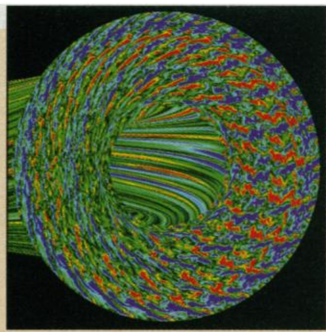
LETTERS

Turbulence

Readers discuss a new model predicting the performance of a large, planned fusion reactor, the International Thermonuclear Experimental Reactor (ITER). The model is based on a new theory of how turbulence (right) disturbs the hot, ionized particles (plasma) contained within powerful magnetic fields in the reactor. And readers suggest how the Internet could "favor interdisciplinary knowledge and interaction."



R.E. WALTZ AND G.D. KERBEL/NUMERICAL TOKAMAK PROJECT

ITER Forecasts

James Glanz's 6 December News & Comment article (p. 1600) about the performance of ITER draws on a recent model for heat transport in tokamaks, currently one of the most exciting and productive areas of fusion research. The article, however, displays a lack of perspective in reporting the model's application to ITER.

The model uses an innovative method for calculating ion-temperature profiles resulting from fine-scale core turbulence. It calculates these profiles in several present-day tokamaks when key auxiliary information has been provided from measurements—specifically, conditions near the plasma surface—giving insights particularly into the physics of ion-energy transport and indicating a sharp dependence of the core temperature on that of the surface. To use the model predictively for ITER, however, requires that all the auxiliary information be calculated, and the tools for some important elements are not well developed or benchmarked.

We disagree with this leap from an important achievement to sweeping and negative conclusions about ITER's performance. To predict ITER's performance, the international design team and its physics advisory groups use a combination of complementary techniques, based on both theory and scaling from current experiments. What is most important at this time is to test new insights against current experiments and then to take advantage of them to optimize ITER's choice of plasma operating regimes and to make refinements in the design where appropriate.

The transport discussion underscores the continuing scientific character of fusion research and ITER's experimental role in it. ITER is intended to be the first experiment to study burning, magnetized plasmas and test a panoply of fusion science issues at reactor scale. Glanz's article does not reflect this broader perspective: the experimental aspect of ITER, the interplay of experiment and theory, and the continuing maturation of the field of fusion research.

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The numerical technique of William Dorland and Michael Kotschenreuther consists, with some reasonable but not exact assumptions, of patching together several numerical codes, of which only the code treating small amplitude turbulence is close to a "first principles" treatment, while the critical outer regions of the ITER profile are expected to be in the highly nonlinear regime where reliance is put on a fluid "moment closure" technique. The group of independent modeling experts that advises ITER has judged that the agreement of the Institute for Fusion Studies–Princeton Plasma Physics Laboratory (IFS-PPPL) model with present experiments is not adequate for predictive purposes, and is no better than that of other less ambitious models that predict good performance by ITER.

Dorland and Kotschenreuther consider only ion thermal transport and rely on extrapolation for predicting the coupled phenomena of electron transport and density profile evolution. Nonlinear coupling to stable, and presumably damped, electromagnetic oscillations is not treated.

Their work has drawn attention again to the fact that fairly small changes in transport can have dramatic effects on ITER performance and has suggested directions for future experiments and data analysis. But they appear not to have given quantitative attention to the various techniques ITER might use to improve transport. These include central fueling techniques (high-field side pellets and compact toroids), plasma shaping, radio-frequency shearing forces, and more favorable current profiles (the planned ITER "advanced scenario"). At this incomplete stage of progress toward a real first-principles calculation, primary reliance should remain on the physics-constrained (proper dimensionless dependences) empirical scalings that yield optimistic ITER forecasts.

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We are writing to point out key omissions in the article by Glanz that result in an unduly pessimistic view of the likely performance of the ITER fusion device. First, because it is not noted that Dorland and Kotschenreuther, whose work is the subject of the article, are members of the Confinement Modeling and Database Group (an independent international team that advises ITER on these matters), the article could leave readers with the misimpression that they are outsiders.

