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Discussions

Interactive comment on "No Robust Evidence of Future Changes in Major Stratospheric Sudden Warmings: A Multi-model Assessment from CCMI" *by* Blanca Ayarzagüena et al.

Anonymous Referee #2

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Summary The authors look at 12 CCMi models to determine whether the frequency of sudden stratospheric warmings (SSWs) will change by the end of this century for a moderate climate change scenario. They also consider three different SSW metrics. The authors determine that there are no robust changes in the frequency of SSWs by the latter half of this century.

The paper is easy to read and the main message and methodology are clear. In general I feel positive about the paper and the conclusion but I do think the paper lacks much in-depth analysis above what has been done in previous work, most notably Kim et al. 2017. Some of the figures in the supplementary material might be worth including in

the main manuscript (Fig S3 for example). I suggest a major revision to address the points below.

General Comments 1) While this study looks at the CCMi models, which is a likely improvement in terms of interactive chemistry and stratospheric processes, it would be nice if this study more clearly explained how its analysis improves or expands upon those of Kim et al. 2017, which considered a large number of stratosphere-resolving (and non-stratosphere resolving) CMIP5 models and two different SSW definitions (and the more extreme RCP8.5 scenario rather than the RCP6.0 scenario used here). From what I can tell, the results were very similar in both studies (an increase in SSWs in future climate scenarios, though not a significant increase), though the message is quite different. While emphasizing the non-significance of the trend does seem important, the results are basically the same. It would be nice if this analysis had included some more in-depth analysis of the CCMi models in particular, maybe of whether models that had different characteristics (prescribed SSTs vs coupled, internal QBO vs nudged, solar variability or no) had different changes in SSWs. The two points below also outline some areas where more analysis could be considered.

2) This study does look at common SSW definitions, but given that the changes in mean zonal winds at 60N in Figure 4 are barely significant, it would be nice to consider zonal wind reversals at a broader range of latitudes. The authors did look at the 60-90N averaged zonal winds, but if that's cosine weighted it will be dominated by winds near 60N. 60N also seems right on the node between significant weakening winds and significant strengthening winds. It would be interesting to explore (and perhaps more clearly quantify) whether those models that showed no increases (or reductions) in SSWs had significant strengthening mid-latitude winds extending further north (seems to be true considering Fig S3). This might beg the question then, if the jet itself is shifting in the future, is maintaining a definition fixed at a particular latitude like 60N the best way to detect changes? I do agree that the polar cap anomaly/U60-90N results suggest it might not matter too much where it's defined, but exploring that sensitivity

more methodically might be useful.

3) While it does seem clear that there is no significant change in the frequency of SSWs in this analysis, the fact that there appears to be a quite robust weakening of the polar vortex (in a mean sense) is only briefly pointed out in the Discussion. It's worth keeping in mind that since the SSWs represent the tail end of the zonal wind distribution, they may be much more sensitive than the mean to small sample size and higher order moments (e.g., changes in skewness). The authors do mention that the "broadness" (variability) of the distribution does not seem to be changing over time- is it possible that the distribution becomes more skewed? I wonder though whether this weakening of the mean state could have potential climate impacts even if the most extreme events (SSWs) are not significantly changing.

Specific Comments Line 63-64: Some of the more recent papers on this topic should be cited here, including Kim et al. 2017 and Manzini et al. 2014, rather than only at the end.

Line 85: The Kim et al. 2017 results should also perhaps be mentioned here, because they investigated the definition sensitivity as well

Line 105-107: Would it be possible to consider 40-year blocks from 1960-2100âĂŤeither by moving the center of the 40 years by ∼5-10 years over the full period and getting a distribution that way, or by using a smaller time period (20-30 years) and looking at the change in frequency of consecutive 30-year periods over the entire run? I wonder whether that would give you a better sense for how variable the SSW frequency can be for any given 40 year period (maybe the variability between periods is much larger than the trend between the first and last period).

Line 119-121: How did you deal with the temperature criteria here; was the zonal wind first detected and then the temperature gradient had to reverse within a certain number of days?

Line 137-140: Could any other metrics be considered in addition? These are both interesting features but other metrics like the amplitude or depth of the reversal could also be worth considering (to try to further quantify whether these events will still produce significant surface impacts in the future).

Technical Corrections Line 56: change to "weather forecasts on intraseasonal timescales" Line 111: ERA-40 and JRA-55 extend further than 1979, is that what you mean? Maybe instead of "back of", change to "beyond"? Line 208: change to "possibly accounts for at least some of the mismatch between"

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