Author’s Response to Referee #2 Comments

We thank the reviewer for the careful assessment and, despite the number of comments and reservations raised, for thinking that the outcomes of the study could be useful for the scientific community, as well as for the many helpful comments for further improvement. We carefully considered and answered the comments below, starting out with a statement to the overall “Conclusions” of the reviewer (comments quoted in *italic with gray background*, with the answers below each comment).

As part of some answers, as found appropriate, we also included initial versions of revised formulations, to indicate in which way we plan to improve the respective explanations in the revised manuscript. Furthermore, several answers are in an initial form and will be further consolidated along with the actual manuscript revision. Before we start the latter, we plan to seek initial feedback from the editor as to whether our intended revisions are deemed appropriate; in line with the AMT editorial procedures (key quotes: “…submission of a revised manuscript…is encouraged only if you can satisfactorily address all comments… In case of doubt, please ask the handling associate editor directly whether they would encourage submission of a revised manuscript…”).

Referee #2 – Conclusions:

Based on the limited innovations of this paper, some potential flaws in the methodology, and very limited discussion, I suggest a major revision of the paper. In the revised version, the authors should properly address the below mentioned issues. The extra analysis can be added in the supplement or appendix in order not to lengthen the paper. In the current form, the study raises more questions than it undoubtedly answers. Without performing a much more detailed analysis, I find the paper unsuitable for publication in EGU AMT. I am mostly concerned about the innovations of this paper – it seems we have not learned much, or that the authors have only confirmed what is known already. But I still do think the outcomes of the study could be useful for the scientific community.

Introductory author’s statement (as a general answer, aiming to clarify the intended scope of the study):
First of all, we would like to explicitly thank the reviewer for this very thorough assessment of our manuscript, which will clearly contribute to substantial improvements in the revised manuscript. We appreciate the invested time and effort and think that many of the suggestions and editorial comments are helpful indeed. Furthermore, we also plan to carefully re-check, and partly refine, several of the computations and evaluations, since we agree it is important to have a more in-depth basis for some of the results shown and discussed (for example, and in particular, related to what the added value of the v-component, i.e., the meridional wind estimations, actually may be).

This being said, we want to emphasize that the main goal and focus of this study is to test the utility of radio occultation data for wind field monitoring across the equator. Outside the equatorial band in focus here, the utility was analyzed in the “sister study” by Nimac et al. (2023), and we aim to close this remaining gap across the equator. We do have evidence that RO-derived wind fields have a clear potential to provide an “added-value”, with focus on long-term wind field monitoring based on monthly mean fields at meso-scale resolution, where its unique combination of high accuracy and long-term stability (=multi-year to multi-decadal stability) can play out. This is where also the complementary value of re-processed RO-based climate wind field data records may sit, while fully acknowledging that for “weather variability” (sub-monthly timescales of hours and days) the value of the “wind information content” in RO data will best unfold via data assimilation into NWP and other atmospheric

(re)analysis systems. We hence think that this study, accepting its (limited but clear) scope and focus, does definitely have value to the RO-related readership of AMT, but also to the broader atmospheric scientific community reading AMT, since to our knowledge this type of analyses has not been done and published so far.
The paper therefore has a clear methodological focus (and we will improve the introduction, as the reviewer suggested, to make this focus and scope quite clearer at the outset), which is why we submitted it to AMT, and in particular to an AMT Special Issue that focuses on RO-related studies. Clarifying this focus, we admit and agree, that we do not focus on analyzing or describing at the same time atmospheric dynamical processes as such; this is not within the scope of this study (while we confirm, well spotted by the reviewer, that we need to improve on how we present and describe the issues around the meridional wind component).
Specifically, the retrieval of RO climatic wind field data is still a quite new topic within the RO community and basics of the study were presented at the International Radio Occultation Working Group Workshop 2022, which is why we think the study also fits well in this AMT Special Issue; and the interest in this topic is available in the RO community. Once the utility of RO-based wind field datasets is established, atmospheric dynamics aspects, with focus on multi-year to decadal variability and changes, can and will be the focus of future studies. We emphasize this, since we plan to keep the scope of this study as currently laid out, i.e., we do not intend to much expand the discussion of aspects in section 4.3 towards broader application aspects. However, we are indeed thankful to the reviewer for pointing out several important issues of re-checks and evaluations we anyway will have to do for the revised manuscript within the methodological scope. Please see more details in the answers below on how we plan to implement these revisions.

