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Comment on wcd-2020-63

Darryn Waugh (Referee)

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-RC1, 2021

This manuscript analyzes the wave geometry of Final Stratospheric Warming (FSWs) for both northern and Southern Hemispheres. It presents some interesting results that warrant publication. However, it is not clear to me that the differences between wave geometry are important (point 1 below). Also, the analysis of the Elliptical Diagnostics needs to be greatly improved or removed (major point 2). So, I think major revisions are required before the manuscript is suitable for publication.

MAJOR

1. Key results

The presented analysis does find some differences between wave 1 and 2, but overall I find these differences to be small and even when there is a statistically significant difference it is not clear to me that these differences are important. Even the abstract doesn't present the results as having important implications. I actually came away with conclusion that probably not important /necessary to consider wave 1 vrs wave 2. This is an OK conclusion from a study, but I suspect authors disagree with this and if so they need to more clearly present results that will have implications.

I actually was more interested in some of the other results presented and only briefly discussed. In particular I think that there needs to be more discussion of trends, connection of SH FSW date with ozone, and late versus early. Even if some previously discussed you are presenting a longer time series. Also you could making comparisons with wave geometry.

For example:

(a) The existence and lack of trends in Fig 1 is not discussed. Fig 1a appears to show a trend between 1979 and 2010 but I guess an insignificant trend over 1979-2019 or whole record. At least some comment on this is needed. Also possible trends / pause in SH needs to be mentioned (see more below).

(b) The connection between ozone and FSW needs to be expanded. The analysis here focuses on the impact of the FSW on ozone, but for SH (as stated in manuscript) a key point is timing of FSW is linked to ozone depletion. This connection is not actually shown here. Line 155 says SH final warming date tends to occur later during years of strong ozone depletion but no references are given ("As noted previously" is very vague). Even if noted before should be some analysis here. Also, on Line 232 it is stated that "These differences reflect a key point, which is that late events in the SH tend to occur in years with strong ozone depletion that further strengthen the vortex winds and allow the vortex to persist longer." Where is this shown in this paper? If this is a key point then show it.

Returning to trends, does Fig 1b support a trend before 2000 and then a pause since (consistent with ozone recovery)? Banerjee et al reports such a pause in tropospheric circulation, is there also a pause in the FSW date trends?

(c) I am not convinced that a wave separation provides any more (or even as much) insight as separation by timing. Fig 4 shows only small differences between wave 1 and 2, but on line 190 is states that "Early FSW events are associated with a stronger deceleration of the winds as compared to late FSW events due to the seasonally stronger winds earlier in the season". This needs to be shown and contrasted with the wave differences. Note Figure 5 also shows that the difference between late hrs early is more important than difference in wave geometry. Again, I don't think this means not valuable to show wave geometry differences but it just needs to be put in better context.

2. Elliptical Diagnostics

The fact that Elliptical Diagnostics (EDs) are considered is only mentioned in Section 2, and the fact they are used needs to be started earlier (and in conclusions). However, having said this, it is not clear to me that EDs are actually being used correctly or influencing the analysis. I think that additional analysis is needed or they should be removed.

Looking at table A2 it appears that for over half the NH cases and many of the SH cases the ED classification does not agree with the final classification. In other words for many/most years the classification is due to agreement between the two wave number analyses but the ED are saying something else. This needs to be much more clearly

presented (if EDs are kept).

The assumption of split = wave 2 and displacement = wave 1 is not valid, and you should not call a split a wave 2 or a displacement a wave 1. The fact this is not a good assumption is exactly the reason EDs were introduced. Further the disagreement between ED and wave numbers appears to support this.

If you keep the EDs then I think you should do an analysis based on S vrs D. Does this show any significance difference (is it similar / different to wave 1 vrs wave 2 difference).

MINOR

Line 102-105: Quote the standard deviation in date? Also, add a statement comparing April 12 and November 19 in terms of respective seasons (i.e. days after winter solstice / before summer solstice).

Line 109-110: Why not just use JRA for the complete record? It seems strange to add two reanlyses together and further complicates interpretation of the 1958-present day record.

Table 1 and 2: How about Table 1 = NH and table 2 = SH? I don't think any of the analysis compares the FSW in SH with that in NH for same year, and I think better to list all years for one hemisphere in a single table.

Line "133" "Another disadvantage" What was the first disadvantage?