Response to Referee 2: (referee's comments are in blue; the replies are in black)

The authors are grateful to the referee for careful reading of the paper and valuable suggestions and comments. Below we provide our responses point by point, and modify the manuscript accordingly.

This paper considers the ionospheric correction of GPS radio occultation in the troposphere, and how to extrapolate the (alpha_l1-alpha_l2) differences. The information in the paper will be of interest to other GPS-RO data providers, and NWP users who may wish assimilate the data. However, I recommend publish after major revision, because often the results are explained on the basis of simulations that are not presented in any detail. For example, the "ionosphere induced vertical shifts" noted in the abstract, are similar to shifts seen in wave optics modeling. But this modeling is only mentioned, and not presented in any meaningful detail. Hence, it is difficult for the reader to understand how the electron density at the LEO can produce this result (particularly when the ray tracing results differ).

I recommend that the simulation results mentioned in the text be described and presented in more detail before publication. Please see specific comments below.

Answer: We removed the discussion of the modeling from the main text and included it in Appendix A (6 pages + 4 figures) in the revised paper. The description of the modeling contains details sufficient for reproducing the results (this is consistent with the recommendation of the referee).

Specific comments

*Page 7785, line 13: Typo, "and *the* multiple ..."*

Answer: Done.

Page 7785, line 21. How do you know the structure in the observed bending angle profile is caused by moist convection? If statement this based on simulations at the observation location (as suggested on line 1, 7786), please provide more detail, like for example the NWP information used. Source, resolution, forecast-range etc.

Answer: This occultation is from the middle of Pacific Ocean. Most occultations in the ITCZ are affected by moist convection which results in known typical structures in RO signals both in time and impact parameter representation (we provided references). This occultation is used as an example only and, we believe, providing more detailed information about meteorological conditions at the location of the occultation is not needed.

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Page 7786, Paragraph 2, line 7-27. The paragraph is difficult to understand physically. The bending angle profiles in Fig 1c and 1d have sharp vertical gradients near 3.5 km and 2.8 km, respectively. Presumably, the signal to noise is lowest for L1CA and L2C bending angles near these heights, because of atmospheric defocusing. What role does atmospheric defocusing and reduced signal to noise have on the increased (corrected) bending angle errors in these vertical intervals?

The role of the electron density at the LEO is unclear. Why should the electron density at the LEO produce larger bending angle errors when the vertical bending angle gradients are largest?

Based on Schreiner et al (Appendix A, 1999) and also noted in the text, the electron density at the LEO should not significantly affect the retrieved impact parameters. This fact is confirmed by ray tracing, but not in wave optics simulations. Firstly, why would the modeling approaches differ when assessing the impact of the electron density of the LEO? Secondly, the reader needs more details about the simulations mentioned in the text. EG:

**What orbits are used in the simulations? Do they differ in the ray tracing and wave optics simulation?*

**What is the neutral atmosphere model? Horizontal gradients included?*

**What ionosphere model is used? Are horizontal gradients included in the ionosphere model and used to compute the ray path?*

**Can the \Delta h_{1,2} be related to the horizontal refractivity gradients integrated along the ray path?*

**In addition, quantify \Delta h_{1,2} provided by both the ray tracing and wave optics approaches, and note the values in the text. How do they vary with impact height?*

Answer: We are grateful to the referee for raising these important questions. Indeed the effect of the impact height shifts was not completely investigated and clearly described. In the revised paper we included detailed modeling of this effect in Appendix A. The effect is modeled and completely explained by ray-tracing. Its explanation by wave optics is technically more difficult (when including the ionosphere and realistic orbits) and, in fact, is not needed because diffraction effects are not involved. Below are answers to some specific referee's questions above: these answers and more information can also be found in Appendix A in the revised paper. The Appendix A is written with the level of details sufficient for reproducing the results.

The de-focusing and reduced SNR in the presence of inversion layers are not related to the effect of impact parameter shifts.

Spherically-symmetric ionosphere (which extends to the receiver height) does not introduce impact parameter shifts (also mentioned in the original paper).

Circular orbits are used (details specified in Appendix A).

Neutral atmosphere refractivity is spherically-symmetric (details specified in Appendix A).

