Reply to the referee comments (gmdd-7-C2659-2014) on the paper "Vertical resolution dependence of gravity wave momentum flux simulated by an atmospheric general circulation model."

Dear referee,

We would like to thank you for providing constructive comments on our paper, which were really helpful to improve the manuscript. We have revised our paper following your comments as well as those provided by another referee. In the following your individual comments are quoted and our responses follow on.

Best regards,
Shingo Watanabe

The paper is concise and describes sensitivity experiments with an atmospheric general circulation model investigating the effect on gravity wave momentum fluxes of increasing the vertical resolution. A global model with a high model top (150 km) and with high horizontal resolution (0.56_) is used. Five runs with different vertical resolutions above 8 km are carried out. The authors show that the GW momentum fluxes are much stronger (by a factor 2-3) in the simulation with the poorest vertical resolution (dz=1000m), and that they decrease as vertical resolution increases. This is a surprising result, contrary to what is expected based on sensitivity experiments on the horizontal resolution (GW momentum fluxes increase with resolution). The authors illustrate that this is likely due to a lack of filtering in the simulation with poor vertical resolution, resulting in too many waves present in the stratosphere and mesosphere. They use this short paper as a means to open a discussion on possible mechanisms that could lead to this surprising behavior.
On one hand, the paper is well-written and present stimulating results. On the other hand, it is perhaps shorter than it should be, even if the purpose is only to start a discussion by a stimulating, counter-intuitive result. Indeed, with the simulations that are obtained, it seems that it should be possible to explore a bit more the behavior of the gravity waves, especially how the sensitivity to vertical resolution varies with latitude. Hence I recommend to accept the paper after major revision, addressing the points below.

Thank you very much for pointing out the important point. We have re-organized the paper and inserted two more figures and descriptions on the latitudinal variations. We have added Sections 3.5 and 3.6, and revised abstract conclusions.

Major points:

1. All the figures are for latitudes between 35 and 40_N. Is this behavior also observed at other latitudes?

A similar reduction of gravity wave momentum flux with increasing the model's vertical resolution is observed at almost all latitudes. We have inserted a new figure and its description, focusing on the zonal mean meridional distribution of gravity wave momentum flux between 90S and 90N. (Section 3.6)

2. There is significant orography in these latitudes, but the authors only insist on convectively generated waves; are there occurences of orographic waves? In other latitude bands (e.g. in the winter hemisphere, near the Andes) what is the sensitivity to the vertical resolution? It would be expected that the GW generated there are higher frequency, with deeper wavelengths, and so that there is less sensitivity, is this right? On the other hand, non-orographic gravity waves from jets and fronts presumably have near-inertial frequencies, so that there should here again be significant sensitivity. Is this right?

At the latitude we focused in the original manuscript, that was, 35-40N some orographic gravity waves do appear over mountains, but they dissipate near the zero-wind line, so that did not penetrate into the summertime easterly in the stratosphere. We have added a case for orographic gravity waves over the Andes (section 3.5), which we hope answer your question.

3. In locations where deep gravity waves are present (with wavelengths in the vertical of 6 km or more), how comparable are the different simulations?

Please have a look for the Andes case.

4. Figure 4 is obscure and hardly referenced in the text (p7565). The authors should either remove it or make a more significant use of it and improve considerably the figure.

We are sorry for the messy figure. We attempted to present it to stimulate discussions for causal mechanisms of the vertical resolution dependency. We have omitted the discussion section because thorough investigations and discussions on the causality of vertical resolution dependence of GWMF need more time. We have added some speculations on possible suppression effects caused by the thin GWs at the end of Section 3.4.

Minor points:
p7561, line 20: 'wavelengths from 188 to 40 000km': awkward; please rephrase, e.g. 'wavelengths larger than _ 190 km'.

Thank you. We have revised the text following your suggestion.

p7561, lines 23-25: the sentence is mysterious, I do no tunderstand what the meaning is. What are the 'observed phenomena'? In what sense fo explicitly resolved GWs explain these?

We are sorry for the confusing sentence. We have revised the text as follows.

Note, however, that a series of studies has suggested that the model qualitatively reproduces the seasonal and interannual variations of large-scale thermal and wind structures, behaviors of explicitly resolved GWs and vertically fine structures of the extratropical tropopause layer.

p7562, line 22: typo: Metrological

Corrected, thank you.

p7562, line 25: again, why such a precise lower bound (188 km)? Would 'shorter than 950 km' be sufficient? (Same for the caption of figure 1, and in the conclusion...).

This lower boundary corresponds to the minimum horizontal wavelength resolved by the model, which is obtained through 40,000 km / 213 = ~ 188 km. We have omitted repetition of the lower boundary from the text.

p7565, lines 1-2: please provide one or more references to illustrate the observational evidence confirming the existence of a peak at near-inertial frequencies.

We have inserted a reference: Sato and Yoshiki (2000).

p7565, line 2: ’the lowest inertial frequency’: do the authors mean ’lowest intrinsic frequency’?

Yes, we do. Corrected, thank you.