EIGHTEENTH FUSION ENERGY CONFERENCE

SESSION EX2

Friday, 6 October 2000, at 10:50 a.m.

Chair: S. GÜNTER (Germany)

SESSION EX2: Transport 1 (provided by M. KIKUCHI, Japan)

Paper IAEA-CN77/EX2/1 (presented by L.D. Horton)

DISCUSSION

Ph. GHENDRIH: The pellet injection experiments indicate an increase of density and Greenwald fraction leading to a decrease of confinement while edge density profiles are clamped. Do these results confirm that the main vessel neutral pressure has no impact on confinement and especially confinement degradation at high density?

L.D. HORTON: For pulses with moderate confinement degradation and density (H_{97}~0.8; n/n_G~0.8), HFS pellet and gas fuelling produce the same pedestal density but with higher neutral pressure in the gas fuelled pulses. In this sense, the experiments do support the idea that the confinement does not depend on the main chamber neutral pressure. This is consistent with the comparisons made before and after reducing the conductances between the sub-divertor volume and the main chamber. In these experiments, the main chamber neutral pressure was reduced by ~ $\times$ 2 while the confinement remained unchanged.

A. GROSMAN: Are the mentioned narrow scrape off layers related to smaller connection length for configurations used to derive the values of SOL. lengths (i.e., with separatrix high on the vertical plates)?

L.D. HORTON: The connection lengths are only slightly affected by the shifts in the X-point height used in these experiments. These changes are not responsible for the narrow SOL widths observed.

Paper IAEA-CN77/EX2/2 (presented by F. Ryter)

DISCUSSION

H. BERK: Why does confinement improve with density steepening? Is it due to generating higher pedestal temperature or something else?

F. RYTER: The confinement improvement is due to the fact that the stiff temperature profiles do not change whereas density peaks and increases pressure in the core.

K. IDA: Is the difference between χ_e^{PB} and χ_e^{HP} explained by the non-linear temperature dependence of χ_e? Do you need to add the enhancement of χ_e due to the perturbation in the case of heat pulse χ_e^{HP} to explain the difference?

F. RYTER: The difference between χ_e^{HP} and χ_e^{PB} is due to the fact that χ_e^{HP} is the derivative of χ_e at the working point. For instance a strong ∇T_e dependence of χ_e introduces a large difference between χ_e^{HP} and χ_e^{PB} . This is a well-known feature of the modulation analysis.

R. GOLDSTON: The use of steady ECH heating at different radii, plus modulated ECH is a very nice technique. Have you tried it in ITB plasmas?

F. RYTER: Thank you for appreciating our experiment. We have not applied this to election ITB's. However we applied it to the improved H-mode and did not find electron ITB as expected.

A. ROGISTER: You mention that the temperature profiles are stiff ($\nabla T/T$ = constant) but not the density profile. You claim that this is in agreement with ITG and ETG + streamers. However these instabilities thresholds depend on the ratio of the temperature and density gradient lengths.

F. RYTER: The change in density gradient length is not large, even in the peak cases we mention here, this is without pellets. Its value remains above 5. In general the peaking of density happens in the central region of the plasma where we think the ITG and ETG are marginal or not excited. Therefore we think that there is no contradiction in our interpretations.

Paper IAEA-CN77/EX2/3 (presented by D.C. McDonald)

DISCUSSION

H. ZOHM: In your 1999 experiments, the biggest drop in confinement of high triangularity occurred when you moved from type I to type III ELMs at $n/n_G \approx 0.7$. Do your 2000 results with good confinement up to $n/n_G = 0.9$ also exhibit such a transition?

D.C. McDONALD: No, the highly shaped scenario in 2000 reached $n/n_G \approx 0.9$ without a transition to type III ELMs.

T.C. LUCE: Have you tried to evaluate the data in terms of q instead of I & B to discriminate between the effects of rational surfaces and pure geometry?

D.C. McDONALD: Yes, in the high elongation and high triangularity scenario field and current were varied separately and agreed well with the IPB 98 (y, 2) scaling, despite the fact that q_{95} varied from 2.4 to 3.3 to 4.3.

