Reviewer #2

I am happy with the corrections that the authors have brought to the manuscript, so that the reader can better understand that the waves captured by the method largely overlap with the climatological, quasi-stationary Rossby wave features of the extratropical circulation. No method is perfect, but what matters is that the reader is made aware of what each method does (and does not) show.

We highly appreciate the reviewer's efforts in the review process, which helped us to significantly improve the presentation of our methodology in the manuscripts.

I have only two very last issues that can be addressed along the same line.

1) In the reply document, the authors write that

"Furthermore, removing the zonally varying climatology does not necessarily improve the representation of transient ridges and troughs. In fact, this process can introduce artificial ridges and troughs, as illustrated in the schematic (Figure 2 in this response)".

Here, I am not really understanding the point of the authors. The wave patterns shown in the Fig. 2 of the reply document features a wave that is most likely a combination of a wave-4 and a wave-5 harmonics, while the climatology in black looks like a perfect wave-4. It is no surprise that removing the wave-4 climatology makes the remaining wave-5 signature pop out. This is exactly the point of removing the zonally varying climatology: to reveal the transient waves "hidden" by the climatological ones. Furthermore, this behaviour is consistent with the linearity of the Fourier transform and anti-transform: $F(a+b)=F(a)+F(b)$.

To make the point clearer, we here provide another schematic to show our point (Figure 1). Usually for ridges we have one of these options: Either a ridge on a normal day is stronger than the climatology (Figure 1a), which results in a ridge being represented in the probability plot based on anomalies (Figure 2). Or it is weaker, leading to a trough being shown in this probability plot, despite still being a ridge. The probability plot (Figure 2) supports this hypothesis by showing similar probabilities for ridges and troughs in the locations of climatological ridges. The same can be argued for troughs. However, we emphasize that our results are based on Z1-5, which captures quasi-stationary Rossby wave features that are similar to climatology. Thus, it is logical for ridges to correspond with one of the scenarios depicted in Figure 1.

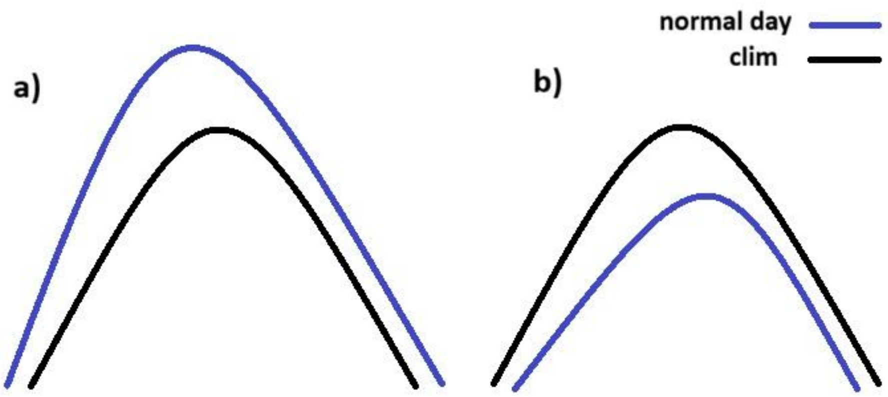

Figure 1 A schematic showing a) a ridge for a normal day and climatology when the normal day ridge is stronger than climatology and b) the same as "a" but when the normal day ridge is weaker than climatology.

