Reply to Referee #2

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.

Thanks a lot for your comments. We have addressed all referee's comments. In particular, we have highlighted more clearly how our results compare and extend upon other previous studies, particularly Kim et al. (2017). We have kept the figures in the supplementary material because they are devoted to the analysis of individual models, and these models agree in the main results. Thus, we think showing the multimodel mean values and its robustness across models for some variables is enough and highlights more easily the main conclusions. Nevertheless, we have extended the work and included new analyses such as the deceleration of the polar night jet during SSWs.

Here is our response to your general, specific and technical comments:

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.

First of all, we would like to highlight that the focus of our study and Kim et al's is different. Whereas ours focuses on the effects of projected climate change on the occurrence of SSWs in CCMI runs, the main goal of Kim et al is to search for a new definition of SSWs that is not sensitive to possible model biases. Kim et al perform indeed an in-depth analysis of the new algorithm in the present period in reanalysis data and CMIP5 models. As a secondary task, they apply the new algorithm to RCP8.5 simulations mainly to examine at what extent the application of their new algorithm may affect the conclusions for future frequency of SSWs. In contrast, in our study, we are interested in the future changes in SSWs and so, we do not only investigate the future changes in the mean frequency of occurrence of SSWs by applying different criteria, but we also examine other aspects such as duration of events, deceleration of the polar night jet or the preceding wave activity. Indeed, as far as we know, this is the first study that also compares the sensitivity of these other features to climate change in several models. In all these SSW features, not only the mean frequency, we do not find a statistically significant future change, and so, that would lead us to highlight the null future change. In the revised manuscript, we have indicated more in detail the strengths of our study in the Introduction and the discussion section (L97-100, and L241-243 and L272-275 in the marked-up manuscript, respectively).

Regarding the suggested analysis of the CCMI models, we agree that it would be a nice exercise to further compare the results for models with different characteristics, if we had found a variety of results among models. However, the same conclusion is reached for all CCMI models, a null change of SSWs in the future. It is true that in the case of the mean frequency of SSWs, a few models show a statistically significant change for one or two criteria. However, the number of these models is so low that we cannot derive any conclusions. Thus, it seems that the different characteristics of the models do not play a relevant role in the impact of climate change on SSWs. We thank the reviewer for this suggestion because it has helped us to highlight this extra point concerning the null change of SSWs. Thus, we have added a short comment about that in section 3.1 (L182-184 in the marked-up manuscript) and another bullet to our main conclusions in Section 4 ("The absence of a future change in SSWs is a robust result across all models examined here, regardless of their biases or different representation of the QBO, coupling to the ocean, solar variability, etc..").

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.

Thanks for the suggestion! We have explored the sensitivity of the results to the latitude chosen for the reversal of the wind in the definition of SSWs (Fig. R2.1). To do so we have computed the mean frequency of SSWs based on the WMO but imposing the reversal of the wind at 55°, 65° and 70°N. First of all, we can see that the main conclusion of our study does not change and most of the models do not show a change. It is true that in a few cases, if we change the latitude of reference, the future change becomes statistically significant at a latitude point. However, only one model (EMAC-L47) shows a systematic significant increase in the SSWs frequency for reference latitudes higher than 60°N. EMAC-L47 is one of the models that displays a latitudinal dipole in the future changes of the climatological zonal winds, as suggested by the reviewer, but other models show that dipole (e.g.: SOCOL3 or IPSL) and there is not a systematic change in the significance of results for higher latitudes.

Although performing this extra analysis was a good suggestion, the results are not relevant for our conclusions and we decided not to include it in the revised version. We prefer to keep the main message of our study clear and avoid confusion in the reader.

