# Response to comments by the co-editor

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*Thank you for your detailed revision and reply to the referees' comments. Based on the reviews and my own evaluation, the manuscript can be acceptable for publication after some minor revision. Here are my suggestions for the paper revision.*

**We would like to thank the co-editor for handling the manuscript and for the following feedback. This has helped us greatly to highlight main messages and to clarify several points more precisely.**

*1. Most of the authors' results on the change of spectral power in the stratosphere show large values over the high zonal wavenumbers (i.e. Figs. 3, 5, 9-11). In the response (i.e. the response to Reviewer 1's comment on Line 325), the authors explain that part of this is likely due to the break-up and filamentation of the polar vortex. Besides this, I wonder whether this is also greatly due to the fact that the authors actually plot the percentage of spectral change in these figures. In the stratosphere, I would expect those high zonal wavenumbers with very small spectral power (or standard deviation for co-spectra) in climatology, which is the denominator when calculating the percentage ("relative anomaly"). Therefore, small change of spectral power in those wavenumbers can result in large and significant change in the percentage. If this is the situation, first, I wonder the change in the absolute value of the spectral power in the high zonal wavenumbers. Is it comparable to the change in planetary scale waves? Are most of these high values in synoptic scales with physical meanings? Second, to avoid any misunderstanding, I suggest the authors further clarify this and add some discussion on this in the manuscript. For example, in the figure caption or title, the authors can state directly that the figures actually plot the percentage of change (In the current caption of figures, the meaning of relative anomalies is not clear enough).*

**Thank you so much for highlighting this point. We discussed this internally already a few times and decided in the end to include the relative anomalies in the figures of the manuscript. The 'raw' anomalies showing the absolute magnitude would point out which harmonics have a relevant contribution in reality from an energetic perspective. Thus, only parts where anomalies of spectral power are high in total would be visible in the figures. This would include mostly harmonics within the black contours that show the NDJFM seasonal mean (Fig. 1).**

**In contrast, the relative anomalies that depict the percentage change highlight rather the signal that one can detect with respect to the variability of spectral power at each harmonic. For example, SSWs and SPVs exhibit a statistically**

