydrodynamic simulation codes. For current direct-drive ICF implosions f=0.06. The next level of complexity in simulation codes solves the Fokker-Planck (FP) equation for the electrons and assumes fluid ions. These codes have shown good agreement with experimental data but typically have less physics modules than hydrocodes and mainly have only one spatial dimension (compared to the two, or three dimensions used in hydrocodes). In typical ICF plasmas the agreement between FP codes and flux-limited Spitzer hydrocodes is very good, apart from the existence of a small preheat “foot” ahead of the main heat front in the FP case. This foot could preheat the fuel (increasing the adiabat and reducing the target gain) and reduce the Rayleigh-Taylor (RT) growth rate at the ablation surface. Flux-limited Spitzer hydrocodes show good agreement with RT growth rates, which implies that the effect of the foot on the target is swamped by the physics effects (non-LTE radiation transport, real EOS) included in the hydrocodes, but absent from FP codes. An intermediate approach involves the construction of a convolution formula for heat flow. These models have proven useful but require validation with FP codes when used in new regimes. Magnetic fields are not included in FP codes. Many hydrocodes have magnetic fields, but the computational expense of including them means that the hydrocodes are routinely run without magnetic fields. S H Glenzer presented a review of transport experiments. These experiments use a 4ω beam to perform Thomson scattering measurements. In hohlraums the temperature measurements are in good agreement with LASNEX simulations when magnetic fields were included (4); however, inclusion of magnetic fields resulted in steep density gradients. These steep gradients could increase light refraction, resulting in decreased symmetry. Such gradients will detune SRS and lead to a smaller reflectivity than calculated for a homogeneous hohlraum plasma. More calculations and experiments are required. J Hammer reviewed the LASNEX simulations, which showed the potential importance of magnetic fields on laser-driven IFE hohlraums.

To summarize, current indirect-drive experiments are adequately modeled using flux-limited Spitzer thermal transport and self-generated magnetic fields. However, IFE hohlraums may have higher temperatures, and nonlocal transport will be more important. Direct-drive experiments are well modeled with flux-limited Spitzer thermal transport. However, direct drive targets are more sensitive to hot-electron transport, therefore controlling the source of hot electrons and accurately modeling their transport are important. The availability of massively parallel architecture computers opens the possibility to incorporate more-sophisticated heat transport models into predictive simulation codes. We therefore have an opportunity to improve our heat transport modeling, which will increase our confidence in achieving laser-driven IFE.

Laser Plasma Instabilities and Turbulence in IFE

The interaction physics between the laser and the plasma is a vital element of laser-driven IFE. The laser energy must efficiently couple to the target without the generation of hot electrons and with excellent spatial and temporal control. W L Kruer presented a review of the potential instabilities that may be excited by the interaction of the laser with the coronal plasma. These include the ion-acoustic, Raman (SRS), Brillouin (SBS), and two-plasmon-decay instabilities and the potential for the filamentation of the laser beam. These nonlinear processes place constraints on the overall IFE target performance. Calculations show that current direct-drive IFE target designs are only susceptible to the two-plasmon-decay instability. This instability can produce hot electrons that can preheat the fuel. Experiments on OMEGA with NIF direct-drive density and temperature scale lengths have shown very low levels of SRS and SBS backscatter (5). Control of nonlinear processes in laser-driven hohlraums is a greater challenge, but has been successfully demonstrated in Nova experiments by the use of laser-beam smoothing (SSD). Plasmas generated on Nova have attempted to simulate NIF conditions and have demonstrated adequate control of instabilities; however, the integrated experimental test awaits the completion of the NIF. Kruer reviewed the theoretical tools used to investigate these phenomena. These range from the 3-D particle-in-a-cell (PIC) codes to 2- and 3-D reduced models (such as F3D). With the rapid advances in computer power, more-realistic (larger system sizes for longer times) simulations have become possible. J C Fernandez reviewed Los Alamos Trident experiments on saturation mechanisms for SRS. He pointed out that NIF-hohlraum conditions could result in high laser plasma instability levels. This result depends on filamentation and saturation mechanisms for SRS. Well diagnosed Trident experiments that model a laser hot spot in a NIF hohlraum will help resolve this important issue.

In summary, current experiments with 3ω smoothed beams that model NIF-scale plasmas have low laser-plasma instability levels, although some uncertainty exists. Some uncertainties also exist for recent high-gain designs. Little work on the effect of 2ω beams on laser-plasma instabilities has been done. The use of 2ω beams increases the damage thresholds of final optics and increases the efficiency of the laser. With the availability of high-performance parallel computers an excellent opportunity exists to develop an integrated predictive model for incorporation in IFE target-design codes.

Fast Ignitor Transport and Turbulence Issues

The fast ignitor concept is at an earlier stage of development than the main-line direct- and indirect-drive approaches outlined in the preceding sections. Thus there are greater uncertainties in the physics modeling; in particular the main issues facing the fast ignitor concept are in the area of transport and turbulence. M Tabak presented an overview of the fast ignitor concept and the theoretical models that have been used. M H Key reviewed fast ignitor experiments performed to date. The key issues are (1) transport of the short-pulse, high-intensity laser through the underdense plasma to the critical surface; (2) efficient coupling of the laser light to hot electrons; and (3) transport of the hot electrons to the high-density compressed-fuel “ignition” region. The 2-D PIC codes are used to study the propagation of the laser through the underdense corona, where filamentation and other nonlinear processes may occur. To theoretically study the transport of the hot electrons through the overdense material, a range of codes (from LASNEX with magnetic fields to GAPH, a PIC code for hot electrons and a fluid background and similar models developed outside the USA) have been used. The forward currents in this region can reach 10^5 Alfven currents, so magnetic fields, current, and charge neutralization will be important. Experiments using gold foils buried at various depths in plastic targets have shown an annular x-ray emission pattern that persists in diameter at depths of 15 - 100 μm. This suggests collimated electron transport over relatively large distances in qualitative agreement with theoretical predictions.

In summary, the fast ignitor concept offers the potential for significantly higher gains (~5 times that of conventional IFE), but the transport of the hot electrons to the cold, dense fuel requires more theoretical and experimental research. This will entail the further development of hybrid PIC/fluid codes and access to a petawatt-class laser facility.

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MFE - Overarching Issues

Introduction

During discussions of specific MFE concepts, a broad set of issues arose repeatedly. These same issues were raised by speakers and within discussions in the sessions of the Plasma Science/Transport group. This convergence suggests that there is indeed a substantial overlap in the interests of the entire community. It seems likely that, at least with these areas, progress made in developing an understanding of transport in one concept will be transferable to others.

There was general agreement that the overall priority should be the development of a predictive capability based on a sound physics base.[*] The consensus was that this was the surest and most direct route for future progress in understanding and controlling transport. Empirical scalings (of such things as the global confinement, or the power threshold for H-modes or internal transport barriers) have been useful, but they have their limits and there are often significant uncertainties in extrapolating them. While a predictive capability has been a goal of

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[*] The term "predictive transport modeling" in MFE has generally been used to refer to transport codes that use models of transport coefficients (along with calculations of sources and sinks such as auxiliary heating and atomic physics) that can be used to "predict" the temperature and density profiles in past or future experiments. This term has been used to distinguish it from "interpretive transport modeling", where the measured temperature and density profiles are combined with detailed calculations and measurements of the sources and sinks in order to determine what the transport coefficients must have been in a given experiment. "Predictive transport modeling" is also used somewhat to distinguish it from purely empirical transport scalings, particularly 0-D global scalings. But because many existing predictive transport models are often semi-empirical (with a theory-motivated scaling but supplemented by adjustable coefficients that are empirically fit to past experiments) or are not yet complete enough to calculate the full profiles (and so are supplemented by measurements or empirical scaling for edge boundary conditions, etc.), there is a sense in which they are not fully predicting new phenomena, though even an empirical scaling makes a "prediction" when extrapolated to a new experimental regime or to an experiment which has not yet been built. It is difficult to know the limits of validity of empirical models, and the complete physics is so complicated that a single analytic empirical scaling has not been able to describe the full range of phenomenon observed in experiments. Thus the goal is to develop comprehensive transport models, based on more complete 3-D turbulence simulations and/or sound theoretical principles, with a minimum of (or no) empirically-determined adjustable parameters, that are accurate enough to compare well with past experiments and can be used to predict and optimize the performance of future experiments.

researchers in the field and a focus of the Transport Task Force, the group felt that it has not been pursued vigorously as a programmatic goal within MFE. Resources would have to be applied - ~~W~~we are still not measuring many of the quantities that are critical to understanding turbulent transport, ~~port space~~ diagnostic access and run time need to be made available, and advanced computational tools need to be further developed and exploited. An important component of this approach is the close interaction of experiment, computation, and theory.

There was substantial agreement on what the components of a physics based transport program would be and on what direction we need to move. Clearly more physics needs to be put into the existing computer codes; our groups tried to enumerate and prioritize the opportunities. New diagnostics need to be developed and deployed, innovations must be explored and exploited. We must expand our efforts to control turbulent transport and profiles. Perhaps most importantly, we must increase the systematic comparisons between theory, computation, and experiments. Improved coordination will be essential; theorist must look to experiments to help guide calculations and experimentalists must look to theory to help determine what to measure.