Paper IAEA-CN77/EX2/4 (presented by M.E. Fenstermacher)

DISCUSSION

J. SNIPES: Your talk indicates that there is a clear degradation of confinement, particularly in the pedestal, above $n/n_G>0.7$, yet Mahdavi's poster yesterday showed that DIII-D can operate quite happily above $n/n_G>1$ with good confinement. Can you explain this discrepancy, please?

M.E. FENSTERMACHER: Degradation of T_{eped} begins when $n_{eped}/n_G>0.75$ or $n_e/n_G>0.9$ for typical profiles. Effects of changes in T_{iped} and $n_e(0)/n_{eped}$ are not examined explicitly here. The direct relationship between confinement and T_{eped} seen at lower density may not hold at higher density. In Mahdavi's paper discharges with pumping showed density that became peaked in the core as the shot progressed leading to high confinement even at some what degraded T_{eped}.

K. LACKNER: A couple of years ago we got the message that confinement degradation at high $n/n_{Greenwald}$ was postponed to higher values of this parameter with increasing triangularity, from JET and ASDEX-upgrade. Your observation of a diminished effect of δ on confinement at higher $n/n_{Greenwald}$ seems apparently in conflict with this. Do you have an idea or a model how this conflict could be reconciled.

M.E. FENSTERMACHER: T_{eped} degradation begins for both triangularities for same n_{eped}/n_G. For fixed profiles this would lead to confinement degradation setting in at the same n_{eped}/n_G. However higher δ has higher confinement when degradation begins and continues to have higher confinement until very high densities ~ $n_{eped}/n_G=1$. So, higher δ still postpones confinement degradation until very high densities.

B. DORLAND: Can you compare these results to C-Mod EDA, which are characterized by high triangularity, up-down asymmetric shapes with interesting ELM behavior?

M.E. FENSTERMACHER: Scrape-off-layer parameters, especially neutral density, are very different in C-Mod and DIII-D due to SOL width to the main chamber wall. Also, the data in this paper are from NBI heated H-modes and EDA H-mode observed only in Ohmic and RF heated discharges on C-Mod. So, despite similarity of δ and dR_{sep} we do not see evidence of EDA H-mode in these experiments.

Paper IAEA-CN77/EX2/5 (presented by E.S. Marmar)

DISCUSSION

V. PARAIL: Now I believe you have all the data to measure heat and particle transport within edge barrier. Did you try to compare transport with, say, neoclassical level?

E.S. MARMAR: The measurements do allow for derivation of particle and energy fluxes. The particle flux profile, inferred from the <EA:ñ> probe measurements, are shown in paper OV2/2 by Hutchinson, et al. Impurity particle transport is detailed in paper EXP/23, by Granetz, et al. Typical thermal diffusivity in the edge barrier is found to be ~ 0.03 m^2/s. Comparisons with neoclassical transport are complicated by the fact that the density and temperature gradient scale lengths (~ 0.002-0.004 m) are approximately the same as the poloidal ion gyroradius (~ 0.005 m), in violation of the usual ordering assumed in the theoretical derivations.

R. GOLDSTON: Back in the early 80's on PDX are found a similar mode, and I think gave it a similar name. We saw that it existed between ELMs, and we could not identify what it was. So my questions are 1) does this mode persist when you have small ELMs? and 2) now that you have such detailed measurements, can you identify what this mode is?

E.S. MARMAR: 1) The QC mode persists through the small ELMs, and does not appear to be affected by them. 2) While we have not yet identified the mode the short wave length, high m character of the mode implies that it is not related to saturated ELM precursors.

D. FRIGIONE: 1) What is R_{DD}: is it the neutron yield or the fusion reaction rate? 2) What is T_i as compared to T_e in the core? 3) What do you mean by moderate q?

