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Comment on acp-2022-229

Anonymous Referee #2

Referee comment on "Exploring the link between austral stratospheric polar vortex anomalies and surface climate in chemistry-climate models" by Nora Bergner et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-229-RC2, 2022

This paper investigates the link between Southern Hemisphere stratospheric polar vortex variability and spring-summer surface climate. It compares the representation of this in both reanalysis and two chemistry-climate models (CCMs). The text concludes that there is, in general, a robust relationship between stratospheric extremes and surface climate, but that there are differing biases in the two models, which may be related to their climatological states.

Southern Hemisphere stratosphere-troposphere coupling is a topical issue, highlighted by recent extremes in surface climate such as Australian wildfires, large interannual stratospheric variability, and a background emergence of ozone hole recovery. Understanding the representation of this in climate models, as this paper aims to do, is therefore of great value. I found this paper to be clearly written, logically structured, and with clear figures that support the conclusions. I have a main concern around the choice of model simulations that are analysed, and consequences for the general applicability of the conclusions. I also include some more minor comments below, and I hope that the authors find these helpful.

Major comment:

1. The motivation for using chemistry climate models for this study is unclear to me, particularly given the analysis in appendix A1 showing that interactive chemistry makes relatively little difference in the stratosphere-troposphere coupling. I think that the relative expense of these simulations, meaning the study has just two models, limits the robustness of the results. For instance, it is very difficult to draw any strong conclusions about the relationship presented between model climatologies and stratosphere-troposphere coupling, as in section 3.4, from just two models. I think that the paper would benefit significantly from the inclusion of a much broader range of models, for at least this part of the analysis. For instance, preindustrial control simulations from CMIP6 are readily available and would be suited for this analysis.

Minor comments:

2. L7-8: I'd encourage against using this bracket construction. While it saves a small amount of space, it requires the reader to read the sentence twice.

3. L81: I question whether linear detrending is appropriate in the case of the reanalysis given that we have a nonlinear forcing (ozone depletion and stabilization/recovery). Perhaps the detrending can be split into two time periods to reflect this, or some more evidence presented that linear detrending is acceptable.

4. L122: A little more detail is needed on how strong/weak polar vortex events are defined. i.e. how are they distinguished from final warmings, and is there a minimum time gap between consecutive events?

5. L138: Is this interpolation to get jet latitude linear? If so it may introduce some errors, and this may be worth checking against the calculations detailed in the TropD package (Adam et al. 2018, Geosci. Model Dev., doi:10.5194/gmd-11-4339-2018)

6. L149: I think this would benefit from some more detail on how the bootstrapping works so that the reader doesn't have to refer to those references to check this important point. My guess is that the same calendar dates are used as the stratospheric events (to preserve seasonality) but the year is randomly varied?

7. L175: I'm not sure that saying the anomalies 'propagate down' is necessarily accurate here. For instance, the appearance of anomalies in the stratosphere and troposphere is at almost the same time for reanalysis and SOCOL (i.e. it appears barotropic).

8. L204-205: "We primarily focus on weak polar vortex events, for which the observed tropospheric SAM response is larger than for strong polar vortex events". Is the larger tropospheric response to weak vortex events due to the fact that weak vortex events are on average stronger (i.e. larger 10 hPa SAM anomaly) or because the stratosphere-troposphere coupling is stronger following them? I think this would be worthwhile expanding on a little.

9. L210: Using the term 'datasets' to refer to model simulations may be a little confusing.

10. L265-269: It is stated that it is unlikely that differences arise from short observational record. I think that this is an important point and suggest that it could this be tested quantitatively through some statistical testing.

11. L288-289: Note that Simpson and Polvani (2016) (cited in the paper) find that the jet latitude-shift relationship does not hold in summer. I think this should be discussed here and perhaps any conflicting conclusions with that study clarified.