Author’s Response to Referee #2

We would like to thank the reviewer for the thorough and helpful comments. Our answers to the comments are written in *italic*.

Reviewer #2 (Comments to Author):

Initial assessment: The paper makes use of ERA5 monthly mean geopotential fields for July and January on pressure levels from 1000 hPa to 10 hPa and 2.5°x2.5° horizontal grid during the time period 2007 to 2020. For the same time period and resolution, monthly mean geopotential is computed from RO data using aggregated atmospheric profiles. From each geopotential data set, local geostrophic wind is computed using the geostrophic wind equation at pressure levels. The ERA5 and RO geostrophic winds are then used to "1) test the validity of the geostrophic approximation for representing monthly-mean winds, and to 2) evaluate the utility of RO derived monthly-mean winds, for their potential added value as a separate wind field monitoring data record providing improved long-term stability".

1) The paper reads as a technical report and provides little scientific understanding.

Yes, the paper has a focus on methodology of atmospheric processing of RO data (which is why we submitted it to AMT and not, e.g., ACP), but we also discuss at various suitable places the scientific implications, in the sense of pointing to the scientific utility of the data for climate monitoring. We agree that we do not focus on analyzing and understanding at the same time atmospheric processes as such, since we consider it would overstretch the scope of this AMT paper. As we try to say clearly from the introduction onward (and we will aim to make this even more clear in the revised version), the main goal of the study is to evaluate the potential to derive geostrophic wind fields from radio occultation satellite measurements, and the geographical and altitudinal scope of their utility. This is a quite new topic within the RO community, as it did not get a lot of attention so far (we of course aim to carefully cite all existing suitable references). Hence, our approach is to first evaluate the quality of the derived RO wind fields, which this paper sent to AMT focuses on. Once the quality is established, scientific applications in the field of atmospheric and climate physics and dynamics are possible, and we would then focus on these. To cite a recent example of this publication approach related to our team, please see Li et al. AMT (2021) (https://doi.org/10.5194/amt-14-2327-2021 - focus methodology) and Li et al. ACP (2023) (https://doi.org/10.5194/acp-23-1259-2023 - focus physics/dynamics).

2) The method of the computation of geopotential from RO data is not well explained, e.g. it is unclear if it is only the hydrostatic height from temperature profile or also moisture was accounted for, whether averaging was carried out on input RO profiles or derived geopotential.

Ok, thank you, we agree that it will be useful to provide more refinement of the description to this end. We hence plan to add a short text like the following (first draft) in the "Data and study method" part in the revised version.

"Based on the atmospheric bending of the GNSS signals during the occultation sounding, it is possible to retrieve atmospheric refractivity profiles. 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. In this way, geopotential height profiles as a function of pressure levels can be obtained with unique accuracy and form the basis for the wind field derivation (for a more detailed description see Scherllin-Pirscher et al., 2017 - https://doi.org/10.1002/2016JD025902)."

3) More importantly, the computation of geostrophic winds is in the opinion of this reviewer unsuitable. The authors compute the geostrophic wind on the local f plane, latitude by latitude. Instead, one should derive geostrophic winds on the sphere that provide a smooth representation of the flow. An appropriate way of doing this is by using the stream function, ideally on model levels to avoid any errors due to interpolation, especially over the orography and in the lower troposphere. This global stream function provides the geostrophic wind on the sphere including the tropics.

On the other hand, this does not allow the comparison with the RO data in terms of geostrophic winds. If this is what the authors really intend to do as argued by their goal 2), the analysis should be described as "... geostrophic winds on a local f-plane..." and limited to the midlatitude free troposphere region.

Thank you, yes, we should make (even) more clear, that in line with predecessor work in the field (cited as references), that we apply the geostrophic approximation in a local-regions sense, i.e., evaluating the respective derivative equations locally on geopotential height fields on isobaric levels, where the horizontal sampling is of order 2.5° x 2.5° (and resolution of order 5° x 5°). Actually, in one more recent of our predecessor works, the paper by Scherllin-Pirscher et al. (2017 - https://doi.org/10.1002/2016JD025902), we alternatively derived the wind field as the gradient vector field of the Montgomery potential at potential temperature surfaces, as well as, for comparison, from the geopotential at isobaric surfaces, as we do in this study. The numerical results were found essentially identical (at ~2.5° sampling with ~5° resolution); and the geopotential-based derivation method has some advantages, since it makes like-to-like comparison of results from RO data to reanalysis data quite straightforward. In summary, we do understand that we need to improve the introduction of our method of computation, so that it more clears also to readers that come more from the dynamical meteorology side, and that we did base on the "local f plane" conception. And also recheck we are really clear that we apply the geostrophic approximation on ERA5 data in the same way we apply it to RO data, to fully understand the ageostrophic contribution.

4) If the authors choose to resubmit the manuscript, more consideration should be given to the interpretation. The differences between the full and geostrophic winds should be called ageostrophic wind (not a "bias"), etc.

Yes, ok, we would strive to improve on the interpretation side, as summarized above; however we will prefer to keep it as a methodology-focsed paper here for AMT, since otherwise the scope would become too broad and the paper too long. Specifically, we also agree with the suggestion to carefully check, and as needed rectify, the use of the word "bias", both in cases where it's actually just a *“systematic difference” (and no one dataset can be set biased against the other) and also in physical interpretation contexts, like if we actually should correctly say „ageostrophic wind“.*