We used both spherically-symmetric and non-spherically-symmetric ionosphere models. Only non-spherically-symmetric model produces impact height shifts (details discussed in Appendix A).

Yes, the impact parameter shifts are related to horizontal refractivity gradients in the ionosphere.

Variation of the impact height shifts with height depends on the structure of the ionosphere. With the ionosphere model used in our simulation study, the shifts were about constant in the troposphere. Exhaustive modeling of all possible ionospheric structures is a difficult task. However, from analysis of the observational data it follows that the shifts may remain about constant in a rather extended height intervals or change within short height intervals, they may have different signs and magnitudes from zero to hundred meters. This is described in section 2 of the revised paper. The results obtained from the modeling in Appendix A are consistent with the magnitudes and different signs of the observed shifts.

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*Page 7788, Line 20. Please provide more detail about why the first two terms in equ.7 are needed for "modeling of the effects of horizontal inhomogeneity of the ionosphere". Again, this appears to rely on simulations that are not presented in any detail, and therefore would be difficult to for a reader to reproduce. What effects of horizontal inhomogeneity are we talking about here? Why is (A+B*h) better in these situations than say D/(300-h)**1.5?*

Answer: If there is no horizontal inhomogeneity, the impact of F2 layer (Chapman layer) can be well modeled by the fitting function D/(300-h)**1.5. However in real RO data, the L1-L2 bending angle can be quite variable, apparently due to horizontal inhomogeneity, since there are no other physical reasons. In the example shown in Figure R1, RO L1-L2 profile (gray) changes sign with the impact height, which cannot be modeled by only the term D/(300-h)**1.5 (cyan). Including the linear term A+B*h clearly improves the modeling in this case. However, in the revised paper we changed the statement to a more general: "... the first two terms are necessary for modeling $\alpha_1(h)-\alpha_2(h)$ that cannot be well modeled by the last two terms: such profiles are sometimes observed in RO data."