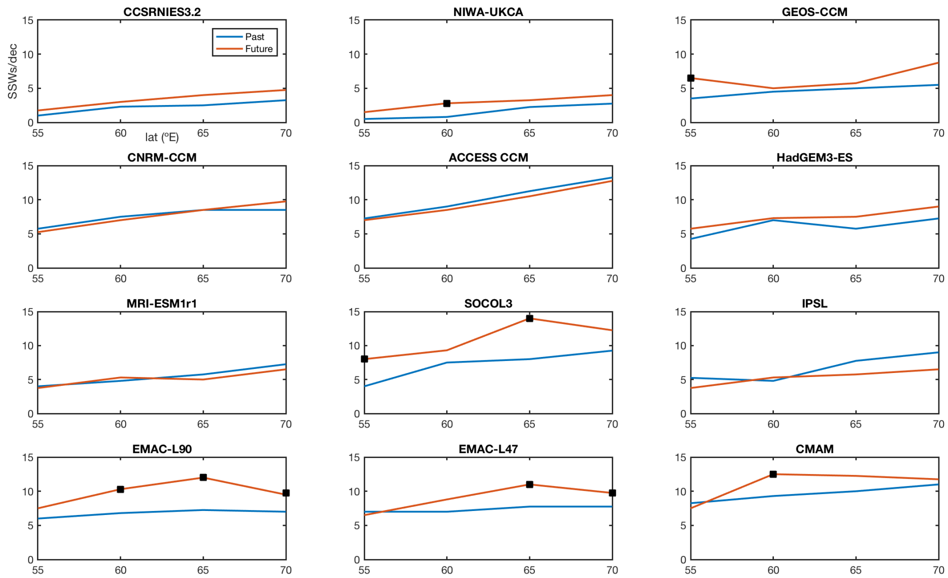

Figure R2.1. Mean frequency of sudden stratospheric warmings per decade for the past (blue line) and the future (orange line) in function of the latitude selected for the reversal of the wind. Black squares indicate that future vales are statistically significantly different from the past ones at the 95% confidence level.

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.

As the referee indicates, the variability of the zonal mean zonal wind at 10hPa had been previously examined and compared in the past and future in each model and we did not find statistically significant differences. Following reviewer's suggestion, we have also plotted the pdf of the zonal mean zonal wind at 10hPa and 60°N for the two periods in typical winter months (December, January, and February) to investigate further possible changes in the distribution of this variable (Fig. R2.2). However, we do not find a future change in the probability distribution of the wind in models. Only a slight shift of the mean value in the future towards lower values is detected in a few models (NIWA-UKCA, CCSRNIES3.2, SOCOL3 and EMAC-L90). This result supports our statement of Section 4 about the lack of statistical significance of the weakening of the vortex in most individual models. The shape of the distribution does not change either, particularly when referring to its asymmetry. The skewness is negative in both periods of study, reflecting that westerly winds tend to be of large amplitude in winter, but the asymmetry is not large though, as the values are not smaller than -0.5 (see values in text boxes of Fig. R2.2).

Based on this additional analysis, we have added a short comment about the general null change of the distribution of the wind data in the past and future period in Section 4. The new statement complements the previous sentence where we had already commented that the future weakening of the vortex was not statistically significant in most of the models (L266 in the marked-up manuscript). We have included the figure in the Supplementary material as Fig. S4.

