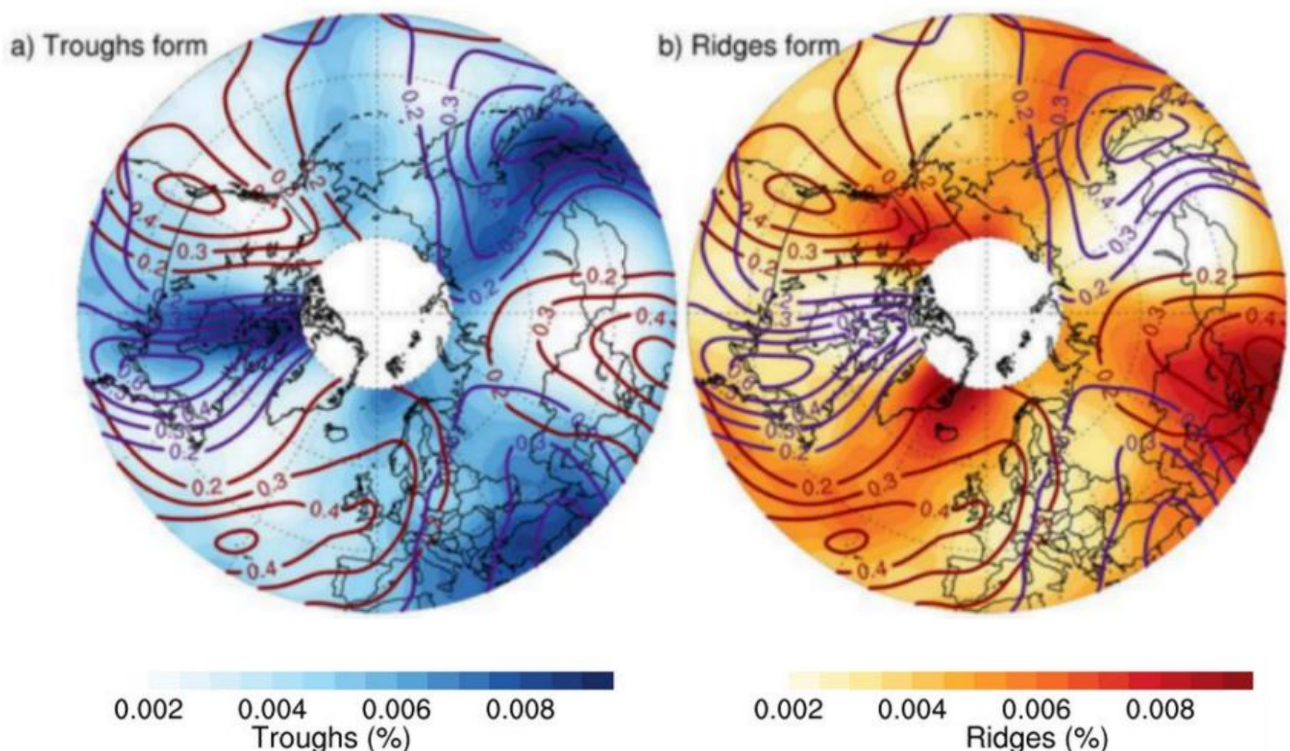


The authors have delivered a very thorough reply to my previous comments, further revising the manuscript to reduce misunderstandings and taking the time to explain the reasoning behind their methodological choices.

These explanations have further highlighted issues with this work that had initially convinced me to propose this manuscript for rejection. However, I will give the authors a last chance to fix these standing issues in the manuscript through a revision focused on three specific points. After this final revision is addressed, I will be ready to either accept or reject the manuscript without further review. Please provide a point-by-point reply to the issues raised below.