Develop Comprehensive 3-D Turbulence Simulations

A high priority goal supported in the discussions was the development of comprehensive predictive transport models based on physically reasonable assumptions. All computer simulations make approximations of the real world in various ways, but the growth of computer power and the advances in physics understanding and algorithms have been sufficiently large that it is believed that fairly realistic 3-D nonlinear simulations of plasma turbulence are becoming possible. Detailed comparisons of such simulations with experiments would help validate the simulations and provide insight into the complicated dynamics of turbulence. Such simulations could improve confidence in the extrapolations to reactor scales, and could help optimize the design of fusion reactors. This goal is perceived as challenging but achievable, and will require a serious effort by a significant number of people. It will require not just advances in computer simulations and use of the latest massively parallel computers but also detailed tests against experiments. There are two parts to this goal: the development of realistic 3-D nonlinear simulations of plasma turbulence (in this section), and their interface with large integrated transport codes (in the next section).

Moore's Law is the observation that computer power has been doubling approximately every 1.5 years. This corresponds to a factor of 100 improvement every 10 years, making simulations practical now that were unthinkable just 10-20 years ago. Simulations have also benefited from advances in theoretical techniques and algorithms, guided by experimental evidence indicating the relevant range of time and spatial scales on which to focus ~~on~~. For example, many orders of magnitude improvement in computational speed have resulted from the development of the nonlinear gyrokinetic equation (which analytically removes high-frequency gyromotion from the Vlasov/Boltzmann equation while retaining nonlinear effects), the delta-~~-~~f particle simulation method, efficient coordinate systems, and fluid closure approximations. Turbulence in plasmas shares some characteristics with regular fluid turbulence, but one of the unique features is the existence of wave-particle resonance effects which provides instability in some ranges of wavenumber k and damping in other ranges of k. At least for certain types of plasma turbulence, these wave-particle resonances and other damping processes cause the range of spatial scales that need to be resolved for a direct numerical simulation to be less severe than for regular fluid turbulence.

Computer simulations in the 1980's focussed primarily on 2-D simulations, though there were some initial 3-D simulations at modest resolution in simplified systems. Recognizing the opportunity for fusion research to make effective use of the new computational power that was becoming available, the Numerical Tokamak Turbulence Project (NTTP) was selected in 1992 as one of the Grand Challenge computational projects supported by DOE’s part of the High Performance Computing and Communications Program. The 1990's saw the development of higher resolution 3D simulations in realistic tokamak geometry, most of them part of the NTTP. These toroidal simulations~~, but~~ focussed primarily on electrostatic adiabatic-electron gyrokinetic and gyrofluid simulations of core ion turbulence, or on electromagnetic collisional fluid simulations of edge turbulence. [The~~se~~ electrostatic simulations allowed ExB fluctuations but neglected magnetic fluctuations.] These simulations have had a significant measure of success, producing fluctuation spectra that are qualitatively similar to observations and leading to transport models that can follow many experimental trends (see the next section on transport modeling). These simulations have also helped build qualitative insight into transport barriers and the important role of ExB zonal flows. However, it is clear that additional physics needs to be added to the simulation codes to make them sufficiently complete and robust for predictions over the full minor radius in a wider range of parameter regimes. Also, previous simulations have tended to focus on primarily the ion thermal transport (though some work has included trapped electrons or collisional fluid electrons which can drive electron thermal and particle transport), while simulations are needed of all transport channels (electron and ion thermal flux, electron and impurity particle flux, momentum flux). The advances that were made in the 1990's involved a lot of work by many people in developing complicated computer programs with more complete physics models, developing and testing algorithms and theoretical methods, cross-checking codes against each other, and comparing code results with experiments. Much of this work was part of the Numerical Tokamak Turbulence Project, which was important in coordinating and focusing these group efforts, in funding some of the work, and in providing access to a large amount of computer time on the latest generation of massively parallel computers. Building on this work and achieving the level of realism set out as a goal here will likewise require a significant effort by many people.

One of the main challenges in the near future is to develop simulations with full electromagnetic fluctuations including kinetic effects and electron dynamics, and to fully explore the resulting turbulence characteristics. This is challenging~~, since~~ because including magnetic fluctuations introduces Alfven waves that require reducing the time step in the simulations by a factor of $1/\mathrm{sqrt}(\beta) \sim 10$. If full electron dynamics are kept, the time step must be reduced relative to electrostatic simulations by a factor of $\mathrm{sqrt}(m_i/m_e) \sim 60$, unless analytic orderings or implicit numerical methods are used to avoid these fast electron transit time scales.

While the electrostatic approximation has been useful in low beta tokamak plasmas, fully electromagnetic simulations are important for higher beta tokamaks and Advanced Tokamak regimes, and are needed to be able to simulate alternate concepts such as ST's, RFP's, high-beta stellarators, etc. Electromagnetic effects and electron dynamics are naturally coupled, and such simulations could also predict all of the transport channels, including particle transport. There is a wealth of experimental evidence on the scaling of particle transport, including puzzling observations of particle pinches in some regimes, that would provide a valuable test of such simulations. The ability to predict particle transport is important because the fusion power improves significantly by density peaking or by increasing the total density (so a physical understanding the empirical density limit is important), and the details of the density profile near the edge could have a significant impact on the height of the H-mode temperature pedestal and on the operation of the divertor.

Because of the importance of electromagnetic effects, there has been some recent work on including them in some simulations. Also, edge turbulence simulations using collisional Braginskii equations have included magnetic fluctuations during the past several years, though this is somewhat more manageable because the Alfven frequency, the electron transit frequency, and the drift wave frequency are all comparable in the edge.

In addition to the electromagnetic goal, another main goal is the study of the interaction of disparate scales in plasma turbulence. Several types of effects are grouped under this goal. Full torus and annular wedge simulations (and perhaps extended versions of flux-tube simulations) will be useful for studying the effects of the equilibrium-scale radial variation of parameters such as $\omega_*(r)$ or $\eta_i(r)$ on the small-scale turbulence. Sometimes such effects may be similar to sheared flows and may reduce the turbulence level. These effects may also lead to a certain degree of radial propagation and nonlocality in the turbulence, or to avalanches or SOC-types of dynamics. These types of effects may be more important near the plasma edge with its sharp gradients. Turbulence-generated zonal ExB flows (poloidal flows with m=n=0) play a major role in the turbulence dynamics and need to be understood better. The formation of transport barriers and the dynamics of ELMs may involve physics that occurs on a wide range of time scales, and so may involve running turbulence simulations for a very long time to follow the slower evolution of the m=n=0 components. In some edge simulations it may be important to include some atomic physics and sources and sinks or the interaction with the scrape-off-layer. Another disparate scale issue is the possible nonlinear interaction of ETG and ITG turbulence.

There are a range of computational techniques in use in present plasma turbulence simulations, including direct full torus gyrokinetic particle simulations, and reduced-volume flux-tube simulations using gyrokinetic particles or grids, or gyrofluid or collisional Braginskii fluid equations. Each of these has strengths for focusing on certain types of problems, and the range of problems being investigated is sufficiently difficult that this multiplicity of approaches is helpful. Comparing different simulations also helps to build insight.

Although there is no substitute for real experiments, computer simulations are easier to diagnose than real plasmas, so they can help develop fundamental understanding of the underlying nonlinear dynamics by detailed comparisons with various analytic theories. Simulations can also help extend the value of experimental measurements by helping to unfold multiple processes that may be affecting a measurement. A particularly fruitful area of experiment/simulation comparisons should be measurements of the spectra in frequency and in space (1D to 3D)~~and frequency spectra~~. While comparisons of measured and predicted temperature and density profiles will continue to be useful, comparisons of the fluctuation spectra provides a more direct link into the underlying turbulence processes.

Predictive Transport Modeling Issues

The objective of predictive transport modeling is to ~~predict~~ understand the plasma temperature, density, current, and momentum profiles measured in previous experiments and to ~~extrapolate to~~ predict them in future experiments and planned devices. Large predictive transport modeling computer codes, such as BALDUR, ONETWO, TSC, and WHIST, use physics-based or empirical models to compute all the sources, sinks, fluxes, large scale instabilities, and boundary conditions needed to predict the plasma profiles. Much of the input data needed to run these codes are similar to the controls that experimentalists use to run experiments. Detailed comparisons with experimental data are then used to test models and to improve our understanding of transport physics.

There are currently at least four theory-based transport models that ~~predict~~ have an RMS scatter of only 15-30 % compared with experimental data ~~within 15 to 30%~~ over a wide range of conditions. These transport models are the Multi-Mode model (MMM) developed by Singer, Bateman, Kinsey and Kritz; IFS/PPPL developed by Kotschenreuther, Dorland, Hammett and Beer; GLF23 developed by Waltz *et al.*; and CDBM developed by Itoh, Itoh, and Fukayama. The best of the empirical transport models also match experimental data equally well.

However, fusion power scales as the square of the temperature, and the feedback of the alpha heating at high gain further increases the sensitivity to transport, so even higher accuracy is needed. Also, there are outliers in the experiment/prediction comparisons, i.e. parameter regimes where the models do not do so well, and it is clear that further improvements in the models are needed.

