Reply to reviews

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RC = Reviewer Comment
AR = Author Reply

Reviewer 2

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:

RC 2.1 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.

AR 2.1 The main motivation to use chemistry climate models is that important processes in the stratosphere are better represented (e.g. Eyring et al., 2010) and observational and model studies suggest the importance of ozone on surface climate based on correlations (Bandoro et al., 2014; Gillett et al., 2019). Including interactive chemistry in simulations is also shown to be important for the surface response in individual events (Hendon et al., 2020), on subseasonal timescales (Jucker and Goyal, 2022), and in the Northern Hemisphere (Friedel et al., 2022). A priori, we did not know that the interactive chemistry in these simulations on seasonal timescales makes relatively little difference in the Southern Hemisphere, which is why we have included these findings in the Appendix.

We agree that it is difficult to draw strong conclusions on the relationship between climatology and stratosphere-troposphere coupling from only these two models. For this reason, we have analyzed ref-C2 simulations from CCMI models (Morgenstern et al., 2017) focusing only on the time period 1980-2020. We chose the CCMI simulations over the CMIP6 preindustrial control, as their boundary conditions are more comparable to the reanalysis data, the stratosphere is better resolved, and they include the chemistry interactions as in our simulations. However, they also come with different caveats, as there are different setups between the simulations: some are forced with observed SSTs, while others are coupled to an ocean model and they also include trends as the reanalysis data. In particular, the imposed SSTs may not be consistent with the stratospheric variability and may interfere with tropospheric SAM responses. In that regard, our simulations with constant boundary conditions, a coupled ocean and 200 model years have an advantage for investigating the interannual variability. In spite of these caveats, the results from the CCMI historical simulations (ref-C2) confirm some of the results from the SOCOL and WACCM time-slice simulations.

We summarize the new findings based on CCMI as follows:

- The tropospheric SAM response (500 hPa) in spring-to-summer (October-January) following weak polar vortex events is inversely related to the stratospheric (50 hPa) SAM timescale (Fig 1), as evident from the statistically significant correlation (r=0.71). The persistence of circulation anomalies in the lower stratosphere has been shown to be an important indicator for the surface response of SSWs in the Northern Hemisphere (Runde et al., 2016; Karpechko et al., 2017). Here, we confirm the existence of this relationship in a very different context (stratosphere-troposphere coupling in the SH).

- The average surface temperature response in spring-to-summer (October-January) in Australia and Antarctica is correlated with the stratospheric (50 hPa) SAM timescale (Fig. 2 and Fig. 3).
- The jet latitude and tropospheric SAM timescale are positively correlated, but the relationship between jet latitude and the spring-to-summer (October-January) surface temperature response in Australia is less clear (Fig. 4). The surface response consists of a complex interplay between these different metrics (SAM timescale and jet latitude), and models with an equatorward jet and long SAM timescales tend to have a larger surface response (e.g. CMAM).

Overall, the CCMI model results support our conclusions that WACCM possibly underestimates and SOCOL overestimates the downward stratospheric impact due to their biases in the SAM timescale and jet latitude. Particularly the models' SAM timescales appear to impact the surface response of stratospheric anomalies. The differing surface patterns as a result of different jet latitudes are not directly confirmed by the CCMI results as some models show a strong warming over Australia despite an even more biased midlatitude jet stream than SOCOL (e.g. CMAM, CCSRNIES-MIROC3.2). However, prescribed vs. modelled SSTs make direct comparison difficult as these likely influence the surface impacts as well. The model simulations that show reasonably similar results to the reanalysis data are also closer to the reanalysis in terms of jet latitude and SAM timescales (e.g.MRI-ESM1r1, NIWA-UKCA).

