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Comment on wcd-2020-63

Anonymous Referee #2

Referee comment on "The Wave Geometry of Final Stratospheric Warming Events" by Amy H. Butler and Daniela I. V. Domeisen, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2020-63-RC3, 2021

Review for 'The wave geometry of final stratospheric warming events'
by Butler and Domeisen (2021)

This paper investigates whether final stratospheric warming events (FSWs) have a wave geometry associated with them, as found in midwinter sudden stratospheric warmings. They find that FSWs can be classified as wave-1 or wave-2 events fairly robustly using 3 different methodologies. They also find that such differences may be useful for improving tropospheric predictability, given that each type can be associated with different near-surface signatures.

I found the paper to be interesting as it examines a topic that has mostly been overlooked in the literature and it is well-written. Although I would like to see more of a mechanistic explanation of some of the differences, particularly in the tropospheric response, which in the SH is quite startling (figure 6d,f), I feel that the paper is mostly good to go and worthy of publication. Hence, my suggestion is of minor corrections.

Comments:

Line 31; Maybe add about the latest SH warming in 2019?

Lines 109-110; It seems strange to stitch together the two reanalysis datasets, especially when JRA-55 could be used for both before and after 1979. Can you better explain why this is done? Why not just use JRA-55 for both periods (from 1979-2009, you say that dates vary only by 1-2 days, so surely it should not matter)?

Lines 100-110; I think something should be said about how your definition compares to other definitions. I understand that FSWs are usually defined using the 50hPa winds, which you mention is not possible in the SH as the winds do not clearly reverse from westerly to easterly at this level. But in the NH for instance, I wonder how the FSW classification at 10hPa and 50hPa may impact your results. I am not entirely sure, but I guess that the vortex breaks up

at one level earlier than the other? This may affect your composites as you may be either capturing (or not) the actual wave pattern in the build up to the FSW. So in conclusion, I think that more should be said about the sensitivity to FSW definition; what if you define an FSW in NH using 50hPa winds, as I think is more traditional.

Line 128; Is it the total number of days in [-10:10] spent with an aspect ratio or centroid latitude satisfying the threshold, or the longest continuous chain of days satisfied, that determines whether it is a wave-2 or wave-1? For instance, an event may overcome the aspect ratio threshold on day -10 but not again until days +7:+10, whereas the same event may overcome the centroid latitude threshold on days -1:2. I would say that the latter is the better classification (i.e., wave-1) as it occurs continuously around the actual FSW date.

Further, what if the number of days are equal for the aspect ratio and centroid latitude fulfilment?

Lines 145-151; Just to clarify, when you say 50-hPa classification here, you only mean for determining wave-1 and wave-2, right? (This is valid for the whole manuscript as
it can sometimes be a bit confusing). If not, then my point just above will be redundant!

Figure 1; I won't go into details here as I realise that both Darryn and Nick have mentioned this substantially in their reviews/comments, but I was aware that there is a well-known trend in the SH final warming dates, linked to ozone depletion. This trend is lacking in fig. 1c and I am wondering why. As Nick states, it may be related to the FSW definition, so discussing the sensitivity of your dates to the level of FSW definition,
as aforementioned in a previous comment of mine, would be useful.

Figure 2; What do the composites look like? Do they show typical wave-1 and wave-2 patterns, or does the longitudinal location of the vortices vary too much between each FSW so that there is cancellation? During SSWs, the vortex locations are pretty constant between events, so I wonder here if it is similar during FSWs. I also think it would be good to show this to see if there are indeed statistically-significant differences between the wave-1 and wave-2 FSWs.

Line 170; 'positive' -> do you mean ratios greater than one?

Line 181; Not strictly a deceleration as has units of m/s. It is just the difference between the two time periods right?

Lines 183-190; Are the differences between the wave-1 and wave-2 events statistically significant?

Lines 193-194; I cannot see this from the plot. It appears that the mean lines (thick) cross
zero at 50hPa after ~20days or so (panel c). Is this what you are referring to?

Figure 5; Can you perhaps include a PV contour marking the 'edge' of the polar vortex to see
how it compares with the ozone anomalies?

Lines 231-232; Can this be shown explicitly? Figure 5 does suggest it, but I would rather it be

shown. For instance, you could calculate the years that have stronger ozone depletion compared
to some mean and compare and perhaps calculate a correlation with the years that have a late FSW.
Just a suggestion for a figure, but I think this point could be better shown in the manuscript.

Lines 260-264; It appears to me that the wave-1 and wave-2 events lead to nearly identically
opposite 500-hPa GPH anomalies in the SH. Any idea why this is? Is this also the case at levels
above, perhaps in the stratosphere? Figure 5g,h did not show such an opposite-signed pattern in
the TCO. Further, are there significant differences lower down, perhaps in the MSLP?