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Atmospheric Measurement Techniques Discussions

Interactive comment on "A simulation study with a new residual ionospheric error model for GPS radio occultation climatologies" *by* J. Danzer et al.

Anonymous Referee #3

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Summary: This paper describes a comprehensive simulation study to assess a new approach to the residual ionospheric correction in atmospheric retrievals using GPS radio occultation. The purpose of the simulation is to assess the efficacy of the correction in a fully-three dimensional ionosphere, whereas it was originally evaluated in a one-dimensional ionosphere (Healy and Culverwell, 2015). A multi-year simulation data set is used so as to sample the full range of solar cycle conditions. As expected, daytime and solar maximum produce the largest residual errors to the standard ionospheric correction, although nighttime impacts are observed as well. Low latitudes are the focus. The correction model produces encouraging results, decreasing residual ionospheric error by greater than 1 K under certain conditions. Future work is suggested.

Review: The paper is an original research contribution to a very important topic: the

residual ionospheric error in radio occultation retrievals, and how to partially correct it. The simulation approach is sound and sufficiently comprehensive to address key aspects of a new algorithm. However, certain aspects of the analysis require further clarification, as described in the detailed comments. In particular, how "noise" becomes a significant contributor to the simulation results is not clear.

Detailed comments: p. 1154, Line 16: I believe the meaning here is that the varying directions of the magnetic field can cancel out a physical correction in a climatological application. I am not sure what is meant. Please be more explicit.

p. 1155, Line 5 (and paragraph): This paragraph is confusing for several reasons. It refers to another paper that can be obtained, but does not direct the reader to specific equations in that document, so it is not clear what the different terms are that are being referenced ("first term" and "second term").

The "second term" depends on "subtly varying parameters" (perhaps a poor choice of words? Why are peak height and thickness "subtle"?). The authors ignore non-spherical symmetry of the ionosphere. Wouldn't this correction approach depend on that? Mannucci et al. (2011) has some discussion of the assumptions required in the usual bending angle correction formula, and I suspect those assumptions are relevant to this new correction factor.

p. 1155, Line 23: "non-spherical" ionosphere is not the correct terminology.

p. 1156, Line 13: The implications of "no magnetic field" are presumably that higher-order ionospheric effects in the Appleton-Hartree formula are being ignored. I suggest that this point be made more explicit and a reference given of why it is appropriate to ignore such effects. This ionospheric correction only deals with residual errors due to L1/L2 raypath separation.

p. 1157, Line 6: It is clear from this discussion that an assumption of ionospheric spherical symmetry is implicit in the correction formula and how it is related to slowly and

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rapidly varying factors. Yet, the authors never refer to this point. This point is referred to in Mannucci et al., 2011 (e.g. see second to last paragraph of p. 2839 for a discussion this point and the relevant references). The authors should consider whether non-spherically symmetric ionospheric structure impacts the correction approach.

p. 1157, Line 13: I would say "product of two factors". Terms are typically additive in an equation. Factors contribute to a term via multiplication.

p. 1157, Equation 4: Is the first equals sign really a definition?

p. 1160: Line 1: By analyzing Januaries only, seasonal effects are not treated. The authors should consider treating this in a future work (season is mentioned earlier in the paper) and mention such in the paper.

p. 1160, Line 10: This is first time of many that "noise" and "noisy events" are mentioned. What is the source of such noise? Is it numerical round-off error? The ionosphere model is smooth, as is the atmospheric model. The authors should provide insight into the source of the noise (is it artificially added to the data?) because it has such a profound effect on the results.

p. 1161, Line 15: Again, how does noise enter the simulation? If the "noise" is due to subtle non-spherically symmetric structure in the ionosphere, is it appropriate to call this "noise"?

p. 1162, Line 16: I have concerns regarding the large variation in kappa. Will this introduce retrieval biases when the actual kappa differs from the assumed one? A statement regarding this is warranted. Suppose kappa is incorrect for a particular month (e.g. a value of 6)? How much might that bias the corrected retrievals and what could be done about it? Could an in-situ or real-time value of kappa be estimated based on data for a particular month? The authors need not solve this problem, but it appears to be a matter requiring further study.

p. 1162, Line 18: The large of impact of noise is counter-intuitive, particular using

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smooth atmospheric models and nighttime ionospheric models which should also be quite smooth. It's not clear to this reviewer why the noise contribution at night is relatively larger, unless the noise is due to round-off error. Is it?

p. 1165, Line 13: What is the altitude of the spacecraft used in the simulation? As shown by Mannucci, et al., 2011, this can affect the results significantly.

p. 1166, Line 3: increasing SNR?

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