

Interactive comment on “New insights into Rossby wave packet properties in the extratropical UTLS using GNSS radio occultations” by Robin Pilch Kedzierski et al.

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Received and published: 15 April 2020

Dear authors,

This SC is not intended to provide a detailed review of the manuscript, I would rather comment on some methodological aspects and ask you for clarification.

First, I would like to point out that the utilization of wet profiles for your analysis does not completely support your claim of superiority of your results over the usage of reanalysis data. Note that the wet retrieval typically relies on a 1D-Var method, which needs assimilation of a background information (ECMWF forecast). In the UTLS, which is

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the focus region of your manuscript, dry profiles (pure observations) are considered to be of sufficient accuracy (e.g. Danzer et al., 2014). In a dry retrieval, however, both the temperature and pressure are derived from the density using hydrostatic balance and are therefore dependent quantities (see e.g. Pisoft et al., 2018). In this light, I suspect that the difference between your results for the temperature and pressure data are to a large extent dominated by a different visualization of the anomalies (absolute in [K] versus relative in [%] for pressure). It would be fair to show, how much additional information contain the pressure data over the temperature data (non-hydrostatic processes, water vapor, etc.).

As a second point I noticed that internal gravity waves (GWs) are not mentioned throughout the manuscript. It is well known (see e.g. Fritts and Alexander, 2003) that the GW sourcing, propagation and breaking processes are influenced by Rossby waves (which can be considered a slowly varying background for them) and so it seems plausible that GWs follow the Rossby wave activity and can contribute to what you interpret as Rossby wave packet properties. I see one potentially elegant way how to prove that your results do not contain the GW signal – you can show that the vertical power spectra of your RWP anomalies (e.g. your Fig. 10, but better with log axes) are significantly different from the slope of a saturated GW spectrum. If you can think of a simpler argument to discern the possible GW imprints in your results, I encourage you to provide it in the manuscript.

References: Danzer, J., Foelsche, U., Scherllin-Pirscher, B., and Schwärz, M.: Influence of changes in humidity on dry temperature in GPS RO climatologies, *Atmos. Meas. Tech.*, 7, 2883–2896, <https://doi.org/10.5194/amt-7-2883-2014>, 2014. Fritts, D. C., and Alexander, M. J. (2003), Gravity wave dynamics and effects in the middle atmosphere, *Rev. Geophys.*, 41, 1003, doi:10.1029/2001RG000106, 1. Pisoft, P., Sacha, P., Miksovsky, J., Huszar, P., Scherllin-Pirscher, B., and Foelsche, U.: Revisiting internal gravity waves analysis using GPS RO density profiles: comparison with temperature profiles and application for wave field stability study, *Atmos. Meas. Tech.*, 11, 515–527,

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