Six issues have been identified for predictive transport modeling research during the next 10 years:

(1) convergence of transport models for reliable extrapolation to fusion reactors;
(2) reliable models for the plasma boundary need to be implemented within integrated modeling codes;
(3) accurate models are needed for momentum transport and flow shear effects (leading to transport barriers);
(4) improved models are needed for sawtooth oscillations and Edge Localized Modes (ELMs);
(5) high quality computer modules need to be shared among transport modeling codes
(6) the practicality of running turbulence simulations within integrated transport modeling codes.

Convergence of transport models Different transport models representing different approximations to the same physics match most of the steady-state experimental data reasonably well, but predict very different fusion reactor performance. The Multi-Mode and CDBM models predict that ITER will ignite easily --- even with L-mode boundary conditions at high enough density --- while the IFS/PPPL and GLF23 models predict that ITER will not ignite unless the H-mode pedestal temperature is very high. Further experimental tests which could help to better distinguish between different transport models include time-dependent pulse propagation experiments, and controlled scans of rotation, plasma shaping, etc. Developing more accurate physics-based transport models based on more complete 3D turbulence simulations (such as described in the previous section) would also be helpful, and comparisons of such simulations with measured fluctuation spectra would provide more direct tests of the causal links in the turbulence. Non-linear gyro-*fluid* simulations of turbulence, which formed the foundation of the IFS/PPPL and GLF23 transport models, have been found to give too much transport compared with non-linear gyro-*kinetic* simulations in the collisionless adiabatic-electron limit. Work is needed to bring the gyrofluid simulations into agreement with the corresponding non-linear gyrokinetic simulations, including the effects of collisions and non-adiabatic electrons which may reduce the differences, and to correct the transport models which were based on the gyrofluid simulations.

In order to develop better transport models during the next ten years, we need to implement more physics in the non-linear gyro-kinetic turbulence simulations --- particularly electromagnetic effects and more detailed electron dynamics physics. Many more non-linear gyro-kinetic turbulence simulations then need to be carried out over a wide range of plasma parameters.

Predicting boundary conditions. Most transport modeling codes use experimentally measured temperatures and densities for their boundary conditions, and the predictions of most modern transport models are very sensitive to these boundary conditions. Computed boundary conditions are needed for completely self-consistent predictions of plasma profiles. There are separate, well-developed computer codes that model the 2-D scrape-off layer at the edge of plasmas that need to be merged together with the core integrated modeling transport codes. In addition, reliable models need to be developed to predict the height of the pedestal that forms at the edge of H-mode plasmas, together with a model for the Edge Localized Modes (ELMs) that periodically sweep this pedestal away. Finally, more reliable edge turbulence models need to be developed for more complete, self-consistent transport predictions from the edge to the center of the plasma.

Self-consistent predictions for toroidal and poloidal momentum profiles and a more accurate model for the effects of flow shear. There are currently ~~two~~ three transport models --- ~~the~~ Houlberg et.al.'s NCLASS model and Ernst' TRV code for neoclassical transport~~,~~ and the GLF23 model for anomalous transport --- that compute the transport of momentum. These need to be implemented within integrated predictive transport codes, along with computations momentum sources and sinks, to produce self-consistent predictions of the net momentum profiles as a function of time and radius across the plasma. In regions where the plasma momentum has strong gradients (flow shear) it is known that transport is reduced and transport barriers are formed. More accurate models are needed for these important effects.

Improved models for sawtooth oscillations and Edge Localized Modes (ELMs). These large scale instabilities can have a large impact on experiments and fusion reactors, because they periodically rearrange the plasma profiles. We need a more reliable model for the frequency and radial extent of these instabilities, and for their effect on fast ions.

Sharing modules in the National Transport Code Collaboration (NTCC) Module Library. Modules that are submitted to the NTCC Module Library are refereed according to published standards. For example, modules must include test cases, they must run correctly on different computers, they must be well documented, and they must have a well isolated input and output. Bringing computer modules up to these standards is expected to make it easier to maintain the large integrated transport modeling codes. Many more modules are needed in this library to build complete integrated transport codes.

Running complete turbulence simulations within integrated transport modeling codes. This is motivated by the fact that transport is found to be a complicated function of many plasma parameters (gradients, geometry, ratios of temperatures and densities of different species, . . .) and transport may be non-local. At first, reduced transport modules that are fast enough to run inside of a large transport code would be fit to comprehensive turbulence simulations. After a transport code runs, the comprehensive turbulence simulations may be rerun in some cases to cross check the results of the reduced transport modules. As computers become faster and their memory becomes larger, we may be able to run turbulence modules directly within large transport codes within the next ten years. Initially, turbulence simulations would be run on only a subset of time steps and radial points, and they could be used to fine-tune the operating transport model on the fly. The objective would be to produce more reliable predictive transport simulations based on first-principles physics.

Improve Experiment/Theory/Computational Comparison and Coordination

In order to further both the scientific goals of understanding turbulence and transport in plasmas and the programmatic goal of utilizing that understanding to control transport and make fusion a practical energy source, an improved comparison between experiment, theory and computation is needed. In order to achieve this goal, improvements (or changes) are needed in all three areas. In the experimental area, dedicated run time must be allocated for experiments specifically designed to test aspects of the theory using relevant diagnostics. This often will mean simply an improved utilization of existing diagnostics in operating regimes thought to overlap with modeling and theory capabilities. However, in some cases, development of novel new diagnostics will be required to make measurements that can be compared with theory and computation (e.g. localized short wavelength fluctuation measurements for comparison with ETG and other theory predictions). In the computational area, simulated diagnostics should be "built" for more realistic comparison with experimental measurements. The models should also be run for as wide a range of "reasonable" parameters (that is, parameters which are both consistent with the assumptions built into the model and in physically realizable regimes) as possible. In the area of theory, experimentally verifiable predictions should, when possible, be a goal. Coupled with the individual efforts, enhanced analysis techniques are needed to facilitate the actual comparisons. This is because all measures are not equal in comparisons (e.g. different. models can give the same fluctuation spectra and fluctuation mean etc). Very often it is in the tails of the probability distribution functions (PDFs), the parts most difficult to investigate, that the differences show up. Therefore, a hierarchy of comparison techniques should be employed. These can include; basic statistics (mean variance etc), spectra (frequency and k, with long enough saturation regime), time evolving spectra (plasma turbulence spectra are rarely steady stationary, wavelets can be useful), bi-spectral techniques to infer growth rates (both linear and nonlinear) nonlinear transfers etc, quantile-quantile (for non-normal comparisons), correlation functions (auto and cross, spatial and local), structure functions (to investigate intermittency etc), and R/S analysis (to investigate system dynamics). All of these techniques can and should also be used for inter-model comparisons, inter-experiment comparisons (to help address the issue crosscuts) and innovative concept experiments and true predictive theory-model comparisons as well as computation-experiment comparisons.

Successful comparisons will form a basis for predictions of new regimes, leading to next step and capabilities.

Diagnostic Improvements

A dramatic improvement in the understanding of the turbulent mechanisms driving anomalously high levels of cross-field energy and particle transport in magnetic confinement devices has been achieved during the past decade. This accomplishment is due in large part to the combination of theoretical simulations of turbulence, detailed profile measurements of density and temperature, and the development of fluctuation diagnostics to experimentally characterize the underlying turbulence. Nevertheless, a great deal remains to be learned and the development of more advanced fluctuation diagnostics, improvements in existing diagnostics and implementation of profile and fluctuation diagnostics on a greater variety of magnetic configurations will play a crucial role in increasing our understanding of the basic physical principles, as well as provide greater confidence in our predictive capabilities. Particular issues to be addressed include turbulent mode identification, LH transition physics, ExB shear dynamics, as well as broad ranging issues including indications of the universality or self-organizing nature of plasma turbulence across various magnetic configurations, and the potential for stabilization in ST and ET devices.

It is desired to obtain fluctuation measurements of several plasma quantities (n, T_i, T_e, ϕ, B, v) over a wide range of wavelengths ($0.5 < k < 50\ cm^{-1}$) to most fully characterize turbulent driven transport ($\Gamma=\langle nv_r\rangle$, $q=\langle nvT\rangle$, ...). Existing fluctuation diagnostics are focussed dominantly, though not exclusively, on the turbulent density field and development has emphasized application to tokamaks. Examples of such existing diagnostics include scattering (microwave, far-infrared, CO_2), (correlation) reflectometry (local ñ, $L_{c,r}$ (radial correlation length)), beam emission spectroscopy (local ñ, $L_{c,r}$ $L_{c,\theta}$), Langmuir probes (local ñ,$\tilde{\phi}$ at edge/SOL), phase contrast imaging (ñ, $S(k_r)$), among others. In addition, electrostatic potential fluctuations are measured with Heavy-Ion Beam Probes, through with limited application. Electron temperature fluctuations can in principle be obtained with correlation ECE, though this technique is under development. Limited ion temperature fluctuation measurements have been obtained using fast charge exchange recombination spectroscopy. Magnetic fluctuations are crucial to understanding RFP (and perhaps other) transport, and possibilities for measuring this include cross-polarization scattering, though further demonstration is required. These techniques have made valuable contributions, yet can all benefit from improvements (e.g., temporal and spatial resolution, signal-to-noise ratio). It is also noted that while the primary focus of fluctuation diagnostics is for transport and turbulence studies, these diagnostics have been applied to examination of MHD phenomena (e.g., TAE, NTM mode structure), and to measurements of RF mode-coupling behavior, and so have a broader application within MFE research.