Major Comments:

1) *In the introduction, the authors state "In this study [you] aim to close the gap in RO wind field computation across the equator". However, I am not able to easily identify this gap based on the introduction you provided. Therefore, I suggest that the authors clearly and directly identify this gap. What are the innovations that this study addresses, how do you aim to expand the present knowledge, why is the potential new knowledge important, can we use it in NWP or climate science, etc.? What new can we learn from this study in comparison to other studies? Please, elaborate in more details in the revised manuscript.*

Please, see introductory statement above, which refers to those questions. Furthermore, we will emphasize these issues more strongly in the introduction.

2) *Throughout the paper, the WMO thresholds for data quality of winds (+- 2 m/s and +- 5m/s) are mentioned. It would be nice to elaborate to which of the following data do the thresholds apply:*

Instantaneous winds

Instantaneous zonal-mean winds

Monthly-mean winds (applied in Nimac et al., 2023)

Monthly-mean zonal-mean winds (e.g., Fig. 2)

Monthly-mean zonal-mean latitudional-band-mean winds (e.g Fig. 3) *something else?*

In the paper, you are mentioning the threshold for different of these options, but the thresholds are not equivalent for e.g., monthly-mean winds and monthly-mean zonal-mean winds, they are certainly more strict for the latter.

Thank you for this reference. We know and acknowledge that WMO also provides more detailed and differentiated requirements, for different spatial and temporal resolutions, as well as for different applications. In general, we focus here on climate-related winds, with a fairly strong spatial and temporal averaging. For these monthly-mean winds with a horizontal averaging around 300 km, we use the 2 m/s requirement as the indicative threshold (we may possibly drop the 5 m/s thereshold). Furthermore, we found in the study of Nimac et al. (2023) that a specifically given threshold has the tendency to be exceeded rather fast/abrupt at a specific geographical location, making not a huge difference whether we use an indicative threshold of, for example, 1 m/s or 2 m/s.
We note that the advantage of RO-based long-term wind records is their unique potential of being also temporally stable, which is another WMO requirement of stability. That is, if we consider monthly winds with accuracy within ±2 m/s, this would be consistent with a decadal stability of roughly ±0.5 m/s per decade, which is the associated WMO-based requirement we use to evaluate long-term stability.
Having said this, we will improve the discussion and reasoning for which indicative thresholds we see fit for our study from the portfolio of requirements of the WMO.

3) *Furthermore, there are inaccurate claims at different instances in the introduction, which need to be revised, in relation to the references pointing to not yet revised studies.*

For example, the study Nimac et al. (2023), in revision at the same journal, does reproduce ERA5 monthly and zonal-mean geostrophic winds rather well (their Fig. 6). It is very important to state it precisely, as suggested by the underlined text above.

On the other hand, Fig. 7 in Nimac et al. demonstrates that the monthly mean ROg-ERAg winds (without zonal averaging) often exceed +- 2 m/s bias threshold. Comparing their Figs. 6 and 7, it is also clear that +- 2 m/s threshold is often only achieved in the zonal-mean monthly-mean winds due to compensating biases along the latitude circle.

We will carefully check the formulations again and revise if necessary.

4) *The computation of the geostrophic winds is very sensitive to the applied resolution of the input data, as you have shown in Figure 1. To avoid the zig-zag pattern at high-resolution, the authors should either use higher-order symmetric approximation of the derivatives (instead of first order forward) or compute the derivatives exactly using a spectral method. At least, the authors should prove that the choice of numerical approximation don't play a major role in the zig-zag pattern. Furthermore, I would be curious to see, how the choice of averaging period affects the "optimal" resolution (only briefly mentioned in line 165). I guess 0.5-degree resolution would not be an issue, if the data averaging was 3 months instead of a single month, but I am eager to see your results. On the other hand, I ask what the reason is for testing equatorial balance in higher-resolution reanalysis data, if the RO data are only available at 2.5-degree resolution.*

In general, we tested different formulations of the derivative operator. We tested forward, backward and central difference, and the differences in the results were essentially negligible. However, we want to take up your suggestion and calculate other approximations of the derivative, to see if it has an impact on the results. We will discuss the outcome in the method section.
About the suggestion of a higher, seasonal-temporal averaging, with a finer spatial resolution. In a short answer, we quickly tested it, and it made no fundamental difference, leading to a zig-zag pattern similar as in Figure 1.

