Collective Author’s Response to Referee #1 and Referee #2

We thank the reviewers for their careful and constructive assessment, for generally 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 all points raised, substantially revised and improved the manuscript along these lines, and provide answers to the comments below (comments quoted in *italic with gray background*, with the answers below each comment).

Author’s Response to Referee #1 Comments

Referee #1 (Specific comments and questions)

1) *Equations 1 and 2: what, precisely, are the variables x and y? I would like to see a level of detail here corresponding to that provided for Equations 3 and 4. Also, latitude is used in Equations 1 and 2 before it is introduced in association with Equations 3 and 4.*

Thank you for noticing. We corrected this and made the formulations of the equations consistent.

2) *RO data retrievals: The key variable used in the study is geopotential height as a function of pressure (or pressure as a function of geopotential height). How is pressure retrieved? It is mentioned that the RO geopotential climatologies are available from 1000 hPa to 5 hPa. That covers atmospheric regions where the "dry" approximation is applicable as well as regions where it is certainly not applicable. Some explanations of how that is handled is needed.*

Thank you for asking. Briefly summarized, based on the atmospheric bending of the GNSS signals during the occultation sounding, it is possible to retrieve atmospheric refractivity profiles via an Abel transform. From these, air density and pressure profiles as a function of altitude, or geopotential height, can be accurately derived based on the refractivity equation, the equation of state, and the downward integration of the hydrostatic equation. Along the retrieval chain we derive the physical atmospheric parameters (e.g., physical pressure) using a moist-air retrieval described in detail in Li et al. (2019) . Roughly down to 700 hPa, but in practice significantly depending on the amount of water vapor, the physical information is dominantly dependent on information from RO measurements. This - of course - varies somewhat depending on the geographical latitude (dry polar troposphere versus moist tropical troposphere). Towards lower tropospheric altitudes, the moist information more strongly relies on background information.

We added this reference as an additional one for the moist-air retrieval:
Li, Y., Kirchengast, G., Scherllin-Pirscher, B., Schwaerz, M., Nielsen, J. K., Ho, S. P., & Yuan, Y. B. (2019). A new algorithm for the retrieval of atmospheric profiles from GNSS radio occultation data in moist air and comparison to 1DVar retrievals. *Remote Sensing*, *11*, 2729, https://doi.org/10.3390/rs11232729

Furthermore, we added the following text insert in Section 3.2, line 153:
“The WEGC OPSv5.6 retrieval system processes the atmospheric parameters as a function of altitude or geopotential height, based on the refractivity equation, the equation of state, and the downward integration of the hydrostatic equation. The physical atmospheric parameters (e.g., physical pressure) are derived using a moist-

air retrieval algorithm, which combines the individual profiles with background information by optimal estimation; see Li et al. (2019) for details."

***3)** You mention that the monthly-mean RO data at the 2.5x2.5 degree grid points are computed by "Gaussian latitude-longitude weighting" within a radius of 600 km. What is the width of the Gaussian? Is it 600 km? Or is 600 km the distance from the grid point within which the profiles contribute to the grid point mean?*

Thank you, we improved the description of the methodology. In general, the 600 km corresponds to the distance from the grid point, defined as the center location of the area of influence, within which the profiles contribute to the grid point mean. In performing the averaging, the profiles are weighted according to their distance from this center location with a bivariate (latitude-longitude) gaussian function which peaks at the center and features a standard deviation of 150 km along latitude and 300 km along longitude, respectively.

We also added the following citation:
Ladstädter et al., OPAC-IROWG 2022 conference, Talk on gridding strategies, Seggau, Austria, September 8, 2022.

***4)** You mention the need to further average to a 5x5 degree grid for the equatorial-balance calculation. Did you try other differencing techniques than forward finite-differences? It may be too simplistic, and other differencing schemes may be more suitable.*

Thank you for this thought. In the very first step of our analysis, we tested different finite-differencing techniques (centered, forward, backward, and centralized with higher-order). We found that while forward and backward differencing is not recommendable, centralized and higher-order centralized methods show very similar results when using ERA5 data on a 2.5° x 2.5° grid. The local approximation bias at individual grid points (V_{EB} – V_O) is slightly smaller when using the standard central method, while the zonal mean bias improves a bit with the higher-order method. These biases are amplified when using the RO data available on a 5° x 5° grid. Here the difference in the local bias is found larger, with the standard central method outperforming the higher-order method. This larger local bias of the higher-order 5-point method compared to the standard 3-point method is likely caused by the fairly large latitudinal range of the former across the central grid point, spanning across four 5° steps. For the zonal-mean bias, again the higher-order method performs somewhat better, with the quality depending on altitude level and month. Overall, since the equatorial balance approximation is, strictly speaking, only fully valid at the equator, the approximation error from including data points outside of the ±5° equator band is considered larger than the gain from applying the higher-order method. For this reason, the standard centered differencing method was finally chosen as the primary method for the respective data analyses in this study.

