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

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The authors thank the referee #1 for his/her valuable comments. We clarify what the referee pointed out. The responses to each of the referee's comments are listed below.

Specific comments:

Page 32640

1. Title. The title seems to be a bit generic. It could be a good idea to include something about jets/fronts in the title, since this appears to be the focus of the paper.

> The title is changed following the suggestion.

2. Abstract L4,11: The shortened labels for the waves (W1,W2,W3,. . .) while useful

in the main body of the paper should not appear in the abstract.

> The shortened labels (W1–W5) are deleted in the abstract in the revised manuscript.

3. L8 ". . .eastward, which is difficult for the waves to propagate. . ." This sentence doesn't make sense. Perhaps split into two sentences e.g. ". . .eastward. These waves have difficulty propagating upward. . ."

> The sentence is modified in the revised manuscript [L7, P2] as suggested.

4. L13 "The generation mechanism . . . is discussed". Please state your results as to what this generation mechanism actually is, i.e. generation at the surface front.

> As the referee suggested, we state the results regarding the generation in the revised manuscript [L12–14, P2].

5. L4,13 It would be better to not use the acronym (GW) in the abstract.

> The acronym (GW) is deleted in the abstract in the revised manuscript.

Page 32641

1. L5/6. Presumably your simulations are initialised in a balanced state, so any mechanism of generation is going to be "spontaneous" – therefore, is geostrophic adjustment (which is the system adjusting to unbalanced initial conditions, e.g. Rossby 1938) actually relevant here? I suggest removing "geostrophic adjustment" and just retaining "spontaneous balance adjustment" – also sometimes called spontaneous adjustment emission (SAE).

> As the referee pointed out, our simulation is initialized in a balanced state, and thus, the geostrophic adjustment is not relevant to our simulation. We remove "geostrophic adjustment" in the revised manuscript [L5, P3].

2. L6/7. What is the difference between unbalanced instabilities and shear instability, or is shear instability a class of unbalanced instability?

ACPD

15, C12847–C12852, 2016

Interactive Comment

> The difference between the shear instability and unbalanced instabilities is that the rotation effect is not relevant for development of the shear instability. The shear instability can occur at very short horizontal scales, and it has been considered mainly in nonrotating flow. On the other hand, unbalanced instabilities typically have been considered in flow with small Rossby numbers (for more details, see Plougonven and Zhang, 2014).

Page 32644

1. L8. "Considerably small amplitudes" – I think you mean "negligibly small amplitudes".

> The phrase is changed following the referee's correction in the revised manuscript [L2, P6].

2. L10. Please define "Running average" a little more carefully. Does this have a time window over which averaging occurs (i.e. a moving average) or is it an average over all time from initialisation to the present instant? Why/how did you make this choice? A mathematical expression defining the background flow field would be helpful.

> The spatially moving average is performed at each instant. We do not perform any temporal averages because the time series of the spatially averaged field were slowly varying. We clarify this averaging method in the revised manuscript [L5, P6].

Page 32647

1. Just a comment. I really like the idea of separating out the wave packets via decomposing the spectral domain into various sectors. I haven't seen this done before but it seems a very useful technique.

> We thank the anonymous referee for the encouraging comment. The difference in the wave vector between the wave packets enables us to separate the waves.

Page 32650

1. L16. ". . .the isoline of c corresponds to an isoline of the vertical wavenumber m

ACPD
15, C12847–C12852, 2016

for a given background state, as m^2=N^2/c^2". I don't understand where this formula came from. I get c^2=w^2/K^2 where w^2= f^2+N^2 K^2/m^2 for hydrostatic waves. This only reduces to your result if you are assuming that K^2/m^2 » f^2/N^2. Are you making this assumption? In either case, please state where the formula comes from and any assumptions involved.

> Yes, we use that approximation ("medium-frequency approximation", see Fritts and Alexander, 2003) which is valid in our case (not shown). We clarify this in the revised manuscript [L17, P12].

2. L26. "Resonant generation of waves". Is this really a mechanism of generation? I agree that the vertical flow structure can be responsible for selecting the dominant scale of the waves. However, I don't see how resonance (which is a scale selection and amplification process) can be responsible for the initial generation of the wave. Surely the generation still requires some sort of flow imbalance, e.g. a sharp front?

> We agree that the resonance effect is not a mechanism for the wave generation but for the spectral shape of the generated waves. The statements regarding this point is revised as pointed out [L16, P12; L1, P21].

3. L29 and following page L1: The formula from previous is stated again here c=N^2/m^2. Note that a power-of-2 is missing on the c. Also refer to my comments above regarding where this formula comes from?

> The typo is corrected in the revised manuscript [L29, P12]. We clarify where the formula comes from in the revised manuscript [L17, P12], regarding the comments above.

Page 32652

1. L3-5. What wavelet function are you using? I don't understand the reasoning for the multiplication by exp(-z/(2H)). Why is this done?

> The Morlet wavelet function is used. This is stated in the revised manuscript [L2,

ACPD
15, C12847–C12852, 2016

P14]. The vertical velocity is normalized before the wave analysis in vertical direction because, in theory, the amplitude of gravity waves increases with height by exp[z/(2H)].

Page 32654

1. L13,17. Here you discuss that the waves might be damped by model diffusion. What are the values of the model diffusivity and viscosity used in these simulations?

> The horizontal Smagorinsky first order closure is used without any other (background) horizontal/vertical diffusion schemes. Therefore, the vertical diffusion coefficient is zero everywhere, and the horizontal diffusion coefficient varies depending on the horizontal deformation value at each model grid. In addition to the explicit diffusion, it should be noted that the significant model-implicit diffusion occurs for small-scale waves, as discussed in L13–18, P32654 in the original manuscript.

Page 32658

1. L12-14. You state that the “GWs are generated by the surface front”. However, there are many mechanisms of surface front generation; e.g. strain flow acting over a front (Shakespeare 2015, JAS) - this is a linear process - and where the front behaves as an obstacle to the surrounding flow (Snyder et al, 1993, JAS, also seen in Shakespeare 2015) - this is a non-linear process. Note that both mechanisms give waves that are stationary relative to the front. From your results, it seems that the second mechanism is the one operating in your simulations, but you could check this by evaluating the magnitude of the large-scale confluence (needs to O(0.2f) or greater for the first mechanism).

> Following the referee’s suggestion, the large-scale confluence is calculated using the background variables. It is confirmed that the magnitudes of the confluence near the fronts at z = 250 m are $\sim$1f on Day 4 and become much larger afterward (not shown). Therefore, we could not exclude the possibility of the first mechanism the referee mentioned. It was difficult to further identify the exact mechanism operating in

ACPD
15, C12847–C12852, 2016

our simulation. The paper mentioned by the referee (Shakespeare, 2015) is referred to in the revised manuscript [L9, P20].

Page 32660

1. I like the analysis using the frontogenesis function. However, it would be useful to label the packets (W1,W2, etc) on figure 11 to avoid the need for complicated descriptions of their locations e.g. “58-65 deg N west of 30 deg E”.

> The wave packets are labeled on Fig. 12 in the revised manuscript (Fig. 11 in the original manuscript), and the complicated descriptions are removed [L6–10, P22].

References

Fritts, D. C. and Alexander, M. J.: Gravity wave dynamics and effects in the middle atmosphere, Rev. Geophys., 41, 1–64, doi:10.1029/2001RG000106, 2003. Plougonven, R. and Zhang, F.: Internal gravity waves from atmospheric jets and fronts, Rev. Geophys., 52, 33–76, doi:10.1002/2012RG000419, 2014.

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