Response to Anonymous Referee #2 (acp-2017-334-RC2)

Our responses are written in italic.

The changes in the manuscript are transferred and marked as quotations.

We thank the reviewer for his/her helpful comments and suggestions to improve the manuscript.

This study addresses the modulation of water vapour in the upper troposphere/lower stratosphere by mountain waves. It draws on a wealth of aircraft measurements made over New Zealand in the context of the DEEPWAVE campaign, and puts them to good use, combined with numerical simulations and soundings. The paper contains a rather thorough processing of these data (for example, using wavelet analysis), with the aim of understanding how mountain waves influence the behaviour of atmospheric water vapour near the tropopause. The work is highly relevant scientifically, namely because it reports on novel data, and may have climate implications, and is suitable for the scope of ACP. Both previous work on the topic and the scientific approach and methods are adequate and discussed in appropriate detail. The number of figures, tables and references included also seems appropriate. The conclusions presented are interesting, relevant and supported by the results. The manuscript is well organized and written in good-quality, clear English.

General comments

Since, as pointed out by the authors, the fluctuations of water vapour in an atmosphere with strong gradients of this substance can be explained using a mixing-length argument, it would be nice to see how well the mixing length obtained from this kind of argument (i.e. defined as the magnitude of the water vapour fluctuations divided by the water vapour gradient) compares with the wave amplitude obtained directly from integrating the vertical velocity. This would, presumably, give indications about the mixing effectiveness, as a mixing length substantially smaller than the diagnosed wave amplitude would suggest considerable fluid parcel dilution.

We fully agree with the reviewer that a calculation of the mixing length and comparison to the wave amplitude would be useful to evaluate the mixing effectiveness. However, the calculation of a mixing length is not possible since we do not have a realistic water vapor profile over the mountains. During the analysis of this case study we thought about calculating the vertical exchange coefficient K_q ($K_q = -\frac{\overline{w'q'}}{\frac{d\bar{q}}{dz}}$) that is similar to the mixing length argument you mentioned ($\frac{\overline{q'}}{\frac{d\bar{q}}{dz}}$) but for both methods a vertical gradient of H_2O is necessary. From our in-situ measurements of water vapor on the Falcon and on the GV we derived an approximate profile from ascent and descent and from changes between the flight levels upstream or downstream the mountains. In the region of the observed mountain waves no vertical water vapor profile exists. Also, we cannot use the water vapor profile from the dropsondes since they were launched at 12.2 km altitude and the humidity sensor does not work properly 2 to 3 km below the launch altitude (very low humidity in tropopause region). A vertical profile of water vapor from model simulations (ECMWF, WRF) is also not applicable because the humidity of the models and the in-situ measurement in the tropopause region shows discrepancies in absolute values and in the amplitude of the fluctuations. A detailed comparison would be necessary. Additionally, we tested a method to determine the vertical gradient of water vapor from a correlation between the vertical displacement and the water vapor fluctuations on each flight leg. The method was adapted from Smith et al. (2008)[1] displayed in their

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[1] Smith, R. B., Woods, B. K., Jensen, J., Cooper, W. A., Doyle, J. D., Jiang, Q. F., and Grubisic, V.: Mountain waves entering the stratosphere, J. Atmos. Sci., 65(8), 2543-2562, doi:10.1175/2007jas2598.1, 2008.

Fig. 9 and eq. 3. The vertical gradient of water vapor can be estimated from the slope of the linear relation between vertical displacement and water vapor fluctuations. However, the correlation scattered too much to derive a linear relationship between the two parameters. Thus, a vertical gradient of water vapor in the sensitive tropopause region has a large uncertainty. Due to this fact, we decided to leave out the discussion on the mixing effectiveness from the manuscript.

