

## ***Interactive comment on “On the convection of ionospheric density features” by John D. de Boer et al.***

**Anonymous Referee #2**

Received and published: 9 September 2018

### General Comments

This paper addresses an important question in the high-latitude ionosphere, namely the convection of, and within, plasma density enhancements in the high-latitude ionosphere. There are some simplifications within the model that make the solution physically unrealistic, however I have no objections to these simplifications, provided that the work presented can help explain observations (see comments below). The work described in this paper is a necessary step towards a more realistic model. They are a sensible advance upon those used in previous publications and are likely to influence the development of more sophisticated models. The work is therefore likely to be of interest to researchers in the field and is, potentially, suitable for publication in *Annales Geophysicae*.

C1

However, before I can recommend full publication, there are some changes that I believe would enhance this manuscript.

### Specific Comments: Minor Changes

Section 1, line 22 (and elsewhere). The authors use the term “polar oval”. Other authors have used ‘polar cap’, ‘auroral oval’ and ‘polar region’, but ‘polar oval’ is not one that I have encountered previously. Please could you define / change this term as appropriate?

Section 1.1, line 9 & section 4, line 24. The steepness of the boundaries modelled is unclear. In section 4, line 24 the authors say that they are studying “sharp, step boundaries”, please add the word ‘sharp’ to section 1.1. Please also add typical values inside and outside the boundary, to allow for comparisons with other studies (see later comment). What is the basis for choosing these values? How do the densities modelled relate to the observations presented in the literature?

Section 3.1 (and elsewhere). Modelling an E-region patch. Polar cap patches are primarily an F-region phenomena. I am confused as to why the authors have modelled a patch in the E-region. Please would you either explain why the E-region has been used (with reference to observational studies in the published literature) or change the arguments to the F-region. Given the assumptions in the model, such a change should be a relatively minor alteration and I don’t see that it would significantly affect the results obtained or the conclusions drawn.

Section 4.1. I found this section very confusing, and needed to re-read this several times. A diagram would significantly help with the clarity.

### Specific Comments: Major Changes

The authors have assumed that the Earth’s magnetic field,  $B$ , and the  $z$ -axis that they have defined are parallel (see, for example, Fig. 2), however this is only true at the geomagnetic pole. Given the other simplifications in this study, I’m not too worried

C2

about the authors making this assumption, but you do need to discuss the effect of this assumption upon your results.

My largest objections to this paper at present is that there is not enough done to relate these results to other modelling & observational studies. Schunk & Sojka (1987) modelled a plasma density structure with a sharp boundary convecting over the polar cap and into the auroral oval. Please discuss how your work relates to, and builds on, this study. Moen et al. (2008) observed that the distribution of polar cap patches around magnetic midnight was asymmetric. Can the arguments which you have presented in section 3.5 explain this asymmetry? Oksavik et al. (2010) observed the rotation of a polar cap patch. Can the work which you have presented in section 3.4 explain this rotation? Numerous authors have presented theoretical or observational results that show small scale structures growing within polar cap patches, and have discussed why happens. I would suggest referring back to a selection of these results, to discuss how your paper relates to / advances our current understanding.

If your paper can explain observations, particularly those of Moen et al. (2008) and/or Oksavik et al. (2010), then this helps to justify the assumptions you have made. The model may not be physically realistic, but can be a step in the right direction towards a more realistic model. Such a discussion would show how your work would build upon, and advance, our knowledge of this field.

I would like to state that I am not an author on any of the publications mentioned below.

#### Technical Comments

Section 4.1, line 13. The authors use the term 'FAC's'. Do you mean 'FACs'?

Writing style: Overall the writing style is good. The article is well-structured and clear. I do, however, have a dislike of sentences starting with the word 'And' (see section 1.2, line 26 and elsewhere). This is just my personal view, but I think the paper would read better if these were rephrased (please ignore this comment if you wish).

C3

#### References

Moen, J., N. Gulbrandsen, D. A. Lorentzen, and H. C. Carlson (2007), On the MLT distribution of F region polar cap patches at night, *Geophys. Res. Lett.*, 34, L14113, doi:10.1029/2007GL029632.

Oksavik, K., V. L. Barth, J. Moen, and M. Lester (2010), On the entry and transit of high-density plasma across the polar cap, *J. Geophys. Res.*, 115, A12308, doi:10.1029/2010JA015817.

Schunk, R. W., and J. J. Sojka (1987), A theoretical study of the lifetime and transport of large ionospheric density structures, *J. Geophys. Res.*, 92, 12,343–12,351, doi:10.1029/JA092iA11p12343.

---

Interactive comment on *Ann. Geophys. Discuss.*, <https://doi.org/10.5194/angeo-2018-13>, 2018.

C4