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Interactive comment on "Propagation of gravity waves and its effects on pseudomomentum flux in a sudden stratospheric warming event" *by* In-Sun Song et al.

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General comments

> **Recommendation**: Accept with minor revisions

> This is a nicely done, comprehensive study of gravity wave (GW) propagation from tropospheric sources into the middle atmosphere under background wind conditions prevailing during the sudden stratospheric warming of 2009. The authors use a ray tracing model to show how spatial inhomogeneity and evolution of the background flow alters the characteristics of propagating GW; and how

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Discussion paper

this 4D (x, y, z; t) propagation model differs from 2D (z; t) propagation in many nontrivial ways.

The study is well-written and easy to follow, and the results are relevant and timing, aligning with current efforts to better understand GW processes in order to improve their parameterizations in climate models. I have a few, very minor comments, and I believe the paper is basically publishable as is.

■ Authors would like to thank the reviewer for carefully reading and evaluating the original manuscript. We think we have corrected faithfully our original manuscript according to reviewer's comments. Please refer to the track-change version of revised manuscript for figure, page and line numbers to be mentioned below.

Specific comments (page, line)

1. (1, 16) "may have profound impacts": Why "may"? GW are the main component of the eddy momentum budget in the mesosphere and above. I would have written "have profound impacts".

   • "may" is deleted in the line 19 on the page 1 of the track-change version of the revised manuscript.

2. (1, 24) "radiatively-driven latitudinal temperature gradient across the two poles" → "pole-to-pole radiatively-driven latitudinal temperature gradient".

   • The phrase is modified in the line 4 on the page 2 of the track-change version of the revised manuscript following the reviewer's suggestion.

3. (2, 1) "Irreversible heat and heat fluxes": This does not make sense. I believe you mean to say "irreversible heat and momentum flux divergences".

- Modified sentences can be found in the lines 5–7 on the page 2 of the track-change version of the revised manuscript.

4. (3, 1) "predominance of non-dissipative wave-mean interaction": "Predominance" overstates the case. Kruse and Smith (2018) stated (their abstract) that "Non-dissipative accelerations are non-negligible and influence a [mountain wave's] approach to breaking, *but breaking and dissipative decelerations quickly develop and dominate the subsequent evolution*" (my italics). Perhaps you meant to say "importance" or "relevance" of non-dissipative interactions? In any case, irreversible changes of the background flow ultimately occur only through dissipation.

    - Modified sentences can be found in the lines 6–12 on the page 3 in the track-change version of the revised manuscript.

5. (3, 18) "GW activities" → GW activity (this is the standard usage, "activity" here being used as a collective noun).

    - Modifications can be found in several places on the pages 3 and 21 of the track-change version of the revised manuscript.

6. (4, 19) "where Λ_ns ($n = 1, \cdots, N$) denote": This is awkward and confusing because the trailing "s", which I believe is intended to denote a plural, could be taken to be part of the symbol. The standard usage for mathematical symbols is that they do not normally takes an "s" to denote plural. Replace this with "where Λ_n ($n = 1, \cdots, N$) denote ...". Note that this occurs many other times through the paper when referring to Λ_n and other symbols. Please do a thorough check.

    - Λ_ns is changed into Λ_n in the line 30 on the page 4 of the track-change version of the revised manuscript.

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7. (7, 19) "Then, τ_{def}s are computed" → Then, τ_{def} is computed. See previous comment. In standard usage τ_{def} stands for all cases of the "deformation" time scale. No trailing "s" needed.

  • The trailing "s" is removed in the line 28 on the page 7 of the track-change version of the revised manuscript.

8. (10, 4) Figure 3: I would delete panels (a) and (d) of this figure, which do not contain any information that cannot be succinctly explained in the text. On the other hand, there could be a little more discussion of the interesting panels (b)-(c) and (e)-(f). In particular, panel (b) indicates that OGW flux, F_p, is well organized in space in a single ensemble member. I presume this is due to the fact that F_p is strongly constrained by the OGW source parameterization, which depends explicitly on orography and low-level wind. By contrast, organization of F_p for the NOGW case only emerges in the ensemble because any single ensemble member is completely stochastic (panels (e) vs. (f)).

