

Review of revised version of “Vertical resolution dependence of gravity wave momentum flux simulated by an atmospheric general circulation model” by Watanabe et al.

The authors have addressed all of my concerns with the exception of two, which need to be addressed in the manuscript before I recommend acceptance.

(1) The first concerns one of my general comments:

"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?**"

which they authors replied with:

"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."

but appear to have made no changes to the paper itself. The authors *must* discuss this in the paper, not to me, the reviewer. Their answer clearly indicates that they cannot say with any certainty that the differences in the GWs in the two simulations are not due to differences in the troposphere. They must at least point that out in the paper by adding a statement like "Although differences in the tropospheric circulation in these two runs could account for the differences in the GWs, we believe this not the case...." and then provide a reason why they think this is so.

(2) My second point concerns their third comment:

"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."

which was again in response to my general comment regarding differences in the

tropospheric simulations resulting in differences in the upward propagating GWs. Again, their response ("it is difficult to say that ...") is very subjective and no change made to the paper. They need to add a statement in the paper to that effect, i.e. similar to what they need to say regarding my first point above.