SESSION TH1

Tuesday, 20 October 1998, at 4.10 p.m.

Chairman: F. Troyon (Switzerland)

THEORY 1

Paper IAEA-CN-69/TH1/1 (presented by A.M. Dimits)

DISCUSSION

K. LACKNER: It has been argued that the effects of self-generated flows should be smaller in ITER than in present-day devices due to the smaller ρ^*. Do you see an effect of ρ/L_T on the critical R/L_T for non-linear suppression of turbulence by flows?

A.M. DIMITS: Most of the simulations shown are formally in the $\rho^* = 0$ limit. These runs do show the non-linear suppression effect. For the simulations where ω_{*T} was a function of radius, the effective ρ/L_T was around 1/60, and in these cases the stabilization is probably stronger, because the profile variation “channels” the flux-surface-averaged flows and makes the radial transport barriers less penetrable.

M. KOTSCHENREUTHER: The IFS-PPPL model was computed for a different magnetic geometry than the (s,α) model you use. This shifts the critical gradient. If the same geometry were used, the IFS-PPPL model would look less discrepant.

A.M. DIMITS: My understanding is that the non-linear gyrofluid simulations that form part of the basis of the IFS-PPPL model, and with which the ’94 IFS-PPPL model agrees quite well, were done with the same (s,α) model equilibrium as used in our gyrokinetic simulations. I agree that the linear gyrokinetic code used for the quasilinear calculations that form the other component of the IFS-PPPL model used a different equilibrium.

M. YAGI: Did you check the error bars for χ_i and the convergence of the average value of χ_i statistically, changing the sampling data?

A.M. DIMITS: The error bars have not been checked very quantitatively. It is evident from time averaging the plots shown in the particle-number convergence sequence that the statistical variation in the inferred thermal diffusivities is in the range of 5% or less. These time histories have no secular trend and have good time averages.

Paper IAEA-CN-69/TH1/2 (presented by Y. Kishimoto)

DISCUSSION

B. SCOTT: In this computation, were the electrons adiabatic? Further, what were the boundary conditions at the outermost radius and where was that radius - the outermost radius in the computations - relative to the last closed flux surface?

Y. KISHIMOTO: The simulation is done for the typical ITG mode and adiabatic electrons are used. This is a particle code and the outermost radius is the conducting wall where the particles are reflected. With regard to the waves, the mode rational surfaces are removed from the edge region so that the edge region becomes a heat bath.

Paper IAEA-CN-69/TH1/3 (presented by X. Garbet)

DISCUSSION

E.B. HOOPER: Heat pulse experiments often show higher thermal conductivity than steady-state experiments. Is this predicted by your avalanche theory?

X. GARBET: In simulations at fixed flux, heat pulses are indeed observed to propagate faster than you would expect from the equilibrium thermal diffusivity. So the overall picture is consistent.

V.V. PARAIL: I would expect strong avalanches when the entire system is close to linear stability. In experiment, however, this is often not the case. How can you explain fast heat pulse propagation here?

X. GARBET: Even when the temperature gradient time average is well above the threshold, the temperature profile exhibits transiently steep and flat regions which propagate radially. In other words, avalanches exist even at large heat or particle fluxes.

W.M. NEVINS: In fixed temperature gradient simulations, have you really fixed the temperature gradient, or have you fixed the temperature difference between the left and right boundary? We have used the latter boundary condition and do see avalanches and intermittency.

X. GARBET: In fixed temperature gradient simulations, the whole temperature profile is frozen; all radial modes are removed except the profile itself. It is true that avalanches are observed if radial modes are included with fixed boundary conditions. However, in our experience these avalanches are weaker than when the system is driven by a source.

Paper IAEA-CN-69/TH1/4 (presented by M. Kotschenreuther)

DISCUSSION

B. COPPI: To confirm your suppositions about the favourable effects of reversed current density profiles, I should mention that P. Detragiache (see our paper IAEA-CN-69/FTP/14) has shown that the stability of ideal MHD m = 1, n = 1 modes is greatly improved when "shoulder" q-profiles are considered. On the other hand, I do not think that the ignition machine you propose is technically feasible when all the material and engineering constraints are considered.

M. KOTSCHENREUTHER: I would prefer not to give an opinion on the engineering problems at this stage.

K. LACKNER: Have you checked the classical confinement of alpha particles in the proposed low aspect ratio ignition device?

M. KOTSCHENREUTHER: I have not done a detailed calculation.

R.D. STAMBAUGH: Should we have already seen this favourable transport physics in START?

M. KOTSCHENREUTHER: START is very resistive, so it could have resistive MHD turbulence (which has not been addressed here). Otherwise, I would expect we should.

G.M. STAEBLER: Were the electrons assumed to be neoclassical in the edge barrier region?

