SESSION TH3

Saturday, 24 October 1998, at 10.45 a.m.

Chairman: J. Sheffield (United States of America)

THEORY 3

Paper IAEA-CN-69/TH3/1 (presented by M.N. Rosenbluth)

DISCUSSION

B. COPPI: How do you deal with the fact that the modes excited in a torus are standing along the magnetic field with certain well-defined symmetries?

M.N. ROSENBLUTH: Radial propagation is the critical issue for interaction with zonal flows.

Paper IAEA-CN-69/TH3/2 (presented by H.R. Wilson)

DISCUSSION

K. TOI: You consider the peeling mode in the condition where the rational surface is just outside the last closed flux surface (LCFS). However, for the H-mode in divertor configurations the rational surface is located just inside the LCFS. I have observed modes in low m (m = 4, n = 1 in ASDEX [1998 Phys. Rev. Lett.] and m = 3, n = 1 in the JIPP T-II U tokamak [IAEA Conference 1990][*] having edge tearing type modes in H-mode. How is your conclusion modified if you also take into consideration an edge tearing mode?

H.R. WILSON: I agree that certain features of separatrix geometry require special consideration. For example, in separatrix geometry with the rational surface inside the LCFS, it is important to retain the rapid variation of the magnetic shear across the mode width when deriving the peeling mode stability criterion; then one obtains the same criterion as the case with the rational surface outside the LCFS in limiter geometry. Our existing model is based on ideal MHD but the consideration of tearing modes would be an interesting extension.

W.M. NEVINS: Inductive effects will delay the appearance of the bootstrap current for an (edge) skin time. This opens up the possibility that the discharge might be able to pass through the peeling mode unstable region before the bootstrap current appears.

In any case, if we identify peeling modes with ELMs, we would expect to see a small negative dip in the loop voltage preceding each ELM (and probably a positive spike synchronous with each ELM). Is this signature observed on the loop voltage in the experiment?

H.R. WILSON: I would indeed expect that it would be possible to access the second stability regime by a fast ramp of pressure, passing through the potentially unstable region before the bootstrap current has a chance to build up. Quantification of this requires modelling by a transport code, which is being addressed. It would be instructive to explore the signature associated with the loop voltage further, both theoretically and experimentally.

---

[*] Plasma Physics and Controlled Nuclear Fusion Research 1990 (Proc. 13th Int. Conf. Washington, D.C.,1990) Vol.1, IAEA, Vienna (1991) 301.

Paper IAEA-CN-69/TH3/3 (presented by T. Hayashi)

DISCUSSION

R.J. BUTTERY: Does the model predict modes at low β as well?

T. HAYASHI: Yes. The model can predict both the pressure driven and current driven modes.

R.J. BUTTERY: Does the code predict the stability or just how modes couple/evolve?

T. HAYASHI: It predicts linear and non-linear stability of plasmas.

R.J. BUTTERY: I would like to have details of the model - is it resistive MHD or ideal? What profiles have you used, and do they match START? What boundaries does the code predict and where are the stable and unstable regions in (β, q) space?

T. HAYASHI: The model is described in my response to Dr. Peng's question. The paper focuses on the non-linear development of modes, which is not yet well understood, by picking up several typical initial equilibria.

W. PARK: You have achieved a nice result. I should like to ask why phase-alignment and phase-locking occur? Your result is similar to our earlier result on tokamak thermal quench, where one can show that a pressure bulge tends to further steepen non-linearly because it is energetically favourable. This would give phase-locking. You have used a case where n = 1 and n = 2 mode linear growth are exactly the same; in general, though, they will differ. Any such difference will result in a large difference in amplitude after many e-folding times in the actual experiment, so the above argument would still apply.

T. HAYASHI: Thank you for your favourable evaluation of our result. In your previous result, the ballooning modes are excited in a locally enhanced bad curvature region which is formed by a helical distortion of the torus caused by the growth of the kink mode. Thus, the process may be called the “mother-daughter” type, and the mechanism may be understood in terms of energetically favourable deformation, as you suggest.