However, we emphasize that in this study the aim is to derive monthly wind data for climate analysis and climate monitoring, following the line of work from Nimac et al. (2023). In the future the goal is to derive for the full available time-series, monthly 2.5 x 2.5 wind products (roughly from 2006 onwards).
A higher spatial resolution is on the one hand not recommendable for RO data and this time frame. This would require an even higher daily occultation statistics, which is not the case at the moment (see also Angerer et al. 2017, Ladstädter et al. 2023). On the other hand, as a further physical reason, the geostrophic and equatorial balance will also not hold well at higher temporal or spatial resolution, leading to larger ageostrophic contributions (see also the reviewer's thoughts in comment 5 below). Depending on our re-check results we consider also to drop the 0.5° resolution case in Figure 1, since it is a sub-degree-scale that is not part of the RO-based wind field records we aim at (for which no finer horizontal resolution than 300 km is thought).

5) *The equatorial balance equation for the zonal wind works reasonably well in the stratosphere in the equatorial area, but we know this already from other studies, e.g., Healy et al., 2020. The meridional wind deduced from equatorial balance equation does not seem to reproduce the original winds, as shown in Fig. 2d,e and Fig. 7b,h,e. The explanation why it fails is speculative and unconvincing ("This could be because the v component contains a derivation with respect to latitude as well as longitude which is computationally not as robust as the second derivative with respect to latitude."). Apparently, the balance is not satisfied in the deep tropics.*

Another possible reason is that the steady-state assumption (neglecting temporal derivatives of meridional wind) might not be valid for meridional wind component. As this is one of the key results of this study, the authors should do more effort to analyse and explain it. You could do this by inspecting the magnitude of the terms in the meridional derivative of full Euler equation for meridional wind.

Is the inability of equatorial balance equation to reproduce meridional winds also the reason why other authors opted not to use it? I also find it rather disturbing that the analysis of meridional wind was only performed for a certain longitudinal band, - 10 to 10 degrees longitude? Why not performing similar analysis also for other bands?

First of all, we would like for your insightful feedback, which will help to improve the manuscript. We already tested the meridional wind component on a zonal-mean average (which is close to zero), as well as for different longitude sectors. In the reviewed manuscript, we just showed the results exemplary for the longitude sector around the prime meridian (−10◦ to 10◦ longitude), keeping in mind that the other sectors were also studied and qualitatively showed similar behavior (we acknowledge that this was not made sufficiently clear).
But we agree with your comment that we need to more carefully re-assess the analysis of the meridional wind component, to see how the equatorial balance holds, and where the "added value" can be isolated (if it exists). Dependent on these re-check results we will decide on the way of how we (may) include the meridional wind component in the revised manuscript. Even if we would find it is not working well, it will be important information at least to the RO community, since several (unpublished) discussions linger around on what the "added value" possibly is.

6) *I like the results presented in Section 4.3. These are very interesting, and the revised paper should build on that, while presenting a detailed analysis why the geostrophic approximation provides an even better reconstruction.*

Thank you – we also see this as a specifically interesting result. However, a deeper physical analysis is beyond the scope of this specific study (see our introductory statement as the answer to the "Conclusions" section above).

7) Descriptions in the figure captions should be more accurate, and English should also be improved at many places.

Thanks, we will revise figure captions and aim to improve the writing.

Specific comments:

2-3: Without "availability". Consider the following reformulation:

Greater availability of wind data is particularly needed, especially in tropical regions and the southern hemisphere.

We will revise it accordingly.

9-10: what do you mean by "volatile in derivation"? Please, express it more clearly.

