Point-to-point answer to the Referees' questions

Referee #1

1.

I believe that this work is based on the TEXTOR experiments performed by O. Schmitz and T.E. Evans. I did not find any information about the discharge numbers, but based on the description of the experiment, and on the information in Evans' TTF-2013 talk, I believe that the authors should consider including T.E. Evans as a co-author, or at least acknowledge his contributions in the Letter. Also, please, add discharge information on the figures or in the text.

We agree on this points, and T.E. Evans will be included as co-author, since he originally stimulated the measurement, and discussion on several aspects of the paper.

2.

The paper resents experimental and simulation results from TEXTOR. At the same time, a large portion of the introduction section is devoted to RFX. This distracts the reader by creating a false impression that the paper is focused on the RFX results and simulations. Please, correct this obvious misbalance.

To avoid any misunderstanding, the part on RFX of the Introduction has been reduced.

3.

(p. 2) The authors mix up the concepts of the topological islands and tearing islands. The rest of the paper talks only about field line tracing, Poincare plots and topological islands. There is no reconnection of magnetic field lines in this case. The explanation with reference [3] is totally misleading.

Reference [3] to resistive-tearing modes and reconnection has been cut.

4.

(p. 2) The statement that “it has been observed that magnetic islands modulate the plasma pressure profile” is inaccurate. Reference [7] only discusses the modulation of the flux surfaces and the plasma boundary by the external magnetic perturbation, and does not discuss plasma pressure profiles. I am really questioning if references [6,8,9] involve any discussion of this kind. In reality, it is a very difficult if not an impossible question whether any flattening of the plasma pressure profile can be related to a magnetic island, or simply to a stochastic region.

Actually, the statement was too shallow. It has been corrected. We pointed to present some results from Tokamak, Stellarator and RFP which show that kinetic properties of the plasma, in presence of 3D fields and magnetic chaos in the edge, show macroscopic modulations coherently with the symmetry of the dominant magnetic island. In particular

- electron density and temperature in [R. Moyer, 2012] and [H. Stoschus, 2012]

- electron pressure in [N. Vianello, 2013]

- connection length in [Y. Feng, 2011].

5.

(p. 2) I believe, that the experiments were performed in the L-mode circular (wall-limited?) plasmas. Please, give more details of the experimental set up to make sure that the reader understands what to expect from these plasmas.

Yes, experiments were performed in the L-mode wall limited circular plasmas (as it was the standard in TEXTOR discharges, without impurity seeding). Details on the experimental setup have been added.

6.

(pp. 2-3) The authors claiming to be the first to present evidence that a magnetic island in the plasma edge can act as a convective cell and generate enhanced radial transport, are probably unaware of the following works:

- S. Takamura et al., Phys Fluids 30 (1987) p.144
- S.C. MacCool et al., Nucl. Fusion 30 (1990) p. 167
- T.E. Evans et al., 14th EPS Madrid, 1987, Vol 11D, p.770

Please, cite these works, and correct the claim accordingly.

We agree and corrected our statement in the revised manuscript by pointing to previous results [refs. to Takamura, McCool, Evans, and Ida Nucl. Fusion 2004] on the formation of island convective cells due to ExB flows around magnetic islands. We believe anyway that our paper shows for the first time a full, self-consistent calculation of an ambipolar potential due to differential drifts of ions and electrons, in a fixed magnetic topology produced with RMPs.

In this sense, as stated in the text, even if the experimental observation of convective cells is certainly not new, and it is not new the algebraic way of determining ambipolarity in the stellarator community, it is absolutely new the combination of these two concepts in order to account for the radial electric fields responsible for these flows.

7.

(p. 3) What kind of simulation was done for Poincare plots in GOURDON? I believe that this is a pure vacuum simulation. If so, this needs to be stated. Was there an attempt to do a plasma response simulation (linear or non-linear)? How does that change the results? Please, include these results and the discussion in the Letter.

No, there was no attempt to do a plasma response simulation. The magnetic field topology described by both Orbit and GOURDON are done in vacuum approximation. When we talk of “plasma response” we mean that the magnetic topology is kept fixed, and we analyze particle trajectories (electrons and ions) and their mutual interaction to produce an ambipolar potential.

8.

(p.3 fig.1) Please, be consistent for the radial axis on the figures and use either poloidal flux, or normalized radius.

We modified the picture accordingly.

9.

(p.3 fig.1) Is this the same Poincare plot data that has already been shown in Fig. 5(a,b) in Spizzo et al., Phys Plasmas 2014?

Fig.5(a,b) in Spizzo et al., Phys Plasmas 2014 shows the Poincare plot for the same TEXTOR run at a toroidal angle = 0° (LFS). Fig.1 differs in the toroidal angle cut (180°), at the HFS, where the plasma potential measurements were performed. Moreover, the technique used to determine L|| has been slightly modified (now details of the ergodic fingers and laminar flux tubes are clearer).

10.

(p.3) Please, justify the choice for these ion and electron temperatures, as well as the plasma density.

The energy of the test particles, temperature and background density are chosen to reproduce collisionality in the experimental conditions. Profiles are reconstructed through the transport code EMC3-Eirene in unperturbed conditions (i.e., no RMP applied).

11.

(p.3) Why 30 cycles were performed? Was there a study on the effect of the number of cycles on the solution. Please, justify your choice.