In our case, on the other hand, the phase-alignment occurs among the “sister” modes, and the precise mechanism is still an open question. Our computational experience indicates that such an alignment in phase often occurs after many e-folding times, even for cases where the linear growth is slightly different. I should add that the linear growth time is usually very similar among multiple dominant modes for the case of a pressure-driven mode.

Y.K.M. PENG: Could you explain the physics model used in the simulation and the plasma conditions assumed?

T. HAYASHI: We used the resistive, compressible, full non-linear MHD model in full toroidal geometry. The boundary condition is put only on the surrounding wall which is assumed to be a perfect conductor. The computation region includes the ambient external magnetic field, and the deformation in the plasma boundary can be treated. So far, we have studied several cases of the initial equilibrium, which have been chosen rather arbitrarily: $\beta_T = 2.9 \sim 33.7\%$, $q_0 = 0.91 \sim 1.05$, $A \sim 1.4$, and $\chi = 1.6 \sim 2.4$.

Paper IAEA-CN-69/TH3/4 (presented by W. Park)

DISCUSSION

R.D. GILL: Figure 3 shows the q-profile changes following pellet injection. Can you say on what timescale the new q-profile is established?

W. PARK: The q-profile is measured by averaging on the original flux surfaces before the system reaches a 2-D symmetric state, which generally occurs in the resistive timescale.

M.N. ROSENBLUTH: Are you planning to add pellet ablation to the code? Line shorting may happen more quickly with gradual ablation.

W. PARK: I do not know whether this is something we should pursue in great detail at present. It is extremely difficult to be quantitatively accurate in such a complex phenomenon. What helps to keep our current model relevant is the fact that in a large tokamak, like ITER, the ablation will be faster and more local. Even in ASDEX, where the pellet is ablated to several droplets, each droplet contains a mass similar to that of our numerical pellet. Thus, our simulation can be seen as modelling one such droplet.

P.R. THOMAS: One of the most interesting phenomena following pellet injection is the formation of snakes. Does your model permit explanation of this?

W. PARK: Yes. In fact, we think that the reconnection process after a pellet injection can produce such a snake. Even the outboard injection can produce a reconnection process when the pellet cloud shifts outwards. This will produce a plasmoid which is surrounded by its own separate flux surfaces which are not strongly connected toroidally. This could explain the most puzzling phenomenon in snakes, namely that the snake is little affected when a sawtooth crash process passes through it. We plan to study this in more detail.

Paper IAEA-CN-69/TH3/5 (presented by P. Ghendrih)

DISCUSSION

M. TENDLER: How do you close currents in your model?

P. GHENDRIH: We have not addressed currents or electric potential in the present model but these will undoubtedly play a major role in particle transport.

Paper IAEA-CN-69/TH3/6 (presented by S.I. Krasheninnikov)

DISCUSSION

F. PERKINS: In modelling simulation at the ITER divertor, the upstream SOL density is taken to be a fraction (~0.3) of the core plasma density. You emphasize the relation of upstream plasma density to neutral pressure in the private flux region. Which process is more fundamental in setting the upstream SOL plasma density?

S.I. KRASHENINNIKOV: The neutral pressure in the private region and the core plasma density are not independent parameters. They are coupled through the plasma cross field diffusion, neutral penetration and ionization processes. Therefore, they are equally fundamental in setting the upstream SOL plasma density.

Paper IAEA-CN-69/TH3/7 (presented by B. Coppi)

DISCUSSION

M.N. ROSENBLUTH: Have you looked at mode conversion?

B. COPPI: No, we have not solved the mode conversion problem yet. We believe that the actual coupling is likely to be non-linear.

M.N. ROSENBLUTH: Why is poloidal damping different?

L.E. SUGIYAMA: The major differences with other analyses - numerical and theoretical - that do not see this kind of poloidal damping, are that these results contain the full effects of compressional motion and also the variation of the magnetic field. The analysis is not complete, so this remains to be verified.