In Section 5 and Figure 7, some attention is devoted to the vertical profiles of the potential temperature theta and the wind velocity (U, V), for the purpose of calculating the Richardson number Ri. Although this is obviously highly relevant from the standpoint of turbulence generation, it would also be interesting to add panels to Figure 7 containing Scorer parameter profiles, computed from the same quantities, and discuss the implications of the vertical structure of these profiles in terms of vertical propagation (or trapping) and amplification (or decay) of the mountain waves.

We are grateful for this comment that gives an additional and interesting statement to our manuscript by explaining the observed wavelengths that are responsible for a vertical water vapor flux. The profile of the Scorer parameter is similar for all dropsondes launched during the flight RF16. It shows that linear gravity waves with horizontal wavelengths larger than 10 to 20 km can vertically propagate in the troposphere if they are excited by the flow over the mountain. In the middle and upper troposphere, the critical wavelength increases slightly resulting in an evanescent behavior for horizontal wavelengths smaller than about 22 km. These results of the vertical dependence of the Scorer parameter confirm our observations of horizontal wavelengths larger than 20 km that transport water vapor upward or downward.

“Another characteristic factor is the Scorer parameter ℓ that is shown in Figure 7c for the dropsonde launched at 07:55 UTC (44.39°S, 169.60°E) The Scorer parameter is used to estimate the critical horizontal wavelengths allowing vertical propagation of linear gravity waves under the given atmospheric conditions. The vertical profile of ℓ shows that gravity waves with horizontal wavelengths between 10 and 20 km are able to propagate vertically if they are excited in the lower troposphere. Between 4 and 9 km altitude, wave modes with horizontal wavelengths smaller than the critical wavelength of about 22 km become evanescent and may be attenuated. The magnitude of the estimated critical wavelength based on the Scorer parameter confirms our observations in the power spectra and wavelet cospectrum (Figure 5): the upward transport of water vapor is dominated by horizontal wavelengths larger than 22 km. A downward transport is possible by wavelengths smaller than 22 km due to a wave attenuation in the upper troposphere that is responsible for damping and partial reflecting of gravity waves. The vertical profile of ℓ is similar for all dropsonde launches (upstream and over the mountains) and is also comparable to an upstream ℓ-profile from the IFS forecast shown in Figure 3b in Bramberger et al. (2017).“

Specific comments

Page 2, Lines 23-24: "The transport of trace gas species may be reversible or irreversible, depending on mixing processes on different scales.". This sentence as it stands could be misleading. Any mixing will cause irreversibility, yet the reader gets the impression that reversibility depends on the scale at which mixing occurs. Consider rephrasing to clarify.

We agree and have rephrased the sentence to clarify the issue of (ir)reversibility of transport processes.

Page 3, lines 3-4: "The tracer-tracer correlation are based on a dynamic approach". Please replace "correlation" with "correlations". What is meant by "dynamic approach here? Is the purpose simply making a contrast with "microphysics" mentioned later in the sentence? If yes, this should be better explained.

We refer to dynamic transport processes as the main reason for the behavior of the tracer-tracer correlations and make clear that water vapor is special due to additional microphysical processes (cloud formation and freezing out of water vapor).

"In addition to transport and mixing processes, in cloudy situations, the tracer-tracer correlations for water vapor may additionally be affected by microphysical processes and cloud formation. Then, effects of clouds on the correlations have to be discussed in such situations."

Page 7, line 20: "with a lag of one and a lag of 10". It is not obvious to the reader why these values are used. Perhaps the authors should cite here (again) the reference where these assumptions are motivated.

The used autocorrelation factor in eq. 9 is determined by the method of Portele et al. (2017) in the way that large and small wavelengths are weighted similarly to be significant.

"The original time series is correlated with a delayed copy of itself (time lag) to obtain the significant parts of the cospectrum. The chosen combination includes signals of larger wavelengths (significant for high time lags) and smaller wavelengths (significant for lower lags) without stressing any of them (Portele et al., 2017)."