M. KOTSCHENREUTHER: Yes, on the basis that microtearing modes and electron temperature gradient driven modes appear velocity shear stabilized. I should also point out that, within the extreme limit of the ideas presented here, the magnetic shear is very negative where the pressure gradient occurs, which has been correlated experimentally with electron temperature barriers.

Paper IAEA-CN-69/TH1/5 (presented by B. Scott)

DISCUSSION

I.H. HUTCHINSON: It is interesting to see the importance of realistic shaping. Can you explain further what are the main physical effects of shaping on turbulence; for example, is the local magnetic shear reversed at the outboard in the ASDEX-U cases you analysed?

B. SCOTT: In a flux tube computation like this, the magnetic geometry enters through local and global shear, flux expansion, and the form of normal and geodesic curvature as a function of poloidal position. All of these are affected by finite aspect ratio and shaping. For the ASDEX Upgrade 95% flux surface geometry, global shear is strong enough to keep local shear positive at all poloidal positions. The strongest finite aspect ratio effects are to make the local drift scale, ρ_s, larger on the outside, and to reduce the normalized normal curvature on the outside. Elongation affects both global and local shear; for the same MHD q, the global shear is stronger in an elongated plasma. All of these effects together lead to weaker turbulence for stronger elongation.

V.V. PARAIL: You conclude that electron dynamics influence ITG turbulence even deep in the core. Can you quantify the effect? Do you think that electrostatic approximation is still appropriate?

B. SCOTT: The electrostatic approximation is not appropriate, since the drift Alfvén parameter, $\hat{\beta}$, is of order unity or larger at all radii in the plasma. The only way to avoid strong electron effects at finite $\hat{\beta}$ is apparently to go to flat density (zero gradient). Particle transport is most sensitive, electron heat somewhat less, and ion heat less still. However, as a result of the non-linear synergy, all are enhanced by finite $\hat{\beta}$ and ∇n. To date, the only result is the scan in ∇n at constant n, T_e, T_i, ∇T_e, ∇T_i reported in the paper. The numbers are given in Fig. 4.

T.R. JARBOE: Have you studied the effects of drift Alfvén waves on the current profile?

B. SCOTT: Not really. There is a self-consistent profile modification in the poloidal field in the model, but we can tell from the lack of any noticeable effect on the limiting time step that it is not significant. Ultimately, one would have to update the magnetic flux surface geometry in response to changes in the pressure and poloidal field profiles. Up to now, this has not been attempted in a turbulence code.

Paper IAEA-CN-69/TH1/6 (presented by A. Fukuyama)

DISCUSSION

K. LACKNER: Your model is based on the current diffusive ballooning turbulence paradigm. To what extent has this model been compared with the results of 3-D turbulence codes, like those by Scott or by Drake, Rogers and Zeiler?

A. FUKUYAMA: Since CDBM usually has a short wave length - typically of the order of, or less than, the ion gyroradius - I think it is difficult to study CDBM in the present 3-D turbulence codes.

Papers IAEA-CN-69/THP2/01 and 02 (rapporteured by B.N. Rogers)

DISCUSSION

A.L. ROGISTER: In your model (paper IAEA-CN-69/THP2/01), and also in the models of Fukuyama (paper IAEA-CN-69/TH1/6) and Scott (paper IAEA-CN-69/TH1/5), the turbulence and the profiles must evolve simultaneously. How do you reconcile this requirement with the observations of Moyer et al. (Physics of Plasmas, 1995) regarding the formation of the characteristic negative shear E_r profile before turbulence suppression and any change in the n and T profiles?

B.N. ROGERS: Consistent with the result you mention, we do observe that the steepening of the profiles is triggered by a prior burst of ExB flow.

G.M. STAEBLER: What is the impact of an equilibrium electric field shear on the turbulence in your simulations?

B.N. ROGERS: Local, turbulence-generated ExB flows, rather than equilibrium flows, trigger the transition in our simulations. Once the gradients begin to steepen, however, an equilibrium ExB flow is spontaneously generated that reinforces the transition.

V.V. PARAIL: Can your model predict the radial position and the width of the region with reduced transport? Are you sure that it should be localized near the separatrix?

B.N. ROGERS: In the simulations run so far, the pedestal steepens following the transition until, far above the ideal stability limit, it is destroyed by a rapidly growing global mode. The onset of this mode is the only thing we have (thus far) identified that constrains the pedestal structure.

J.W. CONNOR: Both you in your first paper and Dr. Scott in his paper, IAEA-CN-69/TH1/5, employ similar equations and similar geometry, yet the results seem very different. Can you comment on this disparity?

B.N. ROGERS: The results do appear to be different but the sources of the disparity are not yet understood.

J. WEILAND: In the work described in the first paper, I would expect electron thermal conductivity along B to be important.

B.N. ROGERS: It is indeed important in the higher α_d (diamagnetic) regime.