We will rephrase to:
"From analyzing first the zonal and meridional wind component, we find that the meridional wind component is more sensitive and vulnerable in the numerical implementation, however the total wind speed benefits from a computation of both wind components."

20: Bauer et al. is not a good reference in this context, as it only briefly mentions what is missing in the observing system, but does not actually provide any content. Instead, I suggest citing Baker et al., 2014.

Thank you for this suggestion.

31: I would exclude AMVs here as they are almost global

Thank you for this information.

33: ADM Aeolus does not really perform 3D wind profiling as it only measures a profile of a projection of the wind perpendicular to the satellite track, which is quite similar to the zonal wind component.

32-34: This needs to be reworded. Not only that Aeolus "has potential", but it has also demonstrated its usefulness, which has been described in several studies, such as Rennie et al., 2021, Pourret et al., 2022

Yes, thank you for this information. We will rewrite the sentence accordingly.

30-35: I think it is important to mention that much of the wind information is nowadays obtained also implicitly in NWP to initialise the forecast, i.e. through 4D-Var humidity and/or ozone tracing (Geer et al., 2018 ; Zaplotnik et al., 2023), as well as through the geostrophic adjustment, and directly through the background-error covariances, especially where the geostrophic balance applies. The microwave humidity sounders are now the most important observation system in ECMWF IFS, in large part due to aforementioned tracing effect.

Yes, we will do so.

47-52: It would be informative to mention the horizontal resolution as well, not just the vertical resolution. It could also give reasoning for my further comment line 78.

50-51: the so-called sweet spot for GPSRO is 10-32 km, see Semane et al., 2022, their Fig. 1.

We will add the information about the horizontal resolution in line 50. Furthermore, we will also rephrase the sentence regarding the vertical core region of RO in the following way (initial formulation, may be refined).
"RO data cover well the complete stratosphere, with a core region of high quality in the upper troposphere and lower stratosphere (Zeng et al., 2019; Steiner et al., 2020), having a typical horizontal of 200 km to 300 km (e.g., Kursinski et al. 1997, Foelsche et al. 2011)."

Foelsche, U., S. Syndergaard, J. Fritzer, and G. Kirchengast
Errors in GNSS radio occultation data: relevance of the measurement geometry and obliquity of profiles
Atmos. Meas. Tech., 4, 189-199, doi:10.5194/amt-4-189-2011, 2011

64-65: It is important to mention that Healy et al. (2020) applied equatorial balance equation only in the stratosphere, using zonally and monthly averaged data (for apparent reasons). It is not clear, whether such balance holds also instantaneously at particular location and time instance.

Yes, we agree that in the deep tropics the balance probably does not hold. We will carefully re-asses the meridional wind component (see major comment 5), and revise the manuscript based on the results.

70: I would exclude "going further towards equator than other studies", as this might not be entirely justified by results in their Figs 6 and 7.

Yes, we will do that.

78: what is the reasoning for the choice of 2.5deg x 2.5 deg grid for the assessment of the quality of the approximation? Is it done to follow Nimac et al., 2023, or is there any physical reasoning, e.g. the horizontal resolution of the RO data? If so, it has to be explicitly written to avoid speculation. Note that by increasing the resolution, the greater portion of the total wind is represented by ageostrophic motions, which are unbalanced.

86: The magnitude of ageostrophic contributions are vastly influenced by the resolution at which one performs the analysis. See for example the study of Bonavita (2023), their Fig. 5.

Thank you for your consideration. Yes, we used the same approach as in the study of Nimac et al (2023). The goal in the future is to produce a long-term monthly RO wind product on a 2.5 x 2.5 grid. Another reason is that a higher spatial and temporal resolution is not feasible for RO data. Finally, providing a physical reason, finer resolutions (temporal and spatial) will increase the ageostrophic contributions (see also answer in major comment 4).

100-110: It appears a bit strange, that you use derivative over (x,y) in equatorial balance equation and (lambda,phi) in geostrophic balance equation. Choose one set of variables for both.

Yes, we can change the formulation in the paper.

125: do the WMO-OSCAR, 2023 requirements apply to instantaneous winds, monthly means or monthly and zonal-means? This is very important.

Please refer to our answer to major comment 2. Furthermore, please note that we don't refer to instantaneous winds, and focus on monthly climate related winds.