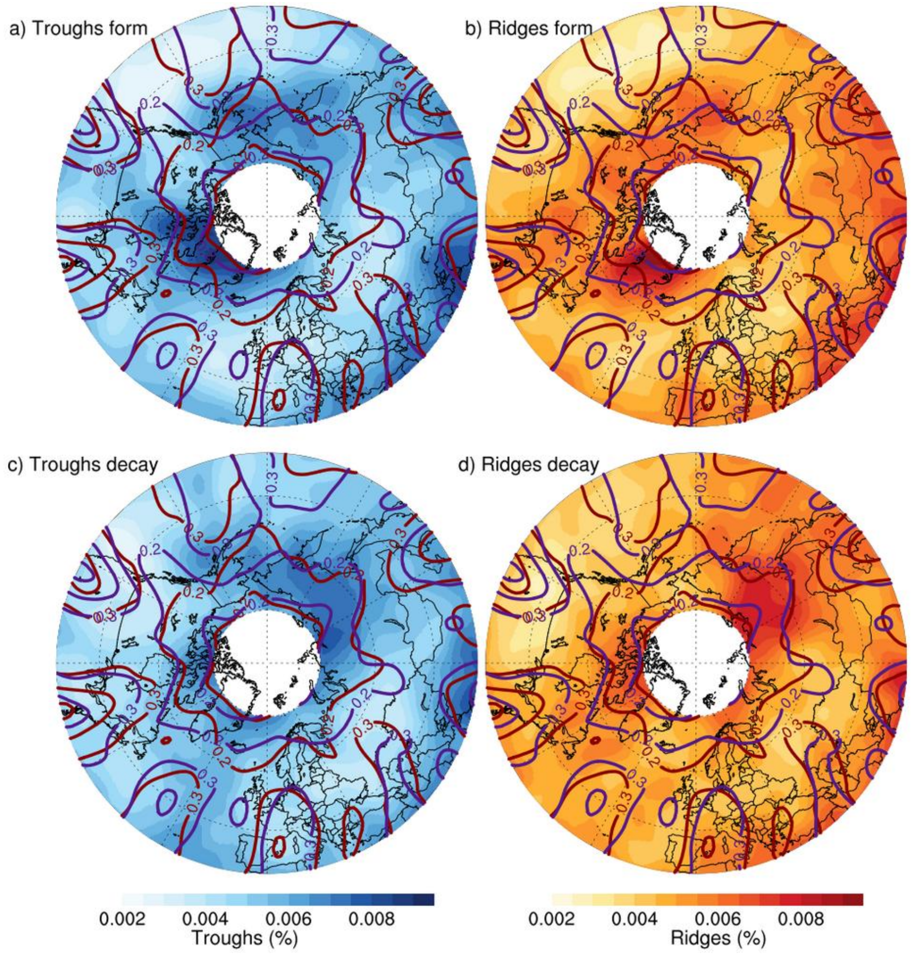

Figure 2 Same as Fig. 4 in the manuscript but using data with removed winter climatology (1981-2020).

The updated ridge/trough climatological frequencies obtained by removing the zonally varying flow look indeed strange, particularly at low latitudes, but this is not because of supposedly "artificial waves" emerging "out of the blue" by removing the climatology. The diagnostic shows values above 0.2 at all latitudes and longitudes except at high latitudes, with local maxima exceeding 0.3 regularly spaced in a wavy pattern. I might have a guess of the reason: have the authors removed the climatological large-scale flow *before* computing the Fourier transform at each latitude and extracting the Z1-5 harmonics (what I would recommend), or have they removed the climatology of the Z1-5 flow *after* having extracted the Z1-5 wavenumbers at each latitude? The example shown in Fig. 2 features very smooth climatology and large-scale flow, with minimal differences: this would make me guess for the second approach.

We followed the reviewer's recommendation by first removing the winter climatology (based on winters from 1981 to 2020) from the gph data. Subsequently, we computed the Fourier transform at each latitude and extracted the Z1-5 harmonics from the climatology-removed gph data. However, note that these operations are linear, and the order of application is therefore not important. This is in line with the reviewer's comment above concerning linearity. In the schematic, we aimed to illustrate both the Z1-5 of climatology and the Z1-5 of normal days to emphasize the unrealistic nature of removing climatology from the data before calculating the Z1-5.

However, there is no need at this stage to go in such a level of detail because this study adopts a different approach. What I would ask is, unless they can prove otherwise, that the authors refrain from making the claim of "artifical ridges and troughs" in the reply document (that is also public and can be quoted by other researchers interested in the topic) as an argument for not removing a zonally varying climatology.

We agree "artificial ridges and troughs" was a somewhat imprecise formulation, which could rather be changed to "ridges and troughs will appear differently in the total field and the anomaly field."

2) I am not completely satisfied with the paragraph contextualizing the results by Schemm et al. (2020). I think that the approach adopted here has the risk to "impose" the presence of ridges and troughs even at low latitudes, where they are not expected to be found (and in this sense Schemm et al. 2020's climatology is more realistic than the one shown here, in my opinion). Rather than being a point of force, the adopted approach might lead to spurious signals at low latitudes, visible in Fig. 3 of the reply document and also in Fig. 4 of the paper, with unrealistically high values of ridge/trough frequencies at latitudes around 30N. I have only realized now that the maxima in trough and ridge frequencies are located at such low latitudes, so much south that their full latitudinal extent cannot be appreciated because the plots are "cut" at 30N. If the authors applied the approach to 10N, they would lso identify ridges and troughs, but what would they represent physically? In conclusion, I would ask the authors to elaborate more upon the differences between their work and Schemm et al. (2020): in the current formulation, it looks like Schemm et al. (2020) simply misses those troughs and ridges located so far south, but are those ridges and troughs actually existing, and if yes what do they represent?

To some extent, we agree with the reviewer's opinion here. Our method can identify subtropical high pressure as Rossby waves, which is why we only plotted our probabilities north of 35° N. However, we believe that Schemm et al.'s (2020) methodology also fails to capture ridges at low latitudes due to local gph flattening. We have revised the paragraph as follows (with new text indicated in bold):

"The probabilities of climatological ridge and trough positions here differ significantly from those in Schemm et al. (2020), mainly due to differing detection methods. Schemm et al. (2020) identify ridges and troughs by comparing nearby grid points, while our method compares values across all longitudes at each latitude. Consequently, their probabilities are higher at high latitudes, where ridges and troughs are more distinct, but lower at low latitudes due to local gph flattening. **Our method, however, still indicates ridges and troughs at subtropical and tropical latitudes, but these could rather be assigned to patterns such as subtropical highs than Rossby waves.** The probabilities estimated by our method represent the likelihood of ridges or troughs occurring at a specific location, rather than over an area defined by exceeding a curvature threshold of the gph field as in Schemm et al. (2020); hence, the probabilities in our study are naturally much lower than those reported by Schemm et al. (2020)."