

Review of the manuscript “Extended-range predictability of sudden stratospheric warming events suggested by mode decomposition” by Wu et al.

General comments

The present study analyzes the dynamics of the breakdown of the polar vortex to extend predictability of sudden stratospheric warmings (SSWs). To do so, a mode decomposition is applied to potential vorticity (PV) equation on 850K isentropic surface by decomposing each term of this equation using the EOFs of PV. The first variability mode of PV is found to be related to the intensity of the polar vortex and more interestingly, its associated principal component (PC) starts increasing 25 days prior to the occurrence of an SSW, which is much earlier than the typical forecast period of SSWs (around 1-2weeks). The detailed analysis of the mode decomposition also provides a way to identify wave-1 or wave-2 SSWs 10 days before the onset of these events.

I find the analysis interesting and the results seem promising, particularly with the aim of extending the predictability of SSWs. Thus, I think this manuscript is worthy of publication in WCD journal. I have only a couple of comments that should be considered before publication.

My first comment refers to the predictability of SSWs by analyzing the tendency of the PC of the first variability mode of PV. I was wondering if the authors have also studied the number of events that present a rapid increase of this PC but they are not SSWs. How many false alarms would you get? I guess these “false alarms” would correspond to minor stratospheric warmings. I am also curious about the opposite case. Have the authors identified any SSW that would not be associated with a fast increase of this PC? In this regard, I am a bit concern about the first assumption of linearity to identify SSWs (i.e. the use of a PCA to identify these events). My concern is based on the importance of nonlinear processes in the development of some of these events (wave-2 SSWs) that is shown later in the study.

Secondly, I am not sure if the authors are aware of the study by Smith and Kushner (2012) where they analyze the evolution of anomalies of eddy heat flux and their contributing terms at different vertical levels for displacement and split SSWs. Their results highly agree with some of this manuscript, particularly the relative role of linear and nonlinear processes and their timing for the development of displacement and split SSWs. It is true that there is not a straight relation between split and displacement SSWs and wave-2 and wave-1 events, but Ayarzagüena et al. (2019) repeated the analysis for the latter and found similar results. I think that it would be interesting to discuss and compare the results of the present manuscript with those of Smith and Kushner (2012).

Specific comments

Lines 340- 343: I was wondering if it would be a good idea to analyze SSWs in long simulations of more complex models such as CCMs. For instance, some CMIP6 models include interactive chemistry and provide daily output of long piControl simulations. I am suggesting this because I had the impression that the conditions in the ISCA model simulations are different from those in the reanalysis. For instance, there is no

interannual variability in SSTs, but a strong warming in the equatorial Pacific is imposed. The evolution of the tendency of the PC#1 is also different in the reanalysis and the model. It seems that SSWs might be only predicted in advance much later in the model than in reanalysis. Thus, it is not clear if the difference between reanalysis and model results is due to the short reanalysis sample or model biases.

Lines 378-385: I would also highlight the positive (and statistically significant) values of the linear term for wave-2 SSWs just before the onset of events. Their values are of opposite sign to those for wave-1 events.

Lines 426-445: I wonder if it would be helpful to represent in the same plot va^* and Pa^{**} , one in contours and the other in shading. This might help to clarify the relation between Pa^{**} and va^* and nonlinear PV flux.

Technical corrections:

Line 348: I could not find the results that the authors mention in Figure 3 of Ayarzagüena et al (2019). Are the authors maybe referring to Figure 3 of Ayarzagüena et al (2018)?

Line 382: I think a “be” before “found” is missing.

References

Ayarzagüena et al. (2018) No robust evidence of future changes in major stratospheric sudden warmings: a multi-model assessment from CCM1, Atmos. Chem. Phys., 18, 11277–11287, <https://doi.org/10.5194/acp-18-11277-2018>.

Smith, K. L. and Kushner, P. J. (2012): Linear interference and the initiation of extratropical stratosphere– troposphere interactions, J. Geophys. Res., 117, D13107, <https://doi.org/10.1029/2012JD017587>.