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# Comment on wcd-2022-13

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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-RC1, 2022

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## General Comments

Overall, I enjoyed reading this manuscript and the results were interesting and novel. I mostly have minor comments, with a few more broad suggestions for improvement.

First, while negative wave driving events (NWDs) are included in the study, they seem almost an after-thought. It would be nice to include something on NWDs in Figures 3 and 4 for example (or in a simple manner to Figures 3 and 4 but in an appendix). More discussion or even a separate section could be given to discussing these events. Or, another option would be to consider not including NWDs here but save these for another study (perhaps comparing to “vortex intensification” events instead of SSWs).

Second, I had quite a few comments on the Discussion/conclusions section below, that I hope the authors will consider also more broadly for the entire manuscript. While I find the results presented in the study very compelling, I also think that the primary goal of the study should not be to dismiss the major SSW definition. In particular, some of the text seems to imply that PWDs and SSWs are different phenomenon, while they really are just two different ways of measuring the same basic dynamical process. While I agree with the authors that the wave driving events do have advantages over SSWs particularly if the interest is in surface impacts, rather than trying to argue that SSWs are not important or the SSW definition not valid, I think the results of the study could be better used to argue for improved dynamical metrics output from forecasting/climate models in order to encourage use of this somewhat more complicated metric (and, I do think it worth acknowledging that while the wave activity metric is relatively straightforward to calculate, it does require much more data than u1060).

## Specific Comments

Title: “sudden” appears twice. More generally, I suggest in the title and in the text (e.g., line 11, line 368) that the acronym for SSW is defined as “sudden stratospheric warming” rather than “stratospheric sudden warming”; reasons for which are described in Butler et al. 2015, footnote 1.

Line 12, line 35: “normal westerly”- to be clearer, specify “climatological wintertime westerly”

Line 13-14: Since the event date itself is presumably determined by the wave activity flux, remove “prior to the onset of the events”.

Line 15-18: phrasing is confusing, plus this point is repeated in the next line. Suggest instead just stating “About half of the wave events are identical to SSWs. However, there exist several advantages for defining stratospheric weak extremes based on wave events rather than using the common SSW definition:…”

Line 31: What is meant by “inner core”? In the NH the warming often seems to start in one particular extratropical sector and move inwards towards the polar cap.

Line 41: Could you be more specific in terms of latitude/level of the CP07 definition?

Line 48: Here, I think you mean “it is unclear how effective this definition of SSWs is in capturing events with downward influence…”

Line 51-52: If the zonal wind reverses, how can it be considered a relatively minor perturbation? According to your prior paragraphs, the reversal is why major SSWs are considered so intense. Since this sentence also repeats the sentiment of the prior one about downward impacts, I would just remove it. The point about it being a based on a fixed threshold is perhaps really only important in the context of climatological model biases; or regarding your next statement on lines 53-54, which is a valid one.

Line 55-57: Is this your wave driving definition applied here to the SH? If not, it seems somewhat out of place to bring this up here (I like the point about the SH in the conclusions, but I would remove it here and focus only on the NH).

Line 17, line 58, line 125, 405: Not sure about the word “traditional”. Technically the traditional WMO one is not exactly the same as the CP07 definition; see Butler et al. 2015, which describes the long history of SSW definitions. Perhaps you should instead use the word “common” to describe this definition and state somewhere that by “common SSW definition” you mean the Charlton and Polvani definition.

Line 60: This paper should be cited here or elsewhere in the manuscript; it is very relevant: https://journals.ametsoc.org/view/journals/clim/30/14/jcli-d-16-0465.1.xml

Line 60-62: I agree, the SSW definition is not based on what precedes the event, but on the other hand is also not based on “their effect”, at least at the surface, which is how this may be interpreted. Maybe “but instead on their stratospheric effect”. I would also argue that a wind reversal is very much a “dynamically-oriented” metric, so I would rephrase (see also, Butler and Gerber 2018, which showed that the particular latitude and height of the CP07 definition does maximize dynamic metrics, at least for definitions based on wind reversals).

Line 61: This is the first time the predictability of SSWs is brought up. Could you make a brief statement about what lead times are expected for SSW prediction? (e.g., Domeisen et al. 2020 part I).

