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Comment on wcd-2022-28

Anonymous Referee #1

Referee comment on "Decadal variability and trends in extratropical Rossby wave packet amplitude, phase, and phase speed" by Georgios Fragkoulidis, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-28-RC1, 2022

Review of "Decadal variability in extratropical Rossby wave packet amplitude, phase, and phase speed" by Georgios Fragkoulidis

This research work applies a set of cutting-edge diagnostics, most of them developed by the author and previously published, to study the decadal variability and the long-term trends in Rossby wave packets and their salient properties (amplitude, phase, zonal propagation). Different analyses are performed and significant trends are discussed, with the aim to disentangle the signal of inter-annual and decadal variability from long-term trends.

The paper is clearly written and full of interesting details about the followed procedures and the results. This nice abundance of details, on the other hand, sometimes hinders the reception of the main message. I would be happy to recommend the publication of this manuscript after few additional methodological clarifications are provided and the text has been streamlined to convey key results more directly.

General comments:

- I have the impression that the main message of the study does not come across in a straightforward manner, "hidden" by the large amount of results. Could such a statement like *"as of 2021, most trends identified in the last decades are likely the result of inter-annual—to–decadal variability. Thus, it seems that extratropical variability still outweighs almost completely any climate change signal visible in Rossby wave packets"* summarize the article? Results of this paper seem to support the hypothesis that the influence of global warming on the midlatitude circulation is mostly thermodynamic and not dynamical, at least for what concerns RWPs. Something on this line is already written at lines 429-434. If this is the case, the author should state that

even more clearly through the text (abstract, summary, etc…).

- Related to previous comment, the Summary and Discussion sections could be made more to the point and easier to read by 1) splitting the body of text and discuss separately and concisely results from each analysis, using subsections and/or bullet points, or even tables or schematics; 2) enhancing the comparison between the obtained results and previous work, now limited only to two papers (lines 435-440), and citing also disagreements between results of this study and published literature.
- The fltering procedure to identify RWPs is not based here on wavenumbers but on wavelengths between 2000 and 10000Km: this is good, because it reduces wavenumber aliasing due to the convergence of meridians toward the poles. However, isn't 10000 km a bit of a long wavelength for a synoptic-scale wave? What is the rationale for this choice?
- A RWP would lose its coherency and get deformed if its northernmost edge were to move faster than the southernmost edge, or vice versa. Given that the phase speed is expressed in m/s, and that the latitude circles have different lengths, the northern part must travel systematically slower than the southern part, and this appears to be the case looking at the meridional gradient of phase speed inside the RWP over the North Pacific in Fig. 1e. How systematic is this feature? Averaging across this gradient would result in a medium phase speed value. How can we be sure that the phase speed metric measures the actual variability in propagation and not variability in latitudinal position/extension of RWPs? This could also influence the lack of covariance between E and phase speed described in Section 3.3. Possible ways to investigate this aspect could be a systematic comparison of RWPs traveling at high and low latitudes, or a phase speed metric expressed as angular velocity.
- Previous literature connecting Rossby waves with extreme weather often conflate together meridionally amplified waves with the occurrence of atmospheric blocking. Several studies are also based on such a tacit assumption: amplified waves, often associated with or resulting in blocking, propagate slowly and increase weather persistence. At line 433, it is said that RWP diagnostics employed in this study are not completely suited to consider the decay and wave-breaking stage of RWPs. If the diagnostics are not able to capture the effect of blocking, this might lead to misunderstandings, for instance concerning the lack of covariance between E and . How do the employed RWP diagnostics capture atmospheric blocking? Do blocking events correspond to compound high E/low c_p events?
- Related to the previous point, but on a more general level: how much are the diagnosed trends in E (Fig. 6) actually due to actual increase/decrease of the amplitude of E, or due to a qualitative change in the structure of the waves that makes them not properly detectable by the employed methodology? (E.g., due to more/less cutoff lows or wave breaking?)
- The lack of covariance between E and is interesting and puzzling. The author already performed a composite analysis to understand this connection in the Supplementary Material of a previous paper (line 361). However, composites of RWP amplitude and phase speed are not obvious to interpret. Are situations with low E and low c_p simply associated with a weak waveguide? Is atmospheric blocking or wave breaking involved in some of those unexpected low E/low c_p or high E/high c_p configurations? A way to to visualize more directly the underlying dynamics would be to select single days exhibiting particularly high/low E/c_p and plot standard quantities during these "snapshots", as upper-level wind or geopotential.

Minor points:

Lines 148-153: an obvious question here would concern the sensitivity of the identified

trends to the chosen 15 m/s threshold for E. Can the author comment on that?

Lines 160-165: why not including meridional variations of phase to account for nonzonal RWP propagation, too?

Line 177: how is the standard deviation computed? From 6-hourly or daily data, i.e., from all 3610 days, of DJF?

Lines 212-213: Very interesting hypothesis, could the author elaborate more on this reduced connection between surface and upper-levels during summer with respect to the other seasons? Has this aspect been discussed by previous literature, and/or is based on some dynamical reasoning?

Lines 293-294: Wouldn't a simpler explanation be, that a shift in the RWP genesis consequently shifts of the same phase angle also downstream troughs and ridges? As it is phrased now, it seems like the change itself in ridge/trough occurrence is inducing a development of transient RWPs.

Lines 295-300: the interpretation of these phase plots is a bit difficult. Would it help to overlay regions of significant E trends?

Lines 305-309: Trying to understand positive phase speed trends in MAM: could they be related to a delayed break-up of the stratospheric polar vortex?

Line 325: the threshold on grid points penalizes features located at low latitudes, as a RWPs with a given size can satisfy the criterion if it propagates at high latitudes and not at low latitudes. It would be better to use a criterion based on the 10% of the area occupied by a RWP object inside a given region.

Lines 375-377: Very interesting observation, it reminds of the phase speed trends analysis of Riboldi et al. (2020). Would there be a qualitative agreement between the two metrics if the phase speed were to be averaged across all the boxes, or across a broad latitude range, to obtain a single value?

Line 454: Fig. S1 is interesting and potentially connected to phase speed trends, but it is not cited or discussed anywhere in the paper. It feels a bit out of place in the closing sentence. Maybe the short Section 3.2.3 (phase speed) is a more appropriate place to introduce and briefly discuss it.

Technical/Typos:

Lines 7-8: sentence too long and weirdly structured, the first sentence can be removed and start with "While many areas…"

Line 9: what is meant by "patterns of robust trends"?

Lines 19-20: this last sentence is very general and not clear, please reformulate.

Lines 22-24: filler sentences that can be omitted or at least shortened?

Line 57 (and other locations): what is meant precisely by "highly-dynamic"?

Line 119: what is meant by "Arg" and "atan2"? Please use standard mathematical notation.

Line 310: what is meant by "organized formation"? Could this sentence be shortened simply to "No significant trends"?

Line 370: is "monotonic" here to be intended as "significant"?

Lines 387-399: the take-home message of this analysis could be emphasized by a summarizing sentence at the end of the paragraph.

Line 421: "Associated with that": the connection with the previous sentence is not obvious, because the sentence above emphasized the role of inter-annual and decadal variability. This sentence, on the other hand, speaks of long-term trends.

Line 447: in which sense is this approach "weather-informed"? In what sense is it more related to weather than the other studies cited at lines 35-55?

Line 450: is “contained” to be intended as “limited”, “attenuated”?