

Review of: “Helical modulation of the electrostatic potential due to magnetic islands in toroidal plasma confinement devices”

General comments:

The biggest problem with this paper is that it is trying to make generalizations that aren't proven and that from the limited information in this paper, it is impossible for a reader to deduce how the experiments and the modeling were conducted. The authors need to strongly reduce the over reaching claims and add a real discussion section that discusses the limitations of the presented work and how it compares with previous work. For example, it is unclear whether ORBIT can handle transport of non-energetic particles and how well it compares to existing neoclassical simulations. Moreover, these are all L-mode plasmas in TEXTOR, where turbulent transport typically dominates over any neoclassical change. So one might question independent of the matching results, whether this physics associated with neoclassical transport should have such a strong effect. Moreover, previous results in TEXTOR with regards to 3D helical deformations of the plasma edge have shown strong changes in turbulent behavior (Y. Xu et al. NF, 47 1696 (2007) Y. Xu et al. PRL 97 165003 (2006) Y. Xu et al. NF 49 035005 (2009)).

Specific comments:

1. The title is misleading. From the title I am assuming that the results in the paper will discuss observations made in multiple toroidal plasma confinement devices. However, all the results are from one single device, a tokamak called TEXTOR. I would strongly suggest that the authors review the title of this paper to something more appropriate that reflects the results being discussed, such as:

“Helical modulation of the electrostatic potential due to magnetic islands in TEXTOR”

2. The abstract does not mention TEXTOR, nor the type of plasmas in which these effects were observed. Considering that the magnetic field topology depends strongly on plasma conditions, this is a serious omission. I would recommend that the authors think carefully of what work was done experimentally and present this as clearly as possible. Then do the same for the simulations. The conclusion that “convective cells are a major radial particle transport driver is generic to 3D plasma boundary layers in toroidal magnetic confinement devices” is a strong exaggeration based on the limited data presented in this paper. It suggests that all the changes in particle transport can be explained through the existence of convective cells. However, 3D modeling has shown that convective cells, although a possible driver for transport, disappear in most regimes in which 3D magnetic fields are playing a role for ELM suppression. It is still an open debate whether one can extrapolate the results found on TEXTOR in a limited, slow rotating L-mode plasma (low beta) to a diverted highly rotating H-mode (high beta) plasma. I would strongly suggest the authors to take a look at the data presented in this paper from an outsider perspective and then write the results up again.

3. The first paragraph of the introduction does not always make much sense. This is due to non-english sentence structures (please read sentence 2 carefully and you will see it does not make any sense). Also you mention spontaneous self-organization (first sentence) and then never talk about turbulence again. Although appreciate the authors trying to making a connection to plasma physics problems, I feel considering the audience of NF, that they would be better suited of talking about the role of 3D fields in magnetic confinement devices and sticking to references for such devices.

4. So reference 3 and 4 are directly related to changing the pressure profiles to stabilize ELMs. Why

not just call a horse a horse. There are plenty of other ways to stabilize a plasma (pick your favorite core mode literature, ECCD, rotation, etc ...). So let's stick to RMPs. Doing so, one should then use the reference 3 for DIII-D, add a reference for MAST, one for AUG, one for KSTAR, one for NSTX and one for EAST and then at least one for TEXTOR, even if no ELMs are suppressed that discusses particle transport (reference 4 is not relevant, since it discusses heat transport, not particle transport and is not the standard reference for ELM suppression on DIII-D).

5. The sentence that start with "In the edge of all fusion devices ..." is a very strong statement to make. I don't think every fusion devices has studied RMPs. So how about naming the devices by name that have observed a 3D boundary deformation when RMPs are applied. There are some excellent overview papers on 3D deformation through boundary perturbations as well as core modes by (Chapman, I. T., et al. NF 54 (2014) 083006, Chapman, I. T., et al. NF 54 (2014) 083007). There is even older data from TEXTOR on the edge topology and how it connects to experimental observations by Jakubowski et al.. Since the radial electric field depends strongly on the diamagnetic component, which all these papers show is modulated along with the 3D perturbed edge, it should not come as a surprise that E_r is modulated as well. Also the symmetry is not with the dominant island, it is a symmetry along with the dominant mode close to the plasma edge. In other devices, which do not have limiter geometries, islands have not been shown yet to exist. Moreover, the Stochus reference actually highlights how plasma rotation can completely change the results. At no point is this limitation discussed in this paper.

Also the work by P. Tamain et al. on MAST on the changes in E_r , shows that changes are independent of the phase of the applied perturbation. So in a diverted L-mode plasma, this 3D deformation was not observed in the radial electric field.

6. First sentence of the second paragraph claims that certain things have been shown, with no reference. Please add references

7. Paragraph boils down to: we have done this before on RFX with good results. At this point, it would be good to let the reader know that you are going to do the same thing on TEXTOR results and that this is the reason why you mention this result. Maybe move this paragraph to later in the introduction, when it makes more sense.

8. In the 4th paragraph you mention a convective cell as potential mechanism to explain confinement changes in confinement. It would probably be wise to include recent modeling results from Nardon et al. and Izzo et al.. Both papers discuss the possibility of convective cells to explain the changes in confinement in strongly rotating, high pressure plasmas.

9. Second page, 1st paragraph. You mention the experiment uses a $m/n=3/1$ but a little further this is suddenly a $4/1$ edge island topology. This cannot be correct. I hope this is only a typo. In fact the paper switches constantly between $4/1$ and $3/1$. This is beyond sloppy !!!!! It makes me question the scientific rigor with which this work was done.

10. Why do you use EMC3-EIRENE input on the temperatures and densities. Were there no measurements available? At this point EMC3-EIRENE does not yet reproduce the experimental results with RMPs, let alone without RMPs.

11. Why is figure 2 from reference 13, which an RFX result. Can't and shouldn't you calculated D_e and D_i for this TEXTOR case? I would strongly recommend making this paper about TEXTOR and TEXTOR only. So calculate the D_e and D_i for this TEXTOR case and then discuss how it compares

with RFX.

12. Figure 3, what is the resolution of the probe measurements? How many data points along the x-axis and how many data points along the y-axis. What are the error bars? Also, discuss why on the inside the probe does not seem to observe the island. The probe only observes the outside deformation.

13. A better way to compare figure 3 and 4 would be to plot the difference between both figures. It would highlight the areas of better and worse agreement and might help uncover physics.

14. Page 6, below the figure in the list of papers that discuss the changes in E_r as a function of the creation of a stochastic field. Please add the work theoretical work from Kaveeva et al., the experimental results from MAST (Tamain et al.) and the existing reference 10, which discusses this feature in great detail. Also, don't forget the existing Coenen reference and all previously mentioned Xu references. AUG also recently published results on this topic. So please revisit the literature to update your reference list.

15. The electron and ion root work is interesting, but feels disconnected from the rest of the work presented in this paper. If the authors want to keep this as a short letter, I would recommend to make this a separate paper and to use the free space to write an actual discussion. Transport in a tokamak is more than particle drifts and the authors should expand on how other transport channels could affect their results. For example, how can this model explain improved confinement observed in Coenen et al.? Moreover, the authors should make very clear that these results can not be extrapolated to a high power fast rotating H-mode plasma, due to clear changes in plasma response. It isn't even clear if the results from a limiter geometry can be simply extrapolated to the results in a diverted geometry with one or multiple X-points.