Response to Referee #3

(***Referee report***: *https://doi.org/10.5194/amt-2023-28-RC3*)

Manuscript:

Innerkofler, J., Kirchengast, G., Schwärz, M., Marquardt, C., and Andres, Y.: *GNSS radio occultation excess phase processing for climate applications including uncertainty estimation*, Atmos. Meas. Tech. Discuss. [preprint], https://doi.org/10.5194/amt-2023-28, in review, 2023.

The authors thank the reviewer very much for his comments, which helped to further improve the manuscript, and his commendation. We thoroughly considered all comments and carefully revised the manuscript accounting for them. Below are our point-by-point responses.

Comments by the reviewer are cited black upright, our responses are red. Line numbers used in our responses refer to the original AMT Discussions paper and text updates in the revised manuscript are quoted below in blue)

Summary

This manuscript provides an overview of Level 1a (L1a) processing of radio occultation (RO), a thorough description of the authors' newly-developed processing system, and demonstration of the quality of said processing system. L1a processing transforms the raw measurements between GNSS satellites and low-earth orbiting RO receivers into measurements of excess phase –the phase "added" by propagation through the atmosphere. Precise estimation of this excess phase is critical to the processing chain of occultation events from GNSS signals to, e.g., temperature –a so-called essential climate variable (ECV). Thorough documentation, quality control, and uncertainty estimation is important for the community to understand what was done and why, and how the choices in processing affect the resultant ECVs.

I acknowledge and applaud the authors for this manuscript. The advancement of RO science at their center is evident in their completeness of the document. I certainly learned a great deal about the steps that go into excess phase retrieval, and I believe that is an important contribution to our subfield and, more broadly, atmospheric science. I think the quality of the retrieval is showcased nicely in their analysis in section 4. And, should they be coming, I look forward to the next publication(s) on later steps in the RO processing chain.

My primary, minor comment for the authors to address relates to the use of ERA5 analysis for their sensitivity analysis. Typically, one thinks that comparing an observation to an analysis that assimilates that observation would lead to artificially small difference statistics. I don't suspect that the use of, say, ERA5 forecasts instead will lead to significant changes in the calculated O-B values. But, I do think it warrants consideration and some discussion.

Regarding the use of ERA5 analysis used in the sensitivity analysis the authors are aware that the analysis assimilates RO measurements as stated in Line 150 of the manuscript. However, the retrieval uses the independent ERA5 short-range forecast fields for the extraction of the modeled excess phase profiles used in the quality and uncertainty evaluation. We also do recognize that the use of ERA5 short-range forecasts instead of the analysis will not change the calculated O-B values significantly and regard the use of the ERA5 analysis for the sensitivity analysis sufficient. However, we of course do plan to incorporate independent datasets in future dedicated validation studies.

Line-by-line comments

Line 35: suggest “By observing Essential…”

Line 56: suggest “Evaluation of basic…”

Line 78: suggest “subdivided into sensitivity…analyses”

Line 152: suggest “…data suit the…”

Line 154: suggest removing “in order”

Line 175: “that builds on”

Thank you, we updated the manuscript following the suggestions above.

Line 205: suggest “surface at the point…line connects between”

The description of the mean tangent point location in space and time was rewritten based on a comment by another reviewer, in order to better describe the actual calculation of this occultation event reference location:

“The selected reference location of an event is defined on the Earth’s ellipsoidal surface at the time when the straight-line connection between receiver and transmitter satellite is tangent to the Earth’s surface (WGS-84/EGM2008, cf. Figure 3 for measurement geometry).”

Line 207: apologies if I missed it, but is the mean tangent point identified in Fig. 3? It’s not needed to add it to the figure if it is not already there.

The term tangent point might be misleading, since it reflects a reference point at the time when the straight line connection between GNSS and LEO is tangent to (i.e., just touches) the Earth’s Ellipsoid. Figure 3 depicts the schematic of the occultation event geometry at that point. We added the following description to the figure caption and identify the mean tangent point in the figure:

“The schematic depicts the occultation event at the time when the straight-line connection between GNSS and LEO satellite is tangent to the Earth’s ellipsoidal and defines the mean tangent point (MTP) in this way.”

