Response to Review#1

General comments

In general, I think the authors addressed a quite interesting and easy to implement approach for the correction of ionospheric residual errors in GNSS-RO data. They also provided a good literature overview, discussing the ongoing work and problems on this topic over the past years. Their style of writing was also good to follow, however, there are some technical errors/typos in this paper, which leave a bit of a sloppy impression. Furthermore, the paper is quite long and hence hard to read and concentrate on. I would prefer a clearer presentation of the main results, maybe providing some of the figures only as supplementary material.

We revised the paper considerably to take care typos and English. We added Appendix A to provide more discussions on 'bending delay and phase advance' for radio wave propagation in plasma.

Personally, I appreciate the extensive analysis the authors conducted, however some of the information might get lost due to the length of the paper. They also add as an additional study the impact of these RIEs on data assimilation. By itself, this is of course interesting and important to discuss, however, I also feel they could have split the study maybe in two papers.

To first introduce the method and precisely discuss the correction of RIEs on phase delays, and a second follow-up study with the data assimilation experiments. It reads more like a scientific report than a scientific publication, which should aim to concisely summarize and present the main/key findings. In that respect, I recommend the authors to improve the general style, structure, readability, and quality of the manuscript.

We moved a large part of the DA impact discussions to Appendix B, and keep the key results and summary in the main section.

Furthermore, I wanted to address, that to my knowledge a correction on phase delays was already discussed in previous literature years ago, leading to the conclusion that a correction on bending angle is to be preferred. The problem here is that the dispersion residual (different ray paths, L1 and L2) is the most dominant residual, compared to higher-order ionospheric effects. Thereby, a correction on bending angles provides better results, since profiles are studied already on a common impact parameter, instead of on excess phase (see also Syndergaard 2000). Please provide a good and high-quality discussion on this issue. Readers should be aware of that, and understand why you don't see this as an issue and recommend this correction approach based on phase delays.

From the review comments, we feel that one of the key points in this paper was not well communicated. Therefore, we added Appendix A to discuss how RIEs can arise in the case without bending. It's a misconception to attribute RIEs solely to the bending effect.

Appendix A provide more discussions on 'bending delay and phase advance' from radio wave propagation in plasma. Especially, the phase advance due to the faster-than-light phase velocity from propagation in plasma can be mistakenly interpreted as a bending. In fact, it is an independent effect from bending (due to group velocity) in the GNSS-RO excess phase measurement. This is also the major reason that this study argues to analyze the excess phase data, rather the bending data, of which the

latter would mislead what might cause the RIE. In Appendix A, we discuss the situation that RIEs can occur even without bending.

Summarized, I recommend a major revision in order to improve readability and a concise presentation of key results, and to get rid of most of the technical errors (I pointed out just a few, please re-check the complete paper carefully).

The manuscript has been revised to take these advices in consideration.

Specific comments:

L 61: ROPP is a processing package. So it is not "RO processing package **or** ROPP", better "a RO processing package **such as** ROPP"

Correction was made in the revision as suggested.

L 71: "However, the GNSS-RO data infusion requires a key assumption about the α _measurements in which ionospheric contributions can be fully removed by using the sounding from two L-band frequencies"; What is meant with "Infusion", what "key assumption". Please rephrase.

The sentence was modified as:

"However, the benefit of GNSS-RO data in DA requires ionospheric contributions to be fully removed for the α measurements."

L 76: Please specify your statement "unrealistic". Why? I suggest dismissing this specific word.

We would like to emphasize the day-night difference in the solar-cycle variations in the bending angle. The sentence was modified as:

"For example, Danzer et al. [2013] highlighted an unrealistic solar cycle variation induced by the daytime ionosphere in the simulated atmospheric bending angle."

L 102: I think there is a "minus-sign and absolute value" missing, α_RIE = - |κ|(α1-α2)2... please check for the correct interpretation of this method.

This has been corrected, along with the sentence that describes this expression.

L 106 to 109, 121-122: please provide a bracket around (α1-α2) in the text.

Changed accordingly.

L 112: Danzer et al. (2020) validated the kappa-corrected RO data against ERAint, ERA5, and MIPAS data. Please correct that statement. Furthermore, the warming was calculated solely based on RO, as a bias between RO-data with and without kappa-correction. The sentence reads wrong.

Changed accordingly. The new sentences read as follows:

"The κ model predicts a lower RIE value during the daytime and higher F10.7. Danzer et al. [2020] further validated the κ-model for RIE correction with the European Center for Medium-range Weather Forecast reanalysis (ERA-Interim, Dee et al., 2011; and ERA5, Hersbach et al., 2020), reporting warming (0.2 – 2 K) effects at 40-45 km prior to the κ-model correction (0.01-0.05 μrad). Using a different model, so-called bi-local correction approach, Liu et al. [2020] showed that the α_{RIE} values are comparable to the κ-model with an amplitude < 0.05 μrad but the standard deviation of α_{RIE} is larger than its mean at all heights."