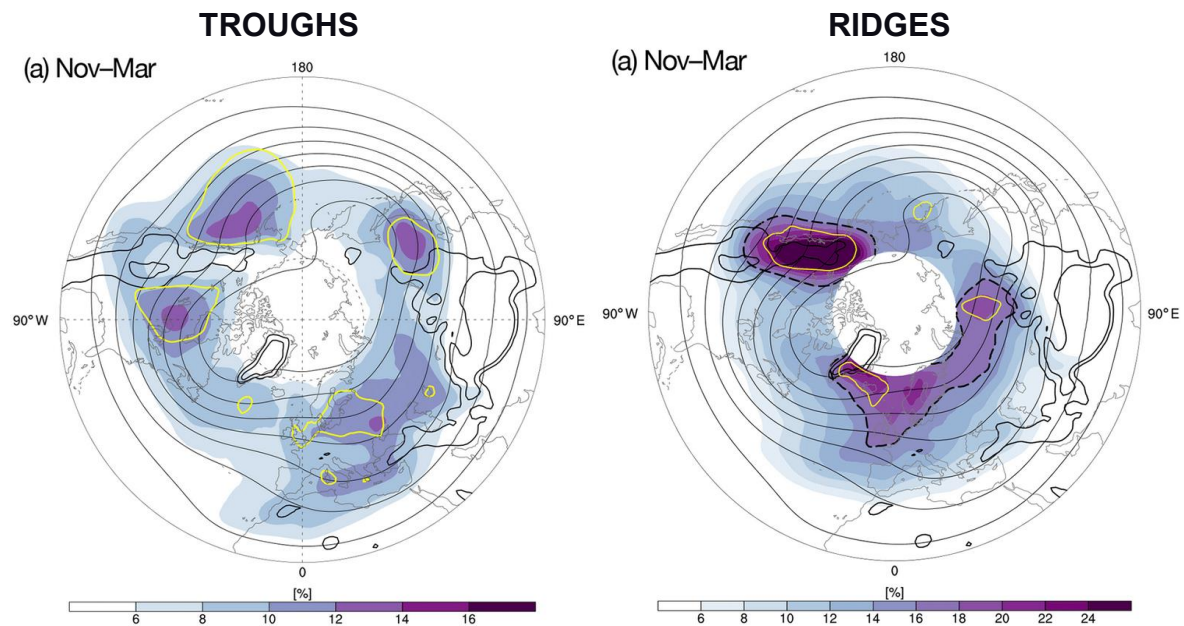
1) The choice of limiting trough and ridge identification to the zonal wavenumbers between 1 and 5 (Z1-5) seems to result in unrealistic trough and ridge frequencies. Considering only the first five Fourier harmonics of geopotential, without removing the time-averaged flow, leads to the systematic identification of the climatological, stationary ridges and troughs due to orography, land-sea contrast and other stationary wave forcing. Despite the reassurance of the authors that tracking is performed "across the entire hemisphere", ridge and trough tracking effectively happens only in the limited longitude sectors corresponding to those stationary waves (e.g., the Ridge over the Rockies, the troughs upstream of the two storm tracks). As visible in the genesis/decay frequency (and noticed by the other reviewer, too), the tracked features are indeed very likely to form to the west of the time-mean ridge/trough and decay right to the east of it with little chance of propagation across, e.g., a continent or an ocean basin. This is visible in the contours of Fig. 4 in the manuscript (pasted below). Furthermore, there is virtually no overlap between trough and ridge frequencies over most of the hemisphere: for instance, it looks like it is extremely uncommon to identify ridges over east Asia, or troughs over the North Atlantic --which are, on the other hand, rather common during winter and tied to, e.g., NAO+ and the Icelandic Low.



For comparison, I copy below a plot from

Schemm, S., Rüdüsühli, S., and Sprenger, M.: The life cycle of upper-level troughs and ridges: a novel detection method, climatologies and Lagrangian characteristics, Weather Clim. Dynam., 1, 459–479, <https://doi.org/10.5194/wcd-1-459-2020>, 2020.

a reference (not cited yet in the manuscript) where one can also find the seasonal climatologies of (left) trough detection and (right) ridge detection frequencies for the cold season (Nov. to Mar.) in ERA-Interim reanalysis.



