Response to referee 1: (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 is a very well-written paper. It describes the current problems associated with ionospheric correction of RO data in the troposphere, and in that context describes relevant aspects of the data from COSMIC and Metop, and the limitations of these data. The authors suggest a method of extrapolating the L1-L2 bending angle down through altitudes where the L2 signal is not available or not useful. The method is based on fitting a function (eq. 8) to the observed L1-L2 bending angle between a transition height (below which the L2 signal is not used) and 80 km. Finally they analyse the difference (mean and std.dev.) between retrieved ionospheric corrected bending angles and forward modelled profiles from ECMWF analyses, using different fixed transition heights. Based on the std.dev. they find optimal fixed transition heights for different cases (COSMIC/Metop; L2P/L2C; rising/setting). The main purpose of the work is to suggest an approach so that all current data from different RO missions are processed in a common way (valuable for climate applications) and for this purpose the authors suggest that all data could be processed with a fixed transition height of 20 km.

I have mostly minor issues, but one is major and will require additional analyses:

I would like the authors to investigate also results for refractivity. I suspect that the results for refractivity might contradict the conclusions on the optimal transition heights. To illustrate my point I have included a figure (figure 1; based on data that I had already available) showing both the bending angle and the refractivity std.dev. for Metop (setting only) with respect to forward modeled profiles from ECMWF forecasts. The processing is based on near real-time bending angle data from EUMETSAT that are further processed to refractivity with the Radio Occultation Processing Package (ROPP), and the data are from a different period (with different firmware settings on Metop) than that used by the authors. In the ROPP processing a constant L1-L2 extrapolation was done from either 25 km or 20 km (sorry, I did not have results for 15 km and 10 km readily available). I make no claim that my processing is any better than the authors. My point is only to show that although the bending angle std.dev. for the two cases (20 km and 25 km) are not very different below ~20 km (somewhat similar to Fig 6b in the discussion paper), the refractivity std.dev. is significantly different at this and lower altitudes. I believe it has to do with the error propagation through the Abel transform and I think an analysis of the vertical correlations mentioned on page 7787 (line 19) is necessary to fully understand these results. Such analysis may be out of the scope of the paper, as the authors say, but it is important to not draw wrong conclusions based only on bending angle. Thus, I urge the authors to also have a look at refractivity (and perhaps also dry temperature) and adjust the paper and the conclusions if necessary.

Answer: We are grateful for the suggestion. We included the statistical results for refractivity in the revision. The main conclusion has not been changed.

That said, I do not understand why the ionospheric correction in this paper is performed with eq. (1) instead of a modification that has been used in practice for many years (e.g., Kuo et al., JMSJ, 2004; Sokolovskiy et al., JTECH, 2009; Schreiner et al. AMT, 2011), namely strong smoothing of the L1-L2 bending angle used in an equation similar to eq. (3) (with the last term being a smoothed version of the observed L1-L2 bending angle). Such a method does not amplify small-scale fluctuations (but leaves the ones in L1). In a few places the authors argue that extrapolation is necessary to avoid the amplification of small-scale fluctuations (line 20 page 7783; line 6 page 7786), but they don't seem to be considering the approach of smoothing the observed L1-L2 bending angles (above the transition heights).

Why was such an approach not used here?

Wouldn't it reduce the noise significantly (in particular reducing the std.dev. shown by green and blue curves above 10 and 15 km, respectively, in figs. 4-6)?

If it were to be used, would it change the conclusions on optimal transition heights?

Answer: We are grateful for pointing to this. Because we were focused on the extrapolation, we missed to mention that the modified ionospheric correction method ($\alpha = \alpha_1 + c_2(\bar{\alpha}_1 - \bar{\alpha}_2)$) (which is routinely applied in the CDAAC processing) was applied in this study as well. We modified the text accordingly to clarify this in the revised version. There is no change of the results and conclusion.

Although my request and questions above (and below) may lead to different conclusions/values of the optimal transition heights, I think the paper is very important. It may be that the authors need to shift the main purpose of the paper from defining optimal fixed transition heights for climate applications to instead give a broader picture of the issue and its complications. I think this would be very valuable.