L 240: The RIE varies, as you state, with local time, season, solar cycle, solar activity, and RO receiver type. Maybe mention also geomagnetic term here. However, what I wanted to state, the bi-local correction is able to compute these variations. Please see, (i) Syndergaard and Kirchengast (2022) introducing the theory, and as application studies (ii) Liu et al. (2020): comparing kappa and bi-local as an initial study on bending angle, (iii) Liu et al. (2024): comparing kappa and bi-local on a larger scale also on temperature.

We have included a brief review on the magnetic field impact in the introduction, as well as the papers by Syndergaard and Kirchengast (2022) for the 3D effect and Liu et al. (2020) for bi-local modeling. We can't find the reference Liu et al. (2024) to comment on the k-method and bi-local comparisons. We did observe and cited the similar RIEs amplitudes [Liu et al., 2020, Fig.5 therein] between the two approaches, which showed mostly negative RIE values.

L 282: Related to that above statement, in Section 3.2, where a discussion is done based on a comparison with the kappa-correction, I suggest adding a discussion based on a comparison with the bi-local correction too. The bi-local correction can account for negative and positive biases resulting from including the geomagnetic term (see especially the analysis of Liu et al., 2024).

We provided a discussion on potential geomagnetic effects in section 3.3 with Fig.13. We found a weak dependence on B-field but a stronger connection to sporadic-E (Es). The latter is perhaps related to the 3D effect in the calculation outlined by Syndergaard and

Kirchengast (2022) who divided the ray trace model into the near and far side of the tangent point. If Es splits the L1 and L2 paths at the tangent point, the RIE would arise due to the near-side propagation both from phase advance in plasma and phase delay in bending.

We added more discussions in section 3.3 on this point.

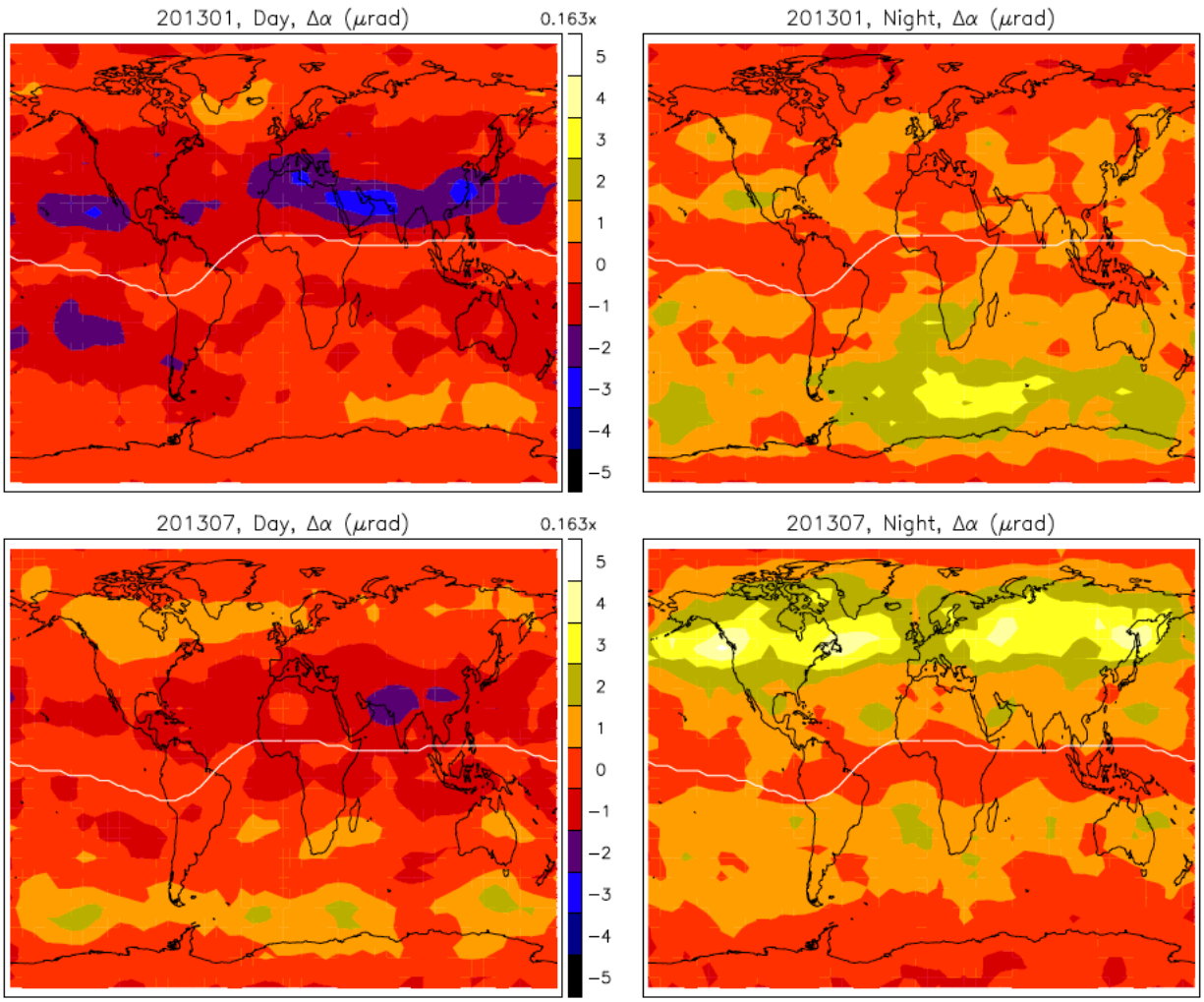

Fig.13. Geographical maps of the $\Delta\alpha$ derived from COSMIC-1 $-d\phi_{exL1}/dh_t$ measurements for January and July 2013; the white lines display positions of the geomagnetic equator.