Line 74-96: There are other studies that suggest caution about how to interpret the 100 hPa eddy heat flux, which should perhaps be mentioned in this section somewhere, see, e.g.:

https://journals.ametsoc.org/view/journals/atsc/74/9/jas-d-17-0136.1.xml

Line 86: Do you test different levels? What about the wave activity at 50 or 10 hPa, for example? 100 hPa could still be muddied by tropospheric variability potentially.

Line 121: what is the model lid height?

Line 125: The CP07 definition is not exactly the WMO-criterion. See Butler et al. 2015.

Line 129, line 151: this should read “separated by at least 20 consecutive days of westerlies”. See CP07 corrigendum: https://journals.ametsoc.org/view/journals/clim/24/22/jcli-d-11-00348.1.xml. In your methodology described on line 151, are your events just separated by 20 days (regardless of wind or PWD sign?). Please clarify.

Line 134: This seems like a broad latitude range to calculate Fz. Did you also try 45-75N or 40-80N as many other studies use?

Lines 134-136: I don’t follow why multiplying by -1 ensures that a positive sign means upward propagation. Which term in Fp leads to upward propagation being negative?

Line 141: “of either sign”- this is confusing because the paragraph starts off describing “positive wave driving events” yet here you are looking for anomalies of Fz of either sign.

Line 150: What does it mean for this quantity to become negative? It’s not clear in a physical sense how that signals a *positive* WD event. Perhaps an example would be useful? (it does become a bit more apparent in Figure 3).

Line 157: du_min is not mentioned on line 129-130 as being saved for SSWs; might do so for consistency. Also the statistics listed here don’t match what is listed in Table 1.

Line 199: This isn’t apparent from the figure (the EX-PWD line falls beneath the SSW value at the 12.9 threshold).

Figure 2: This is a really nice figure. What are the units of event frequency (is this number of events per year multiplied by 100?)

Line 202: It would be nice to at least briefly mention that especially the SLP response in ERA5 is much noisier due to the smaller sample size, which means that the SLP difference between PWD and SSWs and the selected threshold is not significant in ERA5. I think the overall good agreement with the model does support the idea that using ERA5 here alone would be difficult and provides support for needing many samples from model experiments.

Line 220: I don’t know if it will be clear to the general reader what is meant by the final warming here (I don’t think it’s been defined yet) and which zero-wind crossing you are referring to.

Line 229: I can see the “eastern” vs “western” dipole in the lag -15 panel but not so much in the lag -25 panel.

Line 248-253: Here, you have confused the sign of the NAM. The positive values here represent negative values of the NAM (at least, I hope this is the case!). The text should be fixed here; and since the NAM here is approximated by the polar cap geopotential height anomalies, either these should be labeled as such on the figure (instead of as the NAM), or they should be multiplied by -1 to match NAM polarity conventions.

Figure 5: Instead of event day of year, which doesn't seem that useful since it can be somewhat inferred from the location of the triangle, it would be more useful to put the accumulated wave driving value for each event as the small number.

Line 271: "when the polar vortex is weak" – would instead phrase as "when the polar vortex is climatologically weaker"

Line 283-286: I understand your point here, but on the other hand, each SSW also evolves differently, in terms of the morphology of the vortex, so in a similar fashion there is also much uncertainty about how that contributes to the surface response and timing of the response in terms of individual cold air outbreaks. This is of course underscored by your Figure 6 and the following discussion.

Figure 7: It's hard to tell whether the SSW curve is flat at the top or extends above the y-axis. It may be preferable to extend the y-axis so the peak can be seen. Also for panel (d); it's hard to see what is going on because the EX-SSW blocks the U+.

Line 311: Is the narrower seasonality of SSWs in part by construction? Since they cannot be defined past the end of March in the CP07 definition. For a better comparison, you could consider a similar definition that uses the full calendar year- see Butler and Gerber 2018 (this also has the advantage of removing weak March SSWs; which may influence your results on the surface responses to SSWs being slightly weaker).

Line 319: here, U- events are likely dynamic final warmings, correct? I would distinguish these events from radiative final warmings (this is done to later, on lines 361-365, but perhaps that discussion should be moved up to here).

Line 320-321: I think here you are referring to the fact that the CP07 definition doesn't permit April SSWs, but perhaps that should be made clearer. I would find it hard to believe that the wind would reverse in April and still show a "complete vortex recovery"- since the climatological mean date for the final warming is ~April 12 and after that the winds are climatologically easterly, so what would a complete vortex recovery look like at that time of year? I would rephrase or remove this sentence.