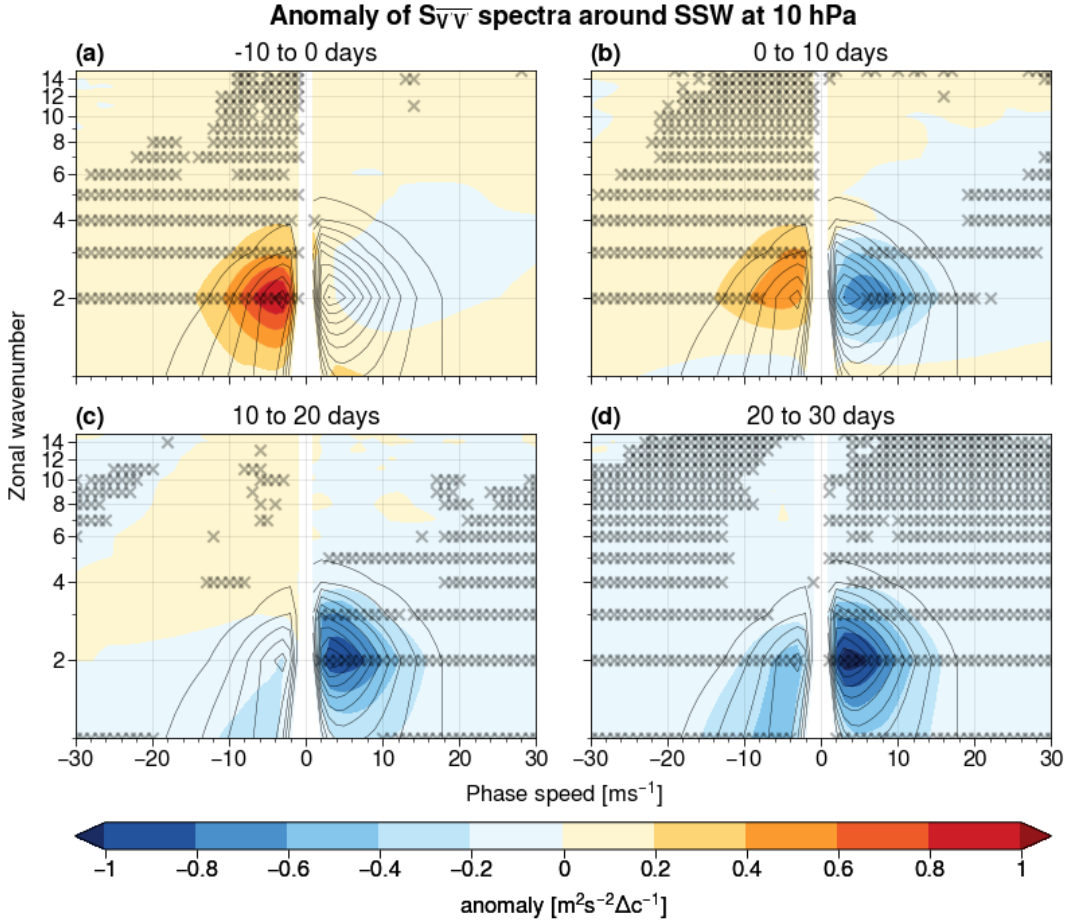


**Figure 1.** Anomalies of spectral power $S_{\overline{V'V'}}$ at 10 hPa averaged over 10-day time intervals around SSW events (shaded). Subplot (a) shows spectra for the period from 10 days prior to the event to the event start, subplot (b) shows spectra between the event start and 10 days after the event start, (c) for 10 to 20 days, and (d) for 20 to 30 days after the event. Wavenumber-phase speed pairs marked with an × exceed the 0.5th or 99.5th percentile of the re-sampled distribution. Black contour lines show the NDJFM climatology ranging from 0.2 $m^2\,s^{-2}\,\Delta c^{-1}$ to 1.8 $m^2\,s^{-2}\,\Delta c^{-1}$ in steps of 0.2 $m^2\,s^{-2}\,\Delta c^{-1}$.

**significant signal from their mean for westward-propagating Rossby waves in the range outside the climatologically relevant harmonics already the days preceding the events. Even though the anomalies might play only a minor role in the overall energy contribution, we decided to highlight this in our manuscript, to point out the importance of anomalies being extremely high or low compared to their climatological mean. Furthermore, since the anomalies are still significant and are part of a coherent pattern across the whole range of harmonics, they are likely also part of the overall Rossby wave behavior during SSWs and SPVs, e.g., the filamentation that we suspect to be represented by higher zonal wavenumbers in the stratospheric spectra during the onset of SSWs.**

**We followed your suggestion and clarified the captions of figures 3-6. The difference between percentage change and the actual change of spectral power is discussed in the very last paragraph of the outlook. We extended that discussion with respect to your comment (line 452-454).**

*2. In the first round of review, both reviewers comment on the use of 48 m/s to define the strong polar vortex (SPV) events and suggest checking the results by using the one standard deviation to define the strong event. I appreciate that the authors tried the later definition in the response letter and showed that the results are not very sensitive to the detailed definition. Though there is no need to add this test in the manuscript, it might be better to mention the result in section 2.1 when introducing the SPV definition, saying that the results are still hold if using an alternative definition.*

**Good point, thank you. We have added a sentence in section 2.1 to highlight that results are still valid with the alternative definition using one standard deviation (line 90-92).**

*3. In the last review, both reviewers hope that the authors use a longer dataset or model simulations to better test the results. Though the authors argue that this could be a topic of future studies, the authors at least can add more discussion on the possible caveat of the manuscript given the limited number of SSW and SPV events in the analysis, and the possibility of testing the results using model simulations in the Outlook part of the paper.*

**Thank you, we extended the paragraph in the discussion about the limited number of events and possibilities to overcome this (line 437-444).**

*4. Line 139: I'm not sure whether it is appropriate to use "interpolation" to describe the conversion of spectra from the frequency space to the phase-speed space. Using the word "convert" or "transfer" seems more accurate.*

**We choose to use the word "interpolation" to be consistent with the relevant piece of literature describing the procedure, i.e., Randel and Held (1991) (cf. the final words of their page 689). The procedure of interpolation is also described in detail in the Supplementary Information of Riboldi et al. (2022). Thus, for clarity and for consistency with previous literature, we believe it would be preferable to keep this wording.**

*Thank you for your patience in waiting the decision.*