Review of Egusphere-2025-3867

The authors present an intersting manuscript about upper-tropospheric drivers of cold extremes. By assessing the meridional extent and the zonal propagation of the upstream ridge and the downstream trough separately, the study promises valuable insight on the role of the large-scale atmospheric dynamics for temperature extremes. The authors develop an original algorithm focused on planetary waves that could potentially complement existing analyses of synoptic-scale variability, such as Rossby wave packets, or hemispheric variability represented by Fourier harmonics. However, it remains unclear to what extent the results are sensitive to filtering choices and to the definition of the study regions. Significant improvements are therefore required to better justify and illustrate the choices underlying the development of this new algorithm.

General comments

1. The introduction falls short in introducing the large variety of waviness and persistence metrics and the scientific discussion around their importance for weather and climate extremes. The first paragraph contains mostly textbook material (line 11-16). The second paragraph and in particular the first sentence (line 19-20) could have been expected at the very beginning. And the third paragraph contains mostly anecdotal evidence. On the other hand, the first paragraph of Section 3.2 (line 156-167) makes good material for the introduction. I suggest restructuring and extending.
2. The sentence in line 65-66 is key to the relevance of the present manuscript. I am surprised to see that the authors choose to move the evidence supporting this statement to the supplement; in particular, since the filtering choices as part of the detection algorithm seem justified by the location of the study regions relative to the climatological position of the troughs and ridges (Fig. 2). Without reading the supplement, it is not clear why these filtering choices are made. Retaining zonal wavenumbers 1-5 and the 30° longitude prominence criterion basically ensure that there will be exactly 3 troughs and 3 ridges (line 137).

3. No information on the tracking itself is provided. I assume that the detection algorithm is run at every time step and at every latitude. How are the troughs and ridges labeled and tracked in time? Do they from and decay in different places? How long do they persist? Is there some correspondence across latitudes? The authors could provide much more detail. Figure 1a is helpful for illustration of the detection. I suggest producing a similar illustration for the tracking.
4. The lead-lag behaviour of wave amplitude and wave speed presented in Figure 5 looks very interesting in light of the literature introduced in line 194-211. However, I am afraid that averaging only over latitudes, that show a significant positive amplitude or a significant negative speed anomaly, compromises the informative value. The Figures S7 and S8 also show latitudes with a significant negative amplitude or positive speed anomaly as well as latitudes with not significance. Is there a lead-lag behavior at those latitudes?

Specific comments

- Line 14-16: The second half of this sentence is basic knowledge and not necessary as introduction of an article for WCD.
- Line 33-34: How does the statement on cold spells under global warming relate to the study by Ribes et al. (2025)?
- Line 60-64: The discussion of the IPCC regions does not seem relevant for the present manuscript.
- Line 79: It seems that Section 2.3 would be entitled more appropriately as “Trough and ridge speed”.
- Line 83: Why is the algorithm called “top-ridge and bottom-trough tracking” instead of a “ridge and trough tracking”?
- Line 86: Is the prominence criterion of 8 % applied to every grid point or just the two grid points exactly 30 degrees to the west and to the east? I suspect the latter. And why is the value of 8 % chosen?
- Line 101: I suspect that “the chaotic nature of the gph field” refers mostly to cut-offs. I am not whether chaos is the correct term here.

- Line 105: What do vertical extremes refer to in this context?
- Line 109: Which method is used for the manuscript? The method that takes account for meridional tilting?
- Line 117: Not even at the North Pole?
- Line 119: What does consistent mean in this context?
- Line 133: For some cases in Figure S2 the anticyclonic (cyclinic) anomaly lies rather poleward (equatorward) of the region.
- Line 137: Be more clear that these are the climatological position, not the troughs and ridges detected in the climatological-mean field.
- Line 145-147: The "intrinsic relationship between relative and planetary vorticity" is not clear. The point that the conservation of potential vorticity gives rise to Rossby waves is textbook knowledge.
- Line 151-153: Figure S6 shows the MCI and does not provide evidence for the role of temperature advection.
- Figure 2 : The colorbar is missing a label. Also, I wonder whether the authors might want to display the mean geopotential height field for comparison with the trough / ridge locations?
- Line 168-173: Are the trough and ridge speeds and amplitudes calculated only in the longitudinal bands of their climatological position or across the globe?
- Figure 3: What are "probabilities of vertical extremes in longitude and latitude"? And why do you show them in relative longitude but absolute latitude?
- Figs. S7 and S8: Related to the question above, are the troughs and ridges assessed in these figures (blue/red lines, black lines, grey lines) necessarily located in the longitudinal bands indicated lines 168-173?
- Line 179: If I am not mistaken, an amplification of the ridge is determined by its meridional extent and, therefore, does not imply the development of higher geopotential height values.
- Line 182: The Figure S6 depicts the MCI, which is not directly related to temperature advection.
- Line 193: "Causality" seems a misleading section title. I suggest something like "lead-lag analysis"

- Line 198: I would not say that the mechanism is unclear. I suggest removing the sentence because it is not necessary to motivate the following.
- Figure 4: Why is the wave speed all positive despite the westward propagation indicated by Fig. 3? Also, it does not seem to agree with Fig. S8.
- Line 224-227: So far, the manuscript dealt specifically with troughs and ridges. But now the authors generalize to “Rossby waves”.
- Line 236: I recommend a reference to Geen et al. (2023) for the influence of Arctic amplification on waviness.

References

Ribes, A., Robin, Y., Tessiot, O., & Cattiaux, J. (2025). Recent extreme cold waves are likely not to happen again this century. *Bulletin of the American Meteorological Society*.

Geen, R., Thomson, S. I., Screen, J. A., Blackport, R., Lewis, N. T., Mudhar, R., ... & Vallis, G. K. (2023). An explanation for the metric dependence of the midlatitude jet-waviness change in response to polar warming. *Geophysical Research Letters*, *50*(21), e2023GL105132.