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

Anonymous Referee #1

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Review of " Origins of Multi-decadal Variability in Sudden Stratospheric Warmings" By Oscar Dimdore-Miles, Lesley Gray, and Scott Osprey

The authors analyze the possible causes that lead to extended periods with and without Stratospheric sudden warmings. They find that such events vary on multi-decadal timescales of period between 60 and 90 years, and that signals on these timescales are present for approximately 450 years of a 1000-year long simulation. While tropical sea surface temperatures and Aleutian Low variability seem relatively unimportant, the amplitude of the stratospheric quasi biennial oscillation (QBO) westerlies in the mid-stratosphere between 15hPa and 30hPa seems more important.

This paper has some interesting results, and it is very likely that the paper will be suitable for publication after revision. However I must admit that I am not an expert

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in the methods the authors use, and I had some questions concerning the manner in which the authors draw conclusions from their results.

Major comments: 1. This reviewer has limited experience with the chief methodology used in this paper, and I suspect that most of the intended audience has a similar lack of familiarity. While advanced methods can help uncover relationships that would otherwise be missed, the interpretability of the resulting effect is often missing. While I appreciate the effort the authors put it to interpreting the results of the wavelet analysis, there are some (perhaps very basic) questions I had.

A. At present the arguments concerning the relative importance of ENSO and the QBO are hand-wavy. The authors claim near line 325 that the ENSO signal is weak in Figure 8. However by eye, the ENSO signal in Figure 8 near the 90 year periodicity is actually little different from the QBO signal in Figure 12. There is no colorbar on either figure, but the shade of red shown is similar, and is located in a similar location. Maybe if I squint I can see the authors point, but there must be a better way to quantify the relatively importance. Is there a way to compute some sort of different plot between figure 12 and figure 8 to more robustly make the point? The simplest way to evaluate this point using more classical techniques is with simple regression. You compute the regression coefficient (including uncertainty) between the QBO signal and SSW_5yr, and between ENSO and SSW_yr, and simply compare the regression coefficients.

B. What exactly is the ENSO signal at a periodicity of 90 years? In addition to the modes of variability considered by the authors, there is an additional oceanic mode: the PDO or IPO (Pacific decadal oscillation or Interdecadal Oscillation). The periodicity in observations is a bit shorter than 90 years, but perhaps in the authors' model the periodicity is longer. The PDO and IPO project strongly onto ENSO, and indeed one leading forcing of the PDO is simply a low-passed version of ENSO (Newman et al 2016). The PDO has been linked to vortex variability (Rao et al 2019, but see the papers cited therein). Please clarify whether the link seen in Figure 8 is just the PDO.

C. I found the QBO index the authors choose a bit strange. The strongest HT relationship on interannual timescales is with the QBO near 40hPa or 50hPa and the vortex. If one focuses on winds near 20hPa, the HT relationship more or less goes away entirely on interannual timescales. The authors claim that on longer timescales, the most important QBO phase is winds between 15hPa and 30hPa, not winds lower in the stratosphere. While it is possibly conceivable that the phase on short and long timescales isn't identical, this much of a mismatch in phases is disturbing, at least to this reviewer. Do the authors have any ideas on possible causes for such a shift in phase? This mismatch casts some doubt on the robustness of the results, as at present the search for the "best" phase seems too much like a data-fishing exercise.

Minor comments: Line 21: there are earlier studies showing an impact of SSW on cold snaps. See Thompson et al 2002 and Lehtonen and Karpechko 2016.

Line 32: Using a subset of the simulations from Garfinkel et al 2017, Garfinkel et al 2015 actually found a slight strengthening of the vortex in late winter due to SSTs over the 30-year period from 1980 to 2009.

Line 52: Garfinkel et al 2018 also showed results regarding the HT effect in GloSea5, which is based on HadGEM3 GC2.0 if I understand the S2S website correctly. The model performed well as compared to reanalysis.

Line 75: The importance of the Aleutian low for stratospheric wave driving was shown earlier than 2015. See Manzini et al 2006, Taguchi and Hartmann 2006, and Garfinkel et al 2010 The former two studies didn't focus on the Aleutian Low per se, but show this effect.

Line 93-94: A competition between Pacific and Indian Ocean SSTs for El Nino was shown earlier by Fletcher and Kushner

Line 214-215 Is the Aleutian index defined for autumn only? Line 339 states you used DJFM. Please clarify

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Line 229: Actually the seasonal evolution of SSWs doesn't match observations all that well (non-overlapping error bars), with too many SSWs later in winter as compared to DJF. Such a model bias is fairly common however, and Horan and Reichler 2017 argue that the observed seasonal distribution may reflect sampling uncertainty.

Caption of Figure A4 and the figure itself don't seem to match. Please correct. For now, the claim near line 352-353 stands unsupported.

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