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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 #1

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Using an idealized model, the paper assesses the role of eddy-eddy interactions (EEIs) on the sudden stratospheric warming (SSW) frequency and the number of splits and displacements. The paper shows that EEIs in the middle and upper stratosphere are important for SSW frequency, but whether the EEIs act to increase or decrease the frequency depends on the wavenumber of the forcing. Moreover, the paper shows that EEIs are important in the troposphere and lower stratosphere for displacements under wave-2 forcing, but that splits and displacements can form under wave-1 forcing without EEIs in the troposphere and lower stratosphere.

I found the paper to be well written and interesting to read and therefore think it will be worth publishing after the authors have addressed my specific comments below.

Specific comments:

1.) P2, L52-55 and more generally: Do the authors understand why forcing by heating perturbation vs. forcing by topography produces different SSW response with regards to split and displacement ratios? It looks to me that the applied heating perturbation (fig1 in Lindgren et al., 2018) is partially at the tropopause or even in the lower stratosphere in the polar regions. Could it be that applying the heating perturbation at the tropopause, where there is a sharp jump in the stratification, leads to a more non-linear behavior (and hence a larger role for eddy-eddy interaction) than a topographic forcing applied at the lower boundary? Do the authors expect the findings in their paper to be the same if forced by topography instead (where the eddy-eddy interactions might not be as important)? Some discussion on this, and why the use of heating perturbation instead of topographic forcing might be more realistic, would be insightful.

2.) It would be helpful if the authors would state more clearly what they mean by eddy-eddy interactions. In the real stratosphere there are also shorter scale gravity waves, that are not resolved in the model at T42 truncation, whose interaction with planetary waves might be important. The manuscript seems to only describe wave-1 interaction with wave-2. The authors should therefore stress that by EEIs they mean wave 1 and 2 EEIs. Additionally, is there any evidence to suggest that EEIs with wave-3 are important (Figure 4h suggests that there is a wave-3 signature)? The authors could plot EP flux and F_p for other wave components and compare magnitudes to those in Figs. S2-S5.

3.) The authors suggest in several places throughout the manuscript that barotropic instability may explain wave-2 structures in simulations forced by wave-1. The authors could easily verify if the necessary condition for barotropic/baroclinic instability is satisfied in their simulations to back up the statement and make the paper stronger.

4.) Would the results regarding SSW frequency and the role of eddy-eddy interactions be the same if instead of reversal of zonal wind at 60N and 10hPa, a sudden stratospheric deceleration events based on 2-sigma threshold (as in Birner & Albers, SOLA, 2017 and de la Camara et al, JClim, 2019) were used as a criteria? As the authors discuss (figs. 1 and 3) the climatology and variability of the polar vortex changes quite a bit as a result of removing/allowing eddy-eddy interactions in the stratosphere/troposphere. Therefore a deceleration event criteria might be less subjective.

More technical comments:

P2, L30: "climate" -> "climate variability" as SSWs do not necessarily affect the climate itself, as is shown in your Fig. 2 (i.e., while the frequency of SSWs change as a result of allowing/removing EEIs, the surface climate does not).

P2, L43-44. Please provide a reference for where a GCM has been extensively tuned to produce realistic SSW frequencies. Unless developed to specifically study SSWs, my understanding is that generally GCMs are tuned to produce realistic climatology. The fact that the SSW frequency is realistic is a by-product of this.

P3, L61: "main" -> "mean"

P3, last paragraph. Model resolution is surely another limiting factor of previous work that should be acknowledged. Model resolution determines what eddy-eddy interactions can be captured (see also my main point 2).

P4, L103: remove parenthesis around "Lindgren et al., 2018".

P7, L200 and L202: "pressure levels below 50hPa" -> "vertical levels below 50hPa". Otherwise it is confusing since "pressure levels below 50hPa" imply pressures 50hPa and lower, which is not what the authors intend.

P7, L208-209 :"at pressure levels greater than 50hPa" -> "at vertical levels below 50hPa"

Since Figs. S2-S5 are referenced quite a lot in the main text, it would make sense

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to put them in the main body rather than supplementary information. Also, why is the vertical extent plotted in Figs. S2 & S3 different to that in Figs. S4 and S5?

Table 1: Please explain what do the square brackets around some of the entries mean. While it is explained in the main text, it would be helpful to have this information in the table caption.

P11, L345: "u(\phi)" to me means just zonal wind at latitude \phi and not "meridional shear of the flow". Perhaps better use "du/d\phi"? Or since this is not really referenced elsewhere, I would remove this altogether.

P13, L387: "confirms" -> "confirm".

P13, L402: "lower pressure levels" -> "lower levels".

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