Reviewer #1

I thank the authors for presenting this revised manuscript. Most of my comments have been sufficiently addressed. The description of the tracking algorithm and the depiction of formation and decay frequencies in Figure 4 are instrumental for assessing the scientific significance of this study.

I realize that not subtracting a zonally varying climatology, in contrast to what is commonly done in Rossby wave packet diagnosis, has important implications for the zonal propagation of disturbances. Figure 4 illustrates how a ridge is likely to decay as it enters a region of climatologically lower geopotential compared to the zonal mean. Although I respect that it lies outside the scope of this manuscript, it would be interesting to see how this affects the wave speed and the meridional extent in a climatological sense.

We thank the reviewer for taking the time to read our manuscript once again and provide constructive feedback.

Regarding Fig. 4, this is meant for providing the climatology of ridges and troughs. Later in the manuscript we discuss the speed of ridges and troughs and the amplitude of the large-scale associated with cold spells and compare these quantities to their climatology (Fig. 8). Hence implicitly, the anomaly of these during cold spells are considered.

Perhaps the reviewer is thinking of calculating anomalies of, e.g., the gph field before calculating the speed of ridges and troughs and amplitude of waves. But that would require a rather different design of the algorithm to estimate these. In addition, it appears most relevant to investigate the behavior of these quantities on the actual waves, at least within the scope of our study.

Despite a clear improvement of the manuscript, I have a few remaining comments.

1. The fully revised introduction is clearly improved. It is however customary and helpful to provide an outlook on the structure of the article. The usage of the MEX index and its relevance for the scope of the paper needs to be better introduced. Furthermore, I would like to suggest additional references for a more comprehensive introduction. The work by Hassanzadeh et al. (2014) on blocking shows once again how Arctic Amplification does not necessarily lead to an increase in amplified quasi-stationary waves. In the context of climate change, we shouldn't forget about the expansion of the tropics and its influence on wave propagation (e.g., Wicker et al., 2025). And for the importance of horizonal advection for cold and hot extremes, I would like to point to Mayer (2025) and references therein.

Finally, I would like to comment on the reference to Geen et al. (2023) in ll. 45 f. In my opinion, the referenced study does not highlight the need for one universal wave metric but points towards the importance of assessing a larger variety of metrics.

We have incorporated your suggestions and added the relevant references. However, a further description of MEX is added to the "Study regions selection" section (see point 2 below), as the MEX index is a minor part of this work, and we therefore do not feel the need to include it in the introduction. Below is the modified section of the introduction reflecting these other changes:

"Increases in extreme event frequency, specifically cold spells, have been linked to Arctic amplification (AA), a phenomenon by which the Arctic warms faster than the mid-latitudes (e.g., Francis and Vavrus, 2012; Cohen et al., 2014; Graversen et al., 2025): the AA is weakening the jet stream, making the large-scale atmospheric waves more wavy (Francis and Vavrus, 2012; Coumou et al., 2015; Stendel et al., 2021); in addition, AA causes Rossby waves to become more stationary, resulting in an increase in the frequency of extreme weather (Francis and Vavrus, 2012). This hypothesis, however, has been questioned by several studies (e.g., Barnes, 2013; Screen and Simmonds, 2013; Hassanzadeh et al., 2014; Blackport et al., 2022, 2024). In addition, a recent study suggests that the poleward expansion of the tropics can reduce the frequency of persistent mid-latitude heat waves by shifting storm tracks and increasing phase speeds (Wicker et al., 2025).

The dispute about the influence of the AA on planetary waves might stem from the sensitivity of waviness assessments to the applied metric (Geen et al., 2023), which emphasizes the importance of evaluating a broader range of wave metrics. However, existing wave amplitude metrics, such as Rossby wave packets (e.g., Fragkoulidis et al., 2018; Röthlisberger et al., 2019), local jet waviness (e.g., Röthlisberger et al., 2016), and hemispheric variability (e.g., Petoukhov et al., 2013; Kornhuber et al., 2019), do not capture the role of the amplitude of each ridge and trough in temperature events. In addition, as far as the authors know, the speed of local ridges and troughs and their role in extreme events have not earlier been quantified. Some studies estimate atmospheric phase propagation by following grid points using lag-correlation in different bandpass-filtered geopotential height maps, tracking the maximum positive correlation center (e.g., Blackmon et al., 1984; Takaya and Nakamura, 2001). Others derive phase-speed spectra for each zonal wavenumber by performing a space–time spectral decomposition of upper-tropospheric winds along latitude circles (e.g., Randel and Held, 1991; Domeisen et al., 2018; Riboldi et al., 2020). A local approach has also been applied to estimate the speed of Rossby wave packets (e.g., Fragkoulidis and Wirth, 2020; Fragkoulidis, 2022). Here, we aim to explore the linkage between cold spells, in particular those that are associated with the advection of cold air from the Arctic, and the amplitude and speed of ridges and troughs in the vicinity of cold spells. Cold spells are particularly of interest in the context of Rossby waves because these cold extremes are argued to be primarily driven by the large-scale advection of cold air from higher latitudes (Bieli et al., 2015; Tuel and Martius, 2024; Mayer, 2025). However, in some regions, especially near the Arctic and Antarctic, diabatic processes are considered to play the dominant role in driving cold extremes (Röthlisberger and Papritz, 2023).

This article is organized as follows: Section 2 describes the study regions, the data used, and the definition of cold spells. Section 3 introduces the newly developed wave metrics. Section 4 presents the application of the wave metric to cold spells, demonstrating its effectiveness. Finally, Section 5 provides the conclusions and discusses the implications of the findings."

2. The top 1% of daily MEX values are presented as a metric to classify cold days (e.g., ll. 78 f.). However, as the mean squared anomaly, extreme hot conditions can equally lead to a high MEX index.

Thank you for bringing this to our attention. We have revised the text to clarify that a high MEX index signifies both cold and warm extremes.

We changed "historical extreme cold days" to "historical extreme 2-meter temperature days" and also added the following text in the "Study regions selection" section.