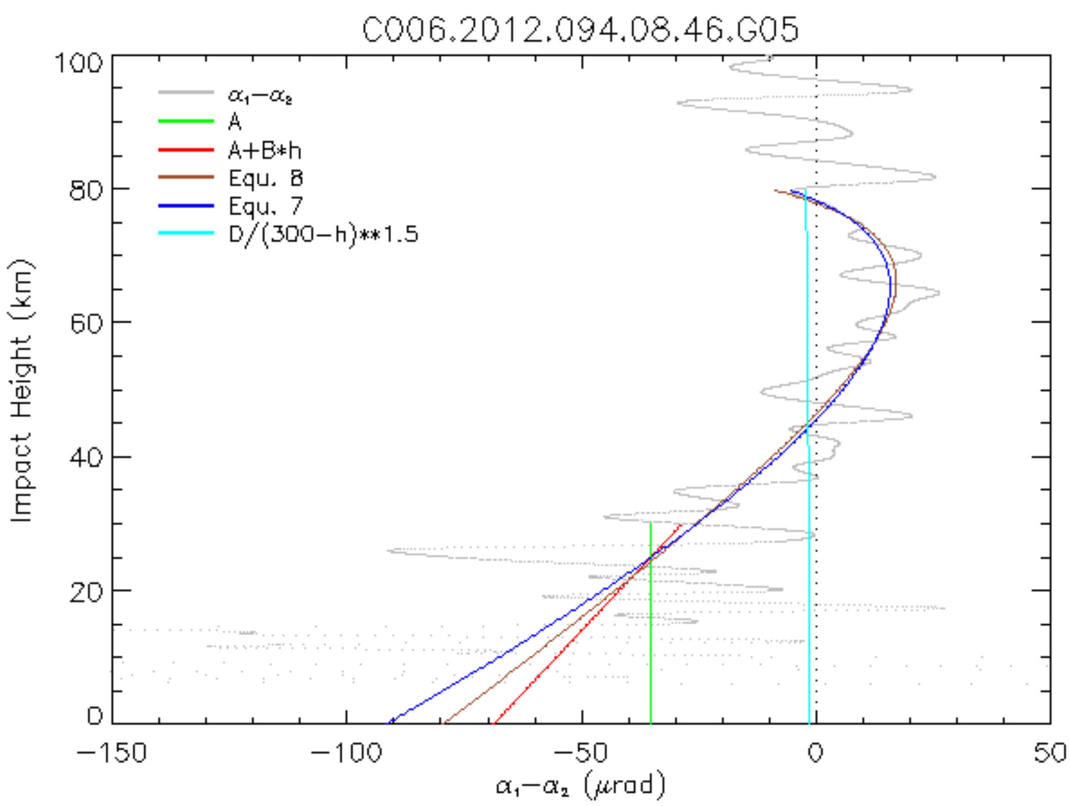

Fig. R1: One case of L1-L2 bending angle profile (gray) and five different fitting functions. The fitting intervals are different for different fitting functions: the fitting interval for constant fit (green) and linear fit (red) is 20-30 km; for all three others, the fitting interval is 20-80 km.

Page 7789: Eq. 8 and Figure 2.

When performing the least square fitting, is it a weighted least square fit, where the assumed (alpha_1-alpha_2) error statistics vary with height? Please confirm that the (alpha_1-alpha_2) differences below 20 km are not used in the least squares fit.

Answer: We didn't apply the weighted least squares fit in this study, and we confirm that no L1-L2 data below the transition height are used in the least squares fit.

*The removal of term 4 clearly improves the pink line (213.02). However, I am surprised that the inclusion of a term D/(300-h)**1.5 produces the unwanted curvature below ~35 km, given that C/(100-h)**1.5 does not appear to. Surely the third term would produce greater curvature in the final solution than the fourth term? What happens if you remove term 3, but keep term 4?*

Answer: Taken RO profiles in Fig. 2 as examples, we demonstrated the fitting results by using different fitting functions, C1+C2*z+C3/(z_E-z)**1.5 (left, the one used in our study) and C1+C2*z+C3/(z_F-z)**1.5 (right), as below. The fitting function with E-layer term (left) performs better than or at least comparable to the one with F-layer term (right).

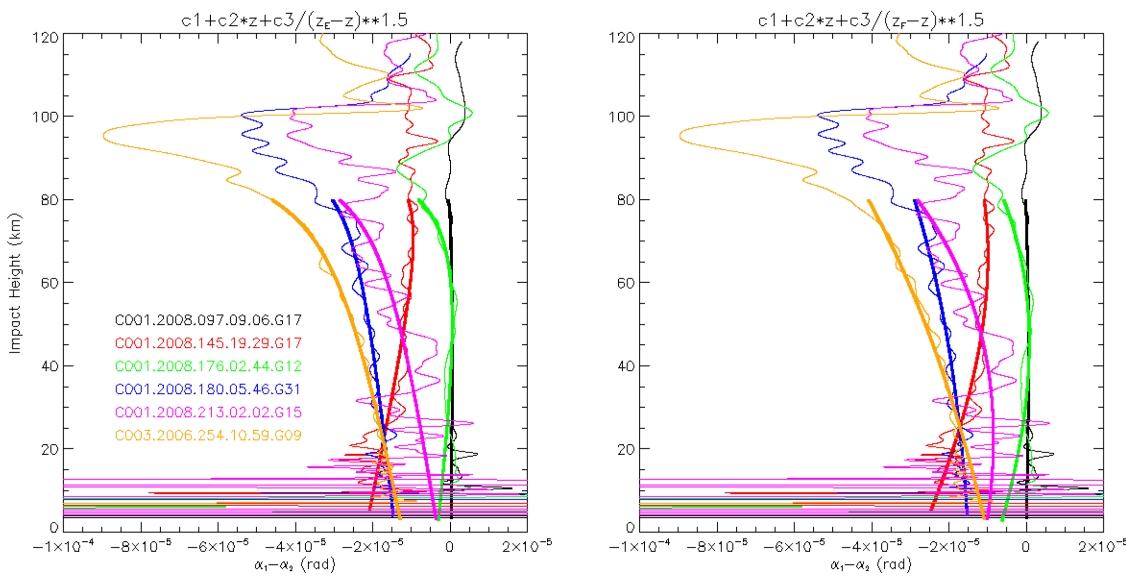

Fig. R2: Examples of RO L1-L2 bending angle profiles (thin) and fitted profiles (thick) by using fitting function of C1+C2*z+C3/(z_E-z)**1.5 (left) and C1+C2*z+C3/(z_F-z)**1.5 (right).