Answer: In this paper we search for optimal fixed transition heights by not stating that dynamic transition should be discarded. There is no universal method, and one or another method may be more optimal for different applications. As for the broader picture, we added statistics for refractivities (as suggested by the referee). As for the complications, we investigated the impact parameter shifts induced by horizontally-inhomogeneous ionosphere (which, at the best of our knowledge, were not observed and investigated before and which introduce a certain challenge to RO data processing). Detailed modeling of the ionosphere-induced impact height shifts is included in the Appendix A in revised paper.

Specific issues:

1) Page 7783 (line 13): '... as noted by several different researchers.'. Please provide references if possible.

Answer: Steiner et al. [1999] mentioned that "the tropospheric refractivity field often exhibits significant small-scale variability mainly due to the presence of a highly variable moisture distribution and temperature inversions. Here, splitting of L1 and L2 raypaths incurred by passing the dispersive ionosphere on the way from the transmitter, causes these rays to often probe quite different local refractivity behavior at the different perigee heights (difference up to several 100 m). Mulitple propagation of the L1 and L2 rays caused by high refractivity gradients can further complicate the received signal." We have included the reference in the revised paper.

2) Page 7783 (line 26): 'In previous studies ...'. Please provide references if possible.

Answer: We provided references in the revised paper, as requested by referee (Rocken et al., 1997; Steiner et al., 1999; Hajj et al., 2002).

3) Page 7784 (line 3-9): These lines from 'A transition too high ...' seems more appropriate in the conclusions, not in the introduction (unless this is a description of results from previous studies, in which case references should be given).

Answer: This statement is just a theoretical perception, which leads to the concept (and expectation of the existence) of an optimal transition height. We think it is reasonable to keep this statement in the introduction and conclusion.

4) Page 7785 (line 26): Sokolovskiy et al. (2014) introduced a method called comparative discrimination that largely eliminates those spikes. Why was it not used here?

Answer: Application of the "comparative discrimination" to processing of large amounts of data is still in research. Besides, the comparative discrimination does not eliminate the necessity for the ionospheric correction, i.e. extrapolation of L1-L2 bending angle is still needed.

5) Page 7786 (line 14 and line 23): It is not clear how Delta(h1) and Delta(h2) are defined. The correction is done at a common impact parameter, which is the independent variable in the phase matching method, so what is exactly meant here?

Answer: Yes. The impact parameter is an independent variable in the phase matching method. However, sharp correlated structures (such as those induced by inversion layers) in L1 and L2 bending angles derived by phase matching are clearly shifted with respect to each other in the impact parameter. These shifts cannot be defined (or measured) from the observation data, but their difference can be measured. Detailed discussion of the impact height shifts is moved from Section 2 to the Appendix A in the revised paper where these effects are modeled and explained by horizontally inhomogeneous ionosphere. In the revised paper, the modeling is discussed in details sufficient for reproducing the results.

6) Page 7786 (line 15-19): Please provide a reference here, e.g. Schreiner et al. (Radio Science, 1999).

Answer: Done.

7) Page 7786 (line 28-29): How can you be sure the error is eliminated? How do you know if the error is in L1 or L2, or both? How do you know that there is an error at all?

Answer: This follows from the results of the modeling included in Appendix A in the revised paper.

8) Page 7788 (line 5): It is not clear that increasing the fitting interval will give better results. What if a function fits well at high altitudes, but is significantly off at the lowest altitudes? Are all heights weighted equally in the fitting? Was it verified that fitting to higher altitudes gives better results statistically? In bending angle? In refractivity?

Answer: We believe that increasing the fitting interval is consistent with the use of the fitting function that models large-scale ionospheric structures such as responses from F and E layers. For constant and linear fitting functions, which are not intended to model responses from the ionospheric layers, naturally, smaller fitting intervals should be used. Implicitly, the use of increased fitting interval is justified by results obtained in response to the next question of the referee (see below).

9) Page 7789 (eq. 8): How does this approach compare to a simple constant extrapolation where the fitting is done only in a small interval above the transition height (e.g. Schreiner et al., AMT, 2011)?

