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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "The effect of interactive ozone chemistry on weak and strong stratospheric polar vortex events" *by* Jessica Oehrlein et al.

**Anonymous Referee #3**

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The goal of the study is to evaluate the effects of ozone chemistry on the stratosphere-troposphere coupling during SSWs and strong vortex events. To this aim, two different configurations of the chemistry climate model WACCM are used: CHEM, the standard version, and NOCHEM, a version with prescribed zonal mean profiles of ozone and other relevant species. Very much like Haase and Matthes (2019), the authors find differences in the basic state and the signal of mid-winter SSWs on surface climate variability. No relevant results are found for March SSWs or for strong vortex events.

The paper is well written and structured, and easy to follow. However, I find the new findings incremental. The results are basically reproducing those from Haase and Matthes (2019), using the same models but with longer runs and different external forcing. The paper also lacks a clear explanation of the mechanisms behind the dif-



ferences in basic state and variability between CHEM and NOCHEM, they are simply attributed to "interactive chemistry". This could be true, but note that this is not the result of a forced experiment versus a control. This is essentially a comparison between two different models, one that has been exhaustively tuned at NCAR to provide the best possible climatology and variability (CHEM), and one that has been "downgraded" by specifying the evolution of ozone and other species (NOCHEM). The reported differences might be at least in part a consequence of this.

I think there is potential in the manuscript, but I cannot recommend publication in its present form. I will reconsider after major revision.

Specific comments:

- Line 18. Interannual variability of what?

- First paragraph of section 3.1. It should be good to at least speculate about a mechanism to explain the stronger westerlies in CHEM. In the light of the given comparison against Haase and Matthes and Smith et al, are high CFC concentrations needed to get this result?

- Line 119. I find it surprising that a 1.7 m/s difference be statistically significant, given the large interannual variability in the strength of the westerlies. But letting aside the statistics, how physically meaningful might this difference be?

- In line 153 it is stated that Fig. 3 reveals the surface impacts of SSWs. In the beginning of the following paragrah it is addressed whether the results of Fig.3 are due to SSWs. Please consider presenting first Fig. 4, and then Fig. 3 (or revise the way the story line is presented).

- Line 163. I am not so sure about this. The positive T signal in Fig. 5a appears at positive lags, generally after the peak of the warming. Without further analysis, this might well be interpreted as a slower temperature recovery in the aftermath of SSWs in CHEM than in NOCHEM.

- Line 168-169. "(. . .) perhaps because of greater wave activity necessary for an SSW to occur with a stronger mean vortex state (...)". The authors provide no proof of this, but it should be easy to check. Please consider plotting composite differences of U60 as a function of lag and pressure (to see if the vortex is stronger before SSWs in CHEM than in NOCHEM), and EP flux divergence over 45N-75N (to see whether there is stronger wave forcing in CHEM than in NOCHEM).

- In this same paragraph, an alternative explanation of stronger dynamical heating in CHEM could be that having a stronger jet in the mid-stratosphere generally implies a stronger vertical gradient of temperature over the pole (see Fig. 3b In Haase and Matthes). And a given w* anomaly, under a stronger temperature vertical gradient, would produce a stronger dynamical heating.

- Figure 5. It would be good to include the temperature tendency to better understand the effects and timing of the dynamical heating (please be explicit, is this w*dt/dz, TEM formalism?), and the radiative heating. Also, please include in the caption the latitude band considered.

- Line 170-172. So would it be possible to discriminate the fraction of the stronger long-wave cooling that comes from having reached higher temperatures, from the fraction that comes from having larger ozone concentrations? This would tell us the importance of the accumulation of ozone over the pole during SSWs to the recovery of the vortex.

- Figure 6. Please consider removing this figure, it does not really add much to the manuscript.

- Lines 183-190. The study of De La Camara et al also shows results on changes of polar ozone during SSWs using a similar version of WACCM as the one used here.

- Line 200. Please take into account that NAM decorrelation timescales are shorter in March-April than in winter (Simpson et al. 2011)âĄă, which may help understand the weaker persistence of surface anomalies after March SSWs.



- I am unsure whether it is worth keeping section 3.4, but 4 figures to only show a very weak signal seems excessive. - Conclusions, first sentence. This has not been shown, at least to my understanding. A mechanism behind the circulation differences between CHEM and NOCHEM has not been provided, nor have the authors discriminated between the effects of ozone chemistry and the effects of transport of ozone during SSWs.

- Line 253-254. It is a speculation that CHEM needs stronger wave forcing to drive SSWs, it has not been proven. Please modify this sentence accordingly.

Technical comments:

- Please refer to individual figure panels using the labels a, b, c, etc. and not top-right etc.

References:

Simpson, I. R., P. Hitchcock, T. G. Shepherd, and J. F. Scinocca, 2011: Stratospheric variability and tropospheric annular-mode timescales. Geophys. Res. Lett., 38, L20806, doi:10.1029/2011GL049304. http://doi.wiley.com/10.1029/2011GL049304.

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