There is significant interest in obtaining two dimensional images of turbulence. Turbulence in magnetically confined plasmas is fundamentally quasi-2D (3D but very elongated along the magnetic field) in nature, and simulations can now model the 3D nonlinear dynamics, while most fluctuation measurements are 1D or point measurements. It is therefore desired to provide comparable 2D measurements for visualization applications as well as to more fully examine the nonlinear mode coupling features. Some concepts under development or being considered for 2D imaging of density fluctuations include edge/SOL Langmuir probe arrays, imaging BES, laser induced fluorescence, reflectometry, fluorescent plate imaging (cool plasmas)....

Another key area in which to develop advanced diagnostics is higher-k measurements. While ion transport appears to be fairly well understood in tokamaks, knowledge of electron particle and energy transport remains far more elusive. An area of intense theoretical and experimental study is higher-k modes (e.g., ETG) that may be controlling electron thermal transport in some regimes. As such, it is desired to develop high-k ($k > 5\ cm^{-1}$) diagnostics for any and all plasma quantities. Possibilities include high-k FIR scattering techniques.

Profile and fluctuation measurements are fairly well developed on the tokamak, yet are required for other configurations. The existing methods often suffer from either not being applicable to other configurations, or simply being too expensive to deploy on smaller concept development machines. Efforts should therefore be made to provide profile diagnostics, required for transport studies, as cheaply as possible. This might be accomplished by developing less expensive profile measurement techniques, as well as by utilizing equipment from “retired” machines, if applicable, to the maximum extent possible. Other options may include more limited profile diagnostics: fewer channels, lower time resolution. Similar issues apply to implementing fluctuation measurements on such devices. Table 1 summarizes a list of existing profile and fluctuation diagnostics and where they have been or could reasonably be deployed.

We also note that it is desired to implement “simulated diagnostics” in the various modeling codes so that nonlinear simulation results can be directly compared with fluctuation measurements. This would involve applying the finite spatial resolution and volume sampling, time resolution, and providing lab-frame equivalent measurements (incorporation of plasma rotation, radial electric field effects). This topic is addressed in more detail in the experiment/theory comparison section.

Through a combination of applying existing diagnostics (profile and fluctuation) to non-tokamak magnetic configurations, further optimizing existing diagnostics, and development of new diagnostics for as yet unmeasured quantities, and direct comparison with turbulence simulations, a more thorough understanding of the fundamental nature of plasma turbulence will be obtained.

TABLE 1. Density fluctuation measurements on existing and developing configurations

	Tokamak	Sph. Tok.	Stellarator	RFP	Spheromak
Scattering	X	A	X	A	A
Reflectometry	X	A/D	X	A/D	-
BES (beam)	X	D	A	-	-
Probes	X	X	X	X	X
PCI	X	A	A	A	A
Thomson scat.	X	A	X	X	A
CER (beam)	X	A	X	A	A
ECE	X	-	X	-	-

X-Experimentally demonstrated, A-Applicable, D-Difficult, - Doesn’t apply

Turbulence Control as a Means of Modifying the Plasma Pressure

Every magnetic fusion concept could benefit from having some degree of control of the plasma transport and the underlying turbulence. For some, developing such capability is critical. For example, for the RFP, confinement remains as one of the most pressing problems that must be addressed if the concept is to achieve viability with respect to fusion energy production. For tokamaks, dramatic improvements in confinement have led to the realization that turbulence control has two other important aspects beyond confinement improvement. First, advanced confinement regimes are often characterized by pressure gradients that are too steep, thereby lowering MHD stability margins. On the other hand, steep pressure gradients are usually accompanied by large values of the bootstrap current. A significant but often overlooked aspect of this is that local transport changes in the core have proven to be the most efficient means of driving current in toroidal devices. Significant relaxation of the demands on external current drive systems might then be achieved if the control tools for local turbulence manipulation and transport control can be developed.

Several of the discussion points in the two Transport Working Groups regarding the need for pressure profile control via turbulence modification are expressed in the following excerpt from the summary of the Workshop on Physics Requirements for Advanced Tokamaks, held at General Atomics in March of 1999:

"The single issue that was virtually unanimously agreed to be the most pressing in terms of the ultimate viability of the Advanced Tokamak is the following: In both the experimental and modeling efforts, there presently exists an inadequate understanding of the transport barrier dynamics. (In particular), significant progress in advanced tokamak research would result if local pressure profile control, through manipulation of the local transport, can be realized. The requirement for this control capability comes from the need to relax and broaden local pressure gradients in order to realize the predicted gains in the stability limits, while maintaining favorable bootstrap alignment with the total current profile."

In addition, it was discussed at Snowmass that there is a need to improve understanding of transport and transport barrier dynamics in both core and edge plasmas of MFE devices. This effort implies continuing and increased focus on dynamical modeling efforts of existing experiments, as well as new investment in existing and novel fluctuation diagnostics and the comparison of their measurements with codes that calculate fluctuation spectral characteristics. Of primary concern, though, is the development of the tools themselves that might be used to modify the turbulence directly, thereby influencing the plasma pressure.

Required investments in control tool development – The proposed transport control tools that would most likely have success in a reactor-scale device would be based on RF flow shear generation. Experimental results that suggest that such a tool would be beneficial can be found on the PBX-M tokamak, as well as recent results from FTU in Italy. In both, launching of Ion Bernstein Waves is thought to be responsible for generation of an ExB flow shear layer and an accompanying reduction in transport. Direct measurements obtained on TFTR with IBW indicate that such flow shear generation indeed can take place. It was also noted that the TFTR experiments were hindered by inefficient coupling of the directly launched wave to the core plasma. This points to perhaps the most pressing problem related to IBW research, namely, developing a means of efficiently coupling the IBW antenna so there is efficient launch and deposition in the plasma core.

Other options for transport control, some of them more speculative and some more appropriate for increasing transport when needed, include: external coils for magnetic breaking or ripple, shallow pellet injection to modify ELMs, impurity seeding which improves edge transport in some cases, compact toroid injection or magnetically-insulated pellets to modify core plasma parameters.

Significant clues that small amounts of externally provided flow shear can have global consequences come from observations of self-generated flows in existing or past experiments. Self-generated flow shears observed in some H mode edges, such as DIII-D, and the TFTR and ASDEX-U core plasmas suggest that significant global confinement changes can be prompted by flow shear changes that begin on a spatial scale that is significantly smaller that the device size. The global changes in transport that result are a consequence of the plasma dynamics that follow these localized changes. These observations provide some hope that the favorable influence of ExB flow shear may not be intrinsically more difficult to obtain in large, reactor-scale devices. Efforts to modify the flow shear externally, with magnetic braking and with the application of external torques via neutral beam heating demonstrate the value of flow shear modification. In these experiments, flow shear levels that were not as extreme as those that accompany the steepest pressure gradients lead to moderated transport rates and thus pressure gradients that did not challenge local stability margins.

Opportunities and needs- Despite the urgency expressed regarding the needs for transport and transport barrier control tool development at Snowmass, and the potential benefits if such a program succeeds, there is little in place in the present MFE research program that suggests this is a programmatic priority. It was noted that experiments are planned on Alcator C-Mod using mode conversion IBW, but current RF models are unclear on whether sufficient flow shear will be generated to significantly modify transport in this case.

Research opportunities and needs appropriate for transport control tool development and understanding include the following:

1. A vigorous research program aimed at developing the antenna technology for efficient, reliable coupling of IBW waves should be pursued on at least one of the magnetic confinement device in the U.S. community.

2. The development of theory of RF wave physics needs to be supported to further the understanding of IBW as a flow shear generation tool. In parallel, such an effort should be directed at the identification of other RF waves that might be suitable for such generation, while being easier to launch. This theory development should occur in concert with the experimental research program.

3. A control tool research program should be accompanied by an appropriate diagnostic set that enables the determination of the local radial electric field and its shear, and perhaps the direct measurement of RF-induced poloidal flows.

4. Understanding of transport barrier dynamics will be significantly enhanced if any means of modifying the flow shear in existing devices can be found, even if they are not reactor-relevant. On DIII-D, reorienting a neutral beamline so as to allow variable degrees of co-and counter neutral beam injection would provide such flexibility. Differences in transport

barrier dynamics that will likely occur as a result of changes in the core flow shear will serve as powerful tests of transport dynamics models, increasing the confidence in projections to more reactor-relevant scenarios.

Interconcept Studies and Exploitation of New Devices

Although the various magnetic fusion concepts under investigation have obvious differences, they also have much in common. Anomalous transport occurs in most magnetic configurations, and in the rare attempts to compare turbulent transport plasma behavior across different concepts, similar features have appeared. For example, the electrostatic turbulence in the edge of tokamak, stellarator, and RFP plasmas has very similar characteristics, and in all three, this turbulence has been shown to be the primary cause of particle transport, despite the likelihood that the sources for this turbulence could be different. One wonders if similar behavior occurs in other magnetic configurations, and if universal physics models can be developed and applied, an example of which might be self organized critical (SOC) transport models.