***5)** In Section 4, the analyses and discussions related to the RO data are focused on three atmospheric layers: 10 hPa, 50 hPa, and 200 hPa. However in Figures 5 and 6, RO data down to 1000 hPa is shown. Whether it makes sense to show RO data in the lower troposphere depends on how the RO data were retrieved. Depending on the answers to comment 2 above, you should consider not to show the full vertical span down to 1000 hPa.*

Since we are using a moist-air retrieval, we consider it not as a problem to show the results in Figure 5 down to 1000 hPa. Of course, in the troposphere the data become significantly influenced by background information (as briefly described in the answer to comment 2 above). Nevertheless, since our focus is the free troposphere (and of course the stratosphere), where frictional forcing can be neglected, we will skip the atmospheric boundary layer and show the results only down to 800 hPa. We added a sentence about this in the discussion of the results. Furthermore, we will also better emphasize that the core range of RO data is from the upper troposphere to the lower stratosphere (see added sentences in the Introduction and Section 4.3).

***6)** Related to comment 5, there is a sentence in Section 4.3 which I don't know how to interpret (lines 259-260): "the larger influence of moisture leads to a higher need of background information in the RO retrieval chain, and as a consequence to an increase in the bias". Is this an indication that you use the "dry" solution all the way down to 1000 hPa?*

We are using a moist-air retrieval, see also answers to questions 2 and 5. Furthermore, we rephrased the sentence in question:
"… This feature clearly relates to the core region of high-quality RO data, which is in the upper troposphere and lower stratosphere. With decreasing altitude and therefore increasing moisture content, the retrieval of atmospheric parameters relies increasingly on background information (e.g., Li et al., 2019). The RO information dominates between about 8 km to 35 km in the tropics (e.g., Scherllin-Pirscher et al., 2011)."

Author's Response to Referee #2 Comments

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 clearly contributed to substantial improvements in the revised manuscript. We appreciate the invested time and effort and found many of the suggestions and editorial comments helpful indeed. We also carefully rechecked, and partly refined, 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 where the added value of the v-component, i.e., the meridional wind estimations, does show up specifically).
This being said, we emphasize that the main goal and focus of this study is to test the utility of RO 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 here 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 will 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 improved the introduction, as the reviewer suggested, to make this focus and scope 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 needed to re-check and 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 decided to keep the scope of this study as originally laid out, i.e., we did 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 having pointed out several important issues for re-checks and evaluations that we have done now for the revised manuscript, within the methodological scope. Please see more details in the answers below on how we implemented the 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 the introductory statement above, which refers to these questions. Furthermore, we emphasized these issues more strongly in the introduction now, but also in the final summary and conclusions section.

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 latitudinal-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 care. 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 main indicative threshold. 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 hence not a huge difference whether we use an indicative threshold of 2 m/s (as we chose) or a bit more tight, like 1 m/s.
The further reason for our choice is related to the aspect that the advantage of RO-based long-term wind records is their unique potential of being also temporally stable, which is another WMO requirement on stability. That is, if we consider monthly winds with accuracy within ±2 m/s, this is roughly consistent with a decadal stability of ±0.5 m/s per decade, which is the associated WMO-based requirement that we use to evaluate long-term stability (see Nimac et al., 2023).
Having said this, we improved the discussion and reasoning for the indicative thresholds we chose for our study from the portfolio of requirements of the WMO in the Method Section 2.

***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.

Thank you for your comment. We reformulated the sentence and hope it is clearer now, as follows:
"It was possible to reproduce the original ERA5 winds rather well, and within the target of ±2 m s^{-1}. However, in the region of the jet stream the difference between the two data sets exceeded this threshold. Furthermore, over large mountain areas (e.g., Himalayan or Andes region) larger deviations were found, since the ageostrophic contribution grows in importance in such regions with massive influence of topography."