E.S. MARMAR: 1) R_{DD} is the fusion reaction rate. The maximum neutron rate observed to date during EDA H-Mode on Alcator C-Mod is 1.8^{14} neutrons per second, corresponding to R_{DD} of 3.6^{14} D-D fusion reactions per second. The volume of the plasma, inside the last closed flux surface, was 0.92 m^3. 2) T_i is equal to T_e in the core of these plasmas, to within the uncertainties of the measurements. This is to be expected, as under these high density conditions, the electron-ion equilibration time is much shorter than the energy transport times. 3) The observed limit for q, to realize EDA H-mode, is q=3.5.

Paper IAEA-CN77/EX2/6 (presented by F.G. Rimini)

DISCUSSION

I.H. HUTCHINSON: You seem to have clear data on the height of the temperature pedestal but your final conclusion returns to the presumption made in earlier JET analysis that pressure gradient is fixed and so the pedestal width is proportional to its height. Our results on C-Mod don't show that. Therefore I ask, have you direct measurements of the pedestal width to support your comments about its scaling?

F.G. RIMINI: No, we only have limited measurements within the barrier region. But there are plans to repeat some of these experiments with a fuller complement of diagnostics, e.g. Li Beam for density pedestal measurement.

M. KIKUCHI: I think this is a very good experiment which agrees with JT-60 experimental results (δ_{edge}~ρ_{pi}). As for the fast ion gyroradius discussion, you mentioned the threshold of fast ion population n_f/n ~ 1%. Does this estimation include charge exchange loss? Next question is that a T_i scan and B_p scan is needed to confirm $\rho_{p,thermal}$ scaling. Did you made such a scan?

F.G. RIMINI: 1) I think the threshold value of ~ 1% includes the charge exchange loss; details of such a model can be found in V. Parail's 1999 Nuclear Fusion Paper. 2) The ρ_{therm} scaling is inferred from comparison of this set of data with other JET experiments without gas fuelling (=>T_i scan) and at different values of the plasma current (B_p scan).

R. GOLDSTON: I am surprised that you say the fast ion density does not include CX losses. The TRANSP analysis should give you a calculation including CX losses and orbit losses near the edge.

F.G. RIMINI: The estimate of n_{fast} has been taken not from TRANSP but from a smaller code which has been developed for JET data. I agree that TRANSP can include CX losses, but in this range of gas fuelling rates there are questions on the accuracy of the TRANSP estimate (permeability to neutrals).

Paper IAEA-CN77/EX2/7 (presented by Y. Miura)

DISCUSSION

R. WEYNANTS: My question is concerned with the criterion on dE_r/dr to sustain the transport barrier. We have recently proposed an expression for the critical dE_r/dr based on an experimental comparison between CHS, TEXTOR and DIII-D and on simple theoretical considerations which yield in your case maybe about 0.6 (in your units). The question which however arises is: how do you experimentally define the critical value of dE_r/dr?

Y. MIURA: Since the H-mode transition occurs gradually step by step with sawtooth crashed in the case of P_{in}~P_{th}, then the value of $(1.20\pm0.4)\times10^3$ kV/m^2 is evaluated in the intermediate state of the L-H transition. It is a step change; therefore, we can not say a clear critical value, but can conclude that the critical dE_r/dr is LESS THAN $(1.20\pm0.4)\times10^3$ kV/m^2.

F. WAGNER: You showed that the H-transition is induced by a sawtooth when the heat wave arrives at the edge. How much of the observed change in electric field is due to the increase in pressure gradient induced by the thermal wave?

Y. MIURA: Since we do not measure the change of the pressure gradient at the edge by the sawooth heat wave, then we could not clearly say about its effect on the change of the potential at the edge. However, there is some sawtooth activity before the L-H transition. If the heat wave by a sawtooth does not trigger the H-mode, a large change of the potential is not observed. Therefore, we think the large pressure gradient that contributed to the rapid potential change might not be formed by the sawtooth heat wave.

A. ROGISTER: In your experiment, the radial electric field is minimum at the position of the last closed magnetic surface. What is the precision on the position of the latter?

Y. MIURA: Since there is a little ambiguity in the estimation of the position of the sample volume, the possible position of the separatrix (ds=0) is in the region labeled "separatrix" in the figure. As shown in the figure, we think that the error is about ±5 mm.