In support of the development of a predictive capability for turbulence and transport in magnetically confined plasmas, the Summer Study turbulence and transport groups identified interconcept studies as a high leverage research element. The importance of such studies in developing predictive capability is at least twofold. First, the commonality —and perhaps more importantly the differences—of the various concepts provide diversity to develop and test robust, universal models. For example, understanding the dominant electromagnetic turbulence and transport in self-organizing plasmas such as the spheromak and RFP might further the understanding of electron transport in tokamak plasmas, for which the inclusion of electromagnetic effects will be crucial. Second, physics understanding developed in more mature concepts (both in terms of applied theoretical models and detailed theory-experiment comparisons) will likely accelerate the understanding of turbulence and transport in less mature concepts. For example, understanding energetic particle modes (e.g., Alfven eigenmodes) at near fusion conditions in tokamaks will develop models testable in the other concepts. The success and/or failure of their predictions in other configurations leads to refined model development. This strategy has an important programmatic benefit. The present nature of fusion research funding makes it much less likely (worldwide) that less mature concepts will see the multiplicity of devices which formed the basis of empirical scaling laws for transport in tokamak and stellarator plasmas. A “smarter” approach based on first principles understanding may be the only way to project performance for next stages of research, a logic which equally well applies to tokamak and stellarator research.

With the commissioning of new devices comes the opportunity to further leverage interconcept studies. The potential contributions of new devices to advance predictive physics understanding should be identified in the planning of both the research programs and facilities as a proactive strategy. Such a strategy would help identify less obvious opportunities, particularly those offered in alternate and emerging concepts research. For example, a Summer Study presentation on Penning trap confinement identified opportunities to study neoclassical transport in new regimes of relevance to magnetic confinement. Truly universal models can only be realized with a concerted effort to apply and test them in many different configurations.

The need for plasma control tools and diagnostics to manipulate and understand turbulence and transport is clearly important to virtually every concept discussed. In a different sense of interconcept study (maybe more appropriately termed cross communication), the development and application of control tools and diagnostics could be made more efficient and effective. As a particular example, plasma flow and/or flow shear is a recurrent theme, either long recognized to be important or anticipated to be important in almost every concept. Cross-concept discussions of flow control and measurement would maximize opportunity and minimize duplication. Other control measures (e.g., current profile) are beginning to appear in the research plans for many of the concepts and would benefit from fostered interconcept communication. A collateral benefit of this approach would be deployment of diagnostics in a cost effective manner, especially the most expensive diagnostics which might not be required for every device in every concept. A nontrivial and timely example is optical imaging diagnostics for fusion grade plasmas. The competition between the need for large photon collection and detector sensitivity and damage in a high radiation environment makes such diagnostics challenging and expensive. Applied to smaller, hydrogen plasmas they would be more economical. A well designed plan to implement and compare results from such diagnostics across device and concept lines would be highly valuable.

ISSUES AND OPPORTUNITIES SPECIFIC TO MFE CONCEPTS

The following sections outline issues and opportunities that were identified during a systematic review of each magnetic confinement configuration. These have necessarily de-emphasized the over arching issues that were covered in detail above. What remains, are issues that are of particular importance to particular configurations. The level of detail and the relationship of theory and experiment vary from section to section as is appropriate for the level of development of each concept.

Tokamak

Tokamak research has led to and benefited from significant progress in the understanding and control of transport. With the extensive diagnostic set in current tokamaks, detailed measurements contribute to the understanding of transport phenomena including the relationship between turbulence and transport, and mechanisms affecting transport in different channels. In several "advanced" regimes obtained in tokamaks, transport (ion thermal, particle, angular momentum) has been reduced at the edge (H-mode, VH-mode, etc.), in the core (PEP, ERS, NCS, high β_P, etc.) and throughout the entire plasma (NCS and high β_P H–modes).

Future progress on the tokamak will address both cross-cutting transport issues and further application of our understanding toward improved fusion performance. In doing so, we will address several remaining major challenges:

Understanding of tokamak transport and the development of an improved predictive capability.

Considerable effort has gone into testing physics-based models against observations made in tokamak experiments. So far, no model has been successful in describing the full set of observed behaviors. In particular, physics-based simulations of electron thermal and particle (electron, main ion and impurity) transport have not been developed and tested as extensively as those of ion thermal transport. Addressing these and other issues will require increased emphasis on physics either not currently included or insufficiently treated, such as short-wavelength turbulence (eg. electron temperature gradient, or ETG modes), electromagnetic dynamics and more realistic geometry. More detailed simulations will also require the plasma community to take advantage of advances in computer technology.

At the same time, experiments must be better prepared to test theoretical predictions. Observations of fluctuations in the generic spatial and frequency range expected for drift/ITG instabilities have been observed, but direct observations distinguishing the ITG (ion temperature gradient) mode from other modes have not been made Direct observations are also lacking for ETG modes, and for the zonal flows which are predicted to damp long wavelength modes. Such tests of theoretical predictions may, in some cases, require the development of new diagnostics. Diagnostic areas identified for improvement include the ability to observe small-scale turbulence, and increased capability to image turbulence in two dimensions. Most of the current diagnostics, especially in the core, are sensitive only to density fluctuations. A more complete picture requires measurements of other fluctuating quantities, including temperature (ion and electron), electric potential and magnetic field.

In the past, extrapolations of plasma behavior to new (larger) devices were done largely through the use of empirical scaling relations. A more reliable predictive capability based on scientific understanding is needed. An intermediate strategy has also been identified. By choosing a set of dimensionless parameters to describe the plasma, it is possible to construct an experiment that matches most of the dimensionless parameters expected in a next-step or reactor class device. Typically, the one such parameter that cannot be matched with a reactor is ρ^*, the normalized ion gyroradius. “Dimensionless scaling” experiments have been done in several devices to empirically determine the scaling of transport with just this one parameter. “Dimensionally identical” discharges have also been produced in several devices, generally supporting the usefulness of these scalings. However, some papers on modeling of such dimensionless scaling experiments have shown that moderate changes in other dimensionless parameters (such as the rotational Mach number, density profile shapes, or Ti/Te) can introduce uncertainties into these extrapolations. Further progress in this approach requires more precise profile measurements on multiple devices, particularly in the edge/pedestal region.

Basic transport processes

Transport in tokamaks is characterized by an irreducible minimum level set by neoclassical theory, which is overlaid by “anomalous” transport processes believed to be primarily turbulent in nature. Until recently, the anomalous component of transport dominated, so that neoclassical predictions of transport were considered irrelevant.

Much progress has been made in understanding ion thermal transport during the last decade. Many observations are consistent with the hypothesis that the “anomalous” ion thermal transport is driven largely by ITG modes, which can be suppressed by sheared **E**×**B** flows. Fluctuation measurements are roughly consistent with the ITG hypothesis, but conclusive proof of the hypothesis is still lacking. With advances in transport suppression (see “Transport barriers,” below), turbulent transport has been eliminated in many cases, at least for the ions. This leads to a situation in which neoclassical transport can once again become important. It also exposes areas where neoclassical theory is weak and needs improvement. One well-known example is the need for inclusion of “orbit squeezing” effects near the axis in discharges with reversed magnetic shear.

Our understanding of behavior in the electron channel is less advanced. In many regimes, it is thought to be due to the non-adiabatic electron response to ITG turbulence due to trapped-electrons or finite-beta effects on passing electrons, though more work is needed to do quantitative tests of this. But there are many cases where ITG turbulence is stabilized by sheared flows, and ion thermal and electron particle transport is significantly reduced but the electron thermal transport is still anomalous. Although ETG turbulence, analogous to the ITG mode, is a hypothesized candidate to drive electron thermal transport in some cases like this, this is less certain. One difficulty is that ETG modes are predicted to appear at very short wavelengths, beyond the range of most current fluctuation diagnostics. Another issue is that other phenomena, both electrostatic and electromagnetic, are also predicted to impact electron thermal transport but are similarly difficult to observe.

Recent theoretical work has proposed several mechanisms that may control transport in tokamaks and other devices. One such phenomenon is self-organized criticality, in which discrete avalanche-like events can determine the plasma profiles. This may be supported by observations of self-similar turbulent spectra in plasmas, with similarities between different experiments and regimes. Zonal flows have been proposed as a possible mechanism for the transport to become self-limiting. In this case, the turbulence itself can generate localized flows, which then feedback on the turbulence through sheared **E**×**B**. Unfortunately, the ability to test this prediction remains a challenge for future diagnostics.

Transport barriers

Achievement of transport barriers, in many cases with ion thermal transport reduced to neoclassical levels, is a recent triumph of transport physics. The hypothesis that **E**×**B** shear stabilizes the long-wavelength turbulence responsible for ion thermal transport qualitatively explains the formation of these regions, and can describe much of the observed dynamics. Simple models have been created which are beginning to predict such bifurcations, but a complete, time-dependent, self-consistent model of transport barrier formation still does not exist.

Understanding of these regimes is complicated by the complex interplay between different terms of the **E**×**B** shearing rate and other factors affecting the microinstability growth rates, resulting in a set of multiple feedback loops. Edge transport barriers (H–mode) are even more difficult to describe, due to the additional complexity associated with high neutral populations and divertor geometry which require a 2– or even 3–D description.

Applicability of core transport barrier regimes in future reactor scenarios depends on our ability to control the barrier position and characteristics. In some cases, we may need the capability to *increase* transport in the barrier region in order to avoid MHD instabilities associated with strong pressure gradients. Also, control of the barrier position will be important both for maximization of fusion performance and for MHD stability considerations.