***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.*

Thank you for this thought. In the very first step of our analysis, we tested different finite-differencing techniques (centered, forward, backward, and centralized with higher-order). We found that while forward and backward differencing is not recommendable, centralized and higher-order centralized methods show very similar results when using ERA5 data on a 2.5° x 2.5° grid. The local approximation bias at individual grid points (V_{EB} – V_O) is slightly smaller when using the standard central method, while the zonal mean bias improves a bit with the higher-order method. These biases are amplified when using the RO data available on a 5° x 5° grid. Here the difference in the local bias is found larger, with the standard central method outperforming the higher-order method. This larger local bias of the higher-order 5-point method compared to the standard 3-point method is likely caused by the fairly large latitudinal range of the former across the central grid point, spanning across four 5° steps. For the zonal-mean bias, again the higher-order method performs somewhat better, with the quality somewhat depending on altitude level and month. Overall, since the equatorial balance approximation is, strictly speaking, only fully valid at the equator, the approximation error from including data points outside of the ±5° equator band is considered larger than the gain from applying the higher-order method. For this reason, the standard centered differencing method was finally chosen as the primary method for the respective data analyses in this study.

For further information see the newly introduced Appendix A.

About the suggestion of a stronger temporal averaging, seasonal rather than monthly, with a finer spatial resolution: we tested it and found 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; see the introductory statement above that summarizes the aims of this work. In the future, the goal is to derive long-term time series, in form of monthly 2.5° x 2.5° wind products (with RO from 2006 onwards). A finer spatial resolution is, on the one hand, not recommendable for RO data and this time frame. This would require more dense global coverage with daily RO events, which is not the available up to now (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 comment 5 below). For this reason, we now dropped the 0.5° resolution variant from Figure 1, since it is a "sub-scale" that is not relevant for the RO-based wind field records we aim at where we will (and can) not got finer than about 300 km. We have worked these arguments into the manuscript text now, at different locations.

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 to thank you for your insightful feedback, which significantly helped to improve the manuscript in this respect. Actually, we analyzed the meridional wind component both for zonal-mean averages (which are close to zero) and in longitude-resolved form for different longitude sectors. In the original manuscript, we just had shown the example results for the one longitude sector around the prime (Greenwich) meridian (-10° to 10° longitude), keeping notice that we had studied the other sectors as well, which qualitatively showed similar behavior. However, we agree with your quest for a closer recheck and evaluation and now show results for all longitude sectors (new Figure 2). Furthermore, we explicitly make clear that later on we will just illustrate the prime meridian sector, as an exemplary longitude band.
Furthermore, agreeing that we needed to more carefully re-assess the detailed behavior of the meridional wind component, to see how the equatorial balance holds and where really the "added value" of this component can be isolated. For this reason, we added further new/updated figures in the revised manuscript (see Figures 2, 3, 4, 6). In addition, and in line with the updated results shown, we revised the Section 4.1 and also the Conclusions. Briefly summarizing here, the updated results include:
The meridional wind component is very small in magnitude in the tropical stratosphere and generally much smaller than the zonal wind (close to zero compared to the zonal wind speed), and its estimated equatorial-balance approximation bias is larger than its (very small) magnitude itself. For these reasons, there is no added-value information that we could derive from including the meridional component estimation in the stratosphere on top of the zonal wind estimation. However, we nevertheless clearly could show that there is demonstrable added value of including the meridional component in the troposphere, where the total wind speed estimation improves due to its inclusion on top of the zonal wind; we show and discuss this now along with Figure 3 (showing absolute and relative differences) as well as Figure 6.

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 detailed introductory statement as the answer to Comment 1 ("Conclusions") above.

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

Thanks, we revised figure caption texts and aimed to improve the English 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 revised it accordingly.

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

Thank you, we revised the abstract during the review process.

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, we exchanged the citation.

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

Thank you for this information. We toned this down somewhat, qualified it better, and revised the sentence in the following way:
"Aircrafts and atmospheric motion vectors (AMVs) from geostationary or polar satellites provide a high temporal and horizontal sampling at several heights, but have distinct limits in accurate vertical geolocation and resolution and global representation."

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 rewrote 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.