Page 11, lines 6-9: "In their study flux-carrying waves are larger than 20km horizontal wavelength. Small scale waves with wavelengths around 20km and less are mainly dominating in the vertical wind motion and do not carry any energy or momentum flux upward". It should be noted that, in the case of momentum or energy, the reason for this behaviour is dynamical, since only large-scale waves that propagate vertically (i.e. are not evanescent) transport momentum and energy vertically. For water vapour, this scale filtering cannot occur for the same reasons, since water vapour may be viewed as an essentially passive tracer.

We thank for this remark and agree that there are dynamical reasons for the behavior of the energy and momentum flux. Nevertheless, for this wavelength scale range (20 – 80 km) water vapor is a passive tracer for this wave dynamics. Since we use similar equation for the flux calculations the scale separation for momentum and energy transport is comparable to that for the trace gas transport. For larger scales (>100 km) other processes would be dominant but this is not in the scope of this analysis.

"In the statistical analysis of all GV flight level data during DEEPWAVE, Smith et al. (2016) also observed small and longer scale waves with different characteristics. In their study flux-carrying waves are larger than 20 km horizontal wavelength. Small scale waves with wavelengths around 20 km and less are mainly dominating in the vertical wind motion and do not carry any energy or momentum flux upward (Smith & Kruse, 2017). This is explained by dynamic reasons since only the longer-scale waves that propagate vertically and are not evanescent transport energy and momentum vertically. For water vapor as passive tracer the reasons for the chosen scale separation are the same in this wavelength range. Transport processes by large-scale waves with horizontal wavelengths larger than 100 km would be presumably different for energy or momentum and water vapor."

Page 14, lines 19-22: "Under the assumption that the change in the climatological distribution of water vapour may also be representative for our case of mixing induced by mountain waves, we estimate a radiative forcing > 1 W m^-2 locally above New Zealand during and after the mountain wave event.". It would be good to discuss the validity of this assumption a bit further. Under what circumstances is it expected to fail?

We discuss our assumption in more detail and emphasize the difference between our case and the study of Riese et al. (2012). For our estimation we use the same latitude range and altitude. Nevertheless, a large uncertainty exists since Riese et al. (2012) has a coarser resolution in vertical and horizontal dimension and is based on *zonally and annually averaged values. Additionally, our assumption is expected to fail in the absence of permanent mixing.*

“Under the assumption that the simulated difference in the distribution of water vapor as a result of enhanced mixing may also be representative for our case of mixing induced by mountain waves, we estimate a radiative forcing larger than 1 W m^{-2} locally above New Zealand during and after the mountain wave event. Riese et al. (2012) do not give a physical reason for the changes in the mixing strength, so our case may present a physical process (among other processes) contributing to the change in the water vapor distribution in the UTLS. While we used the calculations by Riese et al. (2012) at the measurement location, their study has a coarser vertical and horizontal resolution and is averaged over one year. We here neglect the seasonality in the water vapor mixing ratio that is present in the southern hemisphere at this latitude range. Thus, our estimate has a large uncertainty. Nevertheless, it emphasizes the relevance of mountain waves on the water vapor distribution and the radiation budget of the UTLS.”

Page 14, lines 27-28: "Further studies are required to evaluate the radiative forcing caused by changes in the water vapor mixing ratios due to gravity waves in more detail and/or on larger scales.". Why specifically on larger scales? What scales in particular?

We have removed the remark on larger scales (horizontal wavelengths larger than 100 km). The flux calculations refer to horizontal wavelengths smaller than 80 km but in the H_2O-O_3 *correlation all wavelength scales are included. Therefore, our estimation on the radiative forcing covers all wavelength scales. A separation of the impact of different wavelength scales on the radiative forcing would be difficult and is beyond the scope of this study.*

Page 16, lines 4-6: "The locally and temporally limited radiative forcing over the Southern Alps exceeded 1 W mˆ-2 and suggests that mountain waves may have a large effect on climate.". I suspect this may be an overstatement. To ascertain whether this claim is reasonable, the prevalence of mountain waves similar to those addressed in the present study would have to be taken into account. The tone of this remark could be moderated.

