Final and cumulative response to the reviewers' comments on our manuscript

"An intercomparison of stratospheric gravity wave potential energy densities from METOP GPS-radio occultation measurements and ECMWF model data"

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submitted for publication in Atmospheric Measurement Techniques (amt-2017-346)

Weßling, 22.12.2017

We greatly appreciate the reviewers' positive assessment of our work as well as their constructive comments. For the revised version, all comments have been addressed. In the following we respond to all comments point by point. For clarity, we first repeat the comments of the referees (in black), we then respond to these (in blue), and then indicate the changes made to the text where appropriate (in red).

Reviewer 2

[..]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.

Thanks very much for the careful review and the encouraging judgement of our work.

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.

At the end of Section 3 we had already written: "For this reason, we will exclude altitudes below 20 km from our analysis and focus on the altitude range between 20 and 40 km only, knowing, of course, that the largest altitudes need to be treated with care since noise of RO-data is known to pick up significantly above 35 km altitude (Marquardt and Healy, 2005)."

We do agree with the reviewer that our analyses in Section 4 do confirm that the actual useful range of data is below 35 km. This has now also been explicitly pointed out in the conclusions.

Also, it is well known that noise in RO-data picks up substantially above 35 km such that several previous studies have recommended to restrict the useful range of RO-data for GW analysis to below 35 km (e.g., Schmidt et al., 2008). This previous recommendation is clearly supported by our analysis.

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 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.

As already pointed out in our response to reviewer 1, this is an excellent and important comment. In order to address it, we have pointed out more clearly in the revised manuscript that we are using a vertical detrending method in order to treat all data sets with the same analysis procedure such that derived EP-values are directly comparable. The text added in the revised manuscript in response of this comment as well as to comment 5 of reviewer 1 is as follows:

For this study, we follow the approach of Ehard et al. (2015), i.e., we apply a fifth-order Butterworth filter with a cutoff wavelength of 15 km to vertical temperature profiles from the RO-measurements, from ERA Interim, from the IFS, as well as from ground based lidar measurements. [..] we stick to this approach since it has the advantage that all data sets analyzed in this study can be treated with identical analysis routines thus allowing us to directly and quantitatively compare E_P-values from four independent data sets.

In addition, we have added Figure 15 with its corresponding discussion (repeated below from our response to reviewer 1):

We finally attempt to determine the quality of the corrected Ep -values in Figure 14 by comparing them to Ep -values using a horizontal background determination method. Horizontal estimation of T_0 was previously found to be superior to a vertical background determination by Khaykin (2016) and Schmidt et al. (2016). While the sampling statistics of the METOP RO-data on a daily basis (i.e., only 1100 profiles distributed over the whole globe) is too poor to allow us to apply a horizontal background determination to them we may easily perform a corresponding analysis of the high resolution IFS-data. For this purpose, the spectral model output of the IFS for December 2015 has been reconstructed at T42, i.e., at a horizontal grid spacing of 500 km. These fields have then been used as background temperatures $T_0(z,\lambda,\phi)$, where λ is latitude and ϕ is longitude, in order to compute monthly mean zonal mean distributions of Ep . Such monthly mean zonal mean Ep -distributions for December 2015 are presented in Figure 15. In the same figure we also show corresponding fields of the vertical kinetic energy, $VE = 0.5*w^2$ (Geller and Gong, 2010). Note that VE is a good indicator for gravity waves in the stratosphere since vertical velocities due to other air motions are significantly smaller. While VE-values are significantly smaller than Ep -values (by about a factor of 1000 in the IFS-model) it is still instructive to compare the spatial morphology of the corresponding fields. This comparison clearly reveals that the proposed correction of Ep -distributions derived using a vertical background determination (see Figure 14 and related text) improves the comparison between Ep and VE but that it cannot eliminate all features that are apparently not due to gravity waves. Closer inspection of the data sets reveals that this is partly because some of the non-gravity wave structures (mainly the TTIL) are not zonally homogeneous such that correcting for them using zonal mean fields cannot eliminate the non-gravity wave structures completely. We hence conclude that this correction may be recommended for application to data sets that can only be analyzed using a vertical background determination method such as for the METOP data with relatively scarce sampling statistics. However, even after this correction, regions within +/- 30° latitude around the equator need to be considered with care due to additional potential contamination of Ep by Kelvin waves or other planetary scale features. In any case, if the sampling statistics allows, our analysis clearly shows that in general a horizontal background determination is advantageous in that it better avoids contributions to E_P that are not caused by gravity waves.

Likewise, statements in abstract and conclusions were added.

