NINETEENTH FUSION ENERGY CONFERENCE

SESSION TH/1

Wednesday, 16 October 2002, at 8:30

Chair: K. LACKNER (EFDA)

SESSION TH/1: Core Turbulence and Transport

Paper IAEA-CN94/TH/1-1 (presented by Zhihong Lin)

Discussion

R.E. Waltz: I repeat my comments made to you in private because they are of general interest. In my opinion your Bohm to gyro-Bohm transition result is both qualitatively and quantitatively incorrect. At the low ρ* gyro-Bohm end the result for χ is 50–70% larger than the flux tube benchmark obtained by four or five other codes. We have tried to reproduce your results with the GYRO code using your profiles. We find the Cyclone benchmark at low ρ*, but very small breaking of gyro-Bohm scaling at the high ρ* end. This is as we expect since your profiles have very little variation (flat T and n, flat $1/L_T$ and $1/L_n$ except at the box edge). Such profiles are not very physical.

Zhihong Lin: We have shown in this talk that when we use the Cyclone geometry, our gyro-Bohm results agree with Cyclone. To date, our GTC study of size scaling is the only large scale, global simulation ever to have been carried out to demonstrate the convergence to gyro-Bohm. Your semi-local GYRO simulations have an artificial damping at the edge, which dissipates the energy. As shown in Fig. 2 of your recent paper [Phys. Plasmas **9** (2002) 1938], the non-conserved energy is 5–10% of the temperature. To compensate for this, you need to supply an energy source. The dynamics in your simulation could be totally dominated by this artificial damping and source. Furthermore, your GYRO code uses a fixed 11 toroidal mode number for any device size, compared to the full 500+ modes in our GTC code, and your GYRO has a very low resolution in velocity space of 24 grids. In particular, the six pitch angle grids in GYRO may not resolve the trapped–passing boundary, which varies with minor radius and which is critical for zonal flow damping.

Paper IAEA-CN94/TH/1-2 (presented by F. Jenko)

Discussion

R.E. Waltz: Did your finite beta scans use a fluid model or gyrokinetics? Did the simulations include trapped electrons? If not, would you agree that including the trapped electrons could significantly change the beta scan, particularly at low beta?

F. Jenko: A fluid model was used, keeping passing electrons but neglecting particle trapping. Including trapped electrons would certainly change the beta scan quantitatively, but not qualitatively. The purpose of this study is to emphasize the role of finite beta passing electron dynamics, leading to nonadiabaticity and flutter transport.

Paper IAEA-CN94/TH/1-3 (presented by L. Villard)

Discussion

R.E. Waltz: Did the $v_\parallel$ nonlinearity study refer to ITG adiabatic electron simulations? As you know (for ITG in particular), this parallel nonlinearity is usually neglected for small ρ^*. You apparently found it to be significant. What ρ^* value was used (i.e. how many ion gyroradii in the minor radius)?

L. Villard: Yes, the $v_\parallel$ nonlinearity study refers to ITG simulations with adiabatic electrons. The minor radius is 135 ion radii. There are two reasons to retain the $v_\parallel$ nonlinearity. First, we want to preserve the energy conservation property of the system because, as this paper shows, it is a very useful indicator of the quality of the numerical simulation. Second, even though this term would be ordered as smaller than the $E \times B$ nonlinearity, its effect on the nonlinear saturation process is not small. Neglecting it implies neglecting parallel particle trapping and therefore ignoring nonlinear parallel ion Landau damping. As a consequence, we have found that the population of the modes is affected, a sign that the mode cascade process is affected, including the very crucial zonal component (m=0, n=0). Since the zonal component is linearly totally undamped, it seems to us very important to retain the nonlinear parallel ion Landau damping, even though it is a “small order” quantity.

Paper IAEA-CN94/TH/1-4 (presented by M. Yagi)

Discussion

M. Porkolab: What we have learned from the previous three talks is that using the estimate $D \propto \gamma/k^2$ is incorrect and perhaps irrelevant. Yet you are using such estimates in your theory. So how reliable is your theoretical work?

M. Yagi: Notice that γ_{dec}/k^2 in our model is calculated by use of the nonlinear decorrelation rate, not the linear growth rate γ_{lin}. The nonlinear decorrelation rate can deviate greatly from the linear one, because the nonlinear instabilities are the origin of turbulence. We do not use the formula $D \sim \gamma_{lin}/k^2$.

T.S. Hahm: How can your hierarchical model for multi-scale turbulence modify the more conventional picture of transport barrier formation (soft transition) based on an S-shaped curve due to the $E \times B$ flow shear in the (flux, gradient) space?

M. Yagi: One example is that the suppression of semi-micromode fluctuations, which are the origins of ion and electron transport, leads to excitation of microfluctuations, which drive electron transport. The onset of the ion transport barrier and that of the electron transport barrier are possibly different.

Paper IAEA-CN94/TH/1-5 (presented by Y. Kishimoto)

Discussion

M.C. Zarnstorff: In your figure, you showed that χ was reduced for both s = 0.1 and s = 0.4. What do you calculate for even lower shear?

Y. Kishimoto: As the magnetic shear becomes weak, since the distribution of mode rational surfaces decreases, we need to enlarge the system size to have a convergence. Specifically, since the zonal flow saturation sensitively depends on secondary instability such as the Kelvin–Helmholtz mode, the well packed rational distribution has to be kept. We performed the simulation up to s = 0.05 and observed the qualitatively similar soft transition nature, i.e. the flat or decreasing tendency of the electron heat diffusivity as a function of η_e.

K. Lackner: The difficulty in taking the calculations to very low values of shear seems to be linked to the absolute magnitude. Could you make (or have you already made) calculations for negative values of s?

Y. Kishimoto: Our present model is a simple sheared slab configuration, and therefore the system and/or the obtained result is symmetry for positive and negative magnetic shear, so that no difference appears in the heat diffusivity with respect to the magnetic shear. As for the very low magnetic shear case, this is related to the first question by Dr. Zarnstorff, a nonlocal and/or global treatment where the very low and finite magnetic shear coexist may be necessary.

Paper IAEA-CN94/TH/1-6 (presented by Jiaqi Dong)

Discussion

F. Ryter: The comparison of $(R/L_{Te})_{crit}$ in the equation with theory must be calculated very carefully because R/L_{Te} deduced from T_e is not necessarily $(R/L_{Te})_{crit}$.

Jiaqi Dong: Thank you for your good comment. Here, $(R/L_{Te})_{crit}$ corresponds to the value of the temperature gradient parameter above which the electron heat conductivity increases dramatically with temperature gradient. The theoretical results and the experimental observations should be comparable if the driving mechanism is the same and the nonlinear shift of the threshold is not large.