Response to comment by Anonymous Referee #2

Review for "Opposite spectral properties of Rossby waves during weak and strong stratospheric polar vortex events" by Schutte et al. WCD

This manuscript by Schutte et al. (2023) uses observations and a novel technique (which has not been applied before to SSWs) to examine the spectral properties of Rossby waves throughout the lifecycles of SSW events and strong polar vortex events. The technique picks up on large spectral changes throughout the lifecycle with SSWs being characterized by a reduction in the eastward phase speeds in the stratosphere before the event, along with an increase in westward phase speeds, before being associated with an overall reduction in all phase speeds sufficiently far after the onset. This is consistent with enhanced upward propagating waves before the onset (although their characterization of this as being due to stationary waves requires further explanation; see my point below), and planetary wave suppression after the onset throughout the stratosphere and troposphere. SPV events seemingly show opposite-signed results. Although the stratospheric response is very clear, the tropospheric response to such events is less clear using this method which I suspect is due to the small number of observed events and thus strong inter-event variability, particularly given the more chaotic nature of the tropospheric flow.

The paper is well-written and I found it novel and interesting to read. I have only minor comments and so that is my suggestion. I do hope the authors will use a longer dataset or model simulations to better test the method, though!

Thank you so much for your feedback and the time spent reading the paper. We agree that using a longer data set could provide a great opportunity to test the method further and obtain more robust results through a higher number of SSW and SPV events. As mentioned in our conclusions, one could utilize extended-range ensemble forecasts or seasonal hindcasts in a comparable manner as Spaeth and Birner (2022); Kolstad et al. (2022); Bett et al. (2023). However, this would likely provide enough material for an additional paper, as one should also compare the results from the ensemble runs with the reanalysis data. Thus, we have decided to limit our analysis to the data covered by the ERA5 reanalysis data set between 1979 and 2021.

Minor Comments:

Lines 46-49: This sentence is a bit too flippant in its summary of the role of planetary and synoptic waves in the surface impact.

A large consensus in the community is that something initially brings the stratospheric influence to the surface and maintains an exogenous forcing to the troposphere. The two main such processes are 1) related to the changes in the meridional circulation, initially by the wave-induced meridional circulation during the onset stage (the 'downward control' idea), but later by the radiatively-driven circulation due to the persistent lower-stratospheric anomalies (e.g., Thompson et al. 2006 whom you already cite, and White et al. 2022, JAS). On the other hand, planetary wave suppression throughout the entire column can also provide the continued exogenous forcing (e.g., Hitchcock and Simpson 2016, JAS; Hitchcock and Haynes 2016, GRL). However, to amplify the tropospheric response due to the above mechanisms, it is generally well-accepted that the synoptic wave feedback in the troposphere is necessary to yield the wind dipole associated with the negative NAO (the meridional circulation nor the planetary wave effect can yield the full dipole). Both Song and Robinson (2004) and the aforementioned White et al. (2022) show this downward response being caused by the meridional circulation, but with an additional effect by the planetary waves (though much stronger in

Song and Robinson and in Hitchcock and Simpson 2016), followed by a synoptic-wave-induced tropospheric amplification.

Although this is likely too detailed for your paper introduction, comparing the planetary and synoptic wave effects and determining which is the 'main modulator' of the surface response is not well-founded nor the correct to phrase it, as they both play important, but crucially, very different roles in maintaining the tropospheric response.

Thanks for pointing out this weakness in the text. We have now improved this sentence to better reflect the existing literature. It now reads: "For instance, both planetary-scale and synoptic-scale waves are crucial for the tropospheric response to SSWs (Song and Robinson, 2004; White et al., 2022). While planetary wave propagation tends to be suppressed after SSWs (Hitchcock and Simpson 2016, JAS; Hitchcock and Haynes 2016, GRL), the tropospheric response can be amplified by synoptic wave feedbacks (Domeisen et al., 2013; White et al., 2020)."

Lines 75-76: Did you also use the other parts of the SSW definition defined in Charlton and Polvani? I presume that because you used dates directly from the Butler et al. paper, that you did. But for self-containment maybe state the extra conditions for SSW identification mentioned in the Charlton and Polvani paper. For clarity, the definition makes sure that the winds return to westerly for a 20-day period post easterlies, and must also return to westerly before the end of winter (end of April, I think).

Yes, these additional conditions are included in the selection of SSWs in the data set of Butler (2020). We will clarify this.

Line 80: Presumably you excluded the 22nd Feb 1979 event because there are not 60 days in that calendar year before the event occurred? Maybe state it directly?

Yes, that is the reason for not including that event. More precisely, we decided to go winter by winter and start from NDJFM 1979/80. Even though including spectra from February and March 1979 would be possible in theory, time lags of more than -20 days would have been not available. As we need to consider time lags of up to -30 days in other parts of our analysis (e.g., Fig.7 and 8), we decided to exclude the SSW on Feb. 22nd 1979. We will specify that in the manuscript.

