EIGHTEENTH FUSION ENERGY CONFERENCE

SESSION TH4

Monday, 9 October 2000, at 09:00 a.m.

Chair: H. BERK (USA)

SESSION TH4: Transport, Barrier, Edge Physics (provided by J.C. DeBOO, USA)

Paper IAEA-CN77/TH4/1 (presented by G.M. Staebler)

DISCUSSION

A.L. ROGISTER: Your ITG model, and in particular the Weiland model, assumes the ballooning representation. This, by definition, is invalid in the vicinity of the ITB where the magnetic shear vanishes. You rather should apply a slab model corrected for toroidicity. (See THP1/01: Theory of Low Frequency Instabilities Near Transport Barriers.)

G.M. STAEBLER: Both GLF23 and the multi-mode models employ the ballooning representation. This restricts the class of eigenmodes included in the calculation of growth rates. The computed growth rates are continuous through the point $\hat{s} = 0$. Non-ballooning modes near $\hat{s} = 0$ could be included in the formalism in the future taking advantage of recent theoretical advances.

P. DIAMOND: You mention mean ExB shearing and its role in ITB. As you know (I hope), zonal flows, distinct from mean flows, are crucial to regulating transport, even in L-mode. How does GLF treat zonal flows in its saturation recipe? How sensitive are the results to the assumptions regarding zonal flow effects?

G.M. STAEBLER: The GLF 23 model uses a saturation rule which combines the linear growth rate and an estimate of the zonal flow damping rate. The saturation rule was constructed to fit non-linear ITG mode simulations in the gyro-Landau fluid approximation. Recent work has identified flows in the zonal flow treatment in these simulations. The GLF 23 model should be revised to fit the improved gyro-Landau fluid or gyro kinetic turbulence simulations. A saturation rule for ETG modes including streamer physics also needs to be fit to non-linear simulations. These improvements to GLF 23 are planned for next year. The impact will be a reduction in the ITG and an increase in the ETG transport.

V. PARAIL: The set of equations being solved varies from case to case in your simulations. This means that, for example, not all components of the radial electric field have been computed self-consistently. Could this non self-consistency influence the result of your simulations?

G.M. STAEBLER: At this time, a calculation evolving all of the five fields (T_e, T_i, N_i, V_Φ, V_{ExB}) and the q-profile has not been attempted with either GLF 23 or multi-mode. The complete set of transport equations has been determined and a full simulation will be possible in the next year for GLF 23. It is anticipated that self-consistency is important as has been demonstrated in simpler models. Nevertheless, it is not always necessary to evolve all of the fields in order to obtain valid physics insights.

Paper IAEA-CN77/TH4/2 (presented by J.W. Connor)

DISCUSSION

R. GOLDSTON: I am troubled by both this talk and the last. In the last talk every simulation showed an electron temperature transport barrier, and every set of data showed no barrier. In this paper it seems that for RTP case (E) the electron temperature in the simulation peaked well inside of the heating radius of 0.55a, while the data peaked at 0.55a. My impression is that we have a lot more work to do, despite some exciting progress, before we understand the electrons as well as the ions.

J.W. CONNOR: The interpretation of the CUTIE simulation results on the peaking of the electron temperature inside the heating position in case E involves convective, rather than diffusive, transport resulting from the longer wavelength modes near a rational surface at this point. A transport barrier forms at this rational surface and there is also a pinching of the temperature due to convection there. There are differences in detail between the q-profiles and heating profiles used in the simulation and experiment that can affect the exact location of this barrier, relative to the heating, but the simulation provides some qualitative understanding of underlying processes.

F. PORCELLI: In Porcelli et al, PRL 99, a simple model was developed for the explanation of temperature filaments and the main "transport barrier" at q=1 observed in RTP. The model is based on the interaction between m/n = 1 modes and ECRH. It may be that the code CUTIE is capturing the same physics. What is the role of m/n = 1 modes in CUTIE simulations of temperature filaments with ECRH?

J.W. CONNOR: When there is a q=1 surface present, as in the more centrally heated cases A and B, CUTIE includes the effects of the m = 1 mode, central to your theory. The 3/2 rational surface also plays a role in barrier formation in such cases. It would be interesting to compare your results on temperature filamentation with the CUTIE code predictions.

Paper IAEA-CN77/TH4/3 (presented by T. Tamano)

DISCUSSION

G. BATEMAN: There have been experiments carried out in which the neutral beam injection has been changed systematically from co-injection to counter-injection. The toroidal momentum went through zero in these scans. How does your model handle zero toroidal momentum?

T. TAMANO: Our model does not apply to zero toroidal momentum since the mechanical part has to be larger than the vector potential term. However, ∇p_i must be balanced by en (E_r + $U_\perp B$); therefore, there should be a substantial $U_\perp$ or E_r for a high performance plasma. In the former case, u_Φ appears in reality, which is usually observed in experiments. In the latter case, the potential energy term has to be examined.

R. TAYLOR: Excellent work. Pat Diamond just informed me that you have done the theory for the electric tokamak. If this is true then we have a new perspective. Does this mean that E x B shear is not a critical physics concept?

T. TAMANO:

1. Reversed shear is probably not relevant to ITB.
2. It is related to the answer for the previous question. For a high performance plasma, either E_r and or $U_\perp$ must appear. E x B shear may play an important role to redistribute toroidal angular momentum to the optimum profile.

