Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2021-59-RC1, 2021

Comment on wcd-2021-59

Anonymous Referee #1

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-RC1, 2021

General Comments:

This study explores the boreal winter midlatitude circulation features that emerge from the daily variability modes of the upper-tropospheric zonal wavenumber - phase speed spectrum. The authors focus on the origin and evolution of hemispheric-scale Rossby wave patterns that may characterize the first two modes and test hypotheses regarding the role of tropical convection, jet stream location, and low-level baroclinicity. The analyses are novel and valuable outcomes can be drawn from them. I have a few concerns regarding the basic rationale and scope of this study and some parts of the text require technical clarifications. I recommend a major revision of this manuscript and I hope that the specific remarks listed below will be helpful in this regard.

Specific Comments:

- The main goal of this study is to investigate the origin and propagation of CRWPs. In some parts of the introduction and the conclusions, it appears that another objective is to showcase the potential of the two-dimensional spectral analysis on the upper-tropospheric circulation. In the end, I am left wondering what is the exact scope of this study and what should be the take-home message for the readers. Perhaps, my confusion lies in the fact that the introduction (lines 14-67) touches on many themes and not all of them are directly relevant to the analyses. Could the authors clarify in the text the research questions adressed in this study and the respective conclusions?
- Following up on the previous comment, if the motivation of the study is to investigate and better understand CRWPs, then I find the employed methodology rather indirect. Instead of constructing daily n-Cp Fourier spectra of the midlatitudes, doing an EOF analysis on these spectra, and focusing on periods when the PC of the leading modes is large hoping that the composite patterns will exhibit long-lived and/or circumglobal wave patterns, why not directly diagnose CRWP events in the wind field by simply requiring that the RWP amplitude (section 2.2.2) is large enough in a large area for e.g. 5 consecutive days? The second option would result in composite maps that are easier to interpret and actually contain the wave patterns of interest. With the employed

methodology, not all the observed CRWP events are necessarily included in the top PCs of EOF1 and EOF2, while these two leading modes may also contain days with no CRWPs, thus "contaminating" the lag-composite analyses with flow configurations beyond the authors' interest.

- Given that the wavenumber-frequency spectra are based on the spectral coefficients (discrete Fourier Transform), writing down their equation would perhaps be more relevant than eq.1 (inverse discrete Fourier Transform). In addition, something seems to be off with the notation in eq.1. Since this is basically a 2D inverse DFT, I would expect the arguments of the spectral coefficient to be [n,j] rather than [n,ω_j] (thus matching the two summation indices), the exponents to be like: exp[i2π(λ*n/N_L + t*j/N_T)], and ω_j to be equal to -2πj/N_T.
- Is the methodology of constructing the wavenumber - phase speed spectra exactly the same as the one presented in Riboldi et al. (2019)? If so, this might be a useful remark for some readers. In any case, some things are not clearly described in the text. I am not sure what is the mathematical formulation behind the phrases: "smoothed 10 times in frequency using a three-point window" (line 103) and "The periodogram is interpolated along lines of constant phase speed" (line 104)? All the results are based on these steps, so it would greatly help the reader if they are described clearly with minimal jargon.
- Why do the days of top 15% PC1/PC2 (that is, up to 30% of all winter days) are included in the CRWP1/CRWP2 lists of events? Doesn't this result in too many events, given that the first 2 EOFs only explain 22.5% of the total variance? In this regard, it is perhaps no surprise that some days are found in both the CRWP1 and CRWP2 events (e.g. 16-24 January 1987, 25-26 December 2014) and that the typical duration of the events (as reported in the Supplement) is much larger that what the evolution in Fig.5 suggests. I presume that taking the top 5% or 10% PC days in each case would produce lists of less events but more indicative(?) of the respective EOF modes. Can the authors elaborate on their rationale behind the choice of this threshold and comment on the sensitivity of the results to its exact value?
- There are some causal statements in the text that - although they appear plausible at first sight - they cannot really be justified by the presented analyses. For example, in lines 267-268 it is stated that, according to Fig.6a, the strong convection is the source of negative vorticity anomaly at upper-levels a few thousand kilometers to the east. Looking at pentad-mean composites doesn't really allow such a statement and the fact that there is a synoptic-scale negative height anomaly in the area (Fig.6a) and at times an active MJO does not correspond to OLR anomalies (line 298) makes me even more hesistant to draw "causal chains" here. Similarly, line 327 reads that the North Atlantic anticyclone development is preceded by anomalously strong meridional heat fluxes over N. America (Fig.9). The problem here is that the significant patterns are rather weak/small and the plotting (contouring choice) may obscure the real sequence of events. Maybe adding the complete series of daily-mean lag composites in the Supplement would shed light on such aspects.