Line 84: Why is 48ms-1 chosen? As you state, it is arbitrary, but without reading the Oehrlein et al paper, I do not know why it has been chosen? How sensitive are your results to increases or decreases in that value? I would think that a better and less arbitrary criterion would be to use some kind of standard deviation definition (although perhaps that is what the cited paper did already do)

It is a good idea to use exceedance of 1 std of 60N zonal mean zonal wind as alternative definition of SPV events instead of the rather arbitrary value of 48 m/s. Our motivation for defining SPV events as times when zonal mean zonal winds exceed 48 m/s was motivated by making results comparable to existing literature about SPVs (Oehrlein et al., 2020; Scaife et al., 2016; Smith et al., 2018). These authors motivated the definition as an analogous way to the SSW definition of Charlton and Polvani (2007a), stating that the threshold of 48m/s is chosen as it is exceeded with the same frequency as the lower SSW threshold (Scaife et al., 2016). Since we aim to compare the impact of SSWs and SPVs respectively, it seemed like this analogous definition would be the fairest way of doing so. Thus, even though 48 m/s might seem a rather arbitrary value, the motivation for choosing this specific threshold actually considers the variation, and could be seen as a modified way of standard deviation. We will clarify this choice in L84.

Line 117: Why do you use an extra 10degree in the tropospheric average compared to the stratospheric? For consistency across levels, maybe the same should be used? It is fine to do what you have done, but maybe just clarify why you chose those different bands for the reader.

Thanks for pointing out this unclarity. To include only Rossby waves connected to the mid-latitude jet stream in the troposphere we compute Rossby wave spectra between 35°N and 75°N at 250 hPa. Since the stratospheric polar vortex is located mostly between 45°N and 75°N, Rossby wave spectra are computed in the stratosphere for that latitude range. Thus, the different choice of latitudes can be justified by the different latitudinal range of the eddy-driven jet in the troposphere and the polar vortex in the stratosphere. We will add an explanation in this context in the paper.

Lines 145-150: I think this quantity was plotted in figure 2 but it was not clear from the text or figure caption. Can you clarify? Perhaps define this hemispherically-averaged phase speed as a different variable or just make sure to refer to it properly in the figure (also figure 7, for instance).

Thanks for highlighting this. We will refer to the definition of phase speed and ISP in the captions of the figures.

Line 163: Is there a reason you did not exclude the SSW and SPV event days from the resampling procedure?

We wanted to have a realistic data set of the atmosphere for our resampling procedure. This means, that also the extreme states (SSWs and SPVs) are included. As a result, also extreme states can be drawn for each random sample, which gives in our opinion a fair assessment of the statistical significance of the results.

Line 213: You divide the anomaly for a particular SSW or SPV by that season's mean? Or by an overall climatological seasonal mean?

We divide here by the overall climatological seasonal mean. Thanks for asking, we will clarify this in the text.

Lines 222-224: To clarify, a stationary wave pattern is suggested because the anomalies are dominated heavily by westward-propagating wave anomalies with a reduction in the climatologically-dominated eastward-propagating anomalies, and so this would lead to a more stationary pattern? I see that you said the reduction of all wavenumbers simultaneously likely indicates a weakening of the background flow, but the 'stationary' part is not clear to me.

A more stationary pattern was meant here in a way that Rossby waves are propagating at slower phase speeds than usual. Connected with the slow-down of the zonal mean wind, also the propagation of Rossby waves slows down, before Rossby wave activity is reduced across most wave numbers and phase speeds (Fig. 3d). We will clarify this in the manuscript.

Figure 4 (and other relevant figures): Why did you choose 250hPa as the 'tropospheric' level? 250hPa is already in the lower stratosphere at sub-polar latitudes. I guess your latitudinal average of 35-75N will be dominated by the lower latitudes when weighted. But if you stick with 250hPa, can you address how sensitive the results in this and other figures are to surrounding levels?

Thanks for pointing out the unclarity about the spectra at 250 hPa. This level was also chosen to analyze the impact of stratospheric extremes at 10 hPa on lower levels in the atmosphere, mainly the upper troposphere. We agree, that depending on the definition of the tropopause, the pressure level of 250 hPa can be indeed in the stratosphere. However, from a dynamical perspective, the jet stream is a result of the gradient between polar air (with a lower tropopause) and sub-tropical air (with a higher tropopause). Even though the jet stream might be strongest directly below the tropopause, it is still present at levels above and below, and thus also Rossby waves. Furthermore, 250 hPa is also a way of compromise to investigate different effects at the same pressure level: In order to evaluate the spectra of meridional heat fluxes, Sjoberg and Birner (2012) actually advocated for 200hPa as a "fully stratospheric" pressure level, since N^2 is constant. On the other hand, 300 hPa is still half tropospheric and half stratospheric. Going to even lower levels, like 500 hPa, would result in analyzing the flow below the jet maximum. One can additionally argue that it is at the level of the tropopause that PV gradients and PV anomalies are the strongest and the most variable. Thus, 250 hPa seemed the most appropriate level for our analysis of tropospheric impact of stratospheric extremes at 10 hPa.

