Response to Referee #2 – "The Role of Eddy-Eddy Interactions in Sudden Stratospheric Warming Formation"

Erik A. Lindgren and Aditi Sheshadri

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Author comments in red

Anonymous Referee #2

Review of 'The role of eddy-eddy interactions in sudden stratospheric warming formation' by Erik Anders Lindgren and Aditi Sheshadri

General Comments: This is an interesting and well-written paper trying to understand the role that nonlinear interactions between waves with different zonal wavenumbers (termed EEIs) can have in the formation of sudden stratospheric warmings (SSWs. Their main conclusions is that middle/upper stratospheric EEIs only have an influence on SSW frequency if the incoming wave flux from the lower troposphere is of a certain type (i.e., wave 1 as opposed to wave 2). They also show that the upper stratosphere is not simply a passive recipient of wave activity from below, but via EEIs can have a key influence on polar-vortex variability. They further apply their approach to examining the differences in the number of split and displacement type SSWs, finding that when wave-2 forcing is used, EEIs are necessarily required in the troposphere/lower stratosphere to produce displacements, but if wave-1 forcing is used, both splits and displacements are possible without EEIs. My overall suggestion is of minor corrections, which I list below as specific comments and technical comments.

We thank both referees for their careful reviews of the manuscript and their helpful suggestions. The paper has been edited based on the comments by the two referees, and most suggested changes have been made. Line numbers mentioned below refer to lines in the tracked-changes version of the manuscript.

Specific Comments: Line 37-38: Also de la Camara et al. (2019) and White et al. (2019) [both J. Clim] used chemistry-climate GCMs to examine the numbers of SSWs preceded by tropospheric wave activity, finding similar percentages to in Birner and Albers (2017; hereafter BA17). de la Camara et al. (2019) further found the same in the ERA-20C reanalysis.

Thank you, the suggested references have been added (L38-41).

Line 97: This opening sentence is seemingly not supported by the references provided later in the paragraph. From my understanding, neither BA17 nor Polvani and Kushner (2004) used a model to systematically remove EEIs from the stratosphere. Both used reanalysis datasets. Please clarify.

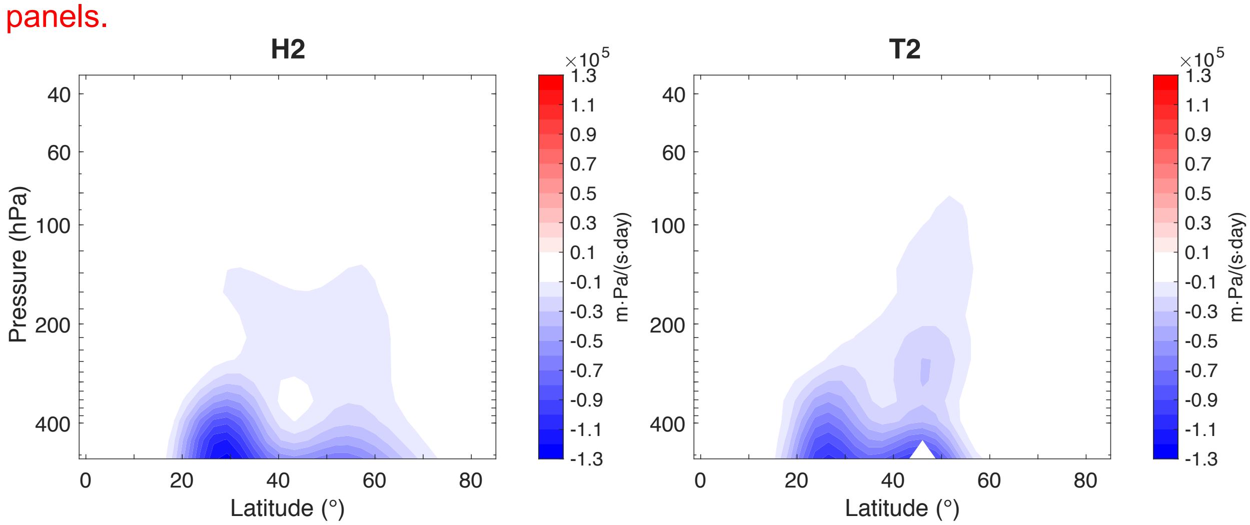
We thank the referee for pointing this out, the sentence was not clear. We mentioned these references to show that wave flux other than tropospheric flux is important, and therefore removing EEIs throughout the atmosphere cannot answer the question of where EEIs are important. The sentence has been edited for clarity (L104-105).

