Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-765-RC2, 2016

Atmospheric Chemistry and Physics Discussions
Open Access
EGU

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

Anonymous Referee #2

Received and published: 30 October 2016

General comments

The authors investigate two mountain wave events over Scandinavia within the GWL-CYCLE 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

Printer-friendly version

Discussion paper

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:

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?

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?

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

Minor comments

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.

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.

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?

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?

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.

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”.

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