Thank you for these thoughts. We imported this aspect in form of an added text as follows:
"Wind information is nowadays obtained also implicitly as part of variational data assimilation ("4D-Var") in numerical weather prediction analyses that initialize the forecasts, such as through the geostrophic adjustment and directly through the background error covariances (especially where the geostrophic balance applies) as well as through 4D-Var of humidity and/or ozone tracing data (Geer et al., 2018; Zaplotnik et al., 2023)."

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 added the information about the horizontal resolution in line 50. Furthermore, we rephrased the sentence regarding the vertical core region of RO in the following way.
"RO data cover well the (free) troposphere and the stratosphere, with a core region of high quality in the upper troposphere and lower stratosphere (e.g., Zeng et al., 2019; Steiner et al., 2020), having a horizontal resolution of about 200 km to 300 km (e.g., Kursinski et al., 1997; Foelsche et al., 2011)."

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

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.

We adapted the sentence to: "Healy et al. (2020), on the other hand, tested the zonal equatorial balance equation around the equator, studying the utility of RO data in a 5°-zonal band in the stratosphere."
For further discussion on the equatorial balance, and the added value of the meridional component, please see the answer to major comment 5.

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.

Ok, we agree and deleted this part of the sentence.

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. The one reason is that a higher spatial and temporal resolution is not feasible given the limits of spatiotemporal sampling by RO events from the available RO missions. The other reason is more physical, i.e., finer resolutions (temporal and spatial) would increase the ageostrophic contributions (see also the answer to major comment 4). We include now an improved discussion about this in Sec. 3.1 and Sec. 3.2.

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.

Thank you. We changed the formulation in the paper to a consistent formulation.

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. We do provide a discussion about it in Sec. 2 (which we also improved). We focus on monthly-mean climate related winds, at spatial resolution not finer than about 300 km.

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

In order to clarify this point, we always analyzed all months and also show some results for the complete year 2009. Where found sufficient, we only show the results for January 2009, as a representative month. For better understanding we rephrased the relevant introductory paragraph of Sec. 3 to:
"… January 2009 was chosen as a representative month in the results section. All other months were analyzed as well and generally showed no major differences in behavior, which justifies the representative-month approach for most result discussions. As we also performed the analysis for the complete year 2009, for both ERA5 and RO data, we draw from these results to discuss aspects of seasonal and interhemispheric changes."

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. We checked at latitudes outside the equatorial band: at latitudes north and south of 40° the zig-zag features become smaller. Note that the regions outside of the equatorial region are not shown in the figure, because they are not the focus of this study; we specifically analyze the equatorial-balance equation across the equator.
Thank you for noticing. We added in the figure caption the description of the dashed line, which represents the ±2 m/s threshold.

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 localization 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 improved the description of the methodology. In general, the 600 km corresponds to the distance from the grid point, defined as the center location of the area of influence, within which the profiles contribute to the grid point mean. In performing the averaging, the profiles are weighted according to their distance from this center location with a bivariate (latitude-longitude) gaussian function which peaks at the center and features a standard deviation of 150 km along latitude and 300 km along longitude, respectively.

We also added the following citation:
Ladstädter et al., OPAC-IROWG 2022 conference, Talk on gridding strategies, Seggau, Austria, September 8, 2022.

About the smoothing procedure: thank you for also pointing that out. The procedure was described in an unclear manner, we apologize for that. We reformulated in the following way:
"Tests revealed that a Gaussian smoothing with a 5° longitudinal smoothing window improved the results. This smoothing was therefore applied to the equatorial-balance wind fields derived from RO data."

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 to major comment 5. We completely revised this section and added the new Figures 2, 3, and 4 to show the results of our updated analyses.

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. We rephrased the sentence:
"Our focus lies on the three representative levels, 200 hPa, 50 hPa and 10 hPa, representing the tropical upper troposphere, lower stratosphere and middle stratosphere, respectively."

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.
We now added a further explanation about this in Section 4.2.
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 is hence considered 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 mitigate this specific problem (see new comment in Section 4.2).

224: provide references to those missions.

Note, we 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 revised the sentence to a more conservative formulation. Furthermore, we cited the work of Scherllin-Pirscher et al. (2011) and 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 our answer to major comment 5 on this.

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

We mention now the important initial pre-work of Healy et al. (2020). Nevertheless, note that this work here is 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.

Please see our answer to major comment 5 on this. We revised some statements of the final section.

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 were implemented.

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