Author's Response to Referee #1 Comments

J. Danzer, M. Pieler, and G. Kirchengast: "Closing the gap in the tropics: the added value of radio-occultation data for wind field monitoring across the equator", submitted to Atmospheric Measurement Techniques. Revised manuscript after first round of comments from reviewers.

The revised manuscript is substantially improved compared to the original version. The authors have carefully addressed the issues raised by both reviewers. I'm particularly pleased to see that they removed the comparison between ERA5- and RO-derived wind fields below 800 hPa.

Minor: Section 4.3, line 326, states that Fig. 8 shows data down to 1000 hPa. This should be 800 hPa.

I recommend publishing the manuscript after correction of this minor typograhical error.

We thank the reviewer very much for this positive general comment on our revised manuscript; we indeed had aimed to carefully improve in all points of concern and address all issues raised. Regarding the minor comment, we corrected the typo and are happy that you recommend the paper for publishing!

Author's Response to Referee #2 Comments

The authors have made substantial improvements to the manuscript and have properly addressed most of my comments. The purpose of this study is now nicely communicated, and the way if fills the scientific gap is described as well. Furthermore, the authors have performed extra analyses, which further confirm the robustness of their results. My suggestion would be to address better a single major point that has to be revised prior to acceptance. I think nothing is wrong with the analysis, but in the conclusions, the authors are overestimating the usefulness of GPSRO and equatorial wind balance to assess the meridional wind component. When this is revised, I would suggest accepting the paper.

We also thank this reviewer very much for this positive overall comment and basic acceptance suggestion, and for the constructive further input, especially on the "single major point". Regarding this one main point remaining (Major comment below), we feel that there possibly has been a partial misunderstanding of our results related to the co-benefit of the meridional component (possibly also since our explanation was in parts not clear and precise enough). We tried to clarify this now in our answer and at the same time improved relevant formulations in the further revised manuscript.

Referee #2 (Major comment)

1) It is clear from Figure 2 in in the manuscript that the equatorial wind balance is not applicable for the meridional wind in the tropical troposphere. The equatorial-balance bias is evidently above the threshold in many longitude bands below 100 hPa (Fig. 2c,f). This is more pronounced for the meridional wind, which can be seen

by comparing Fig. 2d and Fig. 2e. Below, I attach some of my computations for ERA5, April 2010, 2.5x2.5 grid, for 10, 70, 200 and 500 hPa (see Figures 1-4). These plots confirm my suspicions that the equatorial wind balance is inappropriate for the estimation of meridional wind component, while the approximation works out nicely for the zonal wind component (the full fields and not only for the zonal-mean zonal wind as suggested in previous studies, which should be better communicated in this study).

In my opinion, it would be important to see not only the absolute error of the reconstruction (as in Fig 2f) but also the relative error, which often exceeds 100% (see attached Figures 5 and 6 below). I do not fully understand, why Fig.3 compares u_eb to V_o. In my opinion, u_eb should be compared to u_o, v_eb should be compared to v_o and V_eb should be compared to V_o, as in Figure 2. What is the aim of that? The WMO threshold should not be followed blindly. The accurate description of climate trends of tropospheric meridional winds is extremely important as they describe the upper and lower branches of the Hadley circulation, which governs the precipitation distribution in the Tropics and Subtropics. The annual-mean magnitude of meridional wind in the upper branch of HC is around 1.5 m/s (Figure 0). In this respect, the WMO threshold is much too high.

Lines 8-10 and line 16 in the Abstract should therefore be revised – I am not convinced about the added value of meridional wind component for the reasons state above. I think the ability to reconstruct zonal winds (and not only the zonal-mean zonal winds) is still a nice result, but it has to be accurately communicated precisely both in the Abstract as well as in the Conclusions, as well as in the main text (e.g. discussion in lines 238-240).

Thank you for these thoughtful considerations and also the complementary ERA5 figures. We like to first of all clarify that we did not (aim to) make the statement in the manuscript that the meridional wind component itself is well reconstructed in the troposphere. Instead, we stated that the (zonal-mean) total wind speed (in the troposphere) benefits from computing both components, compared to when just using the zonal component as an approximation for the total wind speed. This can be seen, for example, in Figure panels 3b and 3d compared to panels 3a and 3c. And, furthermore, in Figure panels 6g when comparing to 6d, at the 200 hPa level. The zonal-mean total wind speed in the troposphere is always somewhat better approximated when including both, the zonal and meridional wind components. We hence respectfully think that related to this aspect, there was possibly a misunderstanding (possibly also since our text was not fully clear and precise enough to this end). We think this co-benefit aspect of the meridional component (while in itself it is not of sufficient utility) is also a quite interesting result and hence prefer to keep it as one of the messages of this paper.
To help avoid potential misunderstanding, we carefully rechecked the text in the manuscript, and improved at several places, to ensure that we now clearly express this specific kind of co-benefit for the zonal-mean total wind speed in the troposphere (while in the stratosphere, where the absolute values of meridional wind component are very small, also the added value is not there). We also eliminated to show the WMO target requirement in a relevant figure panel (Figure 2f) as if it would apply also to the meridional component on its own; we agree this was a bit sloppy, since this requirement is meant to apply to the total wind speed (and likewise the zonal wind component which strongly dominates the total wind speed).