Glanz does not mention the testing of the IFS/PPPL local transport model advocated by Dorland and Kotschenreuther against the data from the present experiments. This and 10 other models are being systematically tested against the profile data from eight tokamaks of differing sizes, and preliminary results were recently reported (*1*). It was found that the IFS/PPPL model had a standard deviation from the data that was similar in magnitude to that of many other models, not significantly better. Another model that is based on the same type of turbulence appears at this time to be significantly better correlated with the data and yields optimistic predictions for ITER. The paper of which Dorland and Kotschenreuther were co-authors (*1*) concluded that it was not possible to identify a best model at this stage and that further work on model development and testing is urgently required before accurate predictions for ITER can be made using this approach.

The fundamental problem with such local transport models is that they do not reflect the full range of physics that is taking place in tokamaks. For example, none of them, including the IFS/PPPL model, contains a validated model for the plasma edge region. Much of the pessimistic ITER projection from the IFS/PPPL model stems from the assumed pessimistic scaling of the plasma edge temperature; there are experimental counterexamples to these pessimistic scalings. Because of these shortcomings, emphasis has been placed on the direct statistical modeling of the global energy confinement data.

With regard to Glanz's other article (p. 1601), the simple log-linear models that are used by the database group to fit the global confinement data of the present devices have a good track record at predicting future performance with reasonable accuracy. For example, the performance of the present machines was well predicted in the early 1980s, before their construction, from the data generated by smaller devices available at that time.

Glanz does not point out that the more pessimistic projection of Dorland and Kotschenreuther obtained by use of a log-nonlinear fit is just one of a number of such nonlinear empirical models. Other scalings (such as offset linear ones) have been shown to give predictions that fall within the confinement time interval estimate provided by the database group. The results of the nonlinear models should, however, be taken with due caution in the absence of a compelling physical argument because they tend to pick up systematic measurement differences between tokamaks much more easily than do log-linear models. Within the class of log-linear models, the condition of the database (*2*) justifies the inclusion of the plasma density as a regression variable.

In summary, the theoretical work being developed by the IFS/PPPL group is improving our understanding of one of the transport processes at work in a tokamak. However, their model is at the present time incomplete without an experimentally validated model of the edge plasma, and it also requires improvements to the model of the plasma core if it is to accurately describe the experimental data. Dorland and Kotschenreuther have made important contributions to ITER transport studies, and we welcome their continued involvement and help with improving the methods for projecting its performance.

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References

1. J. Connor *et al.*, "Validation of 1D transport and sawtooth models for ITER" (IAEA-F1-CN-64, paper presented at the International Atomic Energy Agency Fusion Energy Conference, Montreal, Canada, 11 October 1996).
2. O. Kardaun *et al.*, *IAEA 14th Conf. Proc.* (Würzburg) **3**, 251 (1993).

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Given the immaturity of the models discussed in Glanz's article, they should not be relied on for predicting the performance of larger devices. By carrying out dimensionless scaling experiments (i) on single facilities matching as closely as possible the ITER conditions, and (ii) between machines, it is possible to predict the performance of ITER. Exploring such data shows that there is no effect of the type predicted (in Glanz's article) so far. This approach and that of fundamental turbulence models with their various uncertainties do lead to a range of predictions for future experiments such as ITER. As in the past—when JET in Europe and the Tokamak Fusion Test Reactor in the United States were under consideration—there is always a degree of uncertainty when extrapolating to future machines. A more focused activity on a range of existing flexible devices should reduce these uncertainties and provide more confidence that a facility such as ITER can produce substantial fusion power (about 1 gigawatt).

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While I have much respect for the work done by those involved with the IFS-PPPL model, a quote attributed to me that the model is "essential" to understanding many results in present tokamaks should be clarified. I was referring to the fact that calculations that serve as the underpinning of the IFS-PPPL model form an "essential" component of one proposed picture describing the suppression of turbulence and improved transport in a particular magnetic configuration. Many elements of plasma theory that are widely regarded as necessary for an accurate physical description of turbulent plasma processes are captured by this model, but the model is incomplete and should not be regarded as essential in a broad sense.

In addition, focusing only on the implications of this particular model for ITER runs the risk of losing sight of the tremendous overall progress in fusion science and the improvements in reactor design that may be implicit in these developments. Owing to significant advances in both experiment and theory in the last several years, fusion plasma research is an extremely exciting field to work in at present. Demonstrations of transport and turbulence control not possible five years ago have been made, and we have much-improved theoretical models that are helping us understand the experimental results. The IFS-PPPL model is one of these, but the advance in theory is much broader. Debate over the IFS-PPPL model with respect to ITER would not be happening at all were it not for profound developments in plasma physics and fusion research in general. While this and other models have significant deficiencies, their level of sophistication is high enough that a fundamental change in the dialogue between experimentalists, theorists, and reactor designers is taking place.