H.L. BERK: Your claim that electromagnetic modes determine transport counteracts the belief that in low beta plasma it is the electrostatic waves that determine the lifetime. How do you resolve this discrepancy?

T. TAMANO: Our model uses the variation method. Therefore, the profile consistency applies only to the best performing plasma. For other plasmas, the constraint could be weaker and the electrostatic waves could determine the plasma lifetime.

Paper IAEA-CN77/TH4/4 (presented by X.Q. Xu)

DISCUSSION

B. SCOTT: You have shown the radial grid with the source terms, with a source term structure indicating that about half of the closed field line region is unaffected by the application of the external sources. This is about 10 grid points in your domain. In my experience one needs to separate the inertial scale from the grid scale, and to do this one needs at least 32 grid points in the inertial region. I therefore feel that the pedestal you have shown in your H-mode phase may be a direct result of this computational profile maintenance rather than of the actual physics.

X.Q. XU: We include the source term in density, not in temperature. What we show is the suppression of electron heat flux which is not directly affected by the numerical source, rather physics associated with the E_r shear suppression. We do not claim we have self-consistent pedestal formation as in experiments yet.

R. GOLDSTON: When you described your calculations you indicated that the profiles were self-consistently derived from sources and fluctuation-driven transport. When you showed calculated fluxes versus radial and poloidal angles, it seemed that the flux went to zero near the separatrix - or at least dropped greatly compared with just inside or outside the separatrix. How is this consistent with the continuity equation?

X.Q. XU: The particle flux that I showed is for a fixed density profile case. It is indeed not self-consistent. For a self-consistent source and turbulent transport as shown in the paper, the radial variation of the particle flux is weak.

Paper IAEA-CN77/TH4/5 (presented by L. Chen)

DISCUSSION

A.L. ROGISTER Your picture of zonal flows as modulation instability of the envelope is very interesting. To me, however, the envelope concept is very much a linear one. It relies on a periodicity property which breaks down when many toroidal mode numbers are involved. My point is that one should compare the strength of the two processes; non-linear interaction of drift waves with different toroidal mode numbers and modulation instability.

L. CHEN: Near marginal stability, the separation of time scales ensures that for each toroidal - n mode, the concept of radial envelope for poloidal harmonics remains valid. Furthermore, we can rigorously demonstrate that in the small ion Larmor limit, drift-drift-drift interactions have smaller coupling coefficients than that of drift-zonal field-drift interactions.

S.A. SABBAGH: Can you comment on the next order effects of aspect ratio and shaping in your theory in a qualitative manner?

L. CHEN: Equilibrium geometrical effects will affect the linear stability properties of the drift-Alfén eigenmodes. Non-linear properties, however, will not be affected, i.e.; modulational instabilities of the radial envelopes will be operative regardless of shapes etc.

W. DORLAND: Do I understand correctly that you are predicting a significant $\delta A_{\|Z}$ ("zonal field") mainly at scales ~ c/ω_{pe}?

L. CHEN: Yes.

Paper IAEA-CN77/TH4/6 (presented by K. Hallatschek)

DISCUSSION

T. ROGNLIEN: What would be the effect of a separatrix (magnetic shear and open field lines) on these edge simulations?

K. HALLATSCHEK: A steady state poloidal asymmetry in anomalous transport, which could drive a stationary flow via Stringer-Windsor effect.

Z. LIN: The GAM frequency is typically higher than the microturbulence decorrelation frequency. It has been shown by Hahm et al. that when the flow oscillation frequency is higher than the turbulence decorrelation rate, the shearing effect is weak.

K. HALLATSCHEK: In the edge the gradients are higher and the sound speed lower than in the core. Therefore the turbulence decorrelation frequency is higher and the GAM frequency lower. In our turbulence simulations, the turbulence time scales are typically somewhat smaller than the GAM time scale.

S. A. SABBAGH: It seems that the present study is for poloidal flows less than or equal to the sound speed. However, transonic and supersonic flows should change the solution (i.e. shocks can evolve). Have you attempted to simulate these regimes?

K. HALLATSCHEK: No. The self-generated flows are a few times the diamagnetic velocity. For this to be of the order of the sound velocity we would need extremely large gradients or extremely small machines.

X.Q. XU: Do you know in your system how different are the ExB velocity phase velocity and group velocity?

K. HALLATSCHEK: All three are different.

B. SCOTT:

1. Drift-scale (ρ_s) affects are in fact dominant in the edge. There is no resistive ballooning regime on closed field lines, and the dominant balance is resonant coupling between drift-mode 3-wave interactions and parallel electron dynamics (paper THP1/12).

2. Is the Stringer-Windsor effect present in flux tube computations? (yes) Given that it is your reason for coherent flow drive, can you comment on the fact that electromagnetic computations run at the same parameters see that as a <u>sink</u> for zonal flows?

K. HALLATSCHEK:

1. Experimental data show that the resistive ballooning regime (α_d=0-0.2, $\varepsilon_n \geq 0.4$) can occur in the edge of AUG. The author has checked this.

2. I have yet to do these simulations with $\beta > 0$. Whether the SW-term is a source or sink depends on the parameters as was shown in the talk.