Author's Response to Referee #1

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

Reviewer #1 (Comments to Author):

1) It would be useful for the reader to better explain how the geopotential is obtained from radio occultation (RO) data, as it is not a quantity obtained directly from RO observations, but derived from the integration of atmospheric density.

We agree that it may be useful if we refine this part a bit. We will hence 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)."

2) Please indicate the major changes that occurred in 2016 in the ERA5 observing system that may explain the changes observed in the difference between ERA5g and ROg.

These changes are mainly related to the changes in the certain input data into the data assimilation system that produced the reanalysis. To better indicate this, we will add a short text like the following (first draft) to the "Discussion" part in the revised version.

*"A change in the bias between the two datasets is especially visible after the year 2016, where certain observing system changes occurred in the observational input data assimilated into ERA5 (Hersbach et al., 2020). Specifically, a salient increase in the number of assimilated observations is seen around this year for surface pressure and specific humidity (Hersbach et al., 2020; Fig. 3 therein). In addition, the inclusion of WIGOS AMDAR (WMO Integrated Global Observing System, Aircraft Meteorological Data Relay) data in 2015 and the exclusion of some wind profilers and ACARS (*Aircraft Communications Addressing and Reporting System*) in 2016 are likely further sources of the inhomogeneities (Hersbach et al., 2020; Fig. 4 therein)."*

3) In the winter middle. stratosphere (10hPa), the geostrophic approximation overestimates the wind speed and the overestimation seems to increase with wind speed. It is greater in the SH where the stratospheric jet is faster than in the NH. If the cyclostrophic term is taken into account (term in V^2/R due to the rapid circulation around the polar vortex), does the agreement with the ERA5 winds become better?

Thank you – but yes, we comment in the discussion part of the manuscript that these larger differences at higher levels (i.e. middle stratosphere) of the winter hemisphere are also a result of neglecting this important cyclostrophic term (i.e., using the geostrophic approximation instead of the gradient wind approximation; cf. e.g., Scherllin-Pirscher et al., 2014). Here, we did not want to go into the details of really including gradient wind estimates also, since we wanted to focus on the validity of the geostrophic approximation, but indeed we do plan to combine various approximations in the future studies (also the so-called equatorial balance approximation across the equator; cf. Danzer et al., AMTD, in review, 2023).

4) Please check the alphabetical order of the publications (i.e. Hierro before Healy)

Thank you for noticing this. We will correct this in the revised version of the manuscript.