"The MEX metric may be somewhat biased because it accounts for both warm and cold anomalies. The simultaneous occurrence of cold anomalies in Eurasia and Northwest America, alongside warm anomalies in Greenland and Beringia, might have contributed to a higher MEX value for the selected days (see supplementary Fig. 1). Nevertheless, our selected regions cover all of Europe and the western part of North America, with the only significant area not included in our analysis being the eastern part of North America, which could be addressed in future research."

3. The new Figure 4 with formation and decay frequencies is instructive. However, I find the overlay of trough and ridges values hard to interpret. For example at 70°N 150°E, there seems to be weird feature resulting from masking values below a certain threshold. The caption says that priority is given to the stronger feature. I suggest, presenting one subplot for the formation and decay of troughs and ridges respectively.

Thank you for the suggestion. We update Figure 4 as recommended, and you can see the revised version in the following picture.

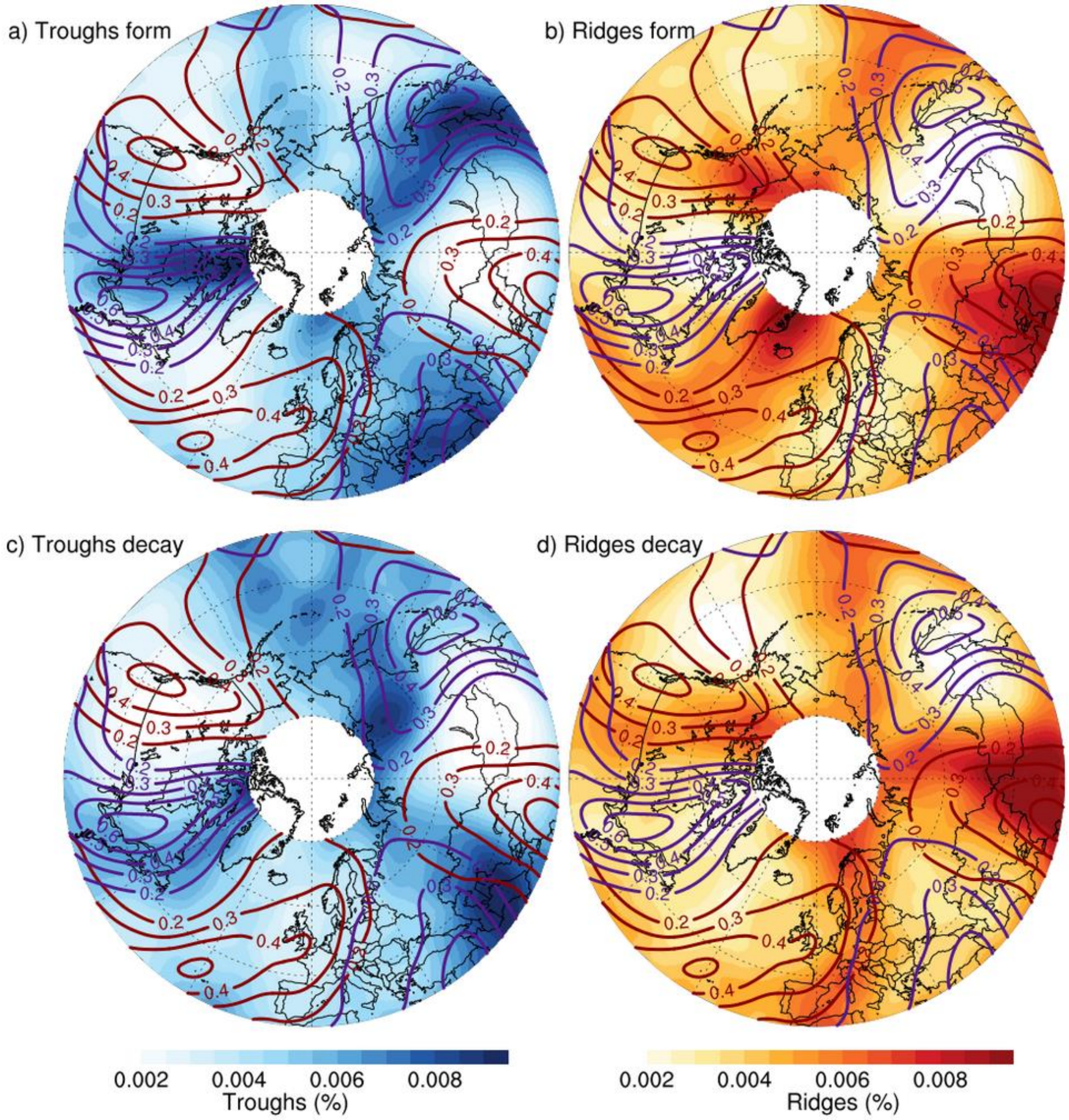


Fig. 4 The probability of ridges and troughs and their formation and decay as a function of location. Shown with contours are the probabilities (in percentage) of 300 hPa ridges (brown) and troughs (purple) during winter climatology as a function of location. The blue shading indicates the climatological probability of a) formation and c) decay of troughs as a function of location. The red shading indicates the climatological probability of b) formation and d) decay of ridges as a function of location. A Gaussian function, using a weighted running average over eight degrees in latitude and over an equal distance in longitude, is used to smooth the probabilities.

4. The Section titles "Methods" and "Results" don't seem appropriate since both Section 2 and Section 3 present results in Figures 1 and 4. Not having introduced the structure of the article in Section 1 makes this confusing.

Thank you for providing insightful feedback. We changed the title of the sections and added the following text to the introduction:

1. Introduction
2. Study regions, data, and cold-spell definition

3. Wave metrics
4. Application of the wave metric to cold spells
5. Conclusions

"This article is organized as follows: Section 2 describes the study regions, the data used, and the definition of cold spells. Section 3 introduces the newly developed wave metrics. Section 4 presents the application of the wave metric to cold spells, demonstrating its effectiveness. Finally, Section 5 provides the conclusions and discusses the implications of the findings."

5. The results presented in Figure 7 are phrased as "retrograding patterns". Given that the composite wave speed is found to be positive (see Figure 8), I wonder whether this term is appropriate.

