

GNSS-RO Residual Ionospheric Error (RIE): A New Method and Assessment

Dong L. Wu et al., amt-2024-51, AMT-Review, Round 2: 26-09-2024

General comments:

In general, the authors addressed finally all questions. (At the beginning not all answers were provided at the AMT platform. Maybe an upload mistake?) However, when I checked for the **proposed modifications, I partially couldn't find them in the manuscript**. I am not sure what has happened. **Maybe the wrong track-changes file was uploaded?** Maybe I made a mistake and downloaded the wrong file. But I suggest to the authors to carefully go through the manuscript and check, if everything is included.

Further remark: After line 240, I stopped comparing if everything is included. I expect the authors to do that.

Please revise the manuscript carefully and provide the correct track-changes version.

A few specific examples and comments:

My old comment: 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.

Answers from the authors: 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."

The correction was not made in the manuscript. Still the old formulation included.

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

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

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

I want to address once more my question here: "**why unrealistic**". It was a study directly performed on RO profiles, calculating the bending angle bias, and a further study with simulated data using NeUoG. The observed RO bending angle bias and simulated RO bending angle bias overlap. Furthermore, the F10.7 index was rather high in 2001/2002 years. Please remove "unrealistic" and soften the wording, such as "For example, Danzer et al. (2013) observe a rather high solar cycle

variation by the daytime ionosphere in the simulated and observed atmospheric bending angle bias.”

My old comment: L 102: I think there is a “minus-sign and absolute value” missing, $\alpha_{RIE} = -|\kappa|(\alpha_1 - \alpha_2)^2$... please check for the correct interpretation of this method.

Answers from the authors: This has been corrected, along with the sentence that describes this expression.

There is still no minus sign in the equation.

My old comment: L 106 to 109, 121-122: please provide a bracket around $(\alpha_1 - \alpha_2)$ in the text.

Answers from the authors: This Changed accordingly.

There is still not always a bracket around $(\alpha_1 - \alpha_2)$

Further comments

L 149: as ‘a’ misconception

L153: “higher-order”

My old comment: 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.

Answers from the authors: 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 eUect 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.

Where is the discussion. I couldn’t find it.

Here is the reference:

Liu, C., Danzer, J., Kirchengast, G., Haas, S. J., Proschek, V., Schwaerz, M., ... & Wang, X. (2024). Understanding ionospheric and geomagnetic effects on residual biases in radio occultation data for stratospheric climate monitoring. *Journal of Geophysical Research: Space Physics*, 129(5), e2023JA032110.

In general, in figures.

My old comment: Please provide units in a square bracket, e.g., [μ rad], also for Latitude [$^{\circ}$], solar local time [h], and so on...

Answers from the authors: 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.

But this is not correct. **Units are supposed to be in a square bracket.** Otherwise, you would read it as an equation. Please correct!

My old comment: References:

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.

Answers from the authors: Corrections are made.

There are still not all references consistent. E.g., 871 and others

L877: Wu is in bold.