Tools for transport barrier control will need to be developed. An important consideration here will be compatibility of pressure and current profile control tools, since both will be important in obtaining a sustained high performance regime. Compatibility with “next-step” and reactor class devices is another important consideration.

Fusion reactors, as currently envisioned, will operate with electron temperatures slightly higher than the ion temperature. In contrast, most core transport barriers thus far have been associated with “hot-ion” regimes ($T_i / T_e >> 1$). This is perhaps to be expected, due to enhanced stability to the ion temperature gradient (ITG) mode expected in such conditions. Nevertheless, in a small number of cases, core barriers have been created with near equal electron and ion temperatures. Although such regimes are more speculative, reactor-relevant hot ion regimes may be possible and could have a high payoff. Such regimes may depend on undeveloped technologies such as “alpha channeling,” or might just rely on poor transport in the electrons and nonzero alpha heating of ions (and require higher alpha power to make up the losses).

Another issue that has been raised is that of flow generation in a reactor-sized device. This may also require the development of new technologies, such as RF-driven flows. The problem may be lessened by improving the efficiency of the tokamak (“Advanced Tokamak”) in order to allow a smaller device size.

Density limits

Density limits observed in many tokamaks are not yet fully understood, but there is some belief that the underlying physics is at least partially a transport effect, in the sense that both turbulence and transport typically are seen to scale as n/n_C, as the density n approaches a critical value n_c. It may represent a more basic issue than the plasma current limit for a specific MHD equilibrium. Since this effect imposes an operational boundary that is directly related to fusion yield, progress in this area represents a high-leverage opportunity for future reactor performance improvement. As experiments move more towards high performance, long-pulse to steady-state discharges, more theoretical and experimental work are needed to elucidate this issue.

Today's large tokamaks are outfitted with advanced, high power heating systems and extensive diagnostic sets. This allows study of plasma behavior under conditions most similar to those in a reactor. Continued study of these issues will allow not only further performance improvements to the tokamak, but should also provide an improved physical understanding of transport processes in fusion relevant plasmas which will be applicable to other confinement concepts.

Spherical Torus/Tokamak (ST)

The Spherical Torus has unique transport properties associated with very low aspect ratio, high equilibrium sheared flows, and high beta. Its parameter domain is characterized, under the best of circumstances, by order-unity local plasma β, absolute "magnetic well" up to 40%, and dielectric constant up to ~10^2. Theoretical studies predict flow shearing rates up to and above 10^6/s, fully aligned ∇p-driven current profiles up to 100% of the plasma current, and supra-Alfvén energetic and possibly thermal particles. The investigation of the Spherical Torus plasmas therefore promises opportunities to advance plasma transport studies.

The Snowmass Summer School discussions by the MFE transport sub-topical group identified transport issues and opportunities for the Spherical Torus. The issues and opportunities were categorized as (1) Edge transport barriers, (2) Suppression, and possible stabilization of micro-turbulence, (3) Hydrodynamic turbulence from large flow and energetic particles, (4) Core transport barriers, (5) Theoretical and computational needs, and (6)Diagnostic and transport measurements. Some of the issues and opportunities are not unique to the ST. There wasn't any discussion prioritizing the issues and opportunities.

1 Edge transport barriers

Several issues related to the ST's particular characteristic of low magnetic field and aspect ratio were identified which will be important in understanding edge transport barriers and H-mode in the ST. The low magnetic field gives rise to a large normalized ion gyroradius compared to the Tokamak Since there are theoretical and experimental scalings of H-mode threshold and edge parameters dependent upon the ion gyroradius, the ST provides a useful extrapolation of these quantities. Furthermore, experimental study of the edge parameters and power threshold for the H-mode in present ST experiments will facilitate the design of future ST devices. However, the potential for naturally occurring large plasma flows, flow shears, and magnetic shear may naturally suppress transport and may make the H-mode less valuable in the ST.

(2) Suppression and possible stabilization of micro-turbulence

There are a number of mechanisms that could suppress and possibly stabilize micro-turbulence even though the small aspect ratio results in a higher trapped particle fraction which can drive higher levels of transport. The diamagnetic flows in the ST could reach sonic values, leading to the possibility of relatively larger shearing rates to suppress microturbulence. Furthermore, the inherent orbit-averaged good curvature at low aspect ratio could stabilize micro-instabilities as well. Considering the high trapped particle fraction and multiple microturbulence suppression mechanisms it will important to characterize the turbulence in both the edge and core of ST plasmas.

3 Hydrodynamic turbulence from large flow and energetic particles

The low magnetic field in the ST will lead to relatively smaller Alfvenic flows which could destabilize hydrodynamic turbulence and lead to energetic particle driven instabilities (e.g. TAE). The confinement of energetic particles in the present of this class of instabilities could be a major issue for the ST.

4 Core transport barriers (both ion and electron)

The much enhanced flow shearing rates in the ST plasma are expected to suppress the micro-turbulence and possibly return the heat and particle transport toward the level of the neoclassical model. These conditions are critical for the formation of core transport barriers in recent Tokamak experiments. However, in those experiments the relative importance of magnetic shear and ExB shear is not clear. ST experiments will be able to explore this issue rather thoroughly, since they have inherently wide ranges of ExB and magnetic shears available in MHD-stable equilibria. Furthermore, in proof-of-principle ST experiments neoclassical transport may likely be a major contributor to the total radial transport regardless to the status of the collisional transport. Consequently, transport barrier formation may be problematic in these experiments and may only emerge in Performance Extension experiments. Therefore, in the proof-of principle experiments it will necessary to characterize the turbulence in regimes with high flow shear.

5 Theoretical and computational needs

ST plasmas have local beta ~ 1 and plasma flows with speeds comparable to the Alfven (and thermal) velocities. Such plasmas are ubiquitous in nature (e.g., in astrophysical and magnetospheric contexts), and have been studied extensively in other communities. Typically, particular emphasis has been placed on hydrodynamic calculations. It is exciting to realize that the development of turbulence and micro-instability theory and computation for the ST will likely be influenced by and contribute to these related fields. There are many opportunities for synergistic developments, particularly with respect to developing turbulence and transport simulations that are directly relevant to astrophysical and magnetospherical systems, and that can be tested in the laboratory.

6 Diagnostic and transport measurements

Experiments focusing on the transport issues in the ST need to be integrated into experimental plans. This includes both heat and particle transport studies using both steady-state and perturbative analysis techniques. The ST's unique characteristics may necessitate alteration of the diagnostics currently in use in the Tokamak. The low magnetic field in the ST has motivated research into new diagnostic techniques to measure the current profile and the electron cyclotron emission.

Stellarator

With the construction of two $1B class stellarators, LHD and W7-X, stellarators are playing an increasingly significant role in the world fusion program. Stellarators present unique challenges and opportunities in the area of transport and turbulence.

From the transport perspective, the primary difference between stellarators and axisymmetric configurations is their relatively large neoclassical transport losses, due to ripple trapped particles in the non-axisymmetric local magnetic wells. This leads to an unfavorable ($1/\nu$) temperature scaling of neoclassical transport which becomes large in the core of current experiments. Recent stellarator experiments have shown that measured transport is consistent with neoclassical levels in the core, but transport remains anomalous in the edge, where it is due to turbulent fluctuations.

Future experiments are designed to significantly reduce neoclassical losses by optimizing the magnetic geometry and exploiting symmetries: quasi-axisymmetry (QAS), quasi-helical symmetry (QHS), and quasi-omnigeneity (QOS) are among the approaches currently being considered. A major goal of the stellarator program is to verify to what extent these symmetries can reduce neoclassical transport, both to test the neoclassical theories, and to test whether the required degree of symmetry can be achieved in an actual experimental realization. For eventual reactor considerations, a cross-cutting issue is achieving good transport at high beta, consistent with MHD stability calculations.

Another clear goal is to compare turbulence simulations and theory with transport and fluctuation measurements on stellarators. Since the neoclassical transport in future stellarators is predicted to be reduced, turbulent transport is expected to play a larger role in future devices, strengthening the links between transport studies in stellarators and tokamaks. Relative to similar experiments in tokamaks, quantitative comparisons of this sort on stellarators are in their infancy. There is a strong need to develop theoretical and numerical tools (3D turbulence simulations and transport analysis codes) and fluctuation diagnostics. Stellarator geometry is more complicated, but since many codes now treat general geometry numerically, this is to some extent a detail, and not a fundamental development issue. More work is clearly needed in this area.

The stellarator has unique issues associated with transport barrier control and formation. In tokamaks, there is an interaction or feedback loop involving transport, pressure gradients, bootstrap currents, current profile evolution, and MHD stability which can be difficult to control, but has lead to the formation of transport barriers, turbulence suppression, and enhanced performance. The stellarator breaks this loop to some extent, since the q-profile (and magnetic shear) is controlled mostly externally, and therefore the problem of q-profile evolution and MHD stability is likely to be less challenging compared to tokamaks. It is not clear, however, that stellarator experiments can be designed with sufficient magnetic flexibility to achieve a comparable degree of q-profile variation transiently available in tokamak experiments.