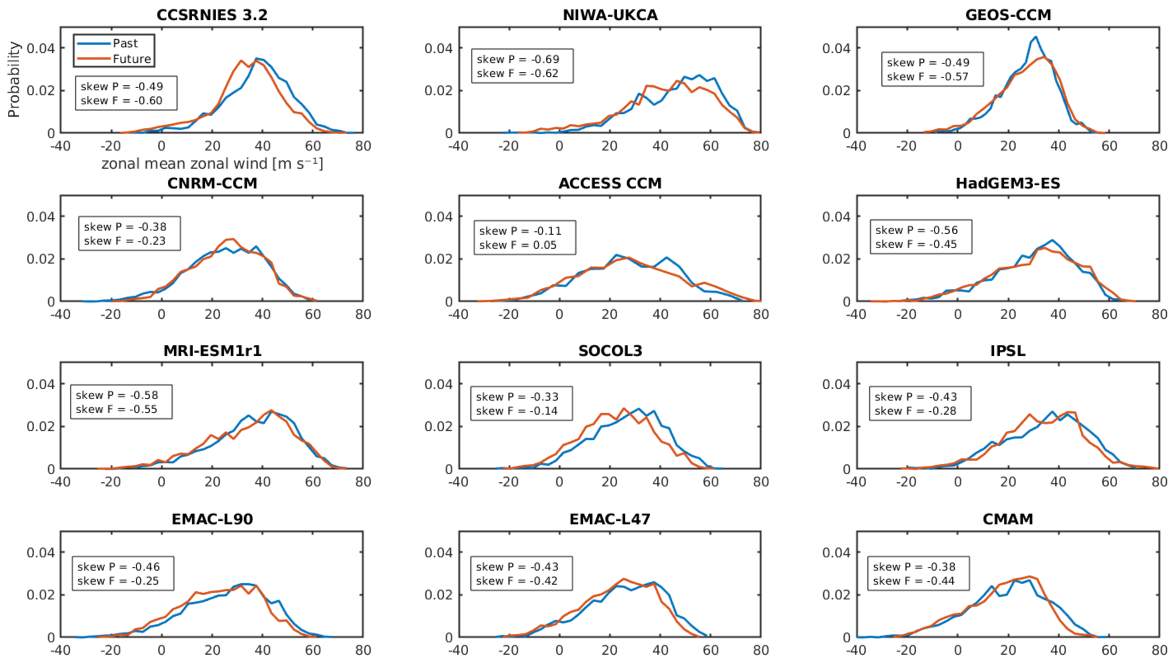

Figure R2.2. Probability distribution function of the daily zonal mean zonal wind at 60ºN and 10hPa in December, January and February in the past (blue line) and future (orange line) for each CCMI model. The skewness of the distribution in each time period is indicated in the text boxes

<u>Specific Comments</u>

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.

We have included Kim et al. 2017, but not Manzini et al. 2014 because the latter does not examine the future changes in sudden stratospheric warmings.

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

Included!

Line 105-107: Would it be possible to consider 40-year blocks from 1960-2100 either 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).

Thanks for the suggestion! Given that SSWs occur randomly and not very often per decade, we found it difficult to get a distribution just picking-up 40-year block by moving the center of 40 year by ~5-10 years. However, inspired by reviewer's suggestion, we have plotted the evolution of these 40-yr blocks of SSWs computed every 10 years (Fig. R2.3). This procedure allows us to visualize whether there is indeed a trend in the occurrence of SSWs or if in contrast, the multi-decadal variability of SSWs is comparable to the change between the past and future period. Only in a few models, and more specifically in EMAC-L90, do SSWs show a clear increasing trend in the future under climate change conditions. The result agrees well with the statistical analysis performed when just comparing the frequency of SSWs in the past and future periods and applying a Student t-test (Figure 1 of the manuscript). Given the agreement in the results, Figure R2.3 adds little to the study; therefore we have not included it in the revised version of the manuscript

