

Weather Clim. Dynam. Discuss., referee comment RC2  
<https://doi.org/10.5194/wcd-2021-14-RC2>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on wcd-2021-14

Anonymous Referee #2

---

Referee comment on "Emergence of representative signals for sudden stratospheric warmings beyond current predictable lead times" by Zheng Wu et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-14-RC2>, 2021

---

Extended-range predictability of sudden stratospheric warming events suggested by mode decomposition  
Z. Wu et al.

The authors have analyzed the dynamical evolution of Ertel's PV on the 850 K isentropic surface by considering a budget analysis of PV fluxes decomposed onto spatial patterns determined from an EOF analysis. They show composites of the evolution of the leading EOF pattern prior to sudden stratospheric warmings using ERA Interim reanalysis data and a longer integration of ISCA, an 'intermediate' complexity model. They further divide the warming events into wave 1 and wave 2-type events based on a wave number decomposition of the 100 hPa meridional heat flux.

Dynamically, they find in ERA Interim that the weakening of the vortex during wave-1 type events are more driven by linear terms over  $\sim 20$  days prior to the event, whereas wave-2 events have large non-linear contributions within five days prior to the central date. The numerical model shows roughly similar patterns.

Statistically, they find in both cases significant precursors roughly three weeks prior to the events, which they argue is evidence that SSWs may be predictable on timescales longer than the generally accepted two week lead time for deterministic predictability.

The mode decomposition analysis is a rather involved and difficult to interpret approach, but the authors have made significant efforts to connect their analysis to PV flux and wave-mean flow interaction diagnostics which are helpful. The results regarding the distinct processes leading to a wave-1 and a wave-2 warming are broadly consistent with other works that argue that wave-2 events evolve more rapidly, more non-linearly, more barotropically, and less predictably than wave-1 events.

However, much is made of the statistically significant precursors found in the time series of the leading EOF, their main metric for describing the evolution of the vortex; it motivates the title of the paper. Unfortunately, statistically significant composite signals preceding events don't imply anything about the predictability of the event in question. Just because the vortex weakens somewhat on average prior to the event does not imply that every time the vortex weakens a sudden warming will follow 25 days later. In other words - the composite suggests at most that this is a necessary condition, not at all that it is sufficient. To demonstrate evidence of predictability, the authors would need to identify relevant precursor events without reference to the warmings themselves, then show that warmings are more likely to occur following that event. This issue comes up in discussion of tropospheric 'precursors' to sudden stratospheric warmings all the time - there are statistically significant signals in the composite evolution prior to sudden warmings, but they turn out to be relatively useless as predictors because only a small subset of the tropospheric events are followed by a warming.

I have a few other comments, some more substantial than others, but addressing this concern is essential, and will amount to major revisions. The authors should either reframe the results from a dynamical point of view and remove reference to predictability, or show evidence for predictability that justifies this emphasis.

Further Comments

Climatology

It's not completely clear from the methodology just how the climatology has been computed, but the authors should consider imposing some kind of low-pass smoothing filter on the climatology if they haven't done so already. Given the finite number of years in the calculation, particularly for observations, there will be considerable residual high-frequency sampling variability that can artificially increase the small scale variance in the anomalies. This probably won't impact the low frequency modes too much, but it will affect the details of the high-frequency modes.

EOF calculation

From what I could understand, the EOF calculation has been carried out in two steps. The first analysis is used to obtain the structure of the leading EOF, then this variability is removed from the PV field and a separate calculation is carried out to obtain the remaining modes. It's not clear why this two step approach is adopted. I think it's connected as well to the fact that EOF 2 explains more variance than EOF 1 (Figs. 1, C1); I'm assuming that the percentage reported for EOF 2 is the fraction of the \*remainin\* variance that is explained by EOF 2.

If this multi-step approach is important, this should be clearly explained and demonstrated.

#### Description of ISCA model

The processes and parameterizations used should be briefly discussed; calling it 'intermediate' complexity is a bit ambiguous. E.g. is there a gravity wave drag parameterization? Realistic radiation? Ozone variability? etc.

#### Bootstrap methodology

The bootstrap methodology for computing statistical significance is reasonable, but the authors should use non-SSW days that have the same seasonal distribution as the SSW days to avoid aliasing with the seasonal cycle.

#### Minor comments:

-Sign convention for leading EOF: At present a positive A1 anomaly corresponds to a weakening of the vortex. This makes the descriptions awkward to me, e.g. in the abstract: the leading PC is an 'indicator of the strength of the polar vortex' but an increase indicates a deceleration. This took me a while to understand. The opposite sign convention might be more intuitive.

l48: the statistics are closer to two in three winters.

-Part 1 of the two papers published by Domeisen et al. in 2020 is labeled Domeisen et al. 2020b, and part 2 is labeled Domeisen et al. 2020a. The citations are appropriate as labeled, but they confused me because I am used to the more sequential ordering.