131: "to limit the length of the paper" is a rather strange argument. You can always provide a supplementary file in the EGU Journals.

We will rephrase that: January was chosen as a representative month for the plots. The other months were analyzed and show no fundamental different behavior.

142: I do not agree with that statement, as mentioned in the General comments.

Please refer to our answer to major comment 4.

Figure 1: are the zig-zag features similar at other latitudes?

Figure 1: it should be mentioned in the figure caption what the dashed lines represent

Figure 1 shows the zonal component computed using the equatorial-balance equation. At latitudes north and south of 40° the zig-zag feature gets smaller. The regions outside of the equatorial region are not shown in the figure, because they are not the focus of this study, since we specifically analyze the equatorial-balance equation across the equator.
Thank you for noticing. We will add in the figure caption the description of the dashed lines, which represent ±2 m/s and ±5 m/s thresholds.

154: does it mean that no correction due to latitudinally varying centrifugal force is applied?

Yes, we focus on geostrophic balance.

161-162: is 600 km the halfwidth of the Gaussian or is this the localisation threshold? If so, what is the halfwidth of the Gaussian smoother?

163: the smoothing procedure is rather strange – first you do a Gaussian smoother, then you further perform binning. Can you provide an example in the supplementary, how the raw fields evolve in your preprocessing routine.

Thank you, we will carefully check the description of the methodology and revise it accordingly. In general, the 600 km corresponds to the distance from the grid point within which the profiles contribute to the grid point mean. The profiles are weighted according to their longitudinal and latitudinal distance to the center with a gaussian function, with a standard deviation of 300 km (lon) and 150 km (lat).

We also intend to add in the description the following citation (in proper formatting, of course):
Ladstädter et al., OPAC-IROWG 2022 conference, Talk on gridding strategies, Seggau, Austria, September 8, 2022.

The smoothing procedure was performed due to the fact that we used preprocessed RO data from Nimac et al. (2023), as we wanted to be as comparable as possible. When working with RO data and the equatorial balance equation, it became clear that 2.5°x2.5° is not feasible. Further binning improved the quality of the reproduced wind field.

193: I would not say "it is not that well reproduced", I would say it is not reproduced at all (Fig 2. d,e). Given the large relative differences between v_o and v_eb, I would suggest to add a new figure of relative differences. Based on Fig 2d,e, I also find it very unconvincing to use equatorial balance for meridional wind component at all.

193-194: I find the explanation for the mismatch between v_eb and v_o rather unconvincing. I would say that the derived physical balance does not apply for meridional wind. If you look at the derivation precisely, there is an important assumption of steady state flow. However, the tropical disturbances are not steady, especially the features involving meridional flow such as MRG waves.

195-196: this might be coincidental. What is the reason for better V_eb if it contains wrong v_eb? How can it be shown?

Please refer to the answer in major comment 5. In a first analysis the total wind speed improved, but we see a further cross-check of the meridional wind component is necessary. Based on the results, we will see if a longitudinally resolved meridional wind component will provide an added value to the total wind speed.

207: tropopause in the deep tropics is rather found between 100 hPa and 70 hPa, instead of 200 hPa

Thank you for this comment. In principle we follow the study of Nimac et al. (2023), using the same respective three levels. But we will rephrase and write:
"Our focus lies on the three representative levels, 200 hPa, 50 hPa and 10 hPa, relating roughly to the upper troposphere, lower stratosphere and middle stratosphere."

213- : It is important to note that the similarity between ERA5 v_eb and RO v_eb does not imply that the use of equatorial balance is meaningful due to large differences in ERA5 v_o and v_eb. It only suggests that the input geopotential data of ERA5 and RO for the computation of u_eb and v_eb are similar. This is not unexpected, as the same COSMIC data were assimilated (albeit in a somewhat different form) in the production of ERA5 reanalysis (Hersbach et al., 2020).