It has changed to:

Fig. 7 shows the time-development of ridges and troughs before and during cold spells for the composites of regions in North America and Europe.

References

Hassanzadeh, P., Kuang, Z., & Farrell, B. F. (2014). Responses of midlatitude blocks and wave amplitude to changes in the meridional temperature gradient in an idealized dry GCM. Geophysical Research Letters, 41(14), 5223-5232.

Wicker, W., Russo, E., & Domeisen, D. I. (2025). A poleward storm track shift reduces mid-latitude heatwave frequency: insights from an idealized atmospheric model. Weather and Climate Dynamics, 6(3), 965-979.

Mayer, A. (2025). A new global Lagrangian analysis of near-surface temperature extremes. Geophysical Research Letters, 52(19), e2025GL116696.wea.3278, 2018.

Reviewer #2

I would like to start by thanking the authors for bringing substantial modifications, that resulted in a more detailed and easier-to-assess manuscript. Given the large differences with respect to the initial version, I treated that as a novel manuscript and provide below a full review.

We thank the reviewer for taking the time to again carefully look over our paper and provide some insightful comments. However, we feel that a large part of the following comments is based on a misunderstanding. It appears as if the reviewer believes that Rossby waves are stationary and have little or no transient behavior. The reviewer provides little evidence for that view, and previous research as well as our study indeed indicate a transient development of large-scale planetary waves.

From a scientific point of view, I believe that the paper still suffers from the ambiguity in the definition of the waves responsible for the analyzed cold spells. Maybe the problem I see behind this study is that it focuses on cold spells, i.e., extreme events that occur on synoptic time scales, while at the same time using a wavenumber-based approach to separate Rossby waves that has been so far applied to climatological, hemispheric-scale studies (about, e.g., energy transport), and not to individual events.

We disagree with the reviewer here. There are many examples of investigations of Rossby waves beyond "climatological, hemispheric-scale studies." In fact, Carl-Gustaf Rossby showed that planetary waves are moving significantly on a weekly (synoptic) time scale (Rossby et al., 1939), although these waves also have a significant stationary part (Held, 1983; and Fig. 1 in our manuscript). Hoskin and Karoly (1981) argued the triggering of Rossby waves by a low-latitude heat source (e.g., latent heat release over the tropical Pacific) results in the propagation of the longer wavelength polewards and eastwards. Regarding more recent studies, Liu and Li (2025) provided an interesting contribution to the theoretical framework of transient Rossby waves, providing examples focusing on zonal wave 1. Additionally, Screen and Simmonds (2014) showed that amplified planetary waves and temperature extremes have the strongest correlation between 5-14 days.

In our previous response to the reviewer's major comment 3, we have illustrated the transient behavior of planetary waves, which the reviewer found interesting (as noted in major comment 4 in this text). Generally, in our study, we utilized the first five zonal wave numbers (Z1-5) to illustrate the general pattern of atmospheric structure. This can be observed in Fig. 3a and Fig. 1 of this response, where the unfiltered patterns of Z300 and Z1-5 closely resemble one another. We clearly showed that these structures display different patterns in terms of their location, speed, and amplitude compared to their typical behavior.

Defining planetary waves at instantaneous time steps is quite tricky and probably inexact because the concept of planetary wave (especially when defined simply through a wavenumber range) does not bear a clear physical fingerprint and assumes the presence of a "low-frequency" dynamics that is distinct from the "high-frequency" one. Isolating slow dynamics in the background using filtering in time or space is extremely tricky as pointed out by, e.g., Wirth and Polster (2021, https://doi.org/10.1175/JAS-D-20-0292.1). I have the impression that the whole paper suffers of this ambiguity, which is still well present despite the efforts of the authors: the first set of major comments try to target this point.

Another important aspect that could be further improved is to highlight the value of the scientific insights that can be gained from the analysis. A way to do so would be to specify more explicitly the research questions that this piece of work tries to answer: these questions should emerge clearly from the Introduction as unanswered by previous research. Some additional suggestions are listed in the detailed comments.

From a methodological point of view, the authors have clarified in which aspects their method is different from others (although the novelty could be emphasized a bit more, see detailed comments). As a side effect of moving many informations from the supplement to the main text, several questions about the choice of the methodology have arisen. They are related partly to the previous point, as methodological choices reflect the theoretical framework of climatological waves: this is visible, for instance, in the decision of tracking transient ridges only in regions where the climatological, stationary ridges are found. I have several detailed comments about it and a major comment with a suggestion about how the methodology can be revised to make it less arbitrary and solve many issues.

In summary, I see again for this paper the necessity of substantial revisions both from a methodological and a scientific point of view. This would warrant at least a request of major revisions or, if the handling editor sees fit, of rejection with encouragement to resubmit.

Introduction to our response:

It seems there is a misunderstanding regarding our tracking algorithm; the reviewer seems to believe that the tracking of ridges and troughs is confined to their location in climatological waves. However, we track all ridges and troughs at each time step, regardless of their location. Our wave speed metric operates based on the following steps:

1. Smooth the geopolitical data to enhance trackability (focusing on Z1-5).
2. Filter out short-lived ridges and troughs.
3. Track all ridges and troughs at each time step (every 3 hours).
4. Plot the probabilities of the winter climatological positions and the cold spell positions for ridges and troughs.
5. Based on regions with higher probabilities, select longitude bands to calculate the daily speed of ridges and troughs. This calculation uses the summation of all tracked ridges (troughs) within the specified longitudinal band.

Thus, we are tracking all ridges and troughs, not climatological waves (for which ridges and troughs are stationary). We assign names to the ridges and troughs within a specific longitude range.

Hence, we are tracking transient troughs and ridges. These ridges and troughs can represent planetary waves in low latitudes as well as planetary and Rossby wave packets in high latitudes, since for smoothing geopotential height in low latitudes we need to keep long wavelengths (more than 5000 km south of 45° N) and both long and shorter wavelengths at the high latitudes (less than 3000 km north of 70° N). Note that all wave numbers can show transient behavior; as Rossby et al. (1939) stated, “long waves must travel westward and shorter waves eastward.” Some of the terminology in the manuscript may have caused confusion by perhaps indicating that we are tracking stationary ridges and troughs. We thank the reviewer for noticing these ambiguous terminologies in the manuscript, and we fixed them.