We further analyzed the relationship between the fitting parameters C1, C2 and C3 for different fitting functions in Fig. R3. It is shown that the fitting parameters are highly correlated with each other once the fitting function with F2-layer term (right) is applied. For the fitting function with E-layer term (left) the correlation is noticeably weaker and this provides additional motivation for the choice of this fitting function.

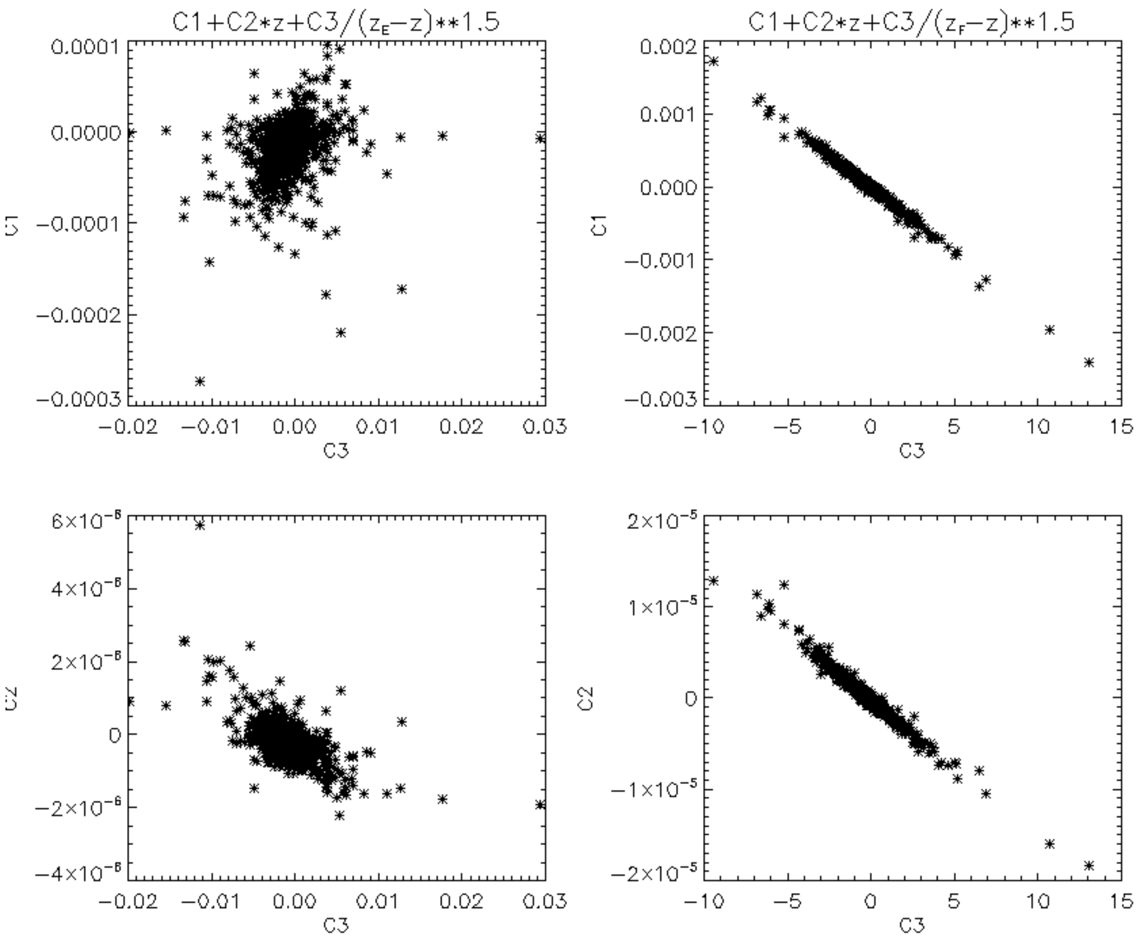

Fig. R3: Scatter plots of fitting parameters (C1 and C3, C2 and C3) for different fitting functions.