A perhaps more significant difference is in the physics determining the radial electric field in stellarators. Since ExB shear suppression of turbulence is the primary candidate for transport barrier formation, this may change the conditions required to produce barriers. Tokamak neoclassical transport is intrinsically ambipolar, and the plasma is free to rotate toroidally so that the radial electric field is in equilibrium with the flows and the pressure gradient. Standard stellarator neoclassical transport is not intrinsically ambipolar, and the radial electric field adjusts until the transport becomes ambipolar, i.e., the plasma charges up until E_r enforces ambipolarity. The E_r which achieves this balance is not unique, and there are usually two stable roots: the "ion root" where E_r holds in the ions and ion energy confinement is good, and the "electron root" where E_r holds in the electrons and electron energy confinement is good. For standard stellarators, a key issue is to demonstrate whether or not the ion root is consistent with transport barrier formation and turbulence suppression. For Q{AHO}S, the key issue is the following: what degree of symmetry is required to restore (quasi-) intrinsic ambipolarity and freedom of rotation (not toroidal, but in some twisted direction)? If this is achievable, transport barrier formation in stellarators could be very similar to tokamaks.

Electric Tokamak (ET)

The goal of the Electric Tokamak (ET) is to induce a bifurcation [1] into a *global* enhanced confinement regime by forcing a strong radial electric field via non-ambipolar ICRF-induced fast ion orbit loss. Because the poloidal flow damping in tokamaks is dominated by trapped particle magnetic pumping, if the bulk plasma rotates faster than the trapped particle bounce frequency, then the poloidal flow damping is greatly reduced because of the destruction of trapped orbits. The plasma will remain in this state even when the drive is greatly reduced. In addition, Ion Temperature Gradient (ITG) modes will be eliminated at high poloidal rotations if the drive provided by resonant trapped ions is eliminated [2]. Shear in both toroidal and poloidal rotation will also be present and beneficial for reducing ion turbulence. This method of inducing a bifurcation by forcing poloidal rotation over the maximum in the damping has been done successfully on CCT [3] and other machines: only edge bifurcations induced in those past experiments. The ET experiment is designed with a high aspect ratio (> 5) in order to have a low magnetic pumping and is optimized for a *global* bifurcation [2]. Partly because the fast particle banana width is the same size as the minor radius, it should be easier in ET to produce the required ion orbit loss than in other tokamaks. The major issue for this experiment is therefore whether this scheme (successfully applied with edge probe biasing) can be extended into the core plasma with ICRF heating.

The resulting strong flow profile can then be controlled through adjustments in the neutral particle density and fully adjustable ripple for toroidal motions. Spectroscopy, reflectometry, and probes will be used to measure these fluctuations and flows with likely upgrades to other techniques as warranted. Because of the high aspect ratio and large volume (~ 180 m^3), there will be up to 64 m^2 of antenna space both *inboard* and outboard for better coupling. Direct momentum input, and current drive as well as ion orbit loss and heating will be explored. The inboard RF launch can also be used for marfe control and therefore density limit control. This unique inboard access can also be employed for a full 2-D picture of the fluctuations.

Once in a fully bifurcated state, the beta should increase. Given that stability requirements are satisfied [4], the beta should rise to near unity where it becomes omnigenous (ions drifts are locked to the magnetic surfaces: therefore classical confinement). The elimination of ion trapped orbits likewise eliminates both ion anomalous as well as ion neoclassical transport. With the large effective aspect ratio due to a high beta magnetic well, electron anomalous transport is reduced but likely not eliminated. The ET experiment is an exploratory experiment and will therefore need to face issues related to the above-described scheme ahead of theory development. In response, theory development should be catalyzed by ET results.

[1] K.C. Shaing and E.C. Crume Jr., Phys. Rev. Lett. 63 (1989) 2369.
[2] M.W. Kissick, et al., submitted to Phys. Plasmas.
[3] R.J. Taylor, et al., Phys. Rev Lett. 63 (1989) 2365.
[4] S.C. Cowley, Phys. Fluids B 3 (1991) 3357.

Reversed Field Pinch (RFP)

Core transport ($r/a < 0.8$): RFP's and other $q < 1$ configurations typically exhibit large-amplitude magnetic fluctuations. In the RFP, these fluctuations are believed to be responsible for sustaining the reversed-toroidal-field configuration through (dynamo) current drive. However, these same fluctuations drive most or all of the core particle and energy transport, which is typically about 100 times larger than the classical prediction. The dominant magnetic fluctuations are core-resonant, global tearing modes with poloidal mode number $m = 1$ and toroidal mode numbers $n \sim 2R/a$. These modes are believed to be driven by a gradient in the parallel current profile. When these modes grow to sufficient amplitude, their associated islands overlap, stochasticizing the core. We have little information on electrostatic fluctuations in the core, but in standard-confinement discharges, they are believed to contribute little to transport. In improved-confinement discharges, however, core magnetic fluctuations decrease, so the electrostatic fluctuations may begin to play a greater role. At present, the reduction of core particle and energy transport is focused on reduction of magnetic fluctuations.

Edge transport ($r/a > 0.8$): Particle transport in the RFP plasma edge is governed by electrostatic fluctuations, as in other configurations, but the origin of these fluctuations in the RFP is not yet known. The cause of energy transport in the edge has not yet been identified, as measurements so far indicate that neither magnetic nor electrostatic fluctuations contribute. However, the electrostatic measurements for energy transport only extend into $r/a \sim 0.9$. In the region $r/a > 0.9$, energy transport may be governed, e.g., by parallel losses due to magnetic field errors. In the region r/a < 0.9, electrostatic fluctuations may yet be important. Several challenges remain with respect to the plasma edge: (1) identifying the origin of electrostatic fluctuations, (2) reducing particle transport through control of electrostatic fluctuations, and (3) identifying the source of and reducing edge energy transport.

Reduction of core and edge fluctuations: Confinement in the RFP has been improved at least five-fold with the addition of auxiliary parallel current in the plasma edge. This auxiliary current is driven with a technique called pulsed poloidal current drive (PPCD), the goal of which is to flatten the edge current profile to reduce the core-resonant tearing fluctuations. Although it provides only coarse control of the current profile, PPCD is quite successful in reducing these fluctuations. With the goal of further reducing the core fluctuations beyond what is possible with PPCD, radio frequency current drive techniques are under development which could allow finer tailoring of the current profile. Sustainment of the RFP magnetic configuration requires some form of edge current drive, and this is normally provided by the tearing modes. One can view PPCD, or any externally driven edge current, as a replacement for the tearing mode current drive, which thereby obviates the need for the tearing fluctuations. In addition to PPCD discharges, there are also discharges with improved confinement that occurs spontaneously, without auxiliary current drive. These discharges also exhibit reduced core tearing fluctuations. In addition, there is a region of strongly sheared E x B flow in the plasma edge and an edge-wide reduction of electrostatic fluctuations. It is now known that PPCD discharges also possess a similar region of flow shear and reduced edge electrostatic fluctuations. There are many questions that must be answered in order to understand the confinement improvement in these discharges. For example: (1) in improving energy confinement, what is the contribution of reduced core fluctuations relative to reductions in the edge? (2) can flow shear contribute to improved energy confinement in the RFP, as it does in other configurations?

Transport modeling: To better understand the issues described above, we need improved modeling of local transport. This will require much more detailed measurements of equilibrium profiles and fluctuations, and diagnostic upgrades are underway to address this need. Using these measurements and a 1D transport code, we will be able to calculate local transport and compare with local measurements of fluctuations.

Computational/theoretical issues: There is a plethora of outstanding computational and theoretical issues related to transport in the RFP, many of which are directly related to the issues described above. One challenge is to improve the ability of MHD modeling codes to simulate plasmas with experimentally relevant parameters, for while existing codes reproduce several basic features of RFP discharges, like toroidal field reversal, there are some important features that the codes do not predict. One code-experiment parameter difference that stands out is the Lundquist number (S, the normalized conductivity). Because of practical limits on computational speed, the codes are limited to S ~ 10^4, while S reaches ~ 10^7 experimentally. Beyond simply matching the code and experimental parameters, simulation of higher-S plasmas is important to test the idea that the core tearing fluctuation amplitudes could scale inversely with S, which would be favorable for an RFP reactor. Assuming that this computational problem is soluble, these codes can be applied to the task of finding an optimal RFP configuration, in terms of the current, pressure, and flow profiles and in terms of the plasma shape and aspect ratio. Optimally, these simulations will be carried out with a code that incorporates two fluids, and such a code is presently under development. Also of potential relevance to the RFP and other configurations is the extension of gyrofluid and/or gyrokinetic codes to model electromagnetic turbulence. Theory can also help to identify the cause(s) of electrostatic fluctuations in the plasma edge, studying the possible influence of, e.g., localized interchange turbulence. Theoretical work is ongoing to describe the mechanism by which flow shear occurs spontaneously in the plasma edge, as well as the possible effect of this flow shear on both electrostatic and magnetic fluctuations at large and small spatial scales.

Spheromak

The spheromak is a toroidal configuration in which plasma current is driven by a dynamo associated with the inward transport of helicity from the edge plasma. Associated with this transport are magnetic fluctuations which break the axisymmetry of the plasma and, by opening magnetic surfaces allows the outward transport of energy. The reconnection events associated with these fluctuations cause anomalous ion heating. (If controlled, this heating might be utilized in hot spheromaks.) An analysis of the decaying plasma in the final CTX devices found that the cross field losses were consistent with Rechester-Rosenbluth diffusion of electron energy in the presence of magnetic field fluctuations which scaled with the Lundquist number, S, as $\left|\tilde{B}/B\right| \sim S^{-\alpha}$, where . This scaling would lead to an interesting reactor; however, the database is very limited so that the loss mechanisms have not been unambiguously identified, and the scaling is highly uncertain. Further, measurements in a sustained plasma are insufficient to even estimate the scaling in a regime interesting to fusion. There are no measurements of electrostatic fluctuations in a spheromak.