Abstract: This correction may be recommended for application to data sets that can only be analyzed using a vertical background determination method such as the METOP data with relatively scarce sampling statistics. However, if the sampling statistics allows, our analysis also shows that in general a horizontal background determination is advantageous in that it better avoids contributions to E_P that are not caused by gravity waves.

Conclusions: In addition, this technique to derive and correct E_P based on vertical profiles was compared to an alternative method applying a horizontal background temperature determination method to IFS-data. We find that the above introduced correction may be recommended for application to data sets that can only be analyzed using a vertical background determination method such as the METOP data with relatively scarce sampling statistics. However, if the sampling statistics allows, our analysis also shows that in general a horizontal background determination is advantageous in that it better avoids contributions to E_P that are not caused by gravity waves like the TTIL and potentially also Kelvin waves and other planetary scale features with short vertical wavelengths (i.e., less than 15 km).

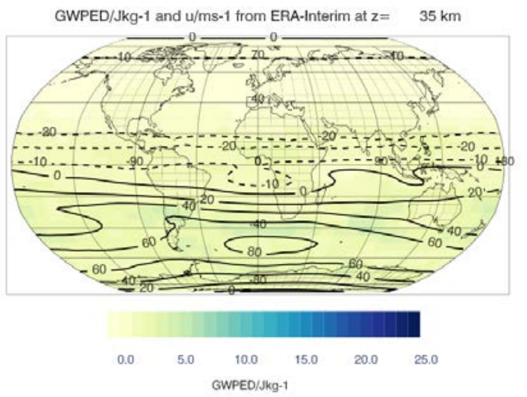
Specific remarks.
P4, L17-19. Although the correct references are in place, the fact that the compared data sets are not independent requires some more specific information on the assimilation of RO data in ECMWF IFS and ERA-Interim.

We have now pointed out more clearly that the data sets are not completely independent.

Thus, ECMWF-model fields and METOP RO-data are obviously not completely independent.

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.

This is a very good suggestion. As suggested we have inspected the E_P-distributions for June 2015 in the Southern hemisphere based on both ERA and IFS-data. While IFS-data do show moderate EP-values over the Patagonian hot spot at 35 km altitude (see lower right panel in Figure 12), ERA-data don't as shown below.



This supports our hypothesis that the coarser resolution of ERA-data leads to the inability to reproduce localized mountain wave activity. A corresponding short statement has been added to the text.

Note that we have checked this interpretation by also comparing Ep -distributions over the well-known Patagonian GW-hot spot for June 2015. While METOP and IFS-data show clear signatures of strong GW activity in this region (see Figure 12), ERA-Interim again misses to reproduce this GW activity (not shown).

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.

The vertical resolution of the two techniques is actually not so much different: 900m for the lidar and ~1.5 km for the RO-data (Kaifler et al., 2005; Kursinski et al., 1997). This information has been added to the text. Also the sentence has been reformulated to make clear that Ep is low-biased and not T.

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

This is correct and has been changed in the text.

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.

Here, one may not confuse the N^2-values shown in Figure 13 and the N^2-values used in the computation of Ep. For Figure 13, the N^2-values were calculated from monthly mean zonal mean T-profiles thus containing information on the climatological temperature profile. For Ep, however, N^2-values are calculated from the filtered individual temperature profiles such that scales smaller than 15 km (such as the TTIL) are suppressed. Hence the N^2-values shown in Figure 13 are an illustration of the climatological small scale structure in the temperature profile but do not enter the Ep-calculation. This has now been clarified in the text.

Note that the N^2-values in Figure 13 were computed from monthly mean zonal mean-temperatures. These must not be confused with the N^2-values used in our EP -calculation which is based on T_0-profiles. Remember that T_0-profiles result from filtering individual temperature profiles with a 5th-order Butterworth-filter with cutoff wavelength at 15 km such that T_0-profiles only contain spatial scales larger than 15 km, and hence do not contain information on the TTIL.

Technical remarks.

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

Done.

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

We changed the wording to “at the altitude levels 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.

We changed the wording to “higher altitudes”. Also, we consistently replaced the commas by points whenever used as a decimal separator throughout Section 6.

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

This must have been a problem of the reviewer’s pdf-file or pdf viewer. The one we see on the AMT-website does show the data.

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

Done as suggested.

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

Done as suggested. We reduced the scale to 0.9 – 1.1

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

Thanks, this has been corrected.

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

We actually found this presentation less confusing than showing the continental contours by mere black lines. Since this appears to be a matter of taste we have left this figure as it was.