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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Mountain waves modulate the water vapor distribution in the UTLS" *by* Romy Heller et al.

Anonymous Referee #2

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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.

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.

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.

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.

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.

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.

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 mˆ{-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?

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?

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.

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.

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.

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.

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?

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.

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

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.

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