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Interactive comment on "An intercomparison of stratospheric gravity wave potential energy densities from METOP GPS-radio occultation measurements and ECMWF model data" *by* Markus Rapp et al.

Anonymous Referee #2

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The paper by Rapp et al. addresses stratospheric gravity wave (GW) activity derived from different data sets: GPS-RO temperature profiling by GRAS onboard Metop A/B, ECMWF operational analysis, ERA-Interim reanalysis as well as Rayleigh lidar measurements at specific locations. The central subject of this study is the intercomparison of GW potential energy densities derived from 'dry' and 'wet' GPS-RO retrievals, ECMWF IFS and ERA-Interim. Generally, a good agreement is found between GW potential energy distributions derived from these data sets, which is not totally surprising given that ECMWF model assimilates GPR-RO 'dry' measurements with a high level of confidence, whereas GPS-RO ‘wet’ retrieval uses ECMWF data as a priori information. Nevertheless, some important discrepancies between RO- and model-based GW Ep fields are pointed out. The interpretation of the data sets is well set in the scientific background of previous publications and largely sound. A few exceptions are listed below. These points need to be corrected. The paper is generally well to read and recommended for publication in AMT after some revisions.

General remarks.

1) Choice of the useful vertical range for GPS-RO.

The choice of lower boundary of 20 km is conditioned by potential aliasing of tropopause layer’s structure and variability as GW-induced perturbations. Although this excludes the entire extra-tropical lower stratosphere from the analysis, it allows to consider and compare the global distributions of GW Ep without heavy stipulations regarding the effects of tropical tropopause. My concern here mostly regards the upper boundary of 40 km. GPS-RO is a powerful tool for temperature profiling, which is why a good knowledge of actual capacities of this technique is crucial for atmospheric community. The majority of RO-based GW studies restrict the analysis to altitudes below 35 km because the uncertainty and the noise become too large at high altitudes. While this is confirmed by the results of this paper, I would love to see among the conclusions some more definitive statements regarding the usefulness of z>35 km RO data for GW retrieval.

2) Choice of Ep derivation method (Sect. 3).

As mentioned in the paper, there is a number of techniques for isolating the GW-induced perturbations in temperature profiles from the background atmospheric state. Here the authors opted to use a vertical detrending method based on a Butterworth filter. The advantage of the vertical detrending is that it can be applied to both local and global observations, enabling direct RO-lidar comparison. At that, I wonder how different would the results be for RO and ECMWF (particularly the TTIL issue) had the

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authors used a horizontal detrending method for GW Ep retrieval, which seems to better handle the lower stratosphere region (Schmidt et al., 2016; Khaykin et al., 2016). Indeed, when the authors subtract the zonal-mean Ep profiles (which is already some sort of horizontal detrending), the TTIL anomaly disappears. A recommendation to use this correction is put forth in the conclusions but I wonder, wouldn't it be better just to use one of the horizontal detrending methods instead? I believe the authors should better justify the choice of Ep derivation method in consideration of its shortcomings before recommending it for future use.

Specific remarks.

P4, L17-19. Although the correct references are in place, the fact that the compared data sets are not independent requires some more specific information on the assimilation of RO data in ECMWF IFS and ERA-Interim.

P6, L20-22. It is indeed surprising that ERA doesn't see the Scandinavian GW hotspot. The validity of the proposed explanation (invoking coarser resolution of ERA) could be verified by examining global Ep distribution for June, when the strong Patagonian GW hot spot is well pronounced.

P.7, L17-31. What is missing in this discussion is the mention of the large difference in vertical resolution of GPS-RO and lidar at high altitudes.

P7, L33-34. The sentence could be reformulated to make it clearer that it is the derived Ep values that are low-biased and not the actual temperature data.

P8, L27. The relatively large bias is rather seem below 23 km.

P.9,L.10-11. What is somewhat controversial here is that the higher N2 values within the TTIL derived from RO should lead to lower Ep, whereas the results suggest the opposite. It is understandable that RO should better resolve the fluctuations, but invoking the differences in N2 field in this context should be done more carefully.

Technical remarks.

P.5,L9. Remove “km” after the right parenthesis

P.8, L.31. “At these altitudes” => at the level of lowest correlation?

P.9, L24. “larger altitudes” => higher altitudes?

Throughout Sect. 6, comma is erroneously used as a decimal separator instead of a point.

Fig. 3. The data are missing in the left-hand panels.

Fig. 3 and 9. The X-axis of right-hand panels could be reduced to enhance the readability of the histograms.

Fig. 4 left panel. The X-axis scale could be reduced to say 0.8 – 1.1

Fig. 5 upper panels. X-axis caption should be T and not T’

Fig. 6. The black-shaded land areas whenever Ep values are too low are somewhat confusing.

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