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

Reply to comments of anonymous referee 1 of manuscript acp-2016-765

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February 7, 2017

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 Summary

The manuscript presents a case study of mountain waves observed over Scandinavia during a field campaign that took place in December 2013. Two orographic wave events were analyzed using field observations and numerical simulations. These two events were simulated using global and mesoscale models over a range of resolutions and with real, smoothed, or no terrain to test the sensitivity of the simulated waves to model resolutions and resolved topography. The simulated waves and wave energy and momentum fluxes were compared with field observations. Their simulations with higher resolutions reproduce some gross features of the waves qualitatively similar to the lidar and airborne in-situ measurements. The authors showed that it is necessary to have high model resolutions and better resolved topography to simulate the observed trapped waves, which usually have shorter wave lengths than propagating hydrostatic waves. In my opinion, their diagnosis methods in general are sound and the results look reasonable. The manuscript is well written, more or less, and the overall figure quality is pretty good. There are a couple of issues that bother me.

3 Comments

1. First, the authors put a lot of emphasis on trapped waves and tropopause reflection. However, I don't think they actually demonstrated that the waves they referred to were trapped waves, or the atmospheric conditions supported trapped waves.

⇒ **We agree and performed additional 2D idealised simulations along the flight legs to simplify the meteorological situation and to demonstrate the formation of interfacial waves during IOP5 along a stratospheric intrusion at an altitude of about 5 km. It is true that interfacial waves were very weak during IOP1 and did not occur in 2D idealised simulations of this event (see section 3.2 and 4.3). Interfacial waves in idealized simulations of IOP5 had the same horizontal wavelength of about 10 km as interfacial waves in CTRL simulations (see Fig. 6, Fig. 10 and section 4.3).**

2. Secondly, overall, this manuscript reads more like a technical report instead of a scientific paper. This can be seen from the conclusion section, which mostly just recaps what's been done. The only conclusion from this study seems to be that topography needs to be well resolved in order to simulate short gravity waves. Of course, this is interesting, but not new at all. It has been known for decades and is the reason for gravity wave drag parameterization in coarse global models.

⇒ **We agree that especially the last sections of the paper repeated already discussed issues. Thus, we revised the conclusion section. By including the additional sensitivity runs we particularly demonstrate that our paper shows the skills and problems of a state of the art mesoscale model when simulating gravity waves. We show that even with horizontal resolutions of 800 m the observed wave field cannot be captured completely and that there are relatively large disagreements between observed and simulated energy and momentum fluxes.**

3. I think there is still plenty of room for improvement before being accepted for publication, and some suggestions are listed below. 1) For the trapped wave case, the authors need to show that those are actually trapping waves, beyond speculation. The vertical cross-section plots and w fluctuations along flight legs are too noisy to tell which and where are trapped waves. The authors showed Scorer parameter profiles calculated from their control simulations, which is helpful, and yet they didn't discuss much about the implication of these profiles. For example, from Fig. 8, it seems that only waves shorter than 30 km may be trapped bellow 5 km. However, in the abstract, the trapped waves ranged from 15 to 40 km. There are a few things they can do to support their argument: a. Solve linear wave equations (e.g., Taylor-Goldstein) for trapped wave modes using observed and simulated profiles, and hope that the observed and simulated trapped waves are consistent with linear wave solutions.

   b. Redo their idealized solutions using profiles approximated from the real profiles and hope the idealized solutions produce trapped waves with wavelengths comparable to the observations.

   c. Check phase relations between different variables and hope they are consistent with trapped waves.

⇒ **We agree that additional effort had to be done to clarify the meteorological situation. We used your proposal b) and performed idealised 2D simulations along the two example flight legs during both IOP1 and IOP5 (see Fig. 9 and section 4.3). Idealised simulations were initialised with mean upstream profiles of CTRL D3 simulations (Fig. 8) and show the wave formation under simplified conditions. It becomes clear that during IOP1 no (or very weak) interfacial waves occured along a tropopaus fold. During IOP5 waves were ducted in a stratified layer around 5 km altitude.**

4. 2) The role tropopause plays in wave reflection was repeatedly mentioned in the text to explain wave trapping, negative energy flux, etc. I don't quite follow the argument. Firstly, it seems that waves were trapped in the lower troposphere and, if so, why the tropopause reflection played a role in wave trapping (line 20, abstract)?

⇒ **We acknowledge the comment and adapted statements in the text to the new results obtained from idealised simulations. It is true that wave trapping did not occur at the tropopause during IOP5, but at a stratospheric intrusion layer at lower altitudes.**

5. Secondly, GW can be reflected by sharp change in stratification or wind, or by wave breaking zone. How can the authors tell it was the tropopause that did the reflection? Again, there are a few things they can do and should do here: a. Figure out where and by what the waves were trapped. If the waves were trapped between the tropopause and the ground surface.

⇒ **We agree and found that wave trapping occured at a stratified layer at about 5 km altitude during IOP5.**

6. b. Repeat the simulation with higher vertical resolution near the tropopause to see if the reflected fluxes increase and the up-going fluxes decrease due to the increased resolution, as they speculated (line 22 in abstract and places in text). This could be one of their most important conclusions from this research and shouldn't be built on speculation.

⇒ **We performed an additional sensitivity run with increased vertical grid resolution, which has a constant level distance of 80 m throughout the troposphere and lower stratosphere (CTRLVR, see Table 2). These simulations improved the leg-averaged energy and momentum fluxes by up to 2 W m^{-2} (Fig. 15). Additionally we performed two sensitivity runs with increased turbulent diffusion (HVDIFF and H2VDIFF, see Table 2 and section 3.1) to enhance non-linear wave effects. These simulations showed significantly reduced energy and momentum fluxes especially for the H2VDIFF run (Fig. 14 and Fig. 15).**

7. c. Compute fluxes at levels right below the wave breaking layer and right below the tropopause to see how much negative energy fluxes at each level. If the latter far exceeded the former, then the authors can conclude, with some confidence, that the tropopause reflection dominates.

⇒ **We computed flux profiles of all D3 simulations (Fig. 15) to show that fluxes were reduced in simulations with increased turbulent diffusion (section 5.2).**

8. 3) By the same token, the authors argued that the simulated trapped waves decayed faster than observed because of weakened reflection associated with lower stratification in the tropopause due to low vertical model resolution. Again, we shouldn't make conclusions based on speculation. There are a couple of things that can be done to help make their case. a. As in 2), according to their argument, the trapped waves should decay much slower in their new simulation with high resolution across the tropopause.

⇒ **We performed additional simulations with higher vertical grid resolution (CTRLVR), which did slightly improve the energy and momentum fluxes but not the decay of waves in the lee of the mountains (not shown). We therefore left out the discussion about lee wave decay.**

9. b. As shown in Smith et al. (2002) and Hills et al. (2016), there are a number of processes that could dissipate trapped waves and caused the rapid decay of their amplitudes with downwind distance. The authors could test the relative importance in their idealized framework.

⇒ **We think that it would be interesting to investigate reasons for stronger decay of trapped waves by means of the idealised simulations. This would, however, go beyond the scope of this study and we focused on the improvement of energy and momentum fluxes by means of additional real-case simulations.**