Lines 232-239: This reduction in upward-propagating low-wavenumber waves strikes me as supporting the idea that planetary waves after an SSW onset become suppressed throughout the entire atmospheric column (not just the stratosphere) in agreement with Hitchcock and Haynes (2016: GRL) and Hitchcock and Simpson (2016; JAS).

Also, can you clarify why this reduction in the V'V' spectra provides evidence of a more persistent tropospheric flow post onset? I would have thought momentum fluxes would indicate this phenomenon. I understand V'V' to be more of an eddy kinetic energy-type quantity and given the reduction at low wavenumbers only, it indicates more of a planetary wave suppression in my eyes. I am happy to be proven wrong though!

Thanks for pointing out that this supports the hypothesis of a reduction in planetary waves throughout the entire column. This is a very interesting point and could be investigated in a future study.

A persistent flow was meant here, similar to the “stationary pattern” in line 222-224, that the jet stream is located over the same region for a longer time than usual. Since planetary-scale waves are suppressed, only synoptic-scale disturbances can chance the position of the jet stream. These are, by definition, smaller and thus, the large-scale pattern is more persistent. This agrees with other research, e.g., a longer persistence of the negative phase of the NAO pattern (Charlton-Perez et al., 2018; Domeisen, 2019).

Line 240: This looks to only be slightly significant at lags 10-20 days and is only sporadically positive at other lags.

We agree, that this signal is relatively noisy. Furthermore, the lack of significance could also be due to compounding the 25 events, where the shift towards higher wave numbers might not happen equally for each event. Nevertheless, a shift towards higher wavenumbers is present for eastward propagating harmonics, especially 10 to 20d after the event (Fig. 4c). Another point that supports this tendency is the symmetry of the SPV response with respect to SSWs (cf. Fig. 4 and 6). This gives us further confidence in our interpretation.

Lines 268-269: yes, but 250hPa is already in the lower portion of the vortex (see my point above).

Let’s see how similar the behavior a of lower level (e.g., 300 hPa) is compared to the chosen 250 hPa.

Lines 273-279: This gradual reduction in the (significant) phase speed anomalies from the middle stratosphere to the lower stratosphere is reminiscent of the downward propagation of critical lines *from the upper to lower stratosphere post SSW, a la Matsuno (1971) and Hitchcock and Haynes (2016).*

Thanks for pointing out this connection. We will consider it in the revised version.

Figure 9: Any ideas as to why the synoptic waves at k=4+ or so, also show a strengthening of westward phase speeds? To me, I wonder if it essentially represents the breaking up of the vortex and filamentation of potential vorticity streamers from the edge of the vortex as planetary waves break, leading to smaller-scale features. In any case, it would be good to mention the interesting, albeit confusing, synoptic-wave features so high up in the stratosphere.

We agree with your impression. These synoptic-scale features are likely due to the break-up of the polar vortex. We discuss this later in lines 360-362. Thanks for highlighting that interpretation. We will stress it more in the manuscript in L224 and 256.

Lines 322-323: Is this because the vortex has weakened (post SPV maximum) to an extent that it is more receptive to upward-propagating waves from below?

This point is discussed in context with Fig. 11 in lines 373-379. We presume that this behavior is indeed connected to a deceleration of the polar vortex, even though the specific Rossby wave signature differs between pre-SSW and post-SPV.

Grammatical Comments:

Lines 29-32: I would rewrite this opening sentence. It is a little confusing to read and disjointed (especially the part before the colon). The part after the colon I would just write as a new sentence.

Thanks for highlighting this sentence. We will rewrite it.

Lines 40-43: The opening sentence is again a bit disjointed. The NAM and AO you put in the first set of parentheses are zonally-averaged themselves, whereas you add about zonally-averaged quantities directly after, and using Hall et al. (2021) as an example. The Baldwin and Dunkerton 2001 and Thompson et al 2006 papers also use zonally-averaged quantities. Multiple sets of parentheses (in this case, 3!) can be confusing and break up the sentence too much. I suggest:

"The impact of SSW and SPV events on the tropospheric circulation is most often analyzed in terms of circulation indices of zonally averaged quantities (such as the Arctic Oscillation or the Northern Annular Mode, e.g., Baldwin and Dunkerton, 2001; Thompson et al., 2006), or in terms of changes in the frequency of weather regimes (e.g., Charlton-Perez et al., 2018; Domeisen et al., 2020c; Hall et al., 2023)."

We agree. Your suggestion improves the readability of that sentence.

Line 121: update end of line to: '...allow ONE to neglect...'

Thanks, we will fix that.

Line 214: change to '... a value twice as large. '

Well spotted, we will change this.

Line 359: 'reflect' --> 'lead to' or 'result in'

Good point, we will adjust the wording.

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