Furthermore, we understand that the reviewer suggests additional visualization of relative errors. However, we feel we already show relative errors to the point we consider needed, which is within Figure 3 in the manuscript. This sufficiently supports the one key result explained just before, i.e., supports the message that the zonal-mean total wind speed is better approximated in the troposphere by including both wind components. One can clearly see the wind speed bias improvements in Figure 3b (absolute difference) and 3d (relative difference), throughout the equatorial troposphere. While in principle we could hence add a further relative difference plot, we consider that this does not add further value to the relevant key messages of this study. Rather than added visualization, we of course tried to carefully improve the related explanations so as to avoid any misunderstanding of the value of the meridional wind component (just the mentioned co-benefit), since nearly all of the wind speed information (and essentially all of the longitudinally resolved one) is indeed carried by the zonal wind component.

Overall, from the line of arguments above, we included in the further revised manuscript the following main improvements:

1. We more strongly emphasized and better clarified in the manuscript, such as in the abstract, the discussion of results, and the conclusions, that while the meridional component itself is not well estimated, it is the zonal-mean total wind speed in the troposphere that benefits from including both components. We also eliminated to indicate the WMO target requirements in a panel showing meridional wind only.
2. We more clearly stated that the equatorial balance approximation works best in the stratosphere (abstract and conclusions). That is indeed a very valuable constructively critical point of the reviewer. We should have emphasized this more strongly before.
3. We preferred not to include another relative difference figure, since it would not add to our message, but we rather aimed to bring the message better along with better discussion especially of Figure 3.

Referee #2 (Minor comments)

I currently do not see the potential of assimilating GPSRO derived winds for NWP/reanalyses applications, opposed to the author's response to reviewers. While the equatorial wind balance applies well particularly in the monthly mean fields in the stratosphere (Fig. 2), it is somewhat less applicable for the upper troposphere and below, and even less applicable for instantaneous fields. Another issue would be the observation correlation between the already assimilated bending angles and the GPSRO winds. On the other hand, I agree there is certainly great potential in the climate scope – both for monitoring as well as climate model verification, as it is also outlined in the paper. While the authors mention that they "do not focus on analysing or describing at the same time atmospheric dynamical processes as such; this is not within the scope of this study", they should note that the atmospheric dynamics are essential to the [potential of the] GPSRO wind retrieval.
Lines 5,6: Considering the previous comment, the equatorial balance approximation becomes important in the temporal mean, while the geostrophic balance mostly applies well for the fields at any time instance too. At any time instance, we do not have a predominant balance between winds and geopotential in the tropical upper-troposphere, the fields are only multivariately coupled through the equatorial modes (e.g. Matsuno, 1966).

Thanks for this comment. However, we want to emphasize that we do not discuss or suggest in this paper the potential to assimilate RO-derived winds in NWP. We rather fully agree with the reviewer that it would be highly unusual (not to say highly suboptimal to useless) to assimilate a derived diagnostic quantity like the RO-derived winds. That is, of course the RO's information content is best made available to data assimilation and forecasting systems by using quite more low-level data such as the well-proven assimilation of RO bending angles (as also done in the ECWMF IFS). In this paper, we purely focus on and discuss the climate-related wind field retrieval, for the benefit of long-term stable climate wind field monitoring.

25: vertical resolution of wind information (in the current form it might be misread as the vertical wind component)

Thank you, we implemented this improvement.

34-37: reformulate the tenses. ADM Aeolus is down now. It also depends on the hydrometeor Mie scattering.

We rephrased to:
"On the other hand, the Atmospheric Dynamics Mission (ADM-Aeolus, operating over August 2018 to July 2023) provided 3D wind profiling with a frequent and high-resolution coverage, filling measurement gaps over the oceans, poles, tropics, and the southern hemisphere, up to an altitude of about 20 km. However, it depended on clear-air molecular scattering (no measurements within clouds) and on hydrometeor Mie scattering, which can

be particularly tricky at tropical latitudes, due to the high-altitude cloud systems (see also Stoffelen et al., 2005, 2020; Kanitz et al., 2019). “

77: what observing system change do you refer to? Please, be more specific.

We rephrased to:
“Our study furthermore showed that within the 2007 to 2020 evaluation period the difference between RO and ERA5 became noticeably smaller from 2016 onward, coinciding with an ERA5 observing systems change including as of 2016 additional information from various sources such as land stations, ships, and buoys.”

83: stratospheric zonal-mean wind field
102: The geostrophic balance breaks down in the tropics, due to the…
107: accuracy

Ok, done.

