Atmos. Chem. Phys. Discuss., 15, C10656–C10662, 2015
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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Characteristics of gravity waves generated in a baroclinic instability simulation" *by* Y.-H. Kim et al.

**Anonymous Referee #2**

Received and published: 19 December 2015

This article presents some detailed simulations and complex analysis (e.g., multi-dimensional Fourier transform, Miller's frontogenesis function) of the high-resolution idealized baroclinic waves with dry dynamics. The analysis on the two-dimensional phase-velocity spectrum of the waves should be considered as the highlights of this article, since it has not been investigated enough by other literature. The conclusion of paper is easy to understand and remember, and it provides new insight on improving the gravity wave parameterizations related to waves from fronts. However, I strongly believe that a MAJOR REVISION will make this study more useful for understanding the dynamics, characteristics, generation mechanism, propagation of the gravity waves in the dry idealized baroclinic jet-front systems. Therefore, I suggest the authors consider these extra analyses in their revised manuscript.



Major comments

1. Limitation of the experimental design

In addition to the current experiments, I would also strongly suggest that the authors spend enough time (e.g., 1-2 months) in updating the current initial profile by including a more realistic upper-level jet, since the biggest concern for me on this paper is on the experimental design, where the background wind in the stratosphere differs from that in the real atmosphere. Based on the description from page 32655 (line 28) to page 32656 (line 19), the upper-tropospheric jet-front system seems so unrealistic that the wave analysis in the stratosphere region is almost impossible. There are two major aspects in this paper, which may be actually related to the current model setting on the background wind.

Firstly, 8 km is chosen as a representative level on wave analysis (e.g., Figure 2-9 in the current manuscript). As far as I am concerned, 8 km is either within or below the jet core region, and it is largely within the upper troposphere as well. Compared to 8 km, many other articles actually choose a relatively higher altitude for wave analysis. For example, Zhang (2004) uses 13 km (e.g., his Figure 5), Wei and Zhang (2014) uses 12 km (e.g., their Figures 3-5), and they are generally above the potential source of upper-tropospheric jet-front systems. Instead, 8 km may be good enough for frontogenesis gravity wave, but it is really be too close to the source of upper-tropospheric jet-front, and source and wave are hard to be separate from each other within this region.

Secondly, I have an impression that the evidence for gravity waves generated by upper-tropospheric jet-front systems is rather weak in this manuscript. For example, based on the summary in the current manuscript, W1-W5 are all generated by the low-level fronts, regardless of the speed or latitude of the fronts. Therefore, I wonder whether the current model setting actually largely constrain the generation of gravity waves by the upper-tropospheric jet-front system. Please note that the authors actually use several sentences at the beginning of the paper and introduce the importance of gravity waves associate with the jets, so it is quite disappointing for me to realize in the end that the source of upper-level jets appears to be rather secondary in their results.

In addition to the above-mentioned suggestion, another concern related to the current study is on the resolution of simulation. For example, the horizontal resolution is ~10 km, and the lower bound of gravity waves in Table 1 could be as short as 40 km (only four times the horizontal resolution). Therefore, it would be interesting to know the sensitivity of the wave characteristics to the enhanced resolution. Similar work has also been done in some of the past studies (e.g., Table 1-4 in Plougonven and Snyder 2007 for dry idealized baroclinic simulations; section 6c in Wei and Zhang 2014 for moist baroclinic jet–front systems with varying degrees of convective instability). However, if there is not enough time for the sensitivity experiment to resolution, I think that it is okay to ignore it temporarily for now and save it for the future study.

2. Wave characteristics analysis and the associated uncertainties

The major part of the wave characteristics analysis is based on the multi-dimensional Fourier transform, which is acceptable and probably one of the best methods for calculating phase-velocity momentum-flux spectrum. However, there are also several limitations or aspects associated with the method. For example, Fourier transform may not be able to calculate the energy/amplitude for very short-scale waves or waves with very high frequency (which is limited by the spatial/temporal resolution). Also, there may be sensitivity to the chosen area for Fourier transform analysis. If the area is too large, it may cover the signals that are not interested. However, if the area is too small, the results may also suffer from the boundary error. Similarly, there may also be sensitivity to the chosen period for Fourier transform analysis. In this study, 24 hour is used as a time window for Fourier transform analysis, which may be rather short for W4 and W5, probably even for W3. Finally, according to Table 1 in the current manuscript, the ranges/uncertainties of the wave characteristics are quite large. For example, the range of the horizontal wavelength in W4 is from 70 km to 400 km. Also, the range of the vertical wavelength in W5 is from 5.8 km to 14 km, which could be twice as long



as the scale height (~7 km). Since there are large uncertainties in the wave characteristics, it is almost impossible to verify the consistency between the estimated wave characteristics and those predicted by linear theory.

