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Comment on gi-2022-12

Alessio Pignalberi (Referee)

Referee comment on "Daedalus Ionospheric Profile Continuation (DIPCont)" by Joachim Vogt et al., Geosci. Instrum. Method. Data Syst. Discuss.,
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The manuscript "Daedalus Ionospheric Profile Continuation (DIPCont)" by Vogt et al. describes the DIPCont project who supports the Daedalus mission proposal to ESA. Specifically, DIPCont investigates how in situ measurements collected by satellites in the lower thermosphere and ionosphere (LTI) region (100-200 km of altitude) can be extrapolated to the lower boundary of this region at about 100 km of altitude. The final goal is obtaining vertical profiles of the Pedersen conductivity based on the knowledge of several physical quantities measured by the satellite. The paper describes the analytical models used in this derivation and, by means of synthetic measurements and Monte Carlo simulations, analyzes which are the uncertainties in the extrapolation of the profiles below the satellite location, given the mission requirements and objectives.

This kind of study is of utmost importance to assess if the Daedalus mission could meet the scientific objectives, and in driving some choices about the satellites' orbit configuration and hardware. This is particularly important for the Daedalus mission because the LTI region is very challenging for satellites.

Overall, the paper is well written and organized. The mathematical part is rigorous and well explained. Python codes and Jupyter notebooks in the supplementary material add value to the manuscript. I did not find any major flaws in the manuscript that could hinder its publication. Below a list of minor comments and suggestions which could improve the paper and stimulate future developments.

Minor comments:

- In the manuscript I did not find any clear information about the magnetic latitudes the

mission is going to cover, or better, which are the latitudes for which the calculations developed here are valid. From the figures shown in the manuscript and from the discussion, I suppose that the main goal is the polar/auroral latitudes where the Pedersen conductivity is of utmost importance at LTI altitudes, but this is not clearly stated in the manuscript. If so, this should be clearly stated in the introduction.

- I wonder if the Daedalus orbit configuration will make possible to get data at low latitudes, and also to estimate the Hall conductivity.
- Line 57: About "*and disregarding the contribution from electron-neutral collisions*", please provide a reference to support this hypothesis or, alternatively, provide a numerical example.
- Line 87: About "*Disregarding altitude changes of atmospheric composition*", I wonder how much the hypothesis of disregarding altitude changes of atmospheric composition could impact on the derivation of the neutral scale height vertical gradient. In fact, as also the authors explained before, in the LTI the atmosphere is not uniform in composition and every constituent obeys to its own barometric law. The hypothesis made here seems to be in contrast with what has been said before. To substantiate your working hypothesis, I would suggest to verify the range of its applicability through the NRLMSISE-00 model.
- Line 152: About "*For simplicity, the ion gyrofrequency is set to a constant.*", I suppose constant with the respect to the altitudinal variation once the location is set, isn't it?
- Lines 304-306: About "*electron density makes the main contribution to the peaked height variation of Pedersen conductivity...*", this is true but, to convince a skeptical reader about this, I would present also the plots for the neutral density, ion temperature and ion-collision frequency for the case shown in Figures 4-6. It is enough to show vertical profiles like Figure 5. These plots would also make clearer the altitudinal variations of these parameters as defined by the equations derived in the paper, and could be useful for the discussion of the results.
- Lines 306-307: About "*Pedersen conductivity controls the height variation of Joule heating*", I would show the analytical dependence between these two parameters. Adding another equation to the paper should not be a problem given the number of equations already present.
- Lines 317-317: About "*In Figure 1 and in the following, latitudinal inhomogeneity of electron density...*", is the crossing of the auroral oval taken just as an example or will be constrained by the orbit configuration?
- Lines 371-372: About "*The DIPCont package contains a parameter to study the effect of F-layer residuals on...*", probably, the dayside F1 layer might slightly affect the electron density in the range 150-200 km of altitude, above all in the summer season. This is a point to check in future as a function of the perigee altitude.
- Lines 410-414: In my opinion, this part is not very clear as it is written. Indeed, the derivation of (A4) on the base of (A3) is based on the fact that $\ln N_n = -dz/H_n^N$ which in turn leads to (A5). As a consequence, in my view, is the adoption of $\ln N_n = -dz/H_n^N$ who leads to (A4) and not vice versa. I am not questioning the correctness of this part but only the way in which it is presented. Moreover, it should be make clearer the difference between the pressure scale height and the density scale height.
- Line 436: About "*In the isothermal limit...*", as a consequence, H^N tells us how the scale height H^P changes for a non-isothermal atmosphere. This will solve my previous comment regarding the relation between H^N and H^P , and should be put in evidence in the text.
- Line 438: About "*Following the approach first presented by Chapman (1931),*", Your derivation is based on the assumption of a single atmospheric constituent, like in the Chapman original derivation. Have you verified the reliability of this assumption in the LTI region and in the formation of the E layer? I suppose that the E layer should be the superposition of Chapman-like layers from O₂⁺, N₂⁺ and NO⁺ ions. This point should be at least discussed.
- Appendix B: From the equations in Appendix B, I suppose that the z axis has been

taken increasing towards the ground. Otherwise, the minus sign should appear in (B1) and in the following equations in the exponential. In my view, this choice is not the best one because it does not make clear that the radiation is absorbed by neutral particles through the radiation path. Anyway, the direction of the z axis should be clearly stated in the text.

Suggestions:

- Line 84: Suggestion about the use of P for the scale height. Many people working in the ionosphere field could confuse it with the plasma scale height because of the presence of P.
- Line 366: controlling --> controlling
- Line 367: the the --> the
- Line 442: precipitaion --> precipitation
- Eq. (B7) is just a repetition of Eq. (A15), it is not necessary to repeat it.
- Line 508: aopgee --> apogee

I recommend acceptance after minor revision.

Best regards,

Alessio Pignalberi