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

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The authors present an analytic solution of a model for convecting ionospheric density structures. They make the point that the drift of the structures depends on the features of the structure, whether depletion or enhancement, and on how strong these are. And this could be relevant for a better understanding real world density structures in the Earth's ionosphere. The manuscript is very well written, I could follow the argumentations easily and enjoyed reading the paper. Proper credit is given to an earlier published similar result, though this was in quite a different context.

There would be quite a few objections, that the real ionosphere is more complicated and things are happening there differently than seen with the simplifying model. But

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Discussion paper

this would not be fair, the point is to develop an elegant model of convecting density structures and relate only a limited aspect of the result to the real ionosphere. However, even when taking this as a sort of Gedankenexperiment, we need to ask: is the model is physically consistent? I'm afraid, that I found a little bit of a spoiler there. The problem can be seen already with the slab model, Figure 1 and correspondig description in the text.

But first, in the E region recombination is effective, and a boundary with a density jump would have to be maintained by a corresponding jump in the production rate of ion-electron pairs. Equations 2 and 3 are only valid in the absence of production and recombination. Taking these into account can change the picture completely: the drift of a density structure would rather simply map the variation of the production rate in space and time (for example as produced by particle precipitation). However, I had promised not to object on grounds like "in the real world things are different". I and hopefully also the reader are willing to follow the authors: in the idealized E region of the model world recombination is switched off. Then a mass transport across the slab boundary needs to occur as described in the manuscript.

But then my alarm bells ring!: how about Newton, the conservation of momentum flux across the boundary? The jump of E across the boundary, required by Ohm's law, implies that the ExB component of the ion drift, the tangential v_t, also jumps, and any tangential acceleration experienced by crossing ions would need to be balanced by some force. The force could be magnetic stress. Then the from the magnetosphere well known jump condition is:

$$\rho_0 v_{n,0} v_{t,0} - B_{t,0} B_{n,0}/\mu_0 = \rho_1 v_{n,1} v_{t,1} - B_{t,1} B_{n,1}/\mu_0$$

(0 and 1 indicating the two sides of the boundary, n for normal direction, t for tangential).

Without magnetic stress, when $B_n = 0$, we have the jump condition of ordinary hydrodynamics

$$\rho_0 v_{n,0} v_{t,0} = \rho_1 v_{n,1} v_{t,1}$$

In the manuscript the first jump condition, coming from the conservation of particle numbers, is used, but this second jump condition, coming from the conservation of momentum is ignored.

One can easily derive, taking into account equation 4 with $v_b = 0$, that v_t cannot jump, $v_{t,0} = v_{t,1}$, if there is transport across the boundary, i.e. v_n is different from zero. But $v_{t,0} = v_{t,1}$ would violate Ohm's law and current continuity. Or, allowing v_t (and E_n) to jump, there cannot be mass transport across, v_n must be zero.

Therefore, I'm afraid, that the model in its present form violates Newtons law, the conservation of momentum. Obviously this is the case not only for the slab, but also for the circular density structure with its complicated ion drift in and near the structure. By disallowing FACs, the model has no forces that could accomplish the derived pattern of particle motion.

Allowing for FACs and their closure would generate magnetic stress B_n. This can then produce a model that would be consistent with respect conservation of both mass and momentum, current continuity and Ohm's law. Whether such a model will then, at least qualitatively, still result in the convection of density structures as obtained in the manuscript, is not clear to me. It seems unlikely that an analytical treatment for the circular structure would be possible, but for the simple slab a fully consistent analytic solution (with non-zero B_n) should be achievable.

My objection of not conserving momentum does not disprove the conclusion of the authors about the convection of the density structure, but it questions whether the toilsome derivation in the manuscript really supports these conclusions. My objection is relatively fundamental, and I would insist that the problem needs to be admitted and discussed in a publication.

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