Wirth and Polster (2021) cautioned against diagnosing slow dynamics from zonal means and time filtering when eddy amplitudes are large. Our approach does not assume a zonal-mean “background state” nor does it invoke linear wave theory. Using a spatial filter at each time step to smooth the gph field is a kinematic, not a dynamical, separation. Consequently, the conceptual and mathematical issues raised by Wirth and Polster (2021) regarding waveguidability diagnoses are not relevant to our analysis.

Below, we offer a point-by-point response to each individual comment.

Major comments about the ambiguity in the type of waves considered in the Z1-5 range

1) The selected range of waves (in the wavenumber range Z1-5) is at times considered capturing climatological waves, sometimes as low-frequency, "planetary" waves, and sometimes as transient troughs and ridges driven by, e.g., Rossby wave packets. The focus on climatological waves is at the basis of the methodology, because trough and ridge tracking are performed only in precise ranges of longitude

mapping into the known stationary ridges and troughs of the northern hemosphere, due to orography or land/sea contrast (e.g., the 180W-90W region upstream of the Rocky Mountains mapping). This explains why it is more likely to identify troughs and ridges in given longitudinal sectors (as depicted in Fig. 4), sectors denoted as the "original locations" of ridges and troughs in the reply document (see the second sentence of the reply to major comment 3). The Z1-5 waves, on the other hand, are treated as being part of transient Rossby wave packets when the results of this work are compared with the ones of Fragkoulidis et al., who specifically focused on transient Rossby waves (e.g., at lines 317-322). The presence of "planetary" waves, a third type distinct from the other two, is assumed implicitely in lines 120-124, where it is stated that Z1-5 waves at high latitudes might have wavelengths that are "too short for their characterization as planetary waves".

We used the term "original locations" in our earlier reply to indicate that ridges and troughs obtained from the Z1-5 waves have the same location as the ridges and troughs in the unfiltered geopotential height (as can be seen in the Fig. 3a example), not where they are found in the climatological mean. We sincerely apologize for misleading the reviewer. Please also see the introduction to our response.

We revised lines 120-124 from “In order to investigate the role of large-scale waves, a Fourier decomposition of gph is employed to separate the field into distinct harmonic waves based on the zonal wave number. The combination of the first five waves is regarded as an estimation of large-scale waves (Z1–5; solid line in Fig. 3a); these are planetary or Rossby waves (except possibly at high latitudes, where it may be argued that their wave length are too short for the categorization as planetary waves).” to:

“To enhance the trackability of atmospheric waves, a zonal Fourier decomposition of the gph field is employed to separate into distinct harmonic waves based on the zonal wave number. The combination of the first five waves captures the major ridges and troughs of atmospheric waves (Z1–5; see solid line in Fig. 3a for an example). These ridges and troughs exhibit transient behavior (Fig. 3b-c); they represent planetary waves in low latitudes and both planetary and synoptic-scale waves in high latitudes.”

2) While distinguishing those types of waves might not be crucial from the perspective of the adopted methodology (which might be seen as "agnostic" about the nature of the waves found between n=1-5), the distinction is on the other hand very important to determine the scientific insights that can be gained from the analysis. For instance, interpreting North American cold spells as the result of the amplification of the climatological ridge upstream of the Rockies might be misleading. This is because the orographic wave dynamics explaining the presence of such a ridge is fundamentally different from the one explaining the genesis, amplification and polar excursion of the midlatitude upper-level ridges occurring downstream of extratropical cyclones (and upstream of cold spells). I see an example of such a (problematic) reasoning at lines 323-324, when it is concluded that "shifts in the location of waves relative to climatology can result in a cold air advection to the cold region": this formulation is quite ambiguous, because it might sound like the displacement of the orographically forced wave from its supposed "original location" is causing the cold spell. What would drive such a displacement? Probably not orographic wave dynamics, but rather the arrival of an upstream Rossby wave train leading to (thanks to constructive interference between different waves) an elongation and potentially a "shift" of the pre-existing orographic ridge: such a shift, however, would be fully due to extratropical Rossby wave dynamics.

Ridges and troughs obtained from the combination of the first five zonal wavenumbers simply represent the general pattern of the atmosphere that is trackable. Please see the response to major comment 1 and the introduction to our response. For some of our interpretations, the reviewer has a point, and we were not careful with our terminology. We have revised the manuscript to clarify our terminology.

We add this in the beginning of the “Trough and ridge amplitude and speed” section: “The following refers to changes in ridges or troughs (such as amplification or slowing down), indicating significant alterations in their behavior during cold spells compared to the climatological average behavior of these features within their respective longitude bands.”

Lines 323-324 are changed to: “Additionally, we discuss the importance of wave location in cold spell development, as different configurations of ridges and troughs, compared to their typical winter climatological positions, can result in a cold air advection from the north to the cold-spell region.”

There are several more of these misleading terminologies that we changed in the new version of the manuscript.

The factors influencing the positioning of ridges and troughs during cold spells (the reviewer mentions upstream Rossby wave trains) are beyond the scope of our study.

3) In this regard I am also not fully sastisfied by the reply of the authors to my Major point 3. First of all, the references brought by the authors to support their statement that "planetary waves (also at mid-latitudes) can exhibit transient behavior" are also based on the same approach criticized here. Baggett and Lee (2015) define their long waves by filtering for wavenumbers between 1 and 3, and therefore using an analogous approach as the present study with the same problematic aspects (incidentally, they note that those waves feature "small propagation speeds"). Graversen and Burtu (2016) also define planetary waves using a wavenumber threshold (between 1 and 5, as in the current study). Incidentally, the conclusions that they reach that planetary waves are the main contributors to energy transport and high-latitude warming" might be fundamentally affected by the choice of defining planetary waves using a wavenumber-based threshold: at high latitdues, the length scales associated with transient baroclinic instability (which indeed is associated with net poleward heat flux) mostly project into that "planetary" range. Blackmon et al. 1994, on the other hand, state in their conclusions that "intermediate and long time scale fluctuations [...] feature little to no phase propagation", with no indication of transient behaviour of planetary waves at the day-to-day scale considered here.