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Response: Glanz's article is a snapshot of the complex, ongoing, and open scientific debate over the causes and effects of plasma turbulence in present tokamaks and in ITER. While the article is factually accurate, it is inevitably incomplete. Because it deals extensively with our research, we would like to discuss in our own words some of the issues raised in the article and in the preceding letters. We agree that many of these issues should be studied further.

As noted in our scientific papers, our present model predicts only core temperature profiles and neglects additional turbulent mechanisms in the relatively cool outer region of the plasma. It does not predict density or momentum profiles, or the near-surface ("pedestal") temperature. We believe that the auxiliary assumptions we use for these quantities are reasonable and have experimental and theoretical support, although further tests would help to reduce the resulting uncertainties.

It is useful to consider the historical context within which the current debate over tokamak confinement extrapolations is taking place.

Since the early 1970s, fusion scientists have improved tokamak performance more than 10,000-fold (by the standard measure of the product of density, temperature, and energy confinement time). Some of this gain came from improvements in our empirical understanding of plasma behavior and some from the construction of larger and more capable devices. While there have been major advances in plasma turbulence theory over the past 30 years, empirical predictions of tokamak performance were clearly more accurate than the resulting theoretical predictions. Consequently, empirical performance projections were used for the ITER design.

Nevertheless, the desirability of a predictive capability with a firmer theoretical foundation was widely recognized and led to a concerted effort over the last several years to understand plasma turbulence better. This effort continues to produce new experimental diagnostics, more dedicated turbulence experiments, valuable multi-tokamak experimental databases (assembled through the ITER project), and advances in theory, including our research with M. A. Beer and R. E. Waltz. We believe that as a result of this broad effort, more reliable theoretical performance estimates are now emerging. However, they remain incomplete. More work is needed, particularly to understand the pedestal physics better.

As Glanz describes, our research highlights several effects important for tokamak performance, including the pedestal temperature, core velocity shear, density peakedness, ion-to-electron temperature ratio, and dilution by impurities and fast ions. Although anticipated in part by antecedent theories, the experimental significance of these effects has only recently begun to be realized, and the empirical scaling methods have not yet adequately taken into account their possible importance. We believe this is why the empirical ITER projections are optimistic relative to ours.

One of the most robust predictions of our model is the sensitivity of high-performance plasmas to the pedestal temperature, a sensitivity that is consistent with much experimental evidence. Although the physics of the pedestal are still a topic of controversy and ongoing analysis, leading theories of pedestal physics imply a relatively low pedestal temperature for ITER, giving the lower range of predictions shown in the graph in Glantz's article. The ITER Confinement Database and Modeling Working Group is planning experiments to investigate the sensitivity of performance to pedestal temperatures. Additional experiments are needed to elucidate the dependence of the pedestal temperature on relevant parameters. We and others are studying possible methods for generating higher pedestal temperatures or providing other stabilization mechanisms.

All of these issues are being reviewed within the ITER project. We believe we have answered the most important questions about our ITER performance projections at meetings within the ITER community, but ours is still a relatively new and controversial theory. Some aspects remain to be disseminated widely. Part of the scientific process will include more fusion scientists looking for mistakes in our analysis or proposing refinements. The U.S. Department of Energy and international review bodies are scheduled to evaluate the ITER design and will also involve the wider fusion community.

Rosenbluth points out several techniques that might improve ITER performance. We agree that these advanced confinement techniques have impressive potential. We feel that more near-term experimental and theoretical analyses of their scaling and accessibility requirements are called for. There are concerns that the present ITER design may not be able to incorporate these techniques.

We are concerned that some readers (perhaps influenced by the headline of Glanz's article) may infer that our projections of potentially low performance for the present ITER design imply a dim future for fusion energy in general. Nothing could be further from the truth. The last few years of magnetic confinement fusion research have been particularly exciting. In recent experiments (see J. Glanz, News & Comment, 28 July 1995, p. 478, for example), several advanced confinement techniques have been discovered that dramatically reduce the core turbulence and associated heat loss. A quantitative theoretical understanding of these techniques is rapidly developing. We are excited about these recent advances, which we believe could lead to more compact and attractive tokamak fusion power plants.

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