Weather Clim. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/wcd-2022-13-RC2, 2022

Comment on wcd-2022-13

Anonymous Referee #2

Referee comment on "Stratospheric wave driving events as an alternative to sudden stratospheric warmings" by Thomas Reichler and Martin Jucker, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-13-RC2, 2022

Review of ‘Stratospheric wave driving events as an alternative to sudden stratospheric sudden warmings’ by Thomas Reichler and Martin Jucker

General Comments:

This is an interesting study that aims to introduce another definition of extreme polar vortex events, related to the magnitude of the lower-stratospheric wave flux. The authors compare the tropospheric response to the most-often used wind-reversal criterion, to their own wave driving definition and find that the latter gives an overall stronger surface response. Although the suggested definition does have some advantages as the authors state, I do not think it is as simple to calculate as the wind reversal criterion or indeed other definitions that require a single zonal-mean field. In some places therefore, the language should be toned down so as to not over-sell this new definition.

A bugging concern I have is that the presented diagnostic is not necessarily capturing a wave driving event, rather an increased wave flux in the lower stratosphere. So the nomenclature should be changed from planetary wave driving (PWD) to planetary wave flux (PWF) events. Even though there is a strong EP flux in the lower stratosphere, it may not drive a weaker vortex and the wave activity could just propagate and break farther equatorward. Indeed, it is the derivative of the wave flux that determines how much the mean flow is affected, and so this should be called the wave driving, rather than your definition.

I would like to see how the presented definition compares to a more dynamical extreme vortex event definition such as the wind tendency definition of Birner and Albers (2017; SOLAS). In my eyes, that definition can more appropriately capture wave driving events, as the wind deceleration is proportional to the wave flux convergence (in the transformed Eulerian mean sense). Nevertheless, the current study is already long enough and self-contained and so this is a suggestion for future work.

The paper is well-written and well thought out and so my comments are generally of a minor nature. Hence, my overall recommendation is of publication subject to minor corrections which I list below.

Comments:

Lines 44-45; It is relatively well understood now that the wave-mean flow interactions associated with the critical layer mechanism for downward propagation (that originally propsed by Matsuno) only reaches the tropopause. See for instance, Hitchcock and Haynes (2016; GRL). So I would rephrase or remove this sentence.

Lines 51-55: I think the following paper should be cited here:
"Defining Sudden Stratospheric Warming in Climate Models: Accounting for Biases in Model Climatologies"
by Kim et al. 2017, J. Clim.
They make this point about the fixed threshold not being ideal for climate models as there are mean
state biases present so that a model with a too strong vortex would likely simulate less SSWs. This is
a sort of similar point to that regarding NH vs SH differences.

Lines 66-67; The heat flux itself is not referred to as the wave driving, as what happens if the wave
activity simply propagates upward through a region? Rather, it is the derivative of the heat flux that
weakens the polar vortex as this represents the convergence/divergence of said wave activity.

General comment on introduction: It is currently very long and I would shorten it to be more to the point.
Also, I think other definitions of extreme vortex events should be mentioned somewhere. There are many
but the current intro only focusses on the wind-reversal one. How does yours fit into the context of others?
I would add a paragraph to discuss previous definitions. Tendency based definitions such as that by
Birner and Albers (2017; SOLAS) is one example that may be better suited to overcome the issue you mention
on defining SSWs in a warming world with underlying polar vortex changes (your lines 58-60). Sentences such
as that on lines 62-64 make it seem that your definition is the only study that has attempted to use a more
dynamically-based definition.

Lins 125-126: I think the WMO criterion also involves a reversal of the temperature gradient. The CP07
definition is a simplification of that.

Lines 139-144: Is there a reason an e-folding timescale of 50 days is chosen? Is this something to do with radiative timescales in the lower stratosphere? Or perhaps related to the 40-day vertically integrated wave flux in Polvani and Waugh (2004)? It would be good to know if results are sensitive to varying this parameter to shorter timescales (which would be a more conservative criterion that reduces the accumulated wave flux). Intuitively 50 days sounds quite long, so would be good to have justification here.

Lines 148-150: 1) What does the timescale physically mean? The time taken for an average accumulated wave packet to die out? 2) Why is a negative Fz set as the end of the wave driving event? I would think that if the wave flux anomaly was close to zero but still positive then this is pretty much the end of the event anyway. For instance, imagine a situation where the wave flux anomaly remained positive but close to zero for an extended period; this would be erroneously counted as an extended event and contribute to the summation. Would a more plausible end of event be related to a criterion on the standard deviations?

Lines 162-164: Physically a positive PWD in the 20 days after an SSW is surely not related to the driving of the SSW? Indeed, Fz remains positive for 2-3 days after an SSW event (as your figure 3 shows), but for 1 week plus, I highly doubt it. How many of your common events fall into this category of large Fz in the 20 days after an identified SSW?

Line 189: What is this 58% and 62% SSW frequency? I presume you are referring to the

number per decade
which in ERA-5 would be 6.2 per decade and so translates to 62% of years having an SSW. Is that right?
I am not saying the statistics are wrong, rather the way they are presented is non-standard.