It seems that we are discussing two entirely different phenomena. One concerns the transient behavior of all zonal wavenumbers, irrespective of their wavelength (Rossby et al. 1939; please see the introduction of our response). The other pertains to the general seasonal patterns of waves influenced by topography and land/sea contrasts (obtained from a low-frequency time filter, similar to Blackmon et al. 1984). We should have been more precise in addressing your earlier major point 3.

Also, not that Graversen and Burtu (2016), as was mentioned by the reviewer, in fact showed that wave number 1 at 70 N provided the largest response on Arctic temperatures from transient behavior (their Fig. 6). Hence, this impact cannot be mainly due to the shorter wave types, as indicated by the reviewer.

Again, we feel that the reviewer seems to think that Rossby waves only have a stationary part, with which we continue to disagree.

4) The authors also perform an interesting analysis by restricting themselves to wavelenghts greater than 6000km to determine planetary waves. However, despite the limitation found especially at high latitudes (where three of the nine study regions are found), no modification has been brought to the manuscript to inform the reader.

Our analysis, which focused on combinations of wavenumbers with wavelengths greater than 6000 km, was intended to demonstrate that all wavenumbers can exhibit transient behavior. However, the ridges and troughs derived from these waves at high latitudes do not align with the locations of the unfiltered geopotential height (this method disregards important ridges and troughs that are intended to be tracked in our study). As a result, this analysis lacks scientific relevance for our study. Please also see the responses to other major comments and the introduction to our response.

Major comment about the methodology

I have a suggestion that would address most methodological comments. Why not performing again the tracking of troughs and ridges across the whole hemisphere, without being bound to the location of climatological waves? This would allow to focus on the characteristics of transient waves (and even moving "planetary" waves, in case they existed) during cold spells, simplifying the interpretation and solving some methodological issues such as the focus on six fixed areas for ridges and troughs. In case the approach needs such fixed longitude intervals, an alternative approach would be to define, for each region affected by cold spell, two fixed windows located 60° to the east and west of the central latitude and perform the tracking for each region separately. The interpretation of the result would then mention "ridges" and "troughs" in general around cold spells, and in my opinion align more closely with the proposed objective of targeting separately the properties of ridges and troughs around cold spells.

We tracked all ridges and troughs across the entire hemisphere and gave them a name for specific longitude sectors. Please see the introduction to our response. Our objective is to analyze ridges and troughs near the locations of cold spells; therefore, averaging over all longitudes contradicts our objective. As illustrated in Fig. 6, using a fixed longitude window centered on each region is not ideal, as ridges and troughs are more frequent in certain longitudes than others. Consequently, limiting the analysis to a 60° window around the cold spell location often leads to missing ridges or troughs on many days, making this approach flawed.

Specific comments

Lines 38-43: given that several studies have addressed the shortcomings of the Francis and Vavrus (2012) hypothesis, wouldn't it make sense to present this paragraph the other way around, giving more emphasis on recent work rather than to the initial, and in many aspects disproven hypothesis?

We believe the paragraph is appropriately structured as it stands. Presenting the hypothesis first creates a logical and organized narrative, followed by a discussion of its shortcomings. The claim that the hypothesis is "in many aspects disproven" is not entirely accurate and in itself deputed, as recent research (Graversen et al., 2025; also added to the manuscript for reference) linked increased weather persistence to Arctic amplification.

Lines 55-57: are there studies that claims otherwise? If yes, it would be great to point them out explicitly here so that the contribution of this study to the scientific debate becomes clear.

Thanks for your feedback; we revised this section:

Cold spells are particularly of interest in the context of Rossby waves because, in many regions, these cold extremes are argued to be primarily driven by the large-scale advection of cold air from higher latitudes (Bieli et al., 2015; Tuel and Martius, 2024; Mayer, 2025). However, in some regions, especially near the Arctic and Antarctic, diabatic processes are considered to play the dominant role in driving cold extremes (Röthlisberger and Papritz, 2023).

Line 78: the MEX corresponds to the squared standardized temperature anomaly and, thus, measures the overall temperature variability, regardless of the sign of the temperature anomaly. Thus, the top 1% would result in days with co-occurring cold AND warm extremes in the considered latitudinal band. Why not using standardized anomalies, then?

We used this metric solely to identify regions for our study, and our selected regions encompass all of Europe and the western part of North America, which largely includes highly populated areas in the midlatitudes. We did not include any regions from the eastern part of North America, but future research can address this gap. We have added the following text to the manuscript:

"The MEX metric may be biased because it accounts for both warm and cold anomalies. The simultaneous occurrence of cold anomalies in Eurasia and Northwest America, alongside warm anomalies in Greenland and Beringia, might have contributed to a higher MEX value for the selected days (see supplementary Fig. 1). Nevertheless, our selected regions cover all of Europe and the western part of North America, with the only significant area not included in our analysis being the eastern part of North America, which could be addressed in future research."

Line 81: follow-up on the previos comment: the fact that in Fig. S1 the chosen approach based on MEX results in composite patterns with low temperatures over the continents is presented by the authors as something obvious. Instead, hidden in those composites might also be days with specular temperature patterns, i.e., warm extremes over the continents (again, because the non-standardized MEX considers squared temperature anomalies regardless of the sign), potentially leading to confusing signals in the interpretation of the results. Have the authors excluded this potential problem?

It is possible some days have positive temperature anomalies over the continents, but as the composite shows, negative anomalies must have a significantly higher percentage than positive anomalies over the continent. Nonetheless, we are only using this metric to locate regions for our study, which encompasses all of Europe and half of North America. So, it is not necessary for us to go into detailed results.

