Reply to the referee comments (gmdd-7-C907-2015) 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 many thoughtful comments on our paper. We have revised the 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

Summary:
This paper examines the impact of a model's vertical resolution on the gravity wave (GW) momentum flux (GWMF) in the stratosphere. Since the model has high horizontal resolution (_0.5 deg), only very short (one-week) simulations could be performed. The authors find that when the vertical resolution in the stratosphere is increased from 1 km to 0.2 km the (eastward) GWMF in the stratosphere is significantly reduced, which is contrary to what happens when horizontal resolution is increased. They attribute the reduced GWMF in the stratosphere in the high-vertical resolution simulation to the presence of short vertical wavelength GWs near the tropopause.

General comments:
This is an interesting study, which could have important implications for GW parameterizations used in coarse horizontal resolution models. My major concern is that the experimental set up the authors have used has somehow strongly impacted on the results by causing the spurious generation or suppression of the longer vertical wavelength GWs that propagate into the stratosphere. There are a number of questions concerning the experimental set up that are unaddressed. To generate the initial conditions for the high resolution run they simply interpolate the initial conditions of the low resolution simulation to the finer grid. Is it possible that this could have somehow changed the longer GWs? The authors consider the possibility that the different initial conditions have resulted in changes in the evolution of the tropospheric circulation, but

the figure they show to demonstrate that this does not impact on the stratospheric GWs (Figure 2) is highly qualitative. Is it possible that the tropospheric circulation (or perhaps the region of deep convection) has changed so that the generation of the longer GWs generated by the convection is different?
The authors therefore need to more deeply examine the possible impact of their experimental set up on the results. They need to demonstrate that the longer vertical wavelength GWs propagating up from the lower troposphere (i.e., the region below 8 km where the model resolution is the same for all simulations) is largely unchanged for the different simulations. They also need to tone down their statements regarding cause and effect that appear in the abstract and conclusions. Based on the scanty evidence that they provide, statements like "GWs with short vertical wavelengths likely play an important role in determining the GWMF in the stratosphere and mesosphere since they are unjustified and misleading.

Thank you very much for provide comments on the experimental settings. We understood your concerns.

1) Indeed, the tropospheric circulation and locations of convection differ in the simulations with different vertical resolution, which can be seen in Figure 2c and 2d for precipitation, and Figure 3a and 3b for instantaneous background wind fields. It is difficult to argue that the observed differences in GWs are not affected by those differences. However, we believe that the systematic and global reduction of GWMF with increasing vertical resolution (Figures 1 and 5) cannot solely be explained by differences in the tropospheric circulation and convection.

2) The vertical interpolation in the preparation of initial conditions destroyed balanced state and generated spurious GWs at the beginning of spin-up. After about 48 hours, obviously strange waves disappeared at least from qualitative point of view. We have added a short sentence at the end of Section 2:

"Because the vertical interpolation used in the preparation of initial conditions disturbed the original dynamical state, spurious GWs appeared during the initial spin-up, though we found them to be negligible during the analysis period."

3) It was found that the longer vertical wavelength GWs observed in the summertime lower stratosphere were not excited well below 8 km. In this sense, it is difficult to

say that GW excitation processes are similar in the runs with different vertical resolutions. Figure 4 (new) shows an example for orographic GWs. In that case phase structures of GWs in the troposphere are qualitatively similar to each other.

4) We agree with you that the original manuscript overemphasized the impact of thin GWs. We have toned down about that. The abstract and conclusion have been revised.

Specific points:
Abstract (l.10-12); "found that inertial GWs with short vertical simulated at higher vertical resolutions likely play an important role in determining the GWMF in the stratosphere and mesosphere." – the word "likely" is far too strong based on the scanty evidence that is provided.

We agree with your comment. We have revised the abstract and use a word "might" instead of "likely".

p.7560 l.16: "deep individual convections" –> "convective systems"

We have revised the text following your suggestion.

p.7560 l.22: "studied in great depth" – change "great" to "considerable" since there hasn't really been that many studies of the effects of horizontal resolution on GWs.

We have revised the text following your suggestion.

p.7561 l.5: "covering" –> "coverage"

Corrected, thank you.

p.7561 l.20: "horizontal wavelengths from 188 to 40000 km". This is true at the equator, but due to the convergence of meridians shorter wavelengths are resolvable at higher latitudes.

We have revised the text: just say "horizontal wavelengths larger than ~ 190 km."

p.7562 l.22: “meteorological” is misspelt

Corrected, thank you.

p.7562 l.23: What is a “spherical” filter?

We have revised the text to clarify the meaning, that is, a high-pass filter based on the spherical harmonics

p.7562 l.26: Explain why only the eastward component of the GWMF is discussed (i.e., easterly background winds in stratosphere filter out westward propagating GWs).

Here we discuss the “net” eastward component of GMWF, because it is of primary importance in the momentum budget in the middle atmosphere.

Revised sentence: *This paper focuses on the net vertical flux of eastward momentum associated with the GW components, because it is of primary importance in the momentum budget in the middle atmosphere.*

p.7564 l.17: Explain why the GWs in the lower stratosphere are found to the east of the strong precipitation. This is presumably a result of filtering by the background winds, since near the convective source region I would expect that the forced GWs would be propagating in all directions.

We agree with your expectation. GWs propagating westward against the westerly winds in the troposphere are filtered near the zero wind line (Figure 3).

p.7566 l.11-14: The existence of low-frequency thin GWs in the lower stratosphere likely causes : : :” – the word “likely” is far too strong based on the scanty evidence that is provided.

We agree with your comment, and changed the word “likely” to “might”.

Figure 3: The purple contours are difficult to see, and the contour labels impossible to read. Since the point of plotting the heating is presumably to show where the strongest convection is, I suggest plotting only a single but thick contour for a value of large

heating.

We are sorry for the messy figure. We have revised the figure to use thicker purple contours. We only plot a single contour level of 0.1 K/h without contour labels.