Figure 2: I am not sure if it is to do with the way it is rendered on my screen, but the shading in this
figure completely masks much of the figure. It not only masks the lines, but the writing next to the lines.
Please fix and make the confidence interval more transparent. Because of its current rendering, lines such
as 201-202 are impossible to make out.

Figure 3: I cannot distinguish the bold from the non-bold lines in panels a-f. You state that bolded lines
represent those differences that are statistically significant. Also, the panels b,e are cut off and do not
show some of the lines around the onset date. Especially in panel e, the sharp increase in SLP just after
day zero is interesting. Finally, what is NNR in the caption?

Lines 216-217: Are you reading off the anomalies in c-d by comparing the solid lines with the dashed lines
and seeing which lies lower?

Lines 240-241: From the left column of figure 3, I would not expect to have such comparable SLP anomalies
to those in the model. In fact PWDs in ERA5 appear to have positive SLP anomalies, for up to 20-30 days
before the onset date, presumably associated with a strong Siberian High.

Lines 267-270: 100hPa where you identify the PWDs is already well within the vortex. Hence, the PWDs events
you capture may also be due to 'stratospheric internal dynamics'. de la Camara et al. (2017) suggested that
300hPa was a better diagnostic level to say that there is cross-tropopause wave propagation. Nevertheless,
this brings up another point: how sensitive are your PWDs to choice of vertical level? It would be good to
raise or lower the level and recalculate the numbers to check that 100hPa is representative of the lower
stratosphere.

Line 275: The word 'tend' here suggests that the majority of NWDs occur close to the onset date of the SSW
(say within +-30 days). I do not see that in figure 5. Rather, there are around as many NWDs that are not
close to an onset date as there are close to an onset date. Can you clarify what you mean here, perhaps
quantitatively. Otherwise I would just consider removing the sentence.

Lines 331-333: This is interesting but unsatisfyingly not further addressed! Do you have any idea as to
why this is? By March-April the vortex is already starting to break down and wave activity to wane
(figure 1a,b) and so is the weaker day 0-59 SLP response simply reflecting the seasonal cycle? The
vortex recovery is too weak by this point as it is the transition time to easterlies and so the
radiative recovery is cut off by the seasonal cycle. Many studies have shown that the persistent
lower-stratospheric anomalies are important for a continued tropospheric response (Hitchcock and
Simpson 2014, JAS; Maycock and Hitchcock 2015, GRL; White et al. 2020, JClim) with it being mechanistically
attributed to the induced meridional circulation by the lower stratospheric radiative recovery (Thompson
et al. 2006, JAS; White et al. 2022, JAS), and this may provide further evidence for that. A NAM index

plot for SSWs occurring only in March-April would help to see if the extended recovery in the lower
stratosphere is indeed cut off by the seasonal cycle with a shorter NAM timescale evidence for that.

Figure 7: Same problem as figure 2 with the shading.

Lines 350-354: It does not look like the SSWs are associated with a SLP<0 response over the North Pacific
compared to the PWDs (comparing panels in column 2) although you state this to be the case. Isn't the strong
SLP<0 anomaly over the North Pacific in the PWDs compared to the SSWs just related to the fact that the PWDs
are wave events themselves? As you say, it represents a deeper Aleutian Low but the difference between the
PWDs and the SSWs is that the planetary wave driving is shut off in the SSWs whereas it continues in the
PWDs until the Fz anomaly goes negative (which could take a while depending on your specified e-folding
timescale). Hence, in the PWDs, I would likely expect a more negative Aleutian Low to persist well after the onset date.

To clarify, my concern is that the presented SLP patterns for the PWDs are simply aliasing the planetary
wave patterns that drove the weaker vortex in the first place and therefore not part of some downward response.
Perhaps a simple way around this is not to use such a broad time-average window that goes all the way to the
onset date (i.e., not use lags 0-59). Or, base the averaging window on the date the minimum stratospheric
winds were found following the maximum Fz.

Further, I thought you had removed the ENSO effects from the timeseries (lines 174-179). Please clarify as
this affects the discussion here. It also affects for instance, line 359.

Line 388: I don't think this is such a simple metric to calculate, particularly compared to the wind reversal one.
The traditional definition can capture most of these events. I would not state that your definition trumps it
so flippantly.

Line 391: Compared to the traditional measure, it appears that the PWD events hint at around an extra week of extreme
Fz (figure 3b) but I would hesitate to state that this so definitely.

Technical Comments:

Title: You use 'sudden' twice!

Line 18: 'criterium' --> 'criterion'

Line 19: Duplicated 'that'

Line 97: 'data' --> 'data'

Line 106: Change the short sentence to 'The results in Section 3...'

Line 134: To clarify, did you use area weighting to average the EP flux?

Figure 1: I think I get it, but can you clarify what the lines mean? I presume thin lines represent
the +-SDs and the thick represents the full value, but it would be nice to not have to work it out...

Line 146: remove the first 'the' on this line.