The number of cycles has been chosen large enough to ensure locally a steady-state distribution of test-particles, as mentioned in the paper. For more details, check on the paper by Spizzo, White and Cappello [PPCF 51, 124026 (2009)] where the method was originally developed.

12.

(p.4 fig. 2) This figure has already been published in Spizzo et al., Phys. Plasmas 2014 (fig.6). Adding symbols to lines and changing the scale on the left axis does not make it a new figure. Please, justify why the figure needs to be repeated. If it is necessary, add a reference to the published paper (adapted from).

Fig.2 slightly differs from fig.6 in Spizzo et al., Phys Plasmas 2014 since some values of De have been corrected (even if the overall result does not change). However, we agree that a reference to Spizzo et al., Phys Plasmas 2014 is appropriate.

13.

(p.5 and p.6) I believe that the separatrix can not be defined in these TEXTOR circular wall limited plasmas, so this is a poor choice of the terminology in the paper. Please, explain what exactly is meant by the “separatrix”. Last closed flux surface? Also, since this surface is mentioned in context of the figures (3 and 4), please, indicate that surface on the figures.

We agree, “last closed flux surface” is the more appropriate terminology.

14.

(p.5 fig.3 and p.6 fig.4) Why are the black dots on the figure form a structure that is inclined to the right (increasing in theta with increasing radius). It seems that all structures on the Poincare plot in figure 1 are oriented the other way. Or is it one of the remnant island highlighted in purple? Please, clarify.

Yes, in Figs.3-4 it is shown the the remnant island highlighted in purple in fig.1 in the poloidal interval [0, ~0.88] rad. This will be clearer by changing the radial axis in fig.1 consistently with Figs.3-4 as suggested above.

15.

(p.5) Can the radial electric field be calculated directly from the plasma potential data as dphi/dr? If yes, what does it look like? If no, why?

We agree that this point was not clear in the original version of the paper. In Fig.3 we added the contour plot of the *modeled* potential, where the correspondence between model and measurements is evident. In this sense, also the modeled and measured Er will be the same, so that Figure 4 answers to the Referee’s question.

16.

(p.7) Using ECCD/ECRH for stability control is not a novel idea, and has been circulating around. The main difficulty seems to be in the amount of power needed to make any effect. Please, discuss these limitations, especially in regards to the RMP ELM control.

A complete answer to this point will be available, when we will perform simulations with the full-profile, energy scattering operator based on the Boozer-

Kuo approach [Boozer & Kuo Petravic, Physics of Fluids **24**, 851-859 (1981)]. Presently, to answer to this point on the use of ECRH/ICRH, we added a picture (Fig.6) with a scan of De/Di at the OP and XP as a function of Te/Ti. It shows that, if Te/Ti<0.5, De/Di<1, which implies that ion heating would flip the system from the electron root (stable in the TEXTOR experimental conditions analyzed) to the ion-root, with the potential well at the OP instead than at the XP.

Referee #2

COMMENTS TO THE AUTHOR(S)

This paper describes the electric field around magnetic islands by resonant magnetic perturbations. This is a hot topic, and is of interest to community. However, for me, it is not clear what is newly found in this paper, which justifies the accelerated publication. More specifically, I have comments on the following points.

[1] Concerning particle transport near magnetic islands and ‘root’, very similar things are already discussed by the authors in Ref. 14.

The similarity with Ref.14, Spizzo, et al., is only apparent: in Spizzo, et al., an initial attempt to determine a potential was based on a *phase* scan at fixed amplitude, which gives a *rough* idea of what the ambipolar potential looks like. In this paper, we apply the complete determination, both phase and amplitude. In particular, the amplitude scan (Figs.3,4,5) allows for the determination of the stability of the ambipolar roots, in a way all similar to stellarators [see Hastings, NF 1985]. This opens up the world of the energy dependence of the potential: to underline the difference with Spizzo et al., we added a sensitivity scan as a function of Te/Ti. This provides also an idea of the amplitude of the ECRH/ICRH needed to make the solution flip from the “electron” to the “ion” root.

[2] The code ORBIT calculates ion and electron fluxes. Then, the authors try to obtain the radial electric field, considering the ambipolar condition. This method is already well known in the stellarator community. It is not very hard to consider that the same method is applicable to tokamaks with symmetry breaking perturbations. Actually, Callen (2011) Nucl. Fusion already mention this point. In addition, the method in this paper relies on the ambipolar condition, and the electric field perturbation by plasma fluids is not self-consistently taken into account.

We were not aware that the method was mentioned as a proof-of-principle in Callen 2011. We added a sentence mentioning that paper, but nevertheless we stressed also that our paper presents for the first time a full calculation of the ambipolar response in presence of a symmetry-breaking perturbation.

[3] Generation of convective cells around magnetic islands are well known in both experiments (Ref. 26, for example) and two fluid simulations (in the context of the drift-tearing mode). In addition, data in Fig. 3 possibly involve higher harmonics of the tearing mode and other instabilities.

We agree, actually the convective cell concept was already discussed in Refs. [Takamura Phys. Fluids 1987, McCool Nucl. Fusion 1990, Evans 14th EPS Madrid, 1987, and Ida Nucl. Fusion 2004], as pointed out also by the 1st referee. We corrected our statement in the revised manuscript. We stress that we actually show for the first time in a tokamak that ambipolar potentials due to differential drifts of ions and electrons in islands can account for the radial electric fields responsible for these flows.