109-110: Try to avoid “strong” and be more specific about the averaging, e.g. as reformulate as: “Since our focus are monthly-averaged mesoscale (might even be synoptic-scale already) winds relevant for the description of climate, …
120: remove new paragraph indent
125: great! Very convincing!
138: equatorial balance approximation takes over. [not the winds themselves]

Ok, thank you, done.

133 and Table 1: you now use the same subscript for globe and geostrophic. At this point, it seems somewhat puzzling.

We are sorry for having had this confusion. The subscript (g) still refers to the geostrophic approximations, which was studied on the complete globe. The description in Table 1 is now clarified by changing to:
“(eb): focus area ±5◦ N/S; (g): studied on complete globe”

Figure 1: is this based on the fields at single time instance or whole 2009, as suggested in line 149.

The figure shows the results for January 2009. We included this information now in the manuscript.

167: convincing!
174: consider “timeframe” à temporal averaging
175: reformulate

Thank you, done.

200: “results”: which results, be more specific? Was the systematic data bias reduced?

Thank you for pointing to this, we reformulated to:
“Tests revealed that a Gaussian smoothing with a 5° longitudinal smoothing window improved the wind data estimation and the systematic difference decreased.”

220: Figure 2: do these statistics apply for time-mean data (year 2009) or for some specific time instance? It should be explained somewhere.

For January 2009, as the text states in line 222. We also added this now in the description of Figure 2.

Figure 3: (a,b) and (c,d) have the same captions, despite first two representing absolute error and the second two the relative error.

Thank you for noticing this. We updated the Figure 3 accordingly.

238-239 I do not agree with the statement: "The difference fluctuates within he ±2 m s−1 threshold, also in the tropical troposphere." Also, such threshold is irrelevant given the small magnitude of the meridional wind.

Thank you for this comment. And indeed, regarding the "threshold", we think you are right and we have been a bit sloppy to this end to show it also for meridional wind. We agree it is sound to show this indicative target requirement of the WMO only for the total wind speed and the zonal wind component (that strongly dominates the total wind speed) but not for the small meridional wind component. For this reason, we have removed the target requirement indication from Figure 2f and made sure, at necessary places, to revise the text accordingly to avoid wording relating the meridional wind to this requirement.

Along with this, we deleted and reformulated the particular statement to:
"Analyzing the equatorial-balance bias shows that the difference fluctuates with amplitudes of about ±2 to 4 m/s in the tropical troposphere (Figure 2f)."

242-244: Again, I do not agree with the reasoning here. The sole reason, why "it was possible to derive the wind fields close to the original wind speed" is because the zonal wind contributes the most to the wind speed and that component is well reconstructed by the equatorial wind balance.
252-257: I am not sure this improvement comes from the right source. The meridional winds could have wrong sign here and the wind speed would still improve. Any meridional wind addition would improve the total wind, i.e. if v<<u, adding v would bring the reconstructed total wind closer to the true total wind.

Please see our answer to the major concern above, related to these arguments. We tried to clarify there, and we further emphasized in the revised manuscript more strongly that while the meridional component itself is not well estimated, it is the zonal-mean total wind speed in the troposphere that benefits from including both components. We reformulated at several places in the manuscript.
Such as:
"This result indicates that while the meridional component itself is not well estimated, the calculation of zonal-mean total wind speed benefits from including the meridional wind component in the troposphere, since it brings the reconstructed wind speed closer to the original wind. However, in the stratosphere, the close-to-zero meridional wind brings in no added value."
As to the point of "right source" for improvement, we think the key is that we now better clarify that the improvement plays out in the zonal-mean wind speed in the troposphere (i.e., not the longitudinally resolved one); and yes, the overall beneficial effect in this zonal-mean wind speed is evidently coming up from the zonal wind component alone providing a little underestimation of the speed (and also yes, we won't consider the data viable to reconstruct a wind direction).

Fig 5f: the reconstruction error in f) is unacceptably large to capture the upper branch of the southern Hadley cell, the main feature of the tropospheric tropical circulation, despite fulfilling WMO requirements.

Yes, we agree and carefully revised the manuscript, such as:
"When considering the differences between the two data sets for meridional wind and wind speed (Figure 5f and 5i) it is seen that these also generally reside within ±2 ms^{-1}. Nevertheless, due to the already small absolute

magnitudes of the meridional wind (Figure 5d and 5e) it is also clear that this component itself is not well reproduced. However, the zonal-mean total wind speed (bottom row) still benefits in the troposphere from including both wind components (Figure 5i), with the dominant contribution coming from the zonal component."

And also, in lines 305 to 308:
"Summarizing the results of the current and previous section, meso-scale climate wind field derivation was possible across the equator using RO data, when focusing on its core vertical region of high quality and resolution. Furthermore, we found that while the meridional component itself is not well estimated, it is the zonal-mean total wind speed in the troposphere that benefits from including both components, while in the stratosphere the meridional component's influence becomes negligible."