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Comment on gi-2022-12

Anonymous Referee #2

Referee comment on "Daedalus Ionospheric Profile Continuation (DIPCont): Monte Carlo studies assessing the quality of in situ measurement extrapolation" by Joachim Vogt et al., Geosci. Instrum. Method. Data Syst. Discuss., https://doi.org/10.5194/gi-2022-12-RC2, 2022

Comments on: "Daedalus Ionospheric Profile Continuation (DIPCont)," by Vogt et al.

This paper presents simulated calculations of altitude profiles of various ionospheric parameters that would be measured by in situ instruments on a low perigee, orbiting platform such as the proposed Daedalus satellite. The paper presents a new perspective on how vertical profiles might be obtained with such satellite measurements and also discusses measurements from two such low perigee satellites in the same plane with somewhat different perigees.

This reviewer found several aspects of the paper difficult to follow. Accordingly, a number of important comments are provided below that the authors are asked to consider prior to publication in *Geospace Instrumentation*.

- Paper organization not clear

The paper's purpose and organization should be reviewed and clarified in the Introduction. What are the main goals of the paper? Is the main objective to show how in situ measurements would ultimately provide ionospheric profiles? A natural question is how many such measurements are needed to obtain a realistic profile. Again, the overall objectives of the study need to be clarified.

It appears as if the authors are considering mid-latitude daytime conditions. If so, this should be stated.

High latitude conditions with auroral input would completely change the approach of this paper, since the ionospheric plasma density is highly variable due to precipitating, energetic (auroral) particles. (See Figure 43 of Pfaff et al., Space Science Reviews, 2012, for an illustration of how the thermal plasma might vary depending on the incoming auroral electron precipitation.) The Daedalus objectives suggest that high latitudes are a key region that that mission seeks to understand.

- Challenges with dual satellite investigation data

The use of two satellites to gather the profile data is a little difficult to follow. Because the satellites have different perigees, their orbital periods would be different. It is hard to believe that two satellites would gather data exactly simultaneously, as shown in numerous figures. How would the results differ if the two orbits were not synchronous or not in the same plane?

- Ions and other parameters not specified

The analysis discusses ion-neutral collisions, but the paper does not specify which ion species are used and which are the most common within the 100-200 km regime. The collision cross section value is given on page 7, so the lack of ion species specification is confusing.

Page 4, equation (1): only one ion and one neutral species are considered in the model. Clearly these two ion species are not the same at all altitudes, so this must be clarified. The collision cross section is given later but it is important to have some explanation of which ions are used.

Page 5, eq (4). It is surprising to have a constant Mn between 100-200 km since the ion mass changes with altitude. According to Appendix A, this mass represents the average mass, but this is not realistic since the collision frequencies are different for different species. Page 6, line 129. The linear variation of Ti is not realistic.

Page 7, line 144. What is the reference for the collision cross section and for which species is this valid?

- Latitude and Local Time not specified for examples shown

Although the paper presents a generic case for the method development and validation, the reader needs to know the latitude, longitude, local time, etc. used for the analysis. Are the simulations for the equator or mid-latitudes? What is the local time? How would the results be different if the passes were at night or in the auroral zone?

- Temporal Variations

The paper presents a case for the method development and validation for static conditions. How does the method react to changes in the environment during a pass? In other words, how sensitive is the analysis to temporal variations? How long is a pass in the simulations shown?

- General Concern with Figures

Figures 1, 7, and 8 are perplexing. Why are there two peaks of the density and Pedersen conductivity near 115 km at +1000 km and -1000 km? Presumably this is mid latitude, daytime, based on the Chapman layer discussion. Why not show continuous plasma density and Pedersen conductivity as in Figure 4?

Figures 5-8. It is not easy to understand the results of these figures, although they appear to be at the core of the paper's objectives. For example, Figures 5 and 6 show Monte Carlo predictions. To what do the percentiles refer and what is the main result that the authors wish to show? This is not explained clearly in the text.

Figures 7-8 show the results of the method for two satellites and one satellite. What are the main results from these figures that the authors seek to convey? Presumably the overall goal is to show altitude profiles of the parameters obtained from the *in situ* measurements which might then be compared with the model. The results are not clear at all.

Minor Comments:

- The paper's title is very confusing. Why say "Continuation" in the title? A suggested title is simply: "Daedalus Ionospheric Profile Study". "Continuation" and "DIPCont" could be explained in the main text but should not be in the title of the paper.
- Page 4, eq (3). On the left-hand side, T should be Tn. Same on line 103 (page 5). Suggest the authors check everywhere where T is used in place of Tn, Te, Ti.

END OF COMMENTS