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Interactive comment on "The Role of Eddy-Eddy Interactions in Sudden Stratospheric Warming Formation" *by* Erik Anders Lindgren and Aditi Sheshadri

Anonymous Referee #2

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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.

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.

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.

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).

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 $\sim$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).

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.

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.

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).

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?

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).

Technical Comments: Line 18: add 'a few' before 'days'

Line 19: add 'can' before 'migrate'

Line 55: 'forced in the troposphere'

Line 61: 'main' > 'mean'

Line 112: 'in an idealized'

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

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

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).

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.

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')?

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