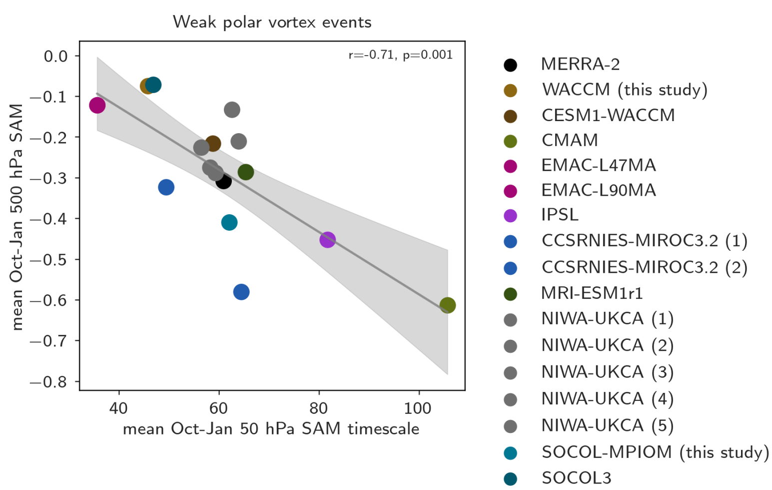

Figure 1: The mean Oct-Jan 50 hPa SAM timecale in each model versus the mean Oct-Jan 500 hPa SAM response following anomalous stratospheric weak events. The Spearman correlation coefficient and p-value are annotated in the upper right corner.

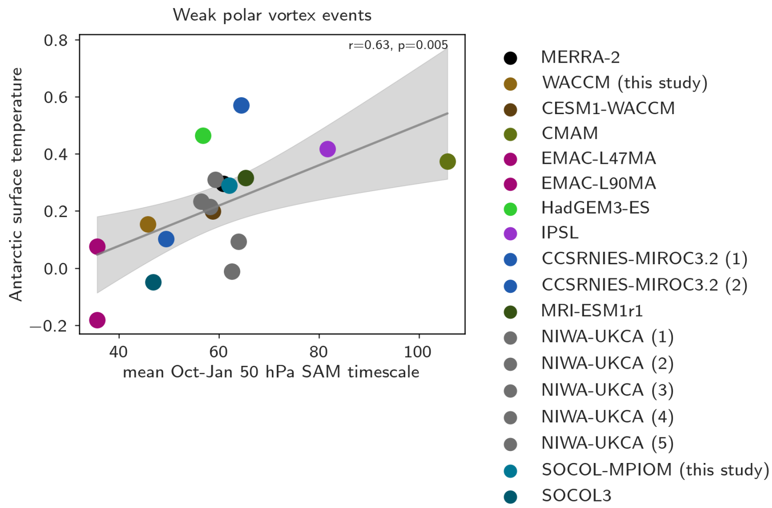

Figure 2: The mean Oct-Jan 50 hPa SAM timecale in each model versus the mean Antarctic temperature response following anomalous stratospheric weak events. The Spearman correlation coefficient and p-value are annotated in the upper right corner.

