Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-765-RC1, 2016


Atmospheric Chemistry and Physics
Open Access
EGU
Discussions

# *Interactive comment on* "Observed versus simulated mountain waves over Scandinavia – improvement by enhanced model resolution?" *by* Johannes Wagner et al.

**Anonymous Referee #1**

Received and published: 25 October 2016

The manuscript presents a case study of mountain waves observed over Scandinavia during a field campaign that took place in the 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 length 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. 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. 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. 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. 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)? 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. 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. 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. 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. 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.

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-765, 2016.