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Comment on wcd-2021-84

Anonymous Referee #2

Referee comment on "Differences in the sub-seasonal predictability of extreme stratospheric events" by Rachel Wai-Ying Wu et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-84-RC2, 2022

This paper examines predictability of wind deceleration and acceleration events using the ensemble hindcasts of the ECMWF for the period of 1998-2018. The variability and predictability of those events are examined according to the magnitude change of the zonal-mean zonal winds, its meridional curvature at 60N, 10 hPa and eddy heat fluxes in the lower stratosphere. It is found that the model can reasonably predict the acceleration events but unable to reproduce extremely deceleration events, which effectively the SSWs. The inability of the model to produce SSWs is linked to weaker-than-observed eddy heat fluxes in the lower stratosphere within the same 10-day interval.

The evaluation of the statistical representation of acceleration and deceleration events are interesting, e.g. the model continues to underestimate the long tails associated with deceleration events, even at short lead times; how the distributions of various quantities compared with reanalysis data sets. I however have major concern in terms of the dynamical reasoning. See comments below for details.

Major comments:

- The mechanisms that the authors identified are entirely consistent with the linear theory, which is adequate in explaining the climatological behaviour of stratosphere wave mean-flow interaction and polar vortex variability, but not sufficient in explaining the SSWs. Thus, the title of the paper does not match its content or key results.
- The results presented shade little new insight onto the predictability of extreme stratospheric events, i.e. SSWs. This is mainly because the authors use upper and lower 60th percentiles of negative (or positive) deltaU within a 10-day window to define the deceleration (or acceleration) events, which is not the standard measure of extreme events. For instance, a normal distribution can approximately capture the 60 percentiles of generalized extreme value (GEV) distribution, but it would fail to model the long tails of the GEV, which normally corresponds to bottom or top 1-5 percentiles

of a distribution. Thus, including small-magnitude events will result in better statistics but potentially hides the responsible mechanisms for the extreme events because the statistics provided by the 60 percentiles of a population is not representative of its extreme values.

- The deceleration and acceleration events appear to include high frequency variability (i.e. < 5 days), the effect is readily seen in Figure A1. The authors need to either justify the extent to which the effects of these high-frequency waves on the polar vortex variability in relation to the SSWs or applying a lowpass filter to the 6-hourly data so that the variation within the 10-day window is truly relevant to extreme stratospheric events.
- The SSWs are known to involve nonlinear processes such as wave breaking, resonance, and internal wave reflection, some of which the model may have failed to capture. For instance, erosion and filamentation due to wave breaking can increase meridional curvature as well as enhance zonal winds at polar vortex edge via PV sharpening. Thus, wave forcing from below does not always result in a weaker polar vortex within a 10-day time window. As such, the meridional curvature term uyy is not a good measure of waveguide.
- A few multi-panel figures are too complicated and some of the panels are redundant. See specific comment below.

Specific comments:

- Line 4, page 1, delete "limit".
- Line 10, page 1, "in a close to linear relationship", it may not be appropriate to study extreme events using linear relationship.
- Line 13, page 1, "wave activity pulses", I do not think that the authors studied wave activity pulses. The exact quantity studied is v'T' averaged within a 10-day window, which can contain only a part of wave pulse or multiple high-frequency wave pulses.
- Lines 35 and 45, page 2, polar vortex can be strengthened via wave breaking and PV sharpening as well.
- Line 59-60, page 3, very good point Re initial stratospheric conditions, but the authors did not study this factor in the rest of the paper. Consider rephrase or remove the sentence.
- Lines 123-134, page 5, using a fixed 10-day moving window to define the acceleration and deceleration events is problematic as it cannot properly differentiate high and low frequency variability thereby wave mean flow interaction. Harnik (2009) demonstrated that low frequency wave activity slows down the zonal winds while transient, high frequency wave pulses act to enhance the polar vortex.
- Lines 136, page 5, the polar vortex generally strengthens in Nov-Dec and weakens in March, how this seasonal cycle affects the classification of these events?
- Lines 141 -155, it is better to condense those roles/conditions and put them into Table 1. Also, the sample size for each subgroup in reanalysis are too small to establish robust statistics or to understand the relevant mechanisms. For instance, a strengthening of a polar vortex can be due to reduced upward wave forcing, PV sharpening via wave breaking, and/or enhanced meridional temperature gradient. It is nearly impossible to differentiate these causes merely based on 25 events.
- Line 153, "the chosen threshold …", at which pressure level and latitude?
- Lines 169-170, I am not convinced that the meridional curvature at 55-75N, 10 hPa is a good measure of refractive index for stationary planetary waves. The climatological EP fluxes at this latitude band and height location are mainly upward, suggesting the dominant role of vertically propagating Rossby waves. This also implies the important

role of the vertical component of the refractive index. It is the first time for me to read that the third term in the equation (5) is highly corrected with meridional curvature term at 55-75N, 10 hPa for the entire winter period from November to March. I would appreciate if the authors can demonstrate the correlation using scatterplots of the reanalysis data also the hindcasts using the 10-day window.

- Lines 173-175, are the uyy at 55-75N, 10 hPa and v’T’ at 45-75N, 100 hPa both calculated using the same 10-day window as well?
- Figure 3, without losing any information, a, b, and c can be combined into one panel. Also, panels d and e can be combined into one panel.
- Figure 4, if all the dashed lines were removed, would it lose any of the key information that the authors want to deliver regarding extreme stratospheric events?
- Figure 6, because the focus of the paper is on the predictability, only panels (b) and (e) are worth shown.
- Figure 7, the temporal evolution of uyy is almost identical to that of u itself, this implies that uyy at the polar vortex edge is not necessarily a good measure of refractive index as it contains the same high frequency variation that u has.
- Also figure 7, I do not see the LTG-xx lines differ from each other much, what is the purpose of showing them as a multiple panel figure if they can effectively be explained by a sentence or two?
- Figure 8, it is evident that the distributions of uyy have larger variance than those of u. This implies that uyy estimated in this study is not a good measure of waveguide. By definition, a waveguide for stationary Rossby waves should be slow varying. It appears to be measure of PV sharpening on top of background waveguide. Thus, the mechanism that the authors want to study is not captured by uyy.
- Figure 9, panels a, b and c of this figure once again suggest that uyy is not a good measure of background waveguide, opposite to what the authors claimed. Its variability is largely associated with wave breaking on both flanks of the polar vortex. Also, this figure be simplified, and the correlations can be summarized by a table or a couple of sentences.