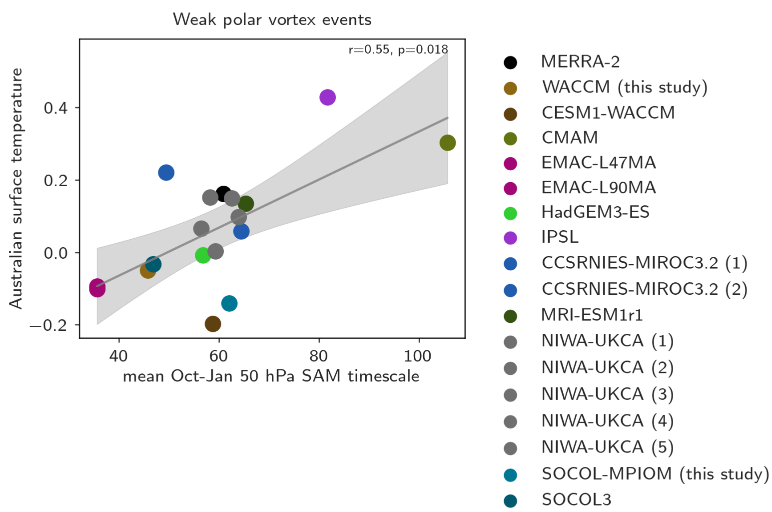

Figure 3: The mean Oct-Jan 50 hPa SAM timecale in each model versus the mean Australian temperature response following anomalous stratospheric weak events. The Spearman correlation coefficient and p-value are annotated in the upper right corner.

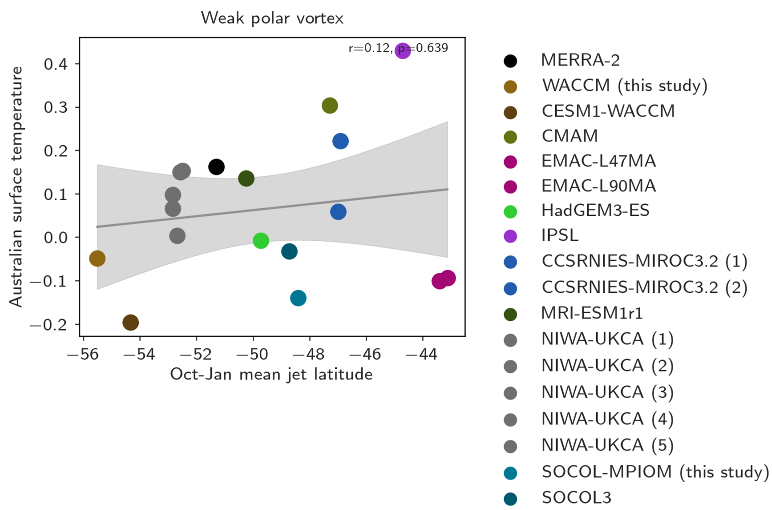

Figure 4: The mean Oct-Jan tropospheric jet latitude in each model versus the mean Australian temperature response following anomalous stratospheric weak events. The Spearman correlation coefficient and p-value are annotated in the upper right corner.

Minor comments:

RC 2.2 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.

AR 2.2 We would rephrase to:
The CCMs show a similar downward propagation of polar vortex anomalies as the reanalysis data: weak polar vortex anomalies are on average followed by a negative tropospheric Southern Annular Mode (SAM) in spring to summer, while strong polar vortex anomalies are on average followed by a positive SAM.

RC 2.3 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.

AR 2.3 We agree that in this time period, nonlinear forcings existed (e.g., ozone depletion and recovery). However, the trends are small compared to the interannual variability and comparing e.g. the tropospheric SAM response or the surface composites including / excluding linear detrending yield very similar results, as can be seen in Fig. 5 and Fig. 6. We also note that linear detrending has been used in previous studies on this subject (Lim et al., 2019), so we use this method for better comparability.

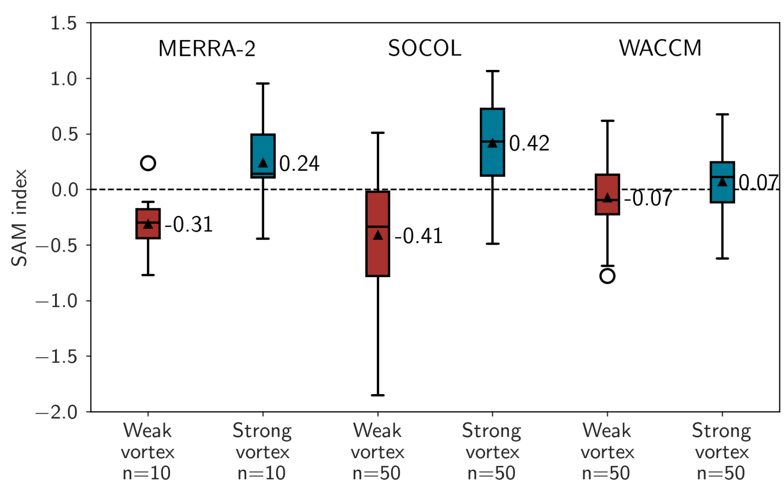

Figure 5: As Fig. 3 in the manuscript, but without detrending the data.