  • Following the reviewer's suggestion, Fig. 3 is modified, and thus panels for the stochastic parameters are removed (see the page 35 of the track-change version of the revised manuscript). Also, more discussions are added at the end of the page 10 and at the beginning of the page 11 of the track-change version of the revised manuscript.

9. (10, 32) "but being weakened" → but is much weakened.

  • The phase is changed as suggested in the line 17 on the page 11 of the track-change version of the revised manuscript.

10. (10, 32) "Transparently shaded areas": This is confusing. "Transparent" implies no shading at all. I believe you are referring to the areas overlain by gray(ish)

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shading. If so, please explain more clearly. Better yet would be to use some other means (e.g., cross-hatching) to denote the regions of non-significant differences to avoid confusion with the color shading meant to denote flux magnitude/sign.

- Following the reviewer's suggestion, the shaded areas are replaced with hatched areas in all the figures that contained the "transparently shaded regions". Also, "transparently shaded" is changed into "hatched" everywhere in the main text of the revised manuscript.

11. (11, 4): "westward Fps in the 4D are about 10 (28) times enhanced ...": This sentence is nearly incomprehensible. Please break it up into two digestible parts, the first referring to the 10X difference between 4D and 2D models in all but one of the parameterizations; and the second referring to the 28X difference in the case of the WM96b non-orographic parameterization. Also, omit the "s" at the end of F_p, here and in many other instances; see comment (4, 19).

- The sentence is broken into two around the lines 21–24 on the page 11 of the track-change version of the revised manuscript.

12. (11, 26) "zonal-mean ks": Here and elsewhere this should be "zonal-mean k"; see comment (4, 19).

- ks is replaced with k everywhere as well as in the line 13 on the page 12 of the track-change version of the revised manuscript.

13. (12, 6) "thermodynamic forcing terms": What are these? Are you referring to the dependence on N?

- "thermodynamic forcing terms" is removed, and some explanations are added in the line 28–29 on the page 12 of the track-change version of the revised manuscript.

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14. (12, 29) "meridional wavenumbers (ls)" → Here and elsewhere, "ls" should be replaced simply by "l"; see comment (4, 19).

   - "ls" is replaced by "l" in the line 18 on the page 13 of the track-change version of the revised manuscript. Plural from of mathematical symbols is modified everywhere in the revised manuscript following the reviewer's comment.

15. (13, 19) Figure 8: This figure shows the striking difference between the 4D and 2D models, especially in the discontinuous (in latitude) appearance of OGW F_p. This is a common problem in comprehensive global models, which usually employ 2D columnar GW parameterizations. Although wave-mean flow interaction will tend to reduce these effects, this does not necessarily happen for the right reasons; see discussion about "compensation" of parameterized vs. resolved wave forcing in Cohen et al. (JAS 2013, 2014). It might be worth mentioning this problem.

   - Following the reviewer's comments, some discussions are added in the lines 9–20 on the page 15 and the lines 12–14 on the page 21 of the track-change version of the revised manuscript.

16. (15, 19) Figure 11: You might consider showing panels (a) through (f) in vector form (vector background wind, U, vector horizontal group velocity, c_g). This would show more clearly the relationship between U and c_g; and also (for the intrinsic group velocity) the regions where that vector is non-negligible.

   - Following the reviewer's comments, some panels on Fig. 11 are replotted in vector field format. Please see the page 43 of the track-change version of the revised manuscript. Related discussions are rewritten for clarification on the page 17 of the track-change version of the revised manuscript.

17. (17, 12) "These enhanced eastward F_ps, if they exist, may induce more rapid recovery of the stratospheric jets, accelerating downward movement of the ES": This is an interesting effect, which would not be captured by the 2D columnar parameterizations used in most comprehensive models. Note again that "F_ps" should be simply "F_p" (no trailing "s").

- Please see the lines 28–29 on the page 18 of the track-change version of the revised manuscript. Again, the plural form of mathematical sysbols is modified everywhere in the revised manuscript as the reviewer suggested.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1046, 2020.

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