In those plots by Schemm et al. (2020), troughs and ridges can be found at all longitudes. The zonal variations leading to relative minima and maxima in frequency also correspond (roughly) to the location of stationary ridges and troughs, but those relative extremes in frequency are not as sharp as in the current manuscript (with the exception maybe of the ridge over the Rockies). The presence of such defined ridge/trough regions in Fig. 4 is possibly due to a phase preference for some of the wavenumbers (1-5) involved in the filtering: for instance, assuming that the Rockies ridge projects on $k=3$, the $k=3$ would systematically be projected on a ridge corresponding to the location of the Rockies and, with it, also the remaining two ridges and three troughs over the rest of the hemisphere will acquire fixed locations. Removing a zonally varying geopotential climatology could help attenuate this problem.

2) Unlike what is suggested by the authors in their reply, I do not believe that "Rossby waves are stationary and have little or no transient behavior": instead, I am rather convinced that the Rossby waves identified following the methodology proposed in this study feature little or no transient behavior. The authors write in their reply that they "... tracked all ridges and troughs across the entire hemisphere and gave them a name for specific longitude sectors" and, later that they are tracking "transient troughs and ridges". This claim is true only in theory, because the presence of a climatological ridge would automatically weaken a trough eventually passing thereby, introducing a risk that the algorithm discards it more often than not. In the practice, what is being tracked is rather the "wobbling" of the same

climatological ridges and troughs around their respective time-mean locations, superimposed with the passage of transient ridges and troughs (by constructive interference with the stationary wave). Climatological, supposedly stationary features can "wobble" because the flow in which they are embedded is not steady. For instance, the ridge upstream of the Rocky Mountains is generated by the interaction between the mountain chain and the background flow: as the latter changes with time, the shape of the resulting orographic wave can also be expected to change. Thus, it is well possible that a orographic wave might feature some degree of movement at synoptic time scales while still remaining anchored, of course, to the topographic feature generating it. This "wobbling" could explain the large negative displacements found in Fig. 3c, as it is quite rare to have a retrograding pattern moving westward at 3-4m/s across half of the hemisphere. **All those issues arise from the choice of performing the tracking on the Z1-5 filtered geopotential height field, obtained through a nonlocal mathematical operation like the Fourier transform.** I am also not aware of other studies who performed tracking of filtered Rossby waves at the scale of individual troughs and ridges, and maybe there is a good reason for that. Furthermore, I am aware that the example brought by the authors their reply document (Fig. 1) would suggest a very good instantaneous match between filtered and unfiltered geopotential, and that those harmonics would represent the flow at high latitudes quite well... but the unrealistic longitudinal partition found in the climatological trough and ridge frequency keep me wondering about how the temporal evolution of the tracked feature actually looks like.

3) The authors conclude (lines 323-327) that "Our findings highlight the critical importance of wave location, emphasizing the need to consider potential shifts in wave positions in a warming world." The sentence above only makes sense in a world where ridges and troughs, regardless of whether they are transient or stationary, can only be found in rather specific longitudinal sectors. This contradicts synoptic weather experience and previous studies. In synthesis, **the results discussed by the authors make sense in a Z1-5 world, but their applicability to the real world --where trough and ridges can be encountered across the whole hemisphere-- remains rather questionable.** This might be an issue when other regions are chosen for investigation: almost all regions chosen in Fig. 2 are located near a time-mean ridge or upstream of a time-mean trough.

From the comments above, I believe that the paper can be made acceptable only if

- the reader is informed explicitly and thoroughly of all the limitations that choosing a "Z1-5 approach" introduces on the distribution and properties of the tracked ridges and troughs, also by comparing it with pre-existing literature (e.g., Schemm et al. 2020);
- or if the analysis is repeated removing a zonally varying climatology (I am aware that this might require more work, but it would help to dissipate doubts and clarify the approach; a re-submission might be indicated).