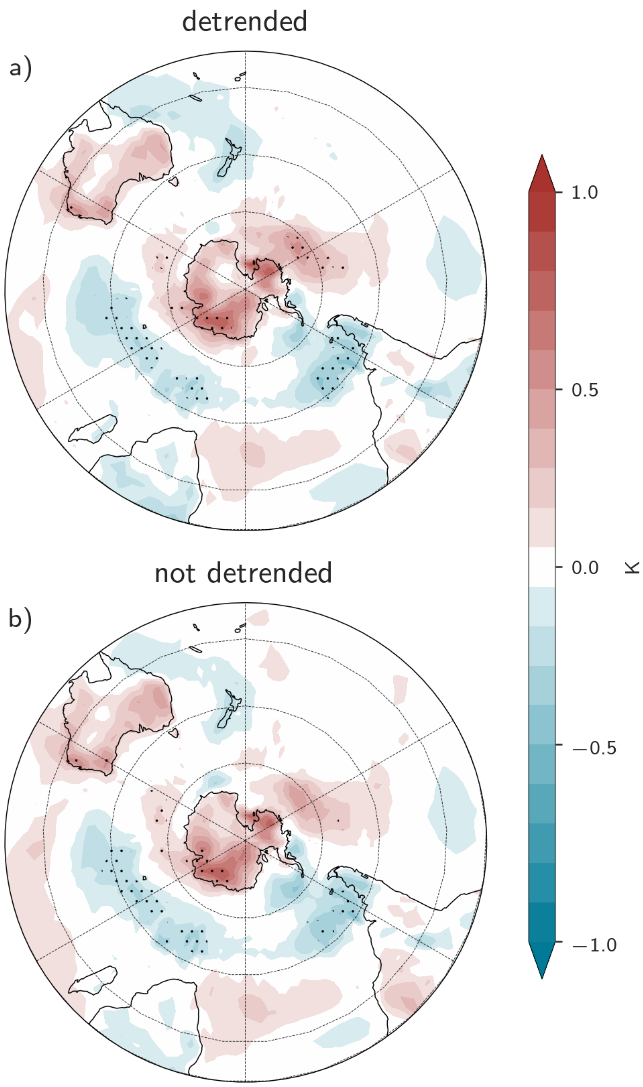

Figure 6: MERRA-2 Oct-Jan surface composite following weak polar vortex anomalies with detrended data (a), and with not detrended data (b).

RC 2.4 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?

AR 2.4 In this study, we only choose one event per year, as the timescale in the SH is so long that the westerly flow typically does not recover after a deceleration (i.e., weak anomaly) and only one event per season is typically observed (Thompson et al., 2005; Gerber et al., 2010). We do not directly distinguish weak vortex events from final warmings, since we only select the events based on ranking bottom/top 25% of SAM anomalies in August-November. Hence, early final warmings could in principle be counted as weak events (approximately one time in MERRA-2 and WACCM, 20 times in SOCOL, when we calculate the final warming dates based on the reversal of the zonal mean zonal wind at 10 hPa and 60°S without returning above the threshold for more than 10 consecutive days (Butler and Domeisen, 2021)). Not making the distinction between mid-winter weak vortex events and final warmings can be justified by the fact that early final warmings – especially in the southern hemisphere – are driven by the same mechanism as mid-winter vortex weakenings.