Line 350: There appears to be no significant Pacific response to SSWs so I would remove that from the sentence. Could more be said about the NWD response? Intriguingly, the Pacific sector stands out much more strongly for NWDs (and for PWDs), compared to SSWs.

Line 350-354: Following the previous comment, could you show these patterns for ENSO-neutral years only as in Figure 4? It seems like for over >4000 samples as in most of the model composites, and *if* the model does not show a strong preference for SSWs/PWDs/NWDs in only one phase of ENSO, then ENSO should basically average out, meaning that Pacific signals shouldn't really be associated with ENSO as suggested here. Is this the case? (i.e., what is the Nino 3.4 composite value for each of these maps?). Could the prominence of the Pacific signal instead be related to the wave driving itself in some way; a fingerprint of the wave driving response? I realize that the intention to investigate this further is mentioned for another paper, but given the strong differences in Pacific response here, I think it would be at least useful to mention the composite Nino 3.4 value in each case.

Lines 350-353: This statement is true but explains the ENSO related precursors to SSWs, not the response to SSWs, which is typically very North Atlantic-centric. The SSW composite in ERA5 in fact shows the opposite anomaly over the N. Pacific (top left Fig 8); this is likely related to ENSO "noise" affecting the composite in the small reanalysis dataset.

Lines 368-69: This sentence overstates the conclusions/implications of this study. This sentence gives the impression that it's the upward wave driving itself that is the most important source for stratospheric signals at the surface, but of course the wave driving has to first cause a change in the polar stratospheric circulation for it to have any influence. True, I think this study does show that that change does not have to be a full reversal of the winds as for the "major SSW" definition, but I think this sentence does a disservice to the role of SSWs in general, which has been well established in the literature. (The rest of the paragraph clarifies this point, but I would consider rewriting this sentence to better reflect the results of the study; lines 383-384 I think is a much better starting conclusion sentence for what the study shows).

Line 372: PWDs had the same frequency as SSWs but by construction of your chosen threshold, which should be clarified here.

Line 374-375: "just like SSWs, PWDs were preceded by increased amounts of wave activity flux" – this is also by definition

Line 377: Here it should perhaps be clarified "half of all PWDs did not concur with major SSWs", since it's very likely that the other half were minor SSWs or final warmings.

Line 378: the more even distribution at least partially comes from PWDs including final warmings, whereas the SSW definition does not include final warmings by construction. While I do think it's an advantage that the PWD definition can detect dynamic final warmings that likely have similar impacts as SSWs, this distinction should be made clearer, as the SSW definition was purposefully designed to exclude final events, so it can't really be considered as a fault of the definition.

Line 378-379: I don't think this was shown anywhere. This assumes that the Pacific response in Figure 8 is from ENSO, but I think more will have to be provided (such as the composite value of Nino 3.4 for each plot in Fig 8) to make this statement.

Line 388: I disagree with this point. If there's any reason to use the common SSW definition, it's for its simplicity, particularly in model data because you just need daily u1060. It's much more intensive to calculate Fz (for which you need gridded daily v and T). I think a stronger stance this study could take is that you've demonstrated that PWDs are valuable measures of stratosphere-troposphere coupling, so there should be more demand for models to output daily v'T' (pre-calculated preferably on model time steps), and to participate in efforts to output these dynamical variables like DynVarMIP (few CMIP6 models did so).

Additionally, while PWDs may include all these types of events, they don't distinguish between them, which is not necessarily an advantage. Instead, perhaps this should be stated as, while other definitions focus on separately defining all these variations of polar vortex variability and looking at differences between them, this goal of this definition is to identify events with greater surface impact, no matter the particular timing or evolution of the polar stratospheric circulation.

Line 391: I don't think this can be stated so strongly here unless you test this explicitly. I agree that Figure 3b hints some lengthening of wave activity flux prior to the event, but it's not a guarantee that this will translate into better predictability of these events. I would soften this statement, e.g., "potentially lengthen the forecast horizon"…

**Technical Corrections**

Line 19, line 56, and throughout: capitalize Southern/Northern Hemisphere

Line 50: I don't think NAM has yet been defined anywhere

Line 55-56: change to "stronger climatological polar vortex"

Line 97: change “date” to “dataset”

Line 106: change to “The results in Section 3…”

Line 145: “past the wave driving” – remove “the”

Line 157: “As for SSWs”- suggest instead “Similarly to SSWs”

Line 233, line 360: don’t need to capitalize “polar cap”