The spheromak can be sustained by injection of helicity from a coaxial "gun," in which a source of helicity is applied across a gap in the flux conserver in the form of a voltage-poloidal flux product. This helicity is carried into the spheromak separatrix by a kink-like, $m = 1$ instability of the current column driven by the injector. Some measurements suggest that the helicity is then transported throughout the toroidal volume by short-scalelength fluctuations, generating the dynamo and balancing the resistive plasma losses.

In the limit of localized (short-scale) magnetic fluctuations (and low beta), the inward helicity transport is predicted to be proportional to $\partial(j_{\|}/B)/\partial\psi$, where $j_{\|}$ is the current density along the magnetic field, $\mathbf{B}$, and ψ is the flux. This gradient is also the driving term for tearing (reconnection) modes which are believed to be the primary source of the fluctuations. In the sustained spheromak, the q-profile need not have an internal, resonant $m = 1$ surface. In that event, the fluctuations should be relatively short-scale and it would be valid to describe the inward helicity and outward energy transports as diffusive. Thus, although spheromak physics is closely related to that in the RFP, if this holds the differences in detailed operation may yield significant differences in energy transport.

The highest electron temperatures (~ 400 eV) were obtained in CTX on after reduction of magnetic field errors by careful flux conserver design and control of impurities and of edge neutral density by wall conditioning and gettering. Further evidence that the fluctuations were relatively low was that ion and electron temperatures were comparable, whereas in earlier experiments the ion temperature was >> than the electron temperature. Unfortunately, the CTX experiment did not continue after obtaining these results. A new experiment, SSPX, has been started to determine the detailed mechanisms and their scalings with *S* and other plasma parameters. An extensive set of profile diagnostics are being assembled to measure the density, temperature, and magnetic field. Measurements of magnetic fluctuations, both by wall-mounted magnetic probes and in the volume by x-mode reflectometry and a Transient Internal Probe will provide data to relate the profile-determined transport coefficients to the fluctuations. Results from this experiment will also contribute to basic plasma science of interest both to fusion and to astronomical plasma physics.

The experimental measurements of transport and its close association with magnetic fluctuations need to be supported by calculations of 3D resistive MHD, e.g. as being carried out by the NIMROD code (and discussed in the MHD Snowmass groups). Such codes, properly benchmarked with experiment, will provide a basis for understanding transport scaling with plasma parameters and modes of operation. The effects on transport and energy losses of plasma flows and non-ideal processes will need to be explored.

Field Reversed Configurations (FRC)

Two-fluid relaxation

The FRC may be describable as a minimum energy state in a two-fluid context. A key element of the two-fluid theory is the existence of a magnetofluid invariant for the ions (ion self helicity), combining magnetic and mechanical (flow) parts, in addition to the usual magnetic helicity. *Issues*. 1) Measurements of the ion self helicity (magnetic, flow profiles) are needed. 2) What is the nature (spectrum, distribution of mode type) of the relaxation turbulence? 3) Measurements of the turbulent fluctuations are needed. 4) What is the transport from the turbulence? 5) Will the FRC minimum energy state evolve toward a Taylor state (*e.g.* a spheromak).

Spontaneous flow-shear generation

A predicted feature of the two-fluid relaxation is global flow and flow shear. This may appear spontaneously in FRCs. *Issues*. 1) Is sheared flow spontaneously generated in FRC experiments, and does it arise in some magnetic configurations and not in others? 2) Will this flow have a stabilizing effect on turbulence? 3) Measurements to detect the flow and flow shear are needed.

Theoretical needs

The value of the two-fluid relaxation theory hinges on the ruggedness of the ion self helicity. *Issues*. 1) The ruggedness of the self helicity needs to be tested in two-fluid codes. 2) Is there a statistical description of the relaxation turbulence? 3) Are relevant theoretical results available in other contexts resembling the FRC, *e.g*. space plasmas?

Role of scrape-off-layer (SOL)

The FRC is embedded in a magnetic mirror with open field lines forming a SOL. It is likely that rapid outflows in the SOL plasma will prevent it from conforming to a state of minimum energy. *Issues*. 1) Do gradients in the SOL cause turbulence which governs global transport by regulating a continuous low-level of relaxation in the core plasma? 2) Does the relaxation proceed continuously or by intermittent events, and what are the transport implications? 3) Can this edge turbulence be controlled if need be?

Classical transport

The ion gyro-radius is relatively large in FRCs: $\rho_i/a > 1/5$ in present experiments and > 1/40 is expected for reactor. *Issue*. Is the classical transport (the irreducible minimum) significant?

Diagnostics and transport measurements

Issue. What types of diagnostics will be effective to measure and characterize the transport in FRCs?

Levitated dipole

The levitated dipole has been predicted to have excellent (possibly classical) confinement which would make it an candidate for burning advanced fuels (like DHe^3). Since the confining magnetic field is the field of a floating ring, there is no toroidal field and therefore there are nor drifts off of flux surfaces which leads to the "neo-classical" degradation of confinement present in tokamak-like systems.

A unique feature of a dipole is that MHD stability results from plasma compressibility. As a result the pressure gradient between the pressure peak and the vacuum chamber wall has to be relatively gentle in order to maintain MHD (interchange) stability and it is expected that the device will operate near this critical pressure gradient. The MHD stability requirement can be shown to require that $\omega^* < 2\,\omega_K$ with ω^* the diamagnetic drift frequency and ω_K the curvature drift frequency. Recent theoretical analyses [Kesner, Phys Plas 4, (1997) 419; 5 (1998) 3675] have indicated that this criterion also predicts the stability of the low beta drift modes. Therefore one can conjecture that when a dipole plasma is MHD stable it is also devoid of drift wave driven transport.

An interesting complication of confinement in a levitated dipole system is the possibility of convective cell formation, as has been observed in systems that have closed field lines [see for example G. Navratil, R.S. Post and A. Butcher Ehrhardt, Phys Fluids, 20 (1977) 157.]. In this regard it is important to observe that convective cells primarily transport particles. In fact in the marginally MHD stable equilibrium convective cells would only transport particles (no energy transport). In a reactor, for example, convective flows could would move ash outward (while cooling) and move fuel inward (while heating) toward the fusing region of hot plasma.

Scheduled speakers and discussion leaders

Discussion leaders for the morning MFE Transport Subgroup were:

- Tokamaks - Chuck Greenfield,
- ST - Phil Efthimion
- Stellarators - Alan Boozer
- ET - Michael Kissick
- RFP - Brett Chapman
- FRC - Loren Steinhauer
- Spheromak - Bick Hooper
- Dipoles - Jay Kesner

Discussion leaders and discussion recorders for the afternoon Transport and Turbulence Science Subgroup included Glenn Bateman, Mike Beer, John DeGroot, Darin Ernst, Greg Hammett, Jim Lyon, David Newman, Scott Robertson, Gary Staebler, Ed Synakowski, Paul Terry, Richard Town. The overview speakers for the afternoon Transport and Turbulence Science Subgroup were:

MFE overview Jim Drake: Turbulence and Transport: a theory/computational perspective

MFE overview Ed Synakowski: Transport physics needs and goals in magnetic fusion energy research over the next 5-10 years

IFE overview Bill Kruer: Waves-Particle Interaction and Turbulence Issues and Opportunities in Inertial Fusion

IFE overview Jean-Pierre Matte: Transport Issues in ICF Plasmas: Overview of past work and of unsolved problems.

IFE overview Sigfried Glenzer: Transport Experiments in ICF

Shorter contributed presentations:

Glenn Bateman: Issues for Predictive Transport Modeling

Juan Fernandez: Wave and Turbulence Experiments in ICF Plasmas: Research on saturation mechanisms for stimulated Raman scattering

Paul Terry: Issues in turbulence and transport with wider scientific implications

Bill Tang: Opportunities in turbulence simulations

Max Tabak: Fast Ignitor Theory

Mike Key: Fast Ignition Energy Transport Experiments

Jim Hammer: Transport in hohlraums/effects of magnetic fields

George Morales: Plans and rationale for a basic science facility

Stuart Zweben: Search for new plasma turbulence diagnostics

Amiya Sen: Need for better nonlinear dynamic models of plasma turbulence, and some promising experimental methods towards it

Stan Luckhardt: 2D and 3D imaging of plasma modes and turbulence: grand diagnostic challenge 2000-2010

Leonid Zhakharov: Liquid wall effects on transport and plasma behavior

David Newman: Improved methods for theory/simulation/experiment comparisons.

Greg Hammett: Opportunities for comprehensive simulations and experimental tests

Scott Parker: Synergies in Kinetic-MHD and Turbulence Simulation

Jim Callen: Future of Plasma Simulations = Chapman-Enskog-like approach?

Wally Manheimer: A Distant Mirror, Some Medieval Thoughts on Marginal Stability and Macro/Micro Interactions

Ron Cohen: Joint discussion with plasma boundary group on boundary turbulence, etc.

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