We would adapt this part in the manuscript to make it clearer: *As the SAM variance peaks in austral spring, we detect the largest and smallest anomaly in the daily 10 hPa SAM index between August and November each year. This allows only one weak or strong vortex event per year, which is reasonable as the dynamical timescales in the SH are long enough that the westerly flow remains weak/strong after a perturbation (Gerber et al., 2010). From these values, we define the highest and lowest* 25 % *as the strong and weak polar vortex events. Therefore, we obtain 10 strong/weak polar vortex events in the reanalysis data and 50 strong/weak events in the CCMs. We do not define a minimum temporal distance to the final stratospheric warming.*

RC 2.5 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)

AR 2.5 The interpolation of the zonal mean wind is based on a spline interpolation of second order (python scipy package). We compared the jet latitude index against the output of the TropD package and got almost identical results for MERRA-2 and WACCM. SOCOL differed a bit, but our jet index appears very reasonable when checking the underlying wind data (interpolated and not interpolated).

RC 2.6 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?

AR 2.6 The idea of the bootstrapping is to create composites of anomalous stratospheric events with different combinations of tropospheric states, similar to running ensemble simulations in a model. We resample different combinations of the 10 observed events, so by necessity repeating some events and leaving others out (resampling with replacement). We describe this resampling in l. 149-153: "The observed composite consists of 10 events with tropospheric states that are unrelated to the stratospheric signal. We randomly resample the 10 observed events with replacement to form 500 synthetic composites. In the synthetic composites, we allow an individual event to be repeated a maximum of three times. We thereby estimate how much the surface signal varies between the synthetic composites and how it relates to the strength of the polar vortex anomaly."

We realize this description may not have been clear enough for the reader to fully grasp how the method works. We would slightly adapt this in the manuscript, to make it clearer: *We randomly resample the 10 observed events with replacement to form 500 synthetic composites consisting of different combinations of the observed events, by necessity repeating some events and leaving other events out. In the synthetic composites, we allow an individual event to be repeated a maximum of three times.*

RC 2.7 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).

AR 2.7 We changed the formulation to: *The anomalies peak in the mid- to upper stratosphere following the onset date (by construction) and persist in the lower stratosphere for up to 90 days (Fig. 2 a,b), consistent with similar previous observational analyses (Thompson et al., 2005; Byrne and Shepherd, 2018).*

RC 2.8 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.

AR 2.8 This is indeed an interesting point and the asymmetry was first noted in Thompson et al. (2005). While it is likely related to the larger stratospheric anomalies, we do not know whether the stratosphere-troposphere coupling is different for the same magnitude of anomalies, which could be investigated in future studies.

RC 2.9 9. L210: Using the term ‘datasets’ to refer to model simulations may be a little confusing.

AR 2.9 We rephrased to: *However, the magnitude and spatial extent of the SLP signal differs among the reanalysis data and model simulations, with a much weaker signal in WACCM than in SOCOL, consistent with the differences among these models in their tropospheric SAM response (Fig. 3).*

RC 2.10 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.

AR 2.10 The mean of the bootstrapped Australian temperature anomalies in MERRA-2 and SOCOL are significantly different from each other based on a two-sample two-sided t-test, and respectively between MERRA-2 and WACCM. The average of the Australian temperature anomaly in MERRA-2 is 0.17 K. The corresponding quantile in the distribution of the subsampled composites is 0.93 in SOCOL, and 0.86 WACCM. Hence, both models are unlikely ($<$33 %) to capture the observed temperature anomaly. We thank the reviewer for raising this point and would like to add this new quantiative estimate to the manuscript.

RC 2.11 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.

AR 2.11 Simpson and Polvani (2016) show that the correlation between SH jet shift (with climate change) and jet latitude only holds in the annual mean, but not in summer. In our study, we do not consider the long-term jet shifts due to climate change. Instead, we explore the relationship between interannual variations in the stratospheric polar vortex and the tropospheric SAM in spring-to-summer. Hence, our results are not directly comparable to Simpson and Polvani (2016).

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