Line 82: what is the advantage of using this two-step approach (through the MEX) in the definition of cold spells and of the study regions?

The two-step approach was done in response to the reviewer's earlier request (line 66) by adding a figure showing the "cold anomaly contribution to the historical extreme cold days."

Lines 104-111: The rationale behind the choice of the regions is still puzzling to me. For instance, why is the region of the North American Great Lakes not emerging from your diagnostic, even though it is also a region anecdotically affected by cold spells? More in general, from the point of view of standardized anomalies sense, every longitude should be equally affected by cold spells: why only some end up

For the first part of your question, please see our response to your previous comment (Line 78). Our research indicates that the location of ridges and troughs is crucial for the potential to advect cold air into a region. Due to the influence of topography and land-sea contrasts, ridges and troughs tend to occur more frequently in certain longitude ranges than in others. Therefore, in a hypothetical world without mountains and land-sea contrasts, all longitudes would be equally affected by cold spells.

Line 95-96: this is an interesting statement, could you provide some more details on it? Or could this signal be due to cancellation between cold and warm spells during winter?

Since we are using a composite analysis of all historical extreme temperature days, we cannot provide additional details on this matter. However, we believe that cold air may become advected from the Siberian High to this region. Further research is necessary to understand the causes of cold spells in this area, which falls outside the scope of our study.

Lines 116-117: In which parts of winter? And more i general, why do you think this is the case? Again, if the standardization is done properly, there should not be a preference for specific dates.

For cold spells, we utilized only the CWMId index. Therefore, the non-standardized MEX results do not impact the identification of cold spell days. Our findings indicate that cold spells are more prevalent in certain regions during specific months, which led us to assign greater weight to these months. The details regarding which regions and months experience more cold spells fall outside the scope of our study and are purely statistical. Nonetheless, using the entire winter season for choosing random days does not alter the observed significant pattern.

Line 123: the correction brought as "Planetary OR Rossby waves" is still ambiguous: which type of planetary-scale waves would not follow Rossby wave dynamics?

Please see our answer for your major comment 1.

Lines 137-138: What if the effect of such a condition on the results? For instance, what happens if the condition is not fulfilled for a single time step? Are there many ridges and troughs that only last a few time steps? To check if this happens often, could you compute the average lifetime of the tracked troughs and ridges? Would the results change if, say, troughs and ridges with track duration lower than 24h were to be excluded?

The probability of decay and formation of ridges and troughs is very small (see Fig. 4). For most of the time steps, we do not have any data. Therefore, it is not possible to study their significant changes during cold spells. We can only show their climatological positions, which are illustrated in Fig. 4.

We attempted to eliminate short-lived ridges and troughs by focusing our tracking solely on strong ridges and troughs. Our analysis revealed that the presence or absence of these features does not significantly affect the results. This information has already been detailed in the manuscript, in lines 125 to 133.

Lines 139-140: This description sounds a bit "textbook" and not convincing. Phase speed corresponds indeed to the speed of individual waves and troughs, which emerge from the local combination of different wavenumbers (only in a mathematical sense, of course: the decomposition along a Fourier basis of circumglobal waves does not necessarily have physical meaning --see the example of a Rossby wave packet initiated locally by baroclinic instability, e.g., Teubler and Riemer 2016, https://doi.org/10.1175/JAS-D-15-0162.1). So, probably what we are looking at is quite similar to the phase speed: would the authors agree?

We thank the reviewer for the helpful clarification. Since previous literature has referred to this as "phase speed," we will adopt the same terminology. Consequently, we removed lines 139-143.

Lines 150-151: although I appreciate the clarification provided by the authors in the revision, it is still not clear to me how those wave features can do more than simply oscillate around their climatological positions dictated by, for example, large-scale orography (as it is the case of the WNA ridge).

Our terminology created confusion regarding our purpose. Please see the responses to your major comments and the introduction to our response. We revised lines 145-146 from “Most often three major ridges and three major troughs can be seen throughout winters (Fig. 4 and supplementary Fig. S4).” to: “Throughout winter, ridges and troughs tend to appear with greater frequency in certain areas compared to others (Fig. 4 and supplementary Fig. S4). This study focuses on three primary locations for ridges and three for troughs, assigning specific names to the ridges and troughs that occur in these regions.” This change is intended to eliminate confusion.

Line 151: A follow-up to the previos comment: unless the tracking is rather done on the portion of transient, high-wavenumber waves in the range Z1-5: then, why limiting to specific longitudinal sectors?.

We do not limit our tracking to specific longitudinal sectors. Please see our answer for your major comments and the introduction to our response.

Lines 152-154: So, what happens once a trough in the ENA sector moves east of 30°W and then re-enters at 0°E the EMed sector? It is not the same trough anymore? Does tracking stop?

As we noted in lines 135-136, we are tracking troughs within a 12-degree range. We are also operating at a high time resolution of 3 hours, ensuring that their movement between two time steps remains minimal to avoid errors. Additionally, it is important to clarify that we are not tracking ridges and troughs in a specific sector; rather, we are tracking all of them on a 3-hourly basis. Please see our response to your major comments and the introduction to our response.

Line 157: this sentence is not so clear to me, does it mean that ridges and troughs form at high latitudes, then "persist longer" at mid-latitudes and move again towards high latitudes to decay? Has this behaviour been documented?

We did not make such claims, as we did not conduct a study on them. In line 135, we specifically mentioned that we are applying our tracking method at each latitude; therefore, the reader should not expect any connections between different latitudes from our text. Based on the observations in Fig. 4, we argue that the WNA ridge, NAO ridge, and ENA trough in the midlatitudes show less decay and formation compared to those in high latitudes, suggesting that they persist longer in the midlatitudes. We believe this sentence is clear and does not require any modifications.

Lines 157-158: How is the EMed trough related to orography? Which orographical feature are the authors referring to?

Based on the location of the EMed trough (Fig. 4), various orographical features—including the Alps, Caucasus Mountains, Carpathians, Zagros Mountains, Taurus Mountains, Armenian Highlands, and Pontic Mountains—can influence it. While we cannot name all of these features in the manuscript, it is widely recognized that numerous mountain ranges exist in the EMed region.