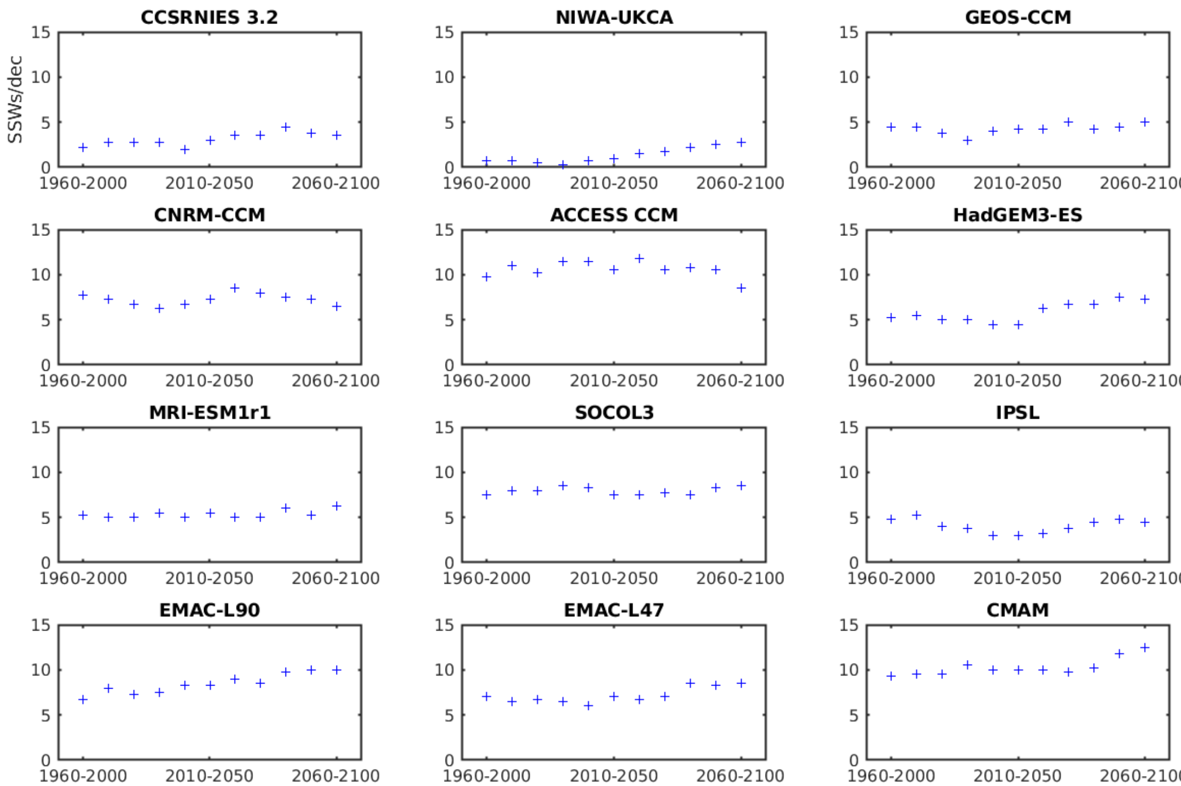

Figure R2.3. 40-yr running-mean frequency of SSWs per decade in each CCMI model.

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?

We have imposed the simultaneous occurrence of zonal mean easterly winds at 60°N and 10hPa and a positive difference of zonal mean temperature at the same level between the pole and 60°N. We have modified the sentence to clarify it.

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).

As part of a good characterization of SSWs, we have computed the associated deceleration of the polar night jet at 10hPa and the results are included in the new Section 3.3 and Figure 2b. Similarly to other SSW features, the MM value does not show a statistically significant change at the 95% confidence level and only two out of 12 models do show a reduction. Thus, the results support the null future change of SSWs.

Regarding the possible impacts of SSWs on surface, it is important to remark that there is still not a clear knowledge about the factors that can modulate the amplitude of this signal in the troposphere. The internal tropospheric variability is much larger than the stratospheric contribution and so, often masks the stratospheric fingerprint in the troposphere (Gerber et al., 2009). For instance, specifically looking at the amplitude of the stratospheric anomalies, Runde et al. (2016) found that strong stratospheric perturbations do not obligatory have associated a strong surface signal.

Technical Corrections

1) Line 56: change to "weather forecasts on intraseasonal timescales"

Done!

2) Line 111: ERA-40 and JRA-55 extend further than 1979, is that what you mean? Maybe instead of "back of", change to "beyond"?

Yes, that is what we mean. We have modified the sentence and clarified the paragraph to make it clearer.

3) Line 208: change to "possibly accounts for at least some of the mismatch between"

Changed!

References

Gerber, E. P., Orbe, C., and Polvani, L. M.: Stratospheric influence on the tropospheric circulation revealed by idealized ensemble forecasts, Geophys. Res. Lett., 36, L24801, doi: 10.1029/2009GL04091, 2009.

Runde, T., Dameris, M.,Garny, H., and Kinnison, D. E.: Classification of stratospheric extreme events according to their downward propagation to the troposphere, Geophys. Res. Lett., 43, , 6665–6672, doi:10.1002/2016GL069569, 2016