Lines 155-160: I feel that some reference should be made to Held and Suarez (1994) and Polvani and Kushner (2002) here as this sounds rather like their original setup(s).

It is correct that the setup is similar, we have added the references (L166-167, L169).

Line 161: At which pressure level is the imposed wave-1 or wave-2 heating perturbation cutoff? Please include here as it likely has an influence on the removal of EEIs in the lower stratosphere. From my understanding of Lindgren et al. (2018), the heating perturbation reached up to ~200hPa; is that correct here too? Further to this, I wonder why the authors did not force planetary waves using topography. Using heating which extends up to the lower stratosphere (at high latitudes), seems like it could have an influence on the removal of the EEIs, especially given the jump in stratification across the tropopause which plays a key role in monitoring the amount of wave activity which can propagate into the stratosphere (Chen and Robinson 1992 – the tropopause valve idea).

While it is true that the top of the heating perturbation extends to 200 hPa while the peak of the 4000 m wave-2 mountain used by Sheshadri et al. (2015) and Lindgren et al. (2018) only reaches about 450 hPa, the wave fluxes produced by these forcings are not very different. The figure below shows vertical EP flux in the upper troposphere and lower stratosphere for the model runs forced with wave-2 heating (H2) and wave-2 topography (T2) in Lindgren et al. (2018). The contour intervals are identical in the two panels.



As the figure shows, the vertical wave flux around the tropopause is very similar in the two model runs. Although the heating perturbation is cut off at 200 hPa the heating perturbation is very weak at high altitudes and the wave forcing is, like in the topographically forced case, primarily located in the troposphere.

Splits and displacements cannot be obtained in comparable amounts with topography (Gerber and Polvani, 2009; Sheshadri et al., 2015; Lindgren et al., 2018). The effect of EEIs on split and displacement ratios would therefore be practically impossible to investigate, and that is the primary reason why heating perturbations were used. This was mentioned in the Introduction, but we have edited the Introduction to make that clearer (L129-130).

Line 194: A citation of Birner et al. (2013; GRL) may be appropriate here who showed the importance of enstrophy fluxes (eddy-eddy interactions) in the region of the subtropical-midlatitude jet.

This is an interesting paper and we thank the referee for suggesting it; however, this sentence refers to the lower stratospheric equatorial region, a region that is further equatorward than the subtropics and midlatitudes studied by Birner et al. (2013).

Lines 204-231: This is a lot of discussion for the supplementary figures. I would consider including some, if not all, of these EP-flux figures in the main article itself rather than the reader continually flicking between the main and supplementary texts.

The figures have been put in the main text, and edited for clarity.

Lines 323-324: This apparent transfer between waves 1 and wave 2 could be quantified using the enstrophy flux term in the potential enstrophy budget. Indeed, Smith (1983) examined this budget for all low wavenumber waves during a particular SSW event. It even seems that it would be possible using your temperature tendency equation (just by decomposing the final term in the right hand side of eq 1 into different wavenumbers).

We thank the referee for this very interesting suggestion; indeed, we are following up on quantifying the precise nature of the interactions between wavenumbers 1 and 2 in the framework of the generalized quasi-linear approximation. In the current work we make the point that the variability of the stratosphere cannot be accurately represented without the presence of EEIs, and we plan on performing a decomposition of wavenumbers in future work.

Lines 349-351: This seems to suggest therefore that using an algorithm based on the absolute vorticity or potential vorticity may be more apt here. The algorithm used cannot capture proper splits or displacements, which I would think would still occur despite the lack of EEIs. On lines 362-363, you state that 'true splits and displacements do not occur in NEs1 or NEs2' – is this really the case? Or is it actually the case that your algorithm is not picking them up?

