Author's Response to Referee #3

We thank referee #3 for the thorough review of our manuscript. We have answered all comments below (for easier comparison the referee comments are included in *italic*).

Detailed comments:

#1: 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.

#1: Yes, your assumption is correct. We expect that the residual errors caused by the Earth's magnetic field average out and do not affect zonal mean climatologies, see also Syndergaard (2000). We decided to add an extra paragraph about this on p. 1155, after line 19, see also answer to referee #1.

"The model does not correct for the residual ionospheric error that arises from horizontal gradients of the ionosphere, or those errors that are caused by the Earth's magnetic field (see companion paper Healy and Culverwell (2015)). These errors could have an effect on individual profiles\footnote{Although Syndergaard (2000) argued that the geomagnetic term has no appreciable impact on the residual ionospheric errors in GPS-RO applications}, but they should average out of the zonal monthly mean climatologies which are the focus of our study here."

#2: 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").

#2: We see that the paragraph is confusing. Hence we decided to add citations of Eqns. 2-4 from the manuscript in the paragraph.

#3: 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.

#3: Thank you, we will change the text in the manuscript in the following way:

"The second factor depends on the peak height and thickness of the ionosphere, which vary, more slowly, with the season and geographic location."

Furthermore we replace “one-dimensional” on p. 1154, line 12 by “spherically symmetric”.

#4: p. 1155, Line 23: “non-spherical” ionosphere is not the correct terminology.

#4: We will correct “non-spherical ionosphere” to non-spherical symmetric ionosphere.

#5: 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.

#5: We will add a citation on p. 1156, lines 12-14 in the following way:

“Vorob’ev and Krasil’nikova (1994) provide an integral expression for the residual ionospheric error $\Delta\alpha$, given for the case of a one-dimensional ionosphere with no magnetic field. (Liu et al (2013) conclude that the magnetic field has no essential impact on bending angle residuals.) "

#6: 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 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.

#6: We refer to spherical symmetry, see last point of answer #3. However, the main point of this work is to examine how well a simple theoretical model can account for the residual errors simulated by a complicated, non-spherically symmetric ionospheric model. We think that is the sensible first step, before complicating the theoretical model. The positive results of this paper are a partial justification for using a spherically symmetric error model.

#7: 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.

#7: Thank you, we will write “product of two factors”. Furthermore we will replace “term” by “factor” throughout the manuscript.

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

#8: We will write \Delta \alpha (a) \equiv \alpha_C(a) - \alpha_N(a) = ...

#9: 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.

#9: We did not yet perform a seasonal study. However, in future work it is the goal to extend the simulation study, where seasons definitely also should be considered. We will add the following sentence on p. 1160, line 3:

“Seasonal effects are not studied, but will be considered in future work. “

#10: . 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.

#10: At the moment there are some ongoing studies at the Wegener Center, trying to understand from a physical point of view exactly how the bending angle alpha(a) is built. Very detailed studies along the ray path (L1,L2, etc) were performed, trying to understand the structure at each point. From this studies we can confirm that the atmospheric model is really smooth, producing negligible numerical noise from the ray tracing. However, the NeUoG model is not completely smooth. The ray tracing through the ionosphere model leads to numerical noise, i.e., discontinuities have been found. This is the dominant source of the noise sign (personal communications with G. Kirchengast).

Furthermore, the problem of the noise in our data is directly related to the fact that we are studying very small numbers. The values of interest, i.e., the studied residual error, is smaller than about -0.3 murad. Hence, the impact of noise is larger than on other quantities of interest, such as the (alpha_1-alpha_2) squared term. So in relative terms, the noise has a definite impact on the data.

There is the goal to improve the NeUoG model. At the moment we suggest, as discussed in the summary and discussion section, to increase the simulated number of profiles, as well as to perform tests with other ionosphere models.

#11: 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"?

#11: Please see the answer to question #10. Furthermore we will add the following sentences to the manuscript on p. 1161, line 19:

"The origin of the noise in the data enters due to discontinuities in the NeUoG model. While the ray tracing through the atmosphere model is really smooth and only negligible numerical noise is produced, the NeUoG ionosphere model has some discontinuities between different layers, leading to noise in the data. Furthermore, with the residual error as one the key quantities of interest, we are studying very small numbers in the order of 10^-7 to 10^-8 rad. Relative in fractions of murad the noise from the ray tracing has a large impact on such small numbers. "

The next sentence will start as a paragraph: "To partially overcome the problem of the noise, we decided to perform a vertical smoothing step ..."

#12: 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.

#12: In principle the statement is correct. Further studies need to be performed in order to assess kappa. Nevertheless, we could show that kappa must be greater than zero (kappa >0), while kappa = 0 is currently assumed in ionospheric corrections. Especially the temperature plot in Fig. 6 shows the positive impact of the model correction on the data. However, prior to being able to apply this approach on observational data, the variation of kappa in time and space must be well studied, which is planned for the future, as also discussed in the companion paper (p. 1187, see lines 20-28).

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

#13: In relative terms the noise in the data has a larger impact on the smaller night time values, compared to the day time values, please see the given answers in question #10 and #11.

#14: 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.

#14: All simulations have been performed at COSMIC altitudes of about 800 km.

#15: p. 1166, Line 3: increasing SNR?

#15: Thank you, we will write: "However, at mid to high latitudes a decreasing signal-to-noise ratio in the simulated data prohibited us from studying correlations."