We have moderated the tone in this statement according to the reviewer’s suggestion. To our knowledge, this is the first study that estimates a radiative forcing in the context of a local mountain wave event. Since we have several mountain wave hot spots all over the world a study of the influence of mixing caused by these mountain waves on the radiation budget in the UTLS would be interesting.

“The enhanced water vapor mixing ratios in the tropopause region strongly influences the radiative transfer in the UTLS. The estimated radiative forcing for our case, locally and temporally limited over the Southern Alps of New Zealand, exceeded 1 W m^{-2} and suggests that mountain waves occurring in many locations all over the world may have a non-negligible effect on climate.”

Page 28, Figure 4: I do not think the large negative flux of water vapour that can be seen in the bottom graph between x=-50 km and x=+50 km is discussed in sufficient detail in the text. This is an intriguing feature, which may seem puzzling to the reader. I advise the authors to include an interpretation of it, even if speculative, justifying its intensity, location and extent.

We now have addressed this issue with an explanation regarding the tropospheric jet stream influence in this region. In Fig. 3 a decrease in the water vapor mixing ratio together with an increase in the potential temperature und changes in the horizontal wind components indicate an impact of the tropospheric jet stream between -50 and + 50 km distance that is also visible in the synoptic plots of Fig. 1. The vertical wind *component is not influenced by the jet stream. Since the decrease in water vapor cannot fully be eliminated by the used filter to obtain the water vapor perturbation we see this issue in the water vapor flux.*

“At the western edge of the mountains (between -50 and +30 km) we also observe a negative flux. This region is located in the vicinity of the tropospheric jet stream which influences the distribution of the water vapor mixing ratio by horizontal transport processes (Figure 3: decrease of H_2O from west to east between -80 km and 0 km distance). This behavior cannot fully be eliminated by the used filter and is thus present in the water vapor perturbations by a few fluctuations with a negative weighting.”

Page 30, Figure 6: In panel(a), the caption does not explain what the red dashed lines represent. Please add that information. In panel (b), the dotted line corresponding to the water vapour flux filtered for waves with wavelengths between 20 km and 80 km does not include a point at z=7.7 km, but the solid line does. Why is that? This choice should be justified convincingly in the text.

We have added the definition for the red dashed lines. In panel (b) we now mark the two lines by different line character and different symbol to clarify that we show both wavelengths at each altitude point. At 7.7 km the values for both wavelengths are very similar which was not obvious in the first version of the plot.

Technical corrections

Page 1, Line 13 in Abstract: "GV research aircraft". Does "GV" stand for anything, or is it just the name of the aircraft? In the first possibility, please expand and explain the acronym.

The acronym is now explained in the text.

Page 7, line 9: "q=H_2 O". If the two notations are equivalent, why use "H_2 O" instead of the shorter and more convenient notation "q", as is done throughout the manuscript? Is there any particular reason for this?

We now use “q” in the whole method description section.

Page 11, lines 29-30: "By the absence of vertical or horizontal transport and the existence of a well-mixed atmosphere, we are expecting no flux divergence.". This sentence does not sound very well. Consider rephrasing.

We have rephrased the sentence.

Page 25, Figure 1(b): Is this simply a magnification of Figure 1(a)? If yes, this should be mentioned in the caption.

Fig. 1(b) is a magnification of Fig. 1(a) which is now mentioned in the caption.

Page 26, caption of Figure 2: "... and topography are shown". It would be good to indicate what denotes the topography, i.e. the grey area at the bottom (as done in e.g. Figure 5).

We have added this remark.

Page 27, caption of Figure 3: "the diagonal blue dashed lines in the bottom panel display the phase shift between the vertical wind motion and perturbations in water vapor and theta". It is not totally clear what this means. Does the phase shift correspond to the horizontal distance between the bottom and top of these lines? Please clarify.

We have rephrased the sentence to clarify the meaning of the phase shift.