Observed versus simulated mountain waves over Scandinavia - improvement by enhanced model resolution?

Reply to comments of anonymous referee 2 of manuscript acp-2016-765

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1 Introduction

We thank the anonymous referee for the comments and acknowledge his effort to improve our manuscript. We performed three additional real-case sensitivity simulations and 2D idealized simulations along the flight legs to clarify the meteorological situation and to improve vertical energy and momentum fluxes. The previous idealised simulations (HYDRO and TRAPPED) were removed as their set-up was more or less arbitrarily chosen and did not have a direct reference to the case study. We also removed the comparison of the observed and simulated reflection coefficient at the tropopause (former Fig. 15), as further investigations showed, that wave trapping did not occur at the tropopause. In addition, we revised the introduction and changed the title to "Observed versus simulated mountain waves over Scandinavia - improvement of vertical winds, energy and momentum fluxes by enhanced model resolution?" to emphasize the focus on GW-induced vertical winds and energy and momentum fluxes.

In the following, comments of the referee are marked with numbers and corresponding replies of the authors are written in bold and labeled with "⇒".

2 General comments

The authors investigate two mountain wave events over Scandinavia within the GWLCYCLE campaign using measurements and simulations. The campaign has a lot of valuable measurements such as airborne in-situ and lidar observations, which allow the authors to analyse the gravity wave (GW) observations in the upper troposphere and compare them with the simulated ones. The presentation of the observations is relevant by itself, showing the gravity wave events. On the other hand, simulations are focussed on exploring the horizontal resolution sensitivity and the topography influence when resolving the GWs, which are very interesting tests. Results show that topography needs to be resolved in order to capture the proper GWs and an horizontal grid around 2.4 km seems to be enough. Simulated GWs also seem to reproduce too small amplitudes and too much decaying, which is a significant result. The momentum and energy fluxes are also investigated, showing that simulations tend to overestimate them comparing with observations. The text is well written and figures and tables are clearly exposed, helping to understand and to illustrate the main results. However, there are a few comments I would like to point out:

3 Major comments

1. Have you tested other vertical level resolution? Does the vertical levels affect the resolution of mountain waves? Maybe near the stratosphere the number of levels is important and changes the resolved inversion layer. Could you run a test increasing the vertical resolution near the tropopause?

⇒ **We acknowledge the comment and performed an additional sensitivity run with increased vertical grid resolution, which has constant level distances of 80 m in the troposphere and lower stratosphere (CTRLVR, see Table 2). Leg-averaged energy and momentum fluxes of these simulations were slightly reduced by up to 2 W m^{-2} (Fig. 15, section 5.2). In addition we performed two further sensitivity runs with increased turbulent diffusion (HVDIFF and H2VDIFF, see section 3.1) to amplify non-linear wave effects. Energy and momentum fluxes of the H2VDIFF simulation were significantly decreased up to 6 W m^{-2} compared to the CTRL run fields (see Fig. 14 and Fig. 15).**

2. The main conclusion is that simulations with mesh sizes larger than 2.4 km cannot simulate the tropospheric GWs. What can you say about the differences between 0.8 km and 2.4 km? Have you checked if the waves propagate within the boundary layer? Are there any differences between these two simulations?

⇒ **The simulations show that the wave patterns are captured well in the D2 runs. However, on average vertical wind speeds are nearly 0.1 m s^{-1} smaller than in D3 runs and especially small-scale interfacial waves are weaker in the D2 simulations. Cross sections of both real-case and idealised simulations show, that waves also propagate in the extremely stable boundary layer (Fig. 6 and Fig. 9). Further idealised simulations with different boundary layer characteristics and their impact on the GW evolution were, however, not performed in this study.**

3. There could be more references in section 5 comparing the obtained results with other previous works.

⇒ **We agree and included some more references.**

4 Minor comments

4. Lines 257-262: It seems that during IOP1 there is a GW breaking at altitudes between 25 and 30 km and the horizontal wind speeds are reduced at these levels at the same time. Does it mean that when GW break the wind speed is reduced? Please explain or provide a reference.

⇒ **We added a reference in the text (L265).**

5. Line 269: "due to critical level dissipation". It is the first time in the manuscript that appears the concept of the "critical level". Could you add a sentence explaining better what is the critical level? Or maybe you could introduce that concept earlier in the introduction.

⇒ **We added the following explanation in the text (L275): "This means that the growing wave amplitude generates regions with nearly zero winds while the vertical wavelength approaches zero. This leads to convective overturning and turbulent wave breaking (?)"**

6. Line 305: I would say the GWs have weaker amplitudes, not the vertical wind. In addition, why do you think vertical winds are weaker compared to observations?

⇒ **We agree and changed the formulation in the text (L339). We think that amplitudes are weaker mainly due to numeric diffusion and added this in the text (L340).**

7. Line 382: "the change in stability at the tropopause is more distinct". Do you mean the inversion is much stronger at the tropopause level? Could you reformulate the sentence to be easier to understand?

⇒ **We left out this sentence and the comparison between observed and simulated stratification at the tropopause, as wave trapping occured along a stratified layer at an altitude of about 5 km.**

8. Figure 3. Colors of CTRL D2 (blue) and CTRL D3 (green) can be easily confused, and lines cannot be distinguished. I would recommend using different colors.

⇒ **We agree and changed the colors in Figs. 3, 7, 8, 12, 13 and 17.**

9. Page 28, caption of Figure 4. "...black and grey lines...". I guess black line for IOP1 is the green line in the plots, so "black" should be replaced by "green".

⇒ **The complete flights are marked with grey and black lines. Only the example flight leg 2 of the respective first research flights during both IOP1 and IOP5, which is used in this study is marked with a green line. We added this in the figure caption (Fig. 4).**