L 294 onwards: is this (Δα1- Δα2)2 a typo? I was confused. Shouldn't it be: (α1- α2)2? Please correct. If I am wrong, please make this clearer in the paper, and introduce the meaning properly. Thanks for this.

This is correct.

In this paper we try to reserve α for conventional definition of bending angle and $\Delta\alpha$ as an approximation of α from the vertical derivative of excess phase profile.

We made this clear right after Eq.7 where $\Delta\alpha$ is introduced.

L326: You introduce σ here for the first time, please make sure to introduce it already together with μ in line 320.

Done.

L 508: As you already address here, the second-order error can have positive and negative contributions. Please discuss it compared to the bi-local correction.

We added more discussions in section 3.3 on this point.

L545: This is an important conclusion: What about missions with a lower RO top height than 120km? Is this approach as a conclusion not recommended? Which missions does this concern? Please discuss. Further, what is the consequence for a complete re-processed multi-satellite data set (climatology), if this correction cannot be applied to all missions. Does this introduce a problem?

We are troubled by these missions as well.

Shortly after we identified the importance of Es in GNSS-RO [Wu et al., 2005], we recommended to all operators to raise the RO top height to >120 km. But Metop, Kompsat, TSX and TDX, and FY3C/D among a few, did not change their operation. Recently, Metop and FY3 have raised the RO profile top in the normal operation.

For those missions with a lower RO top, it would require a climatology built upon other missions that can be parameterized as a function of latitude, longitude, local time and solar cycle.

We added this in the discussion section.

In general, in figures.

- Please provide units in a square bracket, e.g., [μrad], also for Latitude [°], solar local time [h], and so on...

This change would require a lot of rework on the figures made previously. Instead, we made it clear that all variable units are consistent in all figures.

- Also, the colorbars with 0.05x, or 0.07x are very confusing. Please provide a clearer solution here. Usually, one indicates the unit above or below the colorbar, and the range, which I guess means in your case 0.05 times the range from 0 to 5, might be adjusted, or the pre-factor added to the unit.

We added the following clarification in the figure capture:

"All color numbers have a scale factor indicated at the top of each colorbar, and has the variable unit indicated in the () bracket."

- What were the exact definitions for a "day" time and a "night" time window?

We used the solar zenith angle 90 degrees to separate day and night. A clarification is made in the revision.

- Fig. 12: there is a strange offset in the colorbars.

As shown in the time series, the January RIE is generally larger than the July, and both are larger than those from the equinoxes. The colorbars are scaled differently to account for these differences.

- Fig. 20: increase x,y-labels.

The font size is increased in the revision.

- Please make sure that figure captions are located below the figure, and not land on the next page (see Fig. 1, Fig. 12, Fig. 20). At the beginning I thought they are completely missing. This helps the readability.

We will make sure that this shows well in the final print.

Technical corrections:

p. 2: Please remove the table of contents.

Done!

L 13: formulation is off, “therefore residual ionospheric error (RIE) is critical to accurately retrieve atmospheric temperature and refractivity”; reformulate

It is re-phrased as follows:

“ Because the magnitudes of the RO bending angle are small at these altitudes, quantifying and removing residual ionospheric error (RIE) are critical to accurately retrieve atmospheric temperature and refractivity.”

L 21: formulation “and in small-scale temperature variance of the RO retrieval”. That is not a clear sentence.

In the revised manuscript, it is put in a separate sentence:

“RIEs are likely to impact the RO temperature retrieval by inducing a small-scale variance that is solar-cycle dependent.”

L 27: Typo: “RIF”

Corrected.

L 101: introduced “the” so-called kappa-method

Corrected.

L110: delete the word “had”, use instead “Liu et al. estimated”

Corrected.

L 141: “wehre”

Corrected.

L 142: “an RIE”

Corrected.

L 162: In the case „of“ Fig. 1

Corrected.

L 209: Eq. 7: Bracket after the equation "…, with …"

Corrected.

L327: Please insert "commas" between a list of symbols such as Δα, σ, μ and in general at several text places….

Corrected.

L 587: … range, like Es, as well as an extended…

Corrected.

L603: … greater **than** …

Corrected.

References, p. 36 onwards:

Please make sure that the references are given in a uniform way. For example, please compare the style in Angling et al. and Bai et al.

- Years are given after the list of names of the authors. Sometimes you put it at the end of the citation.

- Doi sometimes missing.

- Make sure that all links of the papers are imported as a link.

Corrections are made.