We agree with your statement in the first sentence. However, this was not our intended message in the manuscript. In analyzing the difference between *ERA5* v_eb and RO v_eb, we aim to study the systematic difference between the two data sets, as emphasized in Table 1 and the description in the main text. However, before that, we study in a first step, the bias resulting from the equatorial balance approximation, based on the state-of-the-art reanalysis ERA5 data (e.g., Figure 2); RO does not play any role for this estimation.
Apart from this, we agree that ERA5 has in general RO data assimilated, and hence, is not independent of RO. However, since all major (re)analyses do assimilate RO data since 2006 (start of the "U.S. COSMIC" and "European Metop" RO multi-satellite era), we consider it adequate in this study to quantitatively evaluate the equatorial balance approximation using RO data, and comparing it with the wind field data of the state-of-the-art reanalysis ERA5. From other previous studies that also involve short-range forecasts, or MERRA2, and JRA-55 reanalyses, like for example in the study from von Schuckmann et al. 2023 https://doi.org/10.5194/essd-15-1675-2023, Sect. 3 therein, we know that this will likely result in no major differences, and should be sufficient for the present purpose. Furthermore, the approach of the two-step analysis, as described in Table 1, exactly decomposes the analysis into the bias from the approximation (first step, only ERA5), and the bias between the two data set (ERA5 and RO), aiming to partially circumvent this specific problem.

224: provide references to those missions.

We already provided references in the paragraph from line 155 to 160.

231-233: The alignment of an increase of systematic bias with the drop in the number of RO profiles is a very interesting feature. However, my question is how you can be certain that only this factor explains the increase of bias. No proof is provided, so the statement should be milder and speculative. From the statistical perspective, a reduction of the number of profiles would only increase the random error.

Yes, we will revise the sentence with a more conservative formulation. Furthermore, we cite the work about the uncertainty propagation in RO retrieval from Schwarz et al. (2017).

Figure 7 is another proof, that v_eb (as well as v_g) are likely unable to approximate v_o.

Please refer to major comment 5.

276-277: as this is not some new conclusion, I would say "as in Healy et al. (2020)".

We will mention in that statement the important initial pre-work of Healy et al. (2020). Nevertheless, we see this work here as a more detailed analysis of the wind fields across the equator, providing further insight about the potential of RO data for wind field monitoring.

277: what do you mean by "the resolution was possible to obtain" (I could not understand with going back to the results section). Please, express more clearly.

Yes, we will rephrase. Please see also major comment 5.

1: vertically

7-8: sentence "We analyze the equatorial balance equation within this latitude band." Is redundant in my opinion.

30: several heights but mostly upper troposphere

61: no comma before "isobaric levels".

63: between 15N in 10 S.

65: analyzed instead of "started to analyze"

66: to reproduce ERA5 geostrophic winds ("original winds" sound like total winds). You properly introduce "original" only later in the text, in line 115, leaving the reader confused at this stage.

68: I am not sure whether "Anthes" region is an established geographical term. Did you perhaps mean Andes?

70: equatorial band

70: "approaches" instead of "converges"

71: Reformulate sentence "Interesting was also to see…"

79: latitude-longitude

97-104: it is necessary to mention, that Coriolis parameter is now approximated using equatorial beta-plane approximation.

106: remove "still"

135: "includes"/"provides" instead of "combines"

135 and 137: sometimes you use "data" as singular noun and sometimes as plural, e.g. "The ERA5 reanalysis data combines…" vs. "The data are available…"

138: no comma before "to find", no comma before "for the equatorial balance…" Revise misuse of comma at the other places of the text as well.

178: no comma

Figure 4 should include two more rows: 1) monthly-mean winds V_o and 2) monthly-mean winds V_eb. The caption should be: Temporal development of the wind-speed bias…

222: revise “high occultation statistics”

237: “section”

237: no comma before “to complete”

244-245: “geostrophic break down” to the “the geostrophic approximation does not apply any more”

272: it is again unclear to the reader, what are the “winds calculated using ERA5 data and original winds”. Try forming the Conclusions in a way that is understood even to readers who did not read the whole methodology

276: word order: “we could successfully apply”

287: first comma is excessive

279: this reads as the zonal wind speeds are 1 m/s and meridional wind speeds are 15 m/s. Again, be more precise for which levels in the equatorial +-5 deg channel do this wind speeds apply.

Thank you for noticing! All corrections will be done.

References:

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