Lines 158-159: This is also very interesting, as the genesis and decay rates are very low with respect to the climatological frequencies (I guess the authors refer to an occurrence frequency of 0.007, so to a 0.7% genesis rate per grid point --please double-check and adjust the legend of Fig. 4 accordingly). This lends further support to the hypothesis that (at least at mid-to-low latitudes) what is being actually tracked are the climatological, quasi-stationary features that mostly oscillate around their climatological positions, rather than propagate.

The numbers are correct, and it is 0.007%.

At most time steps, there are three ridges and troughs. Since we are utilizing the ERA5 data with a horizontal resolution of 0.25 degrees, the probability of a ridge occurring at one time step and one longitude is approximately (3/1440 (number of grid points in longitude))*100, which is about 0.2%.

Ridges and troughs typically last several days. For instance, the probability of a decay of a ridge occurring would be calculated as (1/(1440*8 (time steps in one day)*2 (assuming a ridge lasts for 2 days))*100), resulting in approximately 0.0045%. Additionally, it's important to note that the likelihood of ridges and troughs occurring varies by location. Therefore, a probability value around 0.007 is reasonable.

For the final part of your comments, please see the responses to your major comments.

Line 164: is such a short life time over North Pacific related to transient storm-track activity, or to the rather zonal flow configuration indicating a reduced activity of "planetary-scale" waves thereby? If yes, it would be good to discuss why that region is "special".

The region likely exhibits small amplitudes and unstable ridges and troughs due to the absence of significant orographic features such as the Tibetan Plateau or the Rocky Mountains. However, we cannot ascertain the specific causes, as this falls beyond the scope of our study.

Fig. 4: Why those sharp edges at longitudes 60°E, 0°E, and 180°E in Fig. 4a, and 30°E in Fig. 4b? The transition between climatological ridge and trough probability is much smoother and not bound by longitudinal sectors, on the other hand, see for instance over North America (as one would expect). Why do such patterns emerge?

We attempted to plot both ridges and troughs decay (formation) on the same graph, ultimately deciding to highlight the stronger feature, as mentioned in the caption. This choice resulted in sharp edges in the visualization. To address this issue, we will create two separate plots to eliminate the sharp edges. Additionally, the probability of winter climatological positions for ridges and troughs overlaps in certain regions; for example, both features have high probability over the southwest US (Fig. 4). However, since we represented this data using contours, it did not create any issues.

Lines 179-180: does this still hold for regions at low latitudes (35N-45N)?

Yes, it holds; otherwise, we would have addressed it in the manuscript.

Line 188 and following: Please clarify in this sub-section which part corresponds to the Cattiaux et al. (2016) approach and which parts are the improvements/changes suggested by the authors, so that can be emphasized.

In lines 165 to 179, we have already clearly explained the Cattiaux et al. (2016) approach along with our attempt to improve it. Furthermore, lines 182 to 187 explicitly state why even the improved method fails to capture the detailed behavior of ridge and trough amplitudes. Thus, the reader should be able to distinguish between our attempt to enhance the original approach and our novel methodology. This distinction is sufficiently clear in the current manuscript, and no further clarification or changes are necessary in this subsection.

Line 200: which hysoipse? Please give it a name or use some symbols consistently across the article and the schematics, otherwise it is at times very difficult to follow the explanation.

We are not discussing a specific isohypse. It is important for readers to carefully read the "Trough and ridge amplitude" section to understand what we mean by "isohypse" in line 200, particularly in the paragraph preceding this line (lines 188-198).

Line 203: Does this mean that the threshold now is 90 and not 180 degrees? Is this an improvement with respect to Cattiaux et al. (2016)? If yes, please specify it (see previous comments).

The text clearly states that the threshold is 90 degrees, as indicated in lines 202-203, as well as in lines 190 and 194. We have never mentioned 180 degrees in our text. For clarification regarding the similarities to Cattiaux et al. (2016), please refer to our response to your previous comments.

Line 210-211: From reading the paragraph at lines 182-187 I thought the chosen approach would have circumvented this problem. How are cut-offs treated in the novel formulation?

In lines 210-211, we compare our developed metric, which is based on calculating the amplitude of each ridge and trough, with the modified version of Cattiaux et al. (2016) known as PIE. In the PIE version, the issue with cut-offs persists.

Our objective is to calculate the waviness at each latitude separately. However, using a metric such as Cattiaux et al. (2016) is not feasible, as the cut-offs influence the waviness even at considerable distances from their original positions. As depicted in Fig. 5a, the cut-off low generated a significant amplitude when we calculated the amplitude at high latitudes. In Fig. 5b it is evident that the cut-off is centered in the trough location, and its amplitude contributes to the overall amplitude of the trough.

Line 214: in their reply to Major point 3 the authors said that they "avoided the terminology of planetary waves" but they are still mentioned here. Please reframe to avoid confusion by the reader.

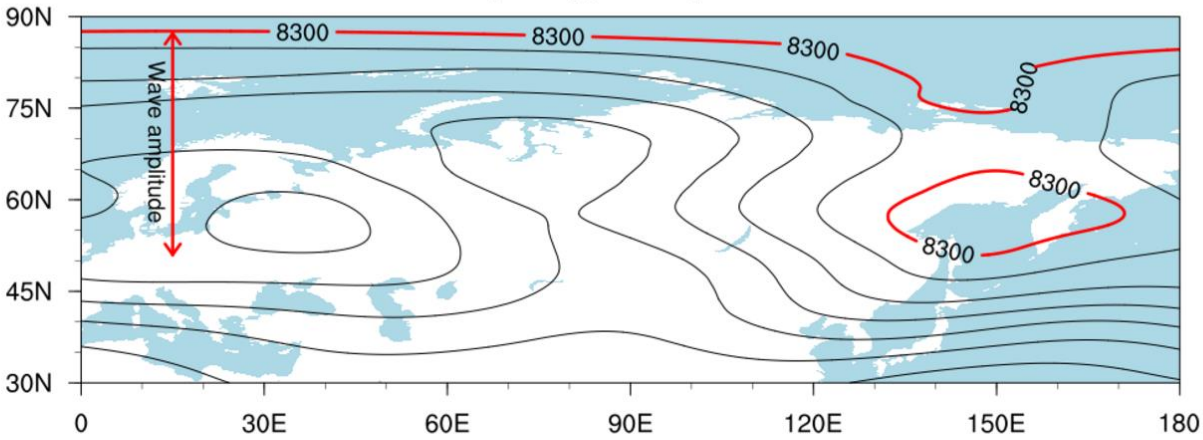
Done

Lines 215-216: this is not clear because MEX conflates together cold and warm spells.

