Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2021-59

Anonymous Referee #2

Referee comment on "Circumglobal Rossby wave patterns during boreal winter highlighted by space–time spectral analysis" by Jacopo Riboldi et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-59-RC2, 2021

This work aims to asses the main observed modes of variability and origins of Rossby wave packets (RWP) in the northern hemisphere winter.

It uses a spectral decomposition method of the upper-level circulation at each latitude to retrieve the most dominant modes of variability in the space of zonal wave number and phase speed. The first two modes are found to be associated with RWPs, and so these are used for further analysis to identify regressions with diagnostics of blocking, large-scale patterns and wave propagation.

The authors also investigate the possible origins of the wavetrains characterizing both modes. They find a likely link between the first mode and tropical convection and the MJO, and elucidate that the second mode is related to extratropical origins, though this link is less clear. They point out other interesting features, such as that both modes exhibit higher synoptic eddy activity and subtropical jet extension that allows the hemispheric wave propagation, which allow a more complete picture of how these modes come about.

The paper is clearly written and conclusions well argued. The results contribute significantly to the field of eddy-mean flow interaction and teleconnections in the northern hemisphere.

I only have minor comments after which I recommend it for publication.

General minor points:
- what is the sensitivity to the windows chosen for the spectral decomposition, and same for the thresholds chosen to isolate PC extremes?
- As I understand it, the authors use the EOFs of the spectral decomposition as the basis for their composites and regressions, because they want to analyse regimes based on the different wavenumbers/phase speeds, groups of which we know have different characteristics (e.g., planetary waves vs synoptioc waves). I like that the analysis is based on reducing dimensionality for physical reasons, but I wonder whether some of the composites, e.g. Fig. 9, would yield a less noisy time series if the v*T* and baroclinicity were directly related to the RWP? It seems that especially Fig. 8(d-f) yield consistent patterns, but with the spread being so large, the authors conclude that these consistent changes in baroclinicity are not significant. â□¨At least comment outlining the disadvantages of this technique would be useful.

- the Figure labels could be enlargedâ□¨.

Specific clarifications
- L20: between one year or the other - you mean inter-annually?
- The next three points aim to differentiate between the two types of mechanisms more clearly:
  - L22: single storm track (internal mechanism)…between the two (hemispheric mechanisms [or something like that])
  - L23: AN internal mechanism
  - L27: [new line] On the other hand… storm tracks (hemispheric mechanism)
- L89: please state how the smoothing was performed
- Eq. 5: it may be worth adding a comment that vT is proportional to the vertical E-vector component and will also be investigated.
- L104: "interpolated along lines of constant phase speed" What exactly do you mean?
- L186: it may be worth including Orlanski (1998) as a reference here, for the interpretation of the horizontal E vectors (https://journals.ametsoc.org/view/journals/atsc/55/16/1520-0469_1998_055_2577_pdost_2.0.co_2.xml)
- Fig. 4: I couldn't see any green arrows