Technical Corrections:

- The abstract ends rather abruptly. It would be worth and nice to add a sentence with the take-home messages of this study.
- Line 14: "Most of the weather systems ... have their dynamical origin ..."
- Line 16: "... form and track across those regions ..."
- Line 18: "... displacements of the jet ..."
- Line 20: "... substantially across seasons and years (Grise et al. ..."

- Lines 36-37: Is this really "another" circulation pattern? The phenomena mentioned in lines 32-35 may also feature the zonal propagation of RWPs.
- Lines 37-38: The readers should clearly visualize in their minds what exactly are the CRWPs that this study focuses on. The sentence "CRWPs can occur if the large-scale flow configuration is conducive to the zonal propagation of RWPs over long distances" explains when do CRWPs occur; not what they are. So, how do they look like in e.g. a snapshot of the upper-tropospheric wind field? Or is this a concept attached to longer-term means?
- Lines 58-60: In these lines the benefit of the spectral analysis is briefly mentioned. Can the authors elaborate a bit more on which frameworks make "a-priori assumptions about the existence of a background flow"? Listing a couple of specific examples upon which the new framework can make a difference would better communicate the significance of this study.
- Line 78: What is meant by "rapid module"?
- Line 89: Do the authors mean "... the annual cycle of meridional wind..."?
- Line 105: The phase speed spectra should also be a function of time, right?
- Line 110: Which coordinate change is meant here?
- Lines 119-120: What is meant by "averaging over different latitudes"? The approach where spectra of different latitudes are averaged, or the approach where wind anomalies are averaged over different latitudes?
- Line 121: Why is a spectrum attributed to every single day? The temporal resolution is 6-hourly, so I would expect 4 global estimates S(n,cp) per day. Is there a daily averaging step that I missed?
- Fig.1: Shouldn't the units be [(m/s)^2/Δc]?
- Line 205: The monopole structure in EOF1 (Fig.2a) suggests variability in the amplitude of Rossby waves (with no apparent shift in wavenumber and phase speed) as shown in Fig.4a,b. Is that what the authors also imply by "enhancement of such harmonics"?
- Line 214: "... wavenumber co-vary with an ..."
- Line 257: "Interestingly, both PC1 and PC2 events are ..."
- Lines 281-283: The procedure to assess the stat. significance of MJO amplitudes is not clear to me. Can the authors reformulate this sentence?
- Figs.4,5,6,9,10: The colorbar labels are particularly small and hard to read. I guess a 50% (or more, for Fig.5) increase in the font would suffice.
- Figs.4,5: Do the black contours depict 250hPa PV as in Fig. 10?
- Fig.6: In lines 384-385 the authors hypothesize an equatorward shift of the jet for PC2 events. Adding contours for negative zonal wind anomalies might help evaluate this hypothesis.
- Lines 308-309: The term "release of baroclinicity" doesn't sound very meaningful. Besides, should the authors briefly describe the Ambaum and Novak (2014) hypothesis, given that big part of this work (section 4.2) is about to test it? Finally, this hypothesis doesn't really talk about a "hemispheric-scale discharge of baroclinicity" as implied in lines 337-338; it focuses on the N. Atlantic storm track.
- Lines 310, 315: Why is a 7-day averaging employed here instead of a 5-day one as before? Why is stat. significance assessed based on the 95th percentile instead of the 90th one as before? This inconsistency is somewhat disconcerting and hinders the comparison of Figs. 6 and 9. The maps in these 2 figures could also have the same projection for consistency.
- Lines 388-390: Why would there be a problem conducting this spectral analysis in the summer season? Weaker storm tracks may also experience distinct modes of variability. Is there a technical issue I miss?