Response to Reviewer Comments

"Origins of Multi-decadal Variability in Sudden Stratospheric Warmings" by Oscar Dimdore-Miles et al.

We thank all the reviewers for providing a second set of comments on our analysis. Their Suggestions have helped us clear up technical points as well as discuss more clearly the lower-than-expected covariation between the Aleutian Low and the vortex which is recommended for further study. Below is a summary of the relevant changes made to the manuscript.

Reviewer 1

The authors satisfactorily addressed the comments from my previous review, and the paper is nearly ready for acceptance. There are just a few remaining issues to clarify.

Major comments:

1. Both another reviewer and I were a bit confused by the relatively weak relationship between the AL index the authors chose and the SSW index. The authors, in their response to reviewer 3, note that if the box is pushed further north to match the region of Garfinkel et al 2010, the correlation is even lower. I find this surprising. Supplemental Figure 6 in Schwartz and Garfinkel 2020 shows the lag correlation between 500hPa height and wave1+2 heat flux at 100hPa in the UKMO model included in the S2S database (which I assume is very similar to the configuration used in this paper), and find that a box further north is indeed more closely associated with heat flux in the lower stratosphere than what the authors include.

I think a full investigation is beyond the scope of this paper, but I wonder if the box further north is more correlated with the seasonal mean vortex, while the definition used in this paper is (apparently) more closely associated with SSW. Even if a full discussion is deferred for future work, I think this issue merits some discussion in the paper itself.

This is a good point to raise and we agree that the nature of the connection between the AL and the vortex is not fully understood in this model. We have amended the results section (L437-442) reporting the lower correlation between the AL and SSWs when we utilise the box recommended in Garfinkel et al. 2010 and added comments in the discussion section (L532-537) alluding to the need for further study to diagnose the potential bias in the model teleconnection.

Minor comments,

Line 106: A more appropriate paper to cite than Garfinkel et al 2010 is Garfinkel et al 2020 (Climate Dynamics).

This citation has been changed (L106).

Line 542-549 and 567: Rao et al 2020 document the Holton Tan effect in the historical simulations of this model, and find that the model has a too-weak HT effect if a simple composite based on lower stratospheric winds is created (similar to most models), but does a better job if care is chosen to focus on the QBO phase that is most closely associated with

the HT effect. This could be mentioned in the discussion when you mention the caveat that this is just one model, as this model does a better job than most for the HT effect.
Reference to this work in the context of QBO and HT representation of UKESM and its impact on the findings on this study (L556).

Figure 10 and 11 are missing colorbars. If the colorbar is the same as other figures, please just note this fact.
This has been rectified with a note that the colour scale is the same as all other wavelet spectra figures.

Reviewer 2

General Comments:

Overall, this is a substantially improved manuscript and includes a better discussion on the physical connections between SSW variability and possible internal/external forcings. I thank the authors for their careful attention in replying to all of the Reviewers' concerns. I only have a few (very minor) technical comments that are listed below.

Technical Comments:

1. L128; Remove the word "unexpected"

This has been removed (L128).

2. L255; So not November as stated in L240?

We have clarified that while we do evaluate the warmings over the whole winter (Nov-Mar), we only use this to compare the SSW rate over the whole season with ERA-Interim. For the wavelet analysis and results studying coverability with other parts of the climate system, we use Dec-Mar warmings (L244-246).

L347; "…shown on the right of figure 6…"

This has been added (L349).

3. L381-384; In the real world there could also be an influence of volcanic aerosols (natural variability) in the stratosphere. But I guess this is not the case in the model, as stated in L145?

This is correct, the volcanic aerosol forcing in the simulation is a constant, prescribed, background contribution so could not account for variability found.

4. L550; Restate the model name here.

This has been added (L563).

5. L567-580; I am not sure all of this context is necessary in the conclusions, as I found it a bit distracting and taking away from the main contributions of your work.

We have removed much of this context to make our contribution clearer in the closing sentences (L579-587).

Reviewer 3

The authors have addressed most of my comments and suggestions. I appreciate the efforts to improve the description of the wavelet technique and to extend the discussion of the potential role of the PDO in defining the multi-decadal variability of SSWs. I have just a couple of small comments, mostly regarding new text included during the review process.

L255: the authors indicate that the month range considered in the Aleutian low index is the same as that used for their SSWs definition. However, the months used for the SSWs definition is stated some sentences later.
This is a good point and the order of introducing the month range over which we evaluate the AL and the SSWs has now been rectified (L244-246).

L282: I would not recommend describing this model bias as cold pole bias. The cold pole bias traditionally refers to the bias of many models towards an anomalously strong climatological vortex (particularly in the Southern Hemisphere). Usually this is due to a lack of wave forcing in models. In the present study, it does not seem that the model simulates a too strong vortex, since the averaged frequency of SSWs is not that different from that in observations. Instead, the vortex is probably too weak in November and so there is an unrealistic frequency of SSWs in that month. As the vortex is recovering in the following months, the occurrence of an SSW becomes less likely. Nevertheless, it is true that it is a common problem across models as shown by Charlton et al. (2007) or more recently by Ayarzagüena et al. (2020).
We have removed reference to a cold polar bias (L275).