Comments on "Tuning of a convective gravity wave source scheme based on HIRDLS observations" by Q. T. Trinh et al.

The present manuscript constitutes an extension of the results described in a very recent Atmos. Meas. Tech (2015) paper, by Trinh et al. A correct parameterization of GWs still represents a great challenge. Here, simulated GWs taking into account different sets of spatial and temporal scales to represent convection sources are generated and propagated following trajectories calculated from the GROGRAT model. In order to compare the simulations with HIRDLS observations, a comprehensive observational filter is applied. The observed spectra reproduce the spectral shape and location of the peak by a combination of four scale sets. The contribution of these waves to the momentum balance is evaluated by calculating zonal mean cross sections of absolute GWMF and its vertical gradients and comparing them to respective observed quantities. Several features regarding wave propagation and visibility in the middle atmosphere as well as zonal average of filtered simulated GWMF and wave drag are discussed against observed GWMF. The horizontal distributions of absolute unfiltered and filtered GWMF are also presented in this work. A good agreement with observed horizontal distributions in the structure as well as the magnitude is claimed. Main convection hot spots are reproduced. The GWMF spectra in terms of zonal phase speed and latitude are shown.

In this manuscript, the scientific results and conclusions are presented in a clear and well-structured way and the results and figures, well presented too. As the authors finally say, I agree that due to the limitations of current global observations, the synergetic use of physics based models, observational filter and observations using both absolute values of GWMF and its vertical gradient is very important to infer the true properties of GWs in the atmosphere. Nevertheless, my main concern is related to the consequences on the final results, after the chain of implicit and explicit accumulated assumptions and hypothesis involved throughout these calculations.

Main points:

1. The 2D restriction of cloud parameterization and the Yonsei 3-layer model scheme. How resistant may be expected to be the filtered and unfiltered GWMF distributions to different schemes for convection and other years, taking into account the interanual variability and climatological departures (e.g., ENSO) from 2006?

2. The exclusion of sources other than convection, mainly orographic sources at mountaineous regions, may represent an important restriction. The importance of penetration of mountain waves into the middle atmosphere and aloft as well as their vertical flux of energy and zonal momentum is broadly accepted (e.g. Preusse et al 2014 and referenes therein).The derivation of eq. (3) for GWMF assumes a single (or prevailing) monochromatic wave and the mid freuencey (hydrostatic) approximation. CGW is a multiscale problem. On the other hand, the GWMF strictly due to mountain waves might be better described by this equation than the contribution due to convection, as far as a common feature in mountain waves spectral analyses is the assumption of one or two prevailing modes of oscillation and a relatively small amount of energy distributed along the remaining spectrum.
3. As the authors clearly state, the adoption of parameter choices determined by deep convection regions for the entire respective hemisphere represents one important limitation of this approach.
4. The absolute value of filtered GWMF is lower, in comparison with this observed magnitude, which, as discussed above can be explained by a lack of other sources different from convection. In addition to this, as the authors state the geographical distribution due to the effect of the observational filter may be different at different regions.
5. The comparison of the unfiltered results with GWMF obtained from other limb instruments as GPR radio occultations, could be useful.
6. In my opinión, in Figures 5-8, besides the agreement in the detection of he hotspots, the considerable differences between subfigures a, c and e, should be better explained and discussed.

After the authors address these poins, I will be able to completely comment about the scientific significance and quality of the manuscript. Besides this, the paper addresses relevant scientific questions within the scope of ACP, as I mentioned above it extends ideas and tools presented in a very recent paper, the main new conclusion may be the global (lat-lon) distribution of GWMF, and the authors give proper credit to related work. The title clearly reflect the contents of the paper and the abstract provide a concise summary. I am not able to judge the english grammar quality as it is not my native language. The number and quality of references is appropriate.