

Interactive comment on “Ionospheric correction of GPS radio occultation data in the troposphere” by Z. Zeng et al.

Anonymous Referee #1

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General comments:

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

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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.

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,

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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?

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.

Specific issues:

- 1) Page 7783 (line 13): '... as noted by several different researchers.'. Please provide references if possible.
- 2) Page 7783 (line 26): 'In previous studies ...'. Please provide references if possible.
- 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).
- 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?

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5) Page 7786 (line 14 and line 23): It is not clear how $\Delta(h_1)$ and $\Delta(h_2)$ 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?

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

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?

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?

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)?

Technical corrections:

- 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]'.¹
- 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'

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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.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 7781, 2015.

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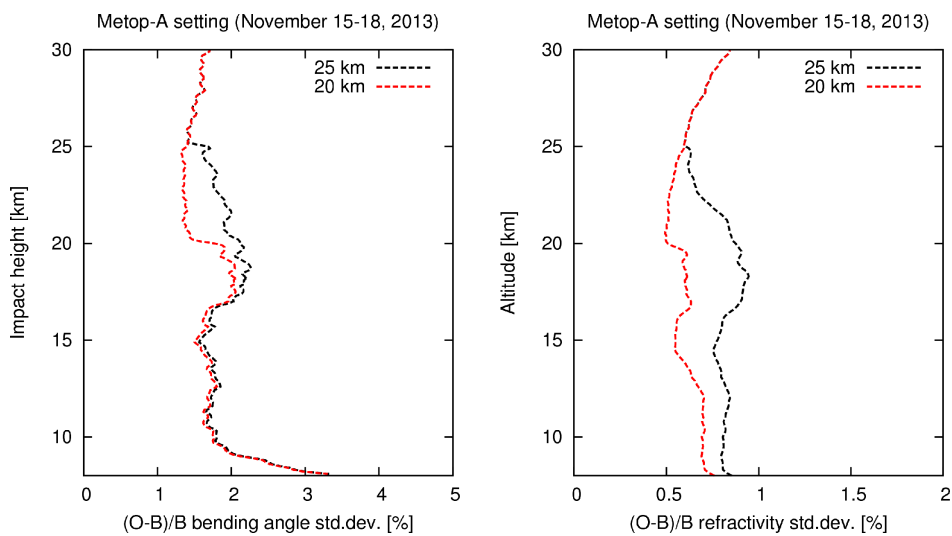


Fig. 1. Standard deviations with respect to ECMWF forecasts for both bending angle (left) and refractivity (right) for Metop-A setting occultations.

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