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

Anonymous Referee #2

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General Comments:

In this new study, the authors using a CMIP6 pre-industrial control run from the UKESM global climate model (GCM) to its evaluate internal variability in sudden stratospheric warming (SSW) events. While there is some limited evidence of SSW multi-decadal variability in observations, the atmospheric reanalysis record is too short to completely address this issue (e.g., internally versus externally-driven). Here, the authors use several transformation methods of wavelet time series analysis to investigate SSW variability in the control simulation and the physical sources that may be associated with it. In particular, the wavelet analysis reveals an important connection between the (deep) westerly phase of the Quasi-Biennial Oscillation (QBO) and multi-decadal periods of little to no SSW activity. In agreement with earlier studies, the vertical structure of the

QBO is important for addressing the Holton-Tan effect, and thus, also SSW variability.

Overall, this is a very interesting study that addresses an open issue of multi-decadal variability in stratosphere-troposphere interactions, which will be of significant interest to the weather and climate communities. In particular, there have been few attempts to assess SSW variability using a GCM with a well-resolved stratosphere and a realistic internal QBO. However, I think a few more caveats should be more explicitly mentioned given that models still struggle to simulate dynamical coupling between the stratosphere and troposphere. Further, this study is only using one model (UKESM). While I am not overly familiar with some of the wavelet transformations employed in this study, more caution (or additional analysis) is needed for interpreting the potential sources of SSW multi-decadal variability (e.g., tropical SSTs – ENSO) that are investigated here.

Recommendation: The paper will be acceptable for publication in Weather and Climate Dynamics after some major revisions.

Specific Comments:

1. L10-11; Do you think they account for some SSW variability or could it just be coincidental internal noise?

2. L20-21; Reference Domeisen et al. (2020) for importance of SSW to S2S forecasts

3. L25-26; Cohen et al. (2009) investigated changes in wave activity/surface forcing on stratospheric variability

4. L29-33; Seviour (2017) attributed the recent weakening of the polar vortex to internal variability

5. L91-95; Reword this sentence to improve clarity

6. L96-101; A very brief discussion would be helpful here to mention other surface forcings that may modulate the strength (and perhaps decadal variability) of the strato-

spheric polar vortex in observation records (Garfinkel et al. 2010). Moreover, recent studies have found that boundary conditions, such as sea ice and snow cover, may modulate the Holton-Tan relationship or even QBO cycle (Hirota et al. 2018, Labe et al. 2019). For example, SSW variability may occur through enhanced vertical wave activity due to Arctic sea ice loss (e.g., Kim et al. 2014; Nakamura et al. 2016) and/or Eurasian snow cover anomalies (e.g., Cohen et al. 2007; Henderson et al. 2018).

7. L110; Why is this unexpected? The vertical structure of the QBO has been identified in numerous studies for its importance to variability of stratosphere-troposphere coupling. Perhaps reword to improve reader clarity.

8. L130-131; Change to something like: "To compare the climate model with the recent observational record, we use ERA-Interim reanalysis (Dee et al. 2011)."

9. L204; How sensitive are the results to your choice of SSW definition?

10. L213-214; Restate the definition of the ENSO3.4 index here.

11. L214-216; Why is this metric chosen as a proxy for the Aleutian Low, instead of something simpler like the central pressure as in Overland et al. (1999)? Reference?

12. L228-229; In my view, there looks to be a statistically significant difference in the number of SSW events distributed per month in Figure 1.

13. L230-240; Although a comparison between model and reanalysis is great, it should still be noted that the samples are not completely comparable if SSWs are influenced by external forcing (climate change) in the real world.

14. L239-246; Again, additional caveats about the use of one model for this analysis are needed... i.e., difference in QBO period (common in high-top models), which could affect the overall conclusions.

15. L321-322; This 90-year periodicity looks somewhat large though in Figure 8? Is there something physically-related to this or is it internal noise? Have you done any

lead-lag or regression composites to further investigate any ENSO-SSW relationship at this time-scale?

16. L360; It could also be just noise in the short reanalysis record.

17. L366-367; This is difficult to see in Figure 10. Could the left six panels be modified slightly?

18. L375-378; This conclusion seems particularly sensitive to the QBO definition. Any thoughts?

19. L409-412; Where is this shown?

20. L416-418; Reword sentence to improve clarity.

Technical Comments:

1. L6; "... coupled Atmosphere-Ocean-Land-Sea ice model." to "coupled global climate model."

2. L46; "...and the [stratospheric polar] vortex."

3. L49; "link" to "effect"

4. L74 and throughout; Unless you are talking about the vertical structure of the Aleutian Low, change "depth" to something like "strength" or "intensity"

5. L91; Lowercase "tropical"

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