Due to the above-mentioned factors, I would like to make the below suggestions for further improvement, even though I think that the authors should still keep most of the results with Fourier transform analysis. Firstly, please give a zoom-in horizontal plot for each of the W1-W5, and mark their corresponding locations in Figure 2 (as well as the areas for D1-D2). If necessary, please also show the corresponding zoom-in cross section plots as well. Figure 2 may be good enough for the overview of the waves, but it is hard to see each WP in detail. Secondly, please choose a representative height, a representative time step, and two neighboring representative phase lines in order to estimate the horizontal wavelength, vertical wavelength, as well as the transient phase velocity within a relatively short time (e.g., 3 hours). Thirdly, please evaluate the representative intrinsic frequency, vertical group velocity, and other parameters if available. Also, please verify the consistency between the estimated wave characteristics and those predicted by linear theory. Similar examples can be found in Zhang (2004; his Figures 5-9; his section 4d), Plougonven and Snyder (2007; their Figures 3, 6, 8), Wang and Zhang (2007; their Table 3), Wei and Zhang (2014; their Figures 6-9; their Table 1), and Wei and Zhang (2015; their Figures 2, 4, 5, 7, 8, 10; their Tables 2-3)

3. Frontogenesis Function analysis

It is very good that the analysis of Frontogenesis Function is included in the last part of the paper. In particular, it is interesting to know that Frontogenesis Function is found to be useful as an indicator for the generation of W1, W3, W4, and W5, but not for W2. However, there are still many questions in my mind, which may not be fully answered in this article. If possible, it would be nice if the authors could try to address part of my questions (if not all of them) listed as below.

3a) Please compare frontogenesis function with the large-scale diagnostics of imbalance in Plougonven and Zhang (2007) based on the spontaneous balance adjustment hypothesis, for the purpose of wave generation study. The authors could also include other available diagnostics or parameters (e.g., PV, horizontal gradient of potential temperature) as well. Practically speaking, which method is the best for gravity wave parameterization associated with fronts?

3b) Please try to reveal the relationship between frontogenesis function and the characteristics of waves from fronts (e.g., horizontal/vertical wavelength, wave amplitude), in addition to the results that gravity waves and fronts are quasi-stationary to each other.

3c) Based on Figure 11, how to choose the launch level or source level for parameterization?

3d) Please highlight the major differences and consistencies between gravity waves from low-level fronts and those from upper-level jets/fronts, including their wave characteristics, large-scale diagnostics for wave generation, and parameterization.

Minor comments

1. Line 9, page 32647. It is okay to use (k,l) here, but it would be much better if the authors could also provide their corresponding wavelength in physical space. I have the same suggestion for the other lines with (k,l), such as line 19 on page 32647, line 24 on page 32648, and so on.

2. Please provide the contour levels or contour intervals for the black contours and green contours in Figure 2. Please provide the contours levels or contour intervals for the background pressure in Figure 4. Also, please provide the meaning of the green lines in Figure 4. Even though the above-mentioned information may be mentioned in the manuscript or somewhere else, it is still necessary to provide them in the figure titles.

3. Section 2. Is any PBL scheme used?



4. Line 22-23, page 32657. Please explain the method of the calculation of intrinsic frequency. Is it again based on the Fourier transform? As I mention in the second part of the major comment, it appears to me that the ranges/uncertainties of the estimated wave characteristics are quite large.

5. Line 14-25, page 32657. I am wondering how to separate gravity waves and other signals (e.g., frontal circulation) at low levels where the background is very complex. Can they easily be separate from each other by scale?

6. What determines/controls the zonal velocity of the fronts at both high-latitudes and low-latitudes?

7. I also have a short comment on source mechanism analysis in this article. I am generally convinced that the source of the waves is the low-level front in this paper. However, strictly speaking, it would be more convincing to the readers if the four-dimensional ray tracing analysis (e.g., Wei and Zhang 2015) is included, in addition to the other studies (e.g., horizontal views at different levels, cross section study and frontogenesis function study). This is especially true for the waves that have travelled for a long distance or for a long time, or waves that have been largely constrained by the complex background (e.g., Plougonven and Snyder 2005).

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32639, 2015.