Line 222: “In principle”

Line 278: recommend removing “also”

Line 279: recommend removing “level”

Line 304: “occultations that may”

Line 320: closed parentheses that is unmatched

Thank you, we updated the manuscript following the recommendations above.

Line 357: the sentence ends with a fragment. Consider rewording.

Rewrote, the sentence now reads as follows:

“The atomic clocks aboard the GNSS satellites are considered stable over the short duration of an occultation event of approximately 1 to 2 minutes with an accuracy of between 10^{-11} and 10^{-12}, while more up-to-date clocks feature even higher accuracy (Griggs et al., 2015; Hauschild et al., 2013).”

Line 442: given that Eq. (9) has a factor 2k*pi, should this be “are always within 2pi”?

This was corrected in the manuscript and now states:

“… are always within $\pm\pi$: …”

Fig. 7: the middle and right panels cut off quite a bit of interesting information due to the bounds at +-15 cm. Is it possible to expand/shift these bounds while retaining resolution of some of the fine-scale features from 10+ km?

I am afraid not, since we already tried to find the best match between indicating the characteristics below an altitude of 10 km and not missing the fine-scale variations at higher altitudes. We hence preferred to keep this as is.

Line 493: “Schwarz et al.” ends up in parentheses when it should be outside of them.

Thank you, changed in manuscript.

Table 4: if it is sensible to do, please include the total rejection fraction in the table caption.

The total rejection rate is now included in the updated version of the manuscript:

“Overview on the rOPS L1a quality processing. All parameters apply to Metop/GRAS data. Middle column separates the total data rejection rate f_{QC} of 4.17 % for all data (9 months) processed by WEGC for this study in rejection fractions for every single quality control step.”

Line 540: suggest “profile is done by checking”

Line 567: “despite averaging”

Line 656: suggest “worthwhile to take into account”

Line 664: references are given in Bibtex format

Line 665: suggest removing “core-strength” or rewording

Line 682: references inside parentheses have parentheses

Thank you, we updated the manuscript following the comments above.

Section 4.1.3: what are the counts of the co-located Metop profiles used in this section? The robustness of the results is not clear without that information. If it was provided elsewhere, please reference to it.

The information was now added in the latest version of the manuscript:

“This results in an adequate number of co-located profiles for robust statistics: Metop-B vs. Metop-A: 2708 (2013-JAS), 1641 (2020-DJF); Metop-C vs. Metop-A: 1797 (2020-DJF).”

Lines 717-718: “with the help of which the RO events” is not clear. Consider rewording.

The authors rewrote this part in order to make it more clear:

“Prior to the comparison and the calculation of profile differences, the external L1a profiles from EUMETSAT and UCAR were converted to rOPS-L1a format, including the calculation of consistent and unambiguous event identifier information and some format alignments of the datasets (Section 2.2). This enables to reliably match the RO events from the three different processing centers.”

Fig. 11 and text: is this given as WEGC minus EUMETSAT? Please make this clear.

The figure header states “WEGC-Bernese vs. EUMETSAT” and we updated the figure caption in order to provide a better description:

“Sensitivity analysis results from comparing WEGC rOPS L1a excess phase profiles from Metop-A (blue), Metop-B (red), and Metop-C (black) based on WEGC-Bernese orbits minus excess phase profiles based on EUMETSAT POD solutions, …”

Also the text in Lines 694-695 was revised:

“Comparison results of rOPS-processed excess phases based on WEGC-Bernese orbit processing against using EUMETSAT POD exhibit a similar agreement across all vertical layers, time periods and satellites.”

Line 779: is there an example percent or range of representative percents that could be given?

A representative percentage envelope number was included in the updated text of the manuscript:

“In the mesosphere and upper stratosphere, the estimated random and systematic uncertainty components are small at millimetric level, while towards lower altitudes, where the absolute excess phase increases rapidly, the estimated random uncertainty component amounts to several centimeters (which is still very small at levels below 0.02 % in relative terms in the troposphere, though).”