

Weather Clim. Dynam. Discuss., referee comment RC2  
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## Comment on wcd-2022-39

Anonymous Referee #2

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Referee comment on "Using large ensembles to quantify the impact of sudden stratospheric warmings and their precursors on the North Atlantic Oscillation" by Philip E. Bett et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2022-39-RC2>, 2022

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Review for "Using large ensembles to quantify the impact of sudden stratospheric warmings on the North Atlantic Oscillation" by Bett et al. WCD

This paper examines how precursory features in the stratosphere and troposphere affect the predictability of the surface response to SSWs using a large ensemble approach. One of the biggest issues in trying to understand and predict how SSWs influence the surface is that there are so few SSWs that have happened since the satellite era began, that one cannot discern any robust signals. The approach used here is a good one to try and improve the signal to noise ratios. The authors find that the precursory features that best correlate with the post-SSW NAO response are the strength of the lower-stratospheric vortex, and the Polar-Cap sea level pressure anomalies at lags -10 to -1 before the onset. Other features such as the type of wave disturbance that dominates the vortex breakdown play a smaller role. Whilst each of these features has been assessed in studies before, this study nicely tests each one, using an approach that will (and should) likely become more commonplace in the literature.

The paper is well-written, well thought out, and interesting, and my comments are all quite minor; my recommendation is therefore of minor revisions. Please see below for a list.

Comments:

Title; The paper focusses on how the precursory state affects the subsequent tropospheric response to an SSW so as to aid in predictability of the response. I think this should be made more explicit in the title (refer to the 'predictability' or 'precursory' focus). The reason is that precursory features can only go so far in explaining the tropospheric response (and only in a probabilistic sense), and I think your correlations highlight this as the maximum correlation is  $\sim 0.3$ . What matters more mechanistically are the lower-stratospheric features after the onset date. Your current title generalises across both of these facets of the problem whereas I think a distinction should be made.

Lines 79-84; A couple of more recent studies that looked at the difference between displacement and splits are Hall et al. (2021; JGR) and White et al. (2021; JGR). Both found that the only salient differences in the surface response between the two occur at lags close to the onset date whereas differences in the surface response at later lags are statistically insignificant. Hall et al used reanalysis whereas White et al used an idealised GCM to artificially force displacement/splits.

Lines 101-102; Why are you only focussing on those three initialisation dates? Why not also use initialisations from later November? Given your focus on DJFM, is that to allow a 'spin-up' of sorts?

Line 104; can you justify why you use ERA Interim rather than ERA-5?

Line 109; Is it resampling with replacement?

Lines 113-114; Can you clarify how you calculated the climatology for both the model hindcasts and the reanalysis?

Presumably this describes the anomalies in the reanalysis, but did you use the same reanalysis climatology to calculate anomalies in the hindcasts?

Lines 120-121; Why not just include the events that occur in 1st-10th December and use the 10 days prior to that to examine the precursor stage? I wonder how many events would have occurred in that period anyway.

Line 147; Can you explain what the 'and significantly different to 50%' means? It is also present in other places, but clarifying once here would be useful.

Line 172; It was not clear to me what panel (d) shows. (c) is also a bit confusing but I think it is the 30-day averaged NAO anomaly after EVERY SSW regardless of hindcast resample. Is that right?

Lines 197-205; The Aleutian Low is usually a tropospheric precursor to SSWs (e.g., Garfinkel et al. 2010 whom you already cite) rather than a response to SSWs. Given the difficulty in teasing apart the response and cause around the onset date in observations, what would the PMSL look like when using an averaging window that does not include that first few days after the onset date, e.g., say, lags 5-30 or 7-30? It may well be that El-Nino is causing this Aleutian Low feature to persist, but it may also be that other factors are contributing.

Figure 4; Why are the number of SSWs in this figure smaller than in figure 3?

Line 219; Can this correlation be read off from Figure 4?

Lines 231-237; There is also a clear region over Eastern Canada that should likely be mentioned. Additionally, the region over the Ural mountains seems to match quite well with the Siberian High as found by White et al. (2019) to be probabilistically important for the downward response.

Figure 5; This is a very interesting plot! In some ways it reframes a somewhat known conclusion that the tropospheric response depends most strongly on the lower-stratospheric anomalies than those higher up. This has conventionally been shown using scatter plots (e.g., Maycock and Hitchcock 2015; Karpechko et al. 2017) but yours highlights the predictability change across all heights and precursory lags.

Section 4.3 and Figures 6-7; Above I mentioned two papers (Hall et al. 2021 and White et al. 2021) that found that the difference between splits and displacements (equivalent to your wave-1 and wave-2 dominated events) was most pronounced

at lags close to the onset date, within about 1-10 days or so. After that, the differences were very small and statistically indistinguishable from one another. Your 30-day window covering lags 1-30 after the onset merges these two periods together and I think it would be worthwhile to recreate these two figures for, say, lags 1-10 and 11-30 to see if indeed there are wave-1 vs wave-2 differences in the earlier period relative to the latter.

Lines 358-360; I feel that the Xu et al. study as described disagrees with your results, no? You find the middle stratosphere to be poorly correlated with the surface response compared to the lower stratosphere.

Summary and Discussion; You do not refer back to many of the studies listed in the introduction. All do not need to be referred to here, but a good proportion should likely be.

#### References:

Hall, R. J., Mitchell, D. M., Seviour, W. J., & Wright, C. J. (2021). Tracking the stratosphere-to-surface impact of sudden stratospheric warmings. *Journal of Geophysical Research*, 126. <https://doi.org/10.1029/2020JD033881>

White, I. P., Garfinkel, C.I. J. Cohen, M. Jucker, and J. Rao, (2021): The impact of split and displacement sudden stratospheric warmings on the troposphere. *J. Geophys. Res. Atmos.*, 126, e2020JD033989, <https://doi.org/10.1029/2020JD033989>