Answer: We compare the 3-term fit (Equ. 8, the one used for the statistical analysis in the paper) with three other fitting methods, including constant fit, linear fit, and 4-term fit (Equ. 7), for extrapolation of L1-L2 bending angle. The fitting interval used for constant fit and linear fit is $h_{ext} < h < h_{ext} + 10$; while the 3-term and 4-term fitting interval is $h_{ext} < h < 80$.

Using the COSMIC RO data from the day 2012.094 as an example, we display all observed L1-L2 bending angle profiles (gray), and their mean (black) in Fig. R1. By using different fitting functions, the mean of fitted individual L1-L2 bending angles is also calculated and shown in Fig. R1. It is seen that except the constant fit which introduces the negative bias below the transition height systematically (ionospheric bending angle, on average, increases with height), all other 3 mean fitting profiles agree well with observed mean L1-L2.

Fig. R2 shows the statistical comparison of the RO-retrieved bending angles by using different fitting methods to the bending angles forward-modeled from the ECMWF analyses for April 2012 (transition height is fixed at 20 km). As shown in Fig. R2, 3-term fit gives slightly smaller stdv below the transition height compared to constant and linear fit. Constant fit brings systematically negative bias, consistent with the result in Fig. R1.

In the manuscript, we already showed that 4-term fit tends to overfit the L1-L2 profile based on RO cases in Fig. 2. The response from the F2 layer far from the hmF2 can be well modeled by the linear term.

Overall, the 3-term fit performs slightly better than three other fitting methods, and it was adopted in this study.

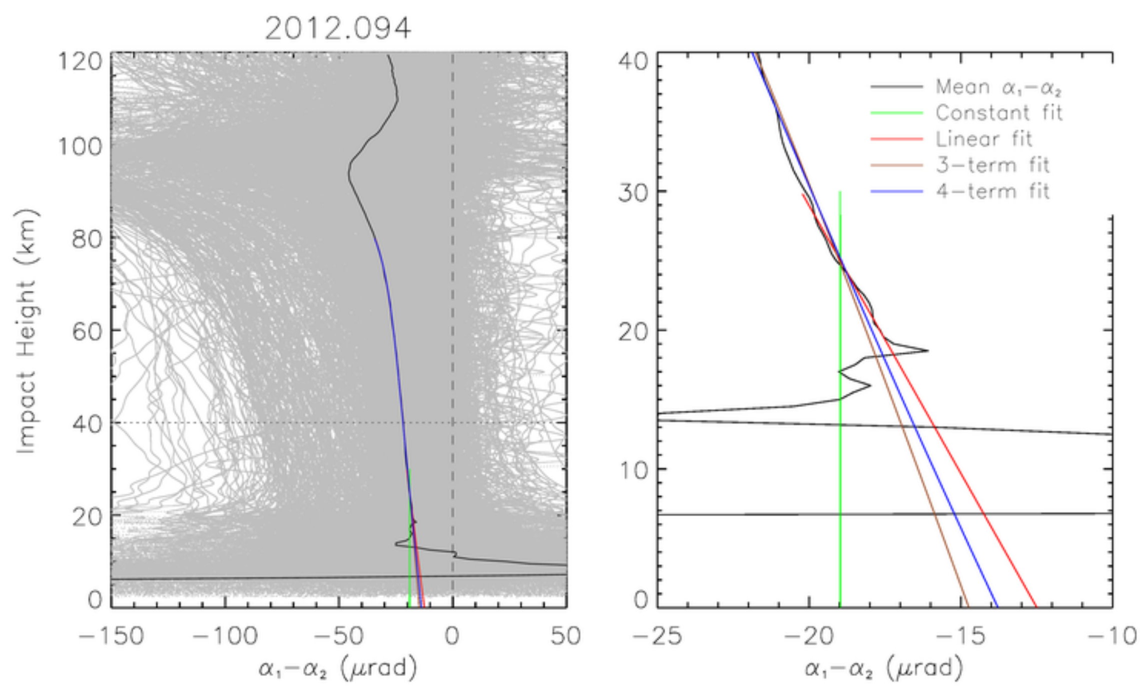

Fig. R1. Left: L1-L2 bending angle profiles (gray) of COSMIC RO data during 2012.094, along with daily mean L1-L2 bending angle (black), and mean fitting profiles by using constant fit (green), linear fit (red), 3-term fit (brown), and 4-term fit (blue). Right: the zoomed mean profiles over 0-40 km, including the observing and fitting ones.