In the revised paper, we summarized the results (presented above in response to referee's question) without details (to not overload the revised paper which already increased due to Appendix A included in response to other important questions from both referees).

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Page 7791, line 15. The use of ECMWF analyses. Given that COSMIC bending angle profiles have been assimilated into the ECMWF analyses, can it be assumed that error arising from uncorrected ionospheric effects are uncorrelated with "the model" (ECMWF analysis?) errors? A short-range ECMWF forecast might be a better comparison.

Answer: In the revised paper, we used the ECMWF forecast data instead of the model analyses for comparison. The comparison between the RO-retrieved bending angle and refractivity for different fixed transition heights and the collocated ones from the ECMWF 24h forecast is shown in Fig. R4. For reference, similar comparison with the analyses (taken from the original paper) is shown in Figure R5. It is seen that the use of the forecast instead of the analysis does not change the estimates of the optimal transition heights, i.e. the main conclusions of the paper remain unchanged. It is worth mentioning that RO data have slightly better agreement (in terms of both mean and standard deviation) with the ECMWF analysis than with the forecast (which is to be expected since the ECMWF analysis data have assimilated RO data).

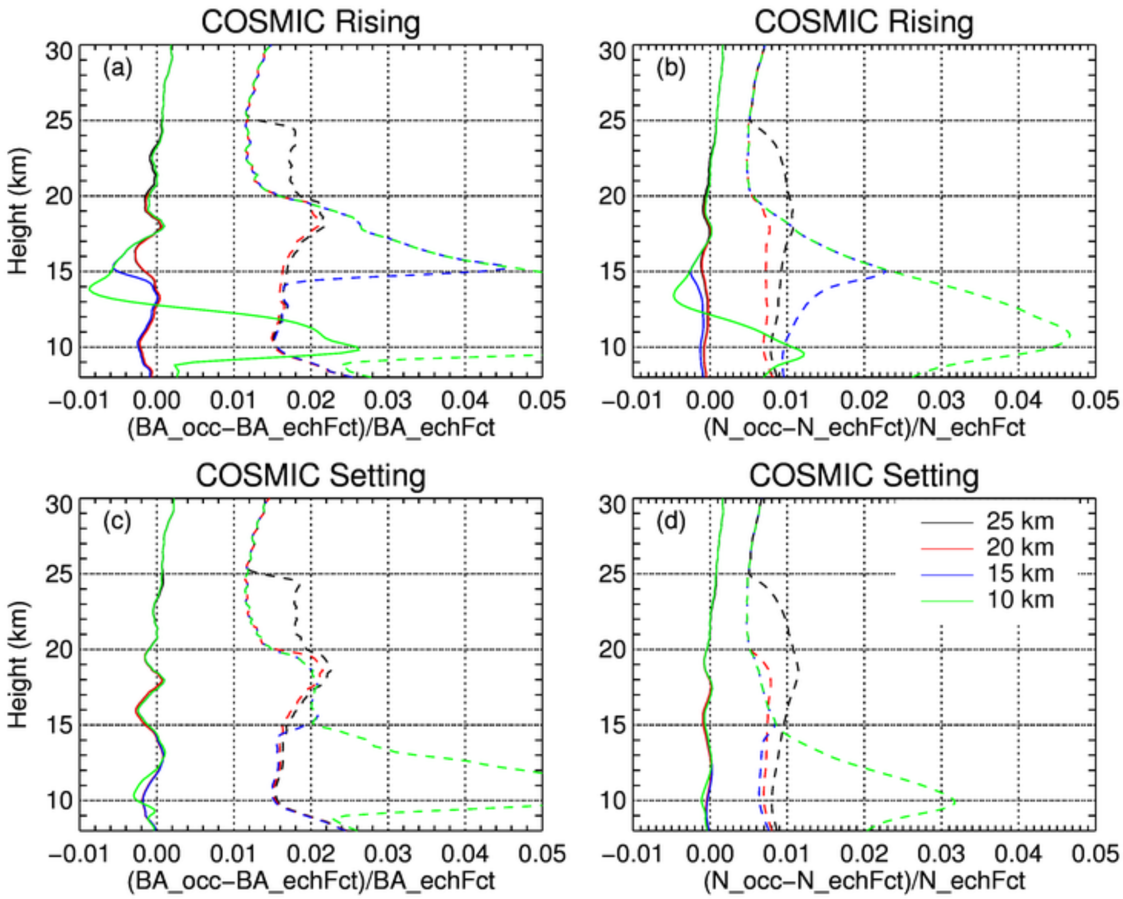

Fig. R4: Mean differences (solid) and standard deviations (dashed) of retrieved COSMIC bending angles (left) and refractivities (right) for RO rising (upper) and setting (lower) occultations for different transition heights (in different colors), relative to the collocated ECMWF 24h forecast data.

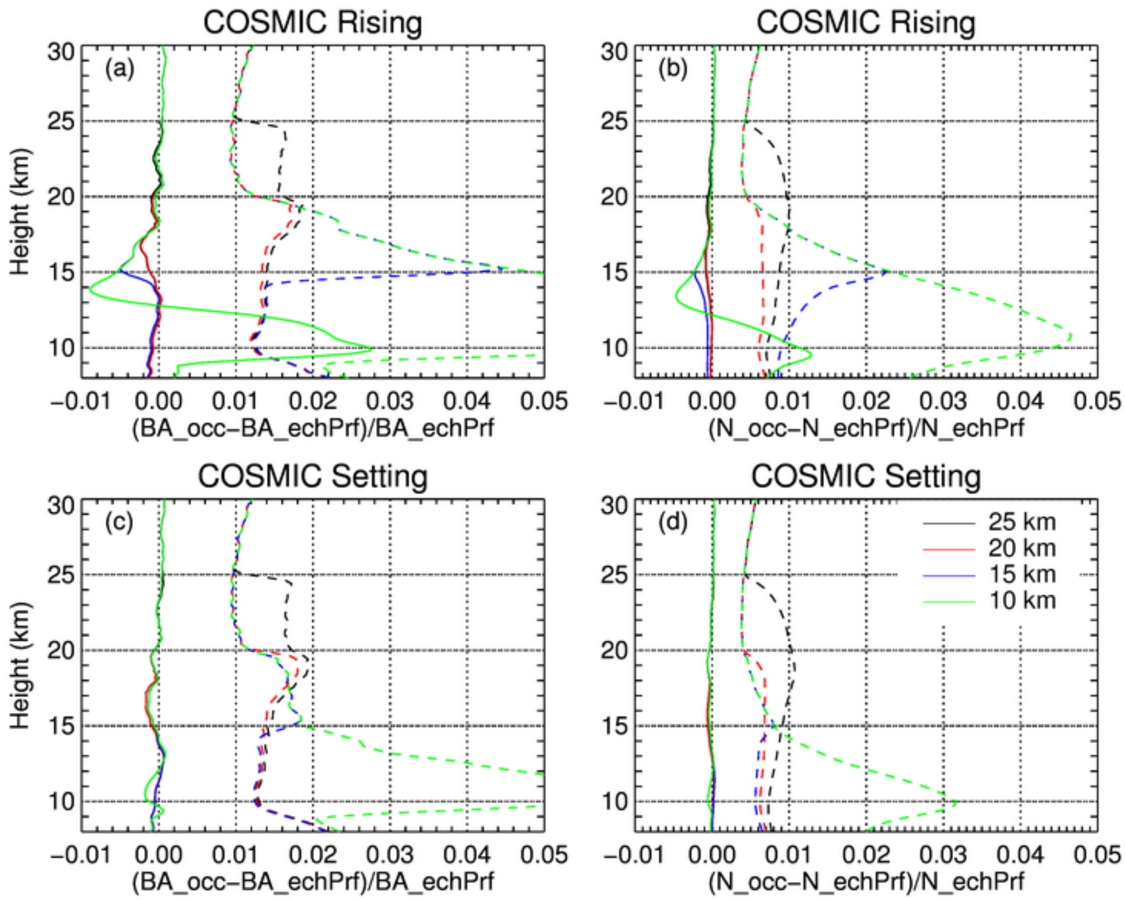

Fig. R5: Similar to Fig. R4, except for statistical comparison with the ECMWF analysis data.