The fault does not lie with the SSW classification: the results of the classification were compared to absolute vorticity videos of each SSW in Lindgren et al. (2018) for H1 and H2 (the latter is referred to as subjective analysis in Lindgren et al., 2018). It was found that the classification agreed very well with subjective analysis. This type of subjective analysis was also performed for the no EEI runs used in this paper, and it was found that the SSW classification did very well in separating splits and displacements.

"True" splits and displacements do not occur without EEIs in the stratosphere simply because events where the vortex is displaced from the pole or split into two daughter vortices do not happen without EEIs. This is evident in Figure 4, where absolute vorticity on the central dates of SSWs are shown. This is even clearer in the supplementary videos show absolute vorticity from 30 days before to 30 days after the events in Figure 4.

The classification sorts SSWs as splits or displacements based on the relative strengths of wavenumbers 1 and 2, so SSWs will be classified as splits or displacements regardless of whether splits or displacements actually occur. This would be the case for any SSW classification that sorts SSWs into either displacements or splits.

Lines 359-361: This is the second time that barotropic instability has been mentioned as a possible candidate for the results found (as well as further mentions in the discussion section). I suggest for the authors to calculate the zonal-mean meridional PV gradient which should not be that difficult as you already have the absolute vorticity shown in Figure 4. From this, one could deduce pretty quickly whether there is any instability using the Charney-Stern criterion (Charney and Stern 1962).

We thank the referee for the suggestion. We calculated the meridional derivative of quasi-geostrophic vorticity (q_y) and found that q_y had opposite signs in the 20N-90N region of the stratosphere during the majority of all days for all runs, showing that the necessary conditions for barotropic instability are satisfied. A few sentences about this have been added to the paper (L355-358).

Technical Comments: Line 18: add ‘a few’ before ‘days’

Edited as suggested (L17).

Line 19: add ‘can’ before ‘migrate’

Edited as suggested (L20).

Line 55: ‘forced in the troposphere’

Edited (L61).

Line 61: ‘main’ > ‘mean’ Line 112: ‘in an idealized’

Edited (L68; L120).

Line 131: Please clarify what is meant by ‘SSWs, splits and displacements’. Currently it doesn’t read well.

Edited for clarity (L141-142).

Figure 2 and all other figures: please make the label axis, contour labels and colorbar labels etc bigger. They are difficult to see currently.

Label sizes have been increased in all figures.

Line 234: Is this definition also using the extra criterion suggested by CP07? Namely, that SSWs are spaced sufficiently far apart to assume independence (they used 20- 30 days I think)? The further conditions (regarding whether winds reversed back to westerly before April 30th, I presume, are irrelevant here).

Yes, that criterion is also used. This has been clarified in the text (L249).

Line 239: Was the difference in SSW numbers tested using a significance test? Line 283-284: i.e., there are less SSWs, as shown in Table 1.

A few sentences about statistical significance of changes in SSW frequencies as well as split and displacement ratios have been added (L253-255, L259, L278, L282-283, L338, L377, L381), and the method is described in detail in the supporting information. All differences in SSW frequencies are statistically significant at a 95% confidence level except those between H2, NE2 and NEt2.

Lines 287-288: Can you quantify this a little more? Over which latitude range does the jet move between runs? Do the SSW frequencies remain approx the same (perhaps that is what you meant by 'variability of polar vortex strength')?

We mean that the time mean positions of maximum jet strength are slightly different in the runs, which can be seen in Figures 2 and S1. The positions range from about 63N to 71N; this has been added to the text (L306-307). The variability of polar vortex strength refers to the latitude of interest in Figure 7 and Table 1 (u1060; 60N).

References

Gerber, E. P., and Polvani, L. M.: Stratosphere-Troposphere Coupling in a Relatively Simple AGCM: The Importance of Stratospheric Variability, J. Climate, 22 (8), 1920-1933, https://doi.org/10.1175/2008JCLI2548.1, 2009.

Lindgren, E. A., Sheshadri, A., and Plumb, R. A.: Sudden Stratospheric Warming Formation in an Idealized General Circulation Model Using Three Types of Tropospheric Forcing, J. Geophys. Res. Atmos., 123 (18), 10125-10139, https://doi.org/10.1029/2018JD028537, 2018.

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