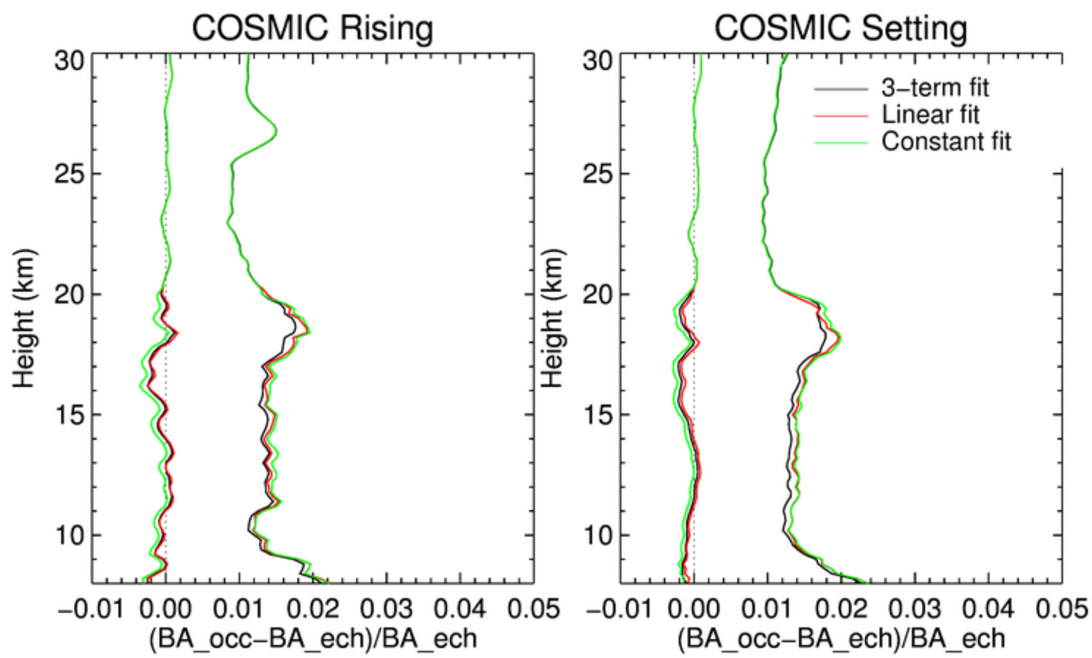

Fig. R2. Mean differences and standard deviations of retrieved COSMIC bending angles for different fitting functions (indicated in different colors), relative to the collocated ECMWF bending angles for rising (left) and setting (right) occultations.

In the revised paper, we summarized these 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).

Technical corrections:

Answer: All is corrected as suggested by referee. Thanks!

10) Page 7782 (line 4 and 5): Would read better with the word 'using' instead of 'by', because 'by' could also refer to 'replacing'.
11) Page 7782 (line 19): Suggesting '... stratosphere [and above].'.
12) Page 7785 (line 8): 'a few' instead of 'the'.
13) Page 7785 (line 11): 'use of [a] wave optics ..'
14) Page 7785 (line 12): 'a' instead of 'the'.
15) Page 7786 (line 10): 'are also' instead of 'also are'.
16) Page 7786 (line 20): Would read better: '... but only introduces an error ...'
17) Page 7788 (eq. 6): There seems to be a few typos in the 'integration by parts' expression. See interactive comment by I. D. Culverwell.
18) Page 7790 (line 13 and other places): Use 'Metop' (EUMETSAT way) or 'MetOp' (ESA way), but not 'METOP'.
19) Page 7791 (line 5): Suggesting: '... different [fixed] transition heights ...'.
20) Page 7791 (line 6 and other places): '... ionosphere-free ...'. In section 3 it was '... ionosphere-corrected ...'. I suppose it is the same. Use one or the other consistently.