We changed it to “historical extreme 2-meter temperature days”

Fig. 5: Would it be possible to use the same example in Fig. 5 and Fig. 1 to improve the clarity of the explanation? Furthermore, would it be possible to compare the geopotential pattern shown here to the unfiltered pattern of Z300?

We assume you are referring to Fig. 3 and Fig. 5, as Fig. 1 presents the composite over all extreme days, while Fig. 5 displays only a single time step. If this is correct, we have specific reasons for presenting two different time steps. In Fig. 3, we chose a case with small-amplitude ridges and troughs over the Pacific Ocean to illustrate to the reader the types of ridges and troughs that will be disregarded during tracking. For Fig. 5, we required an example that included a cut-off to emphasize its impact on wave amplitude at each latitude. The unfiltered patterns of Z300 and Z1-5 nearly exhibit the same pattern, as noted in the manuscript. You can compare the subsequent figure with Fig. 5, which shows the same date but for the unfiltered Z300.


a) Isohypse amplitude

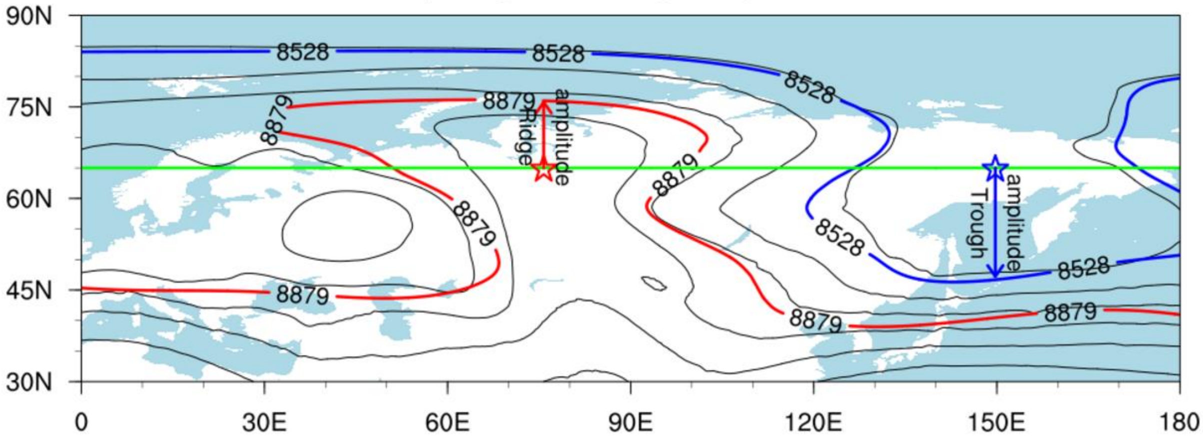

b) Ridge and trough amplitudes

Figure 1 Same as Fig. 5 in the manuscript, but in b) the unfiltered Z300 is plotted.

Line 220-221: here the authors are describing the shift of a climatological feature to the West. Which process is causing it, if we are not talking about Rossby wave packets?

The terminology used in this section was incorrect. They do not shift; rather, they are positioned differently during cold spells compared to their corresponding climatological centers. We revised the text as follows:

“During cold spells, the WNA ridge and the ENA trough are located on the west of their corresponding climatological positions (Fig. 6a, b, and c). This repositioning is more pronounced for R02 and R03 than for R01. The NAO ridge extends from northwest Africa to northwest Europe, specifically exhibiting an eastward relocation at lower latitudes.”

The factors influencing the positioning of ridges and troughs during cold spells are beyond the scope of our study.

Lines 216-235: How can the authors be sure that what is being diagnosed here is a shift of the "planetary" waves and not a projection of synoptic variability on those scales, especially at higher latitudes?

In this section we were not careful with our terminology. Since the gph is going through some changes during cold spells, the ridges and troughs will locate differently compared to normal days. We revised the text to make it clearer.

“To investigate the impact of atmospheric waves on the formation of winter cold spells, nine regions with a high likelihood of cold spells are selected (Fig. 2). These regions are chosen based on their significant contribution to the historical extreme 2-meter temperature days on a hemispheric scale (see methodology section). During cold spells over North America (R01, R02, and R03), large-scale gph anomaly patterns show an anticyclonic gph anomaly over the North Pacific and a cyclonic gph anomaly over North America (supplementary Fig. S3a, b, and c), similar to the findings of other studies (e.g., Xie et al., 2017). During cold spells, the WNA ridge and the ENA trough are located to the west of their corresponding climatological positions (Fig. 6a, b, and c). This repositioning is more pronounced for R02 and R03 than for R01. The NAO ridge extends from northwest Africa to northwest Europe, specifically exhibiting an eastward relocation at lower latitudes. For cold spells over western regions of Europe (R04, R05, and R06), the gph anomaly at 500 hPa shows a positive anomaly located north of a negative anomaly (supplementary Fig. S3d, e, and f). This anomaly resembles a high-over-low North Atlantic blocking situation, which is similar to the findings of other studies (e.g., Trigo et al., 2004; Buehler et al., 2011; Pfahl, 2014). For R04 and R05, at high latitudes, the locations of the NAO ridge and EMed trough tend to be to the west of their respective climatological centers (Fig. 6d and e). During cold spells over Eastern Europe (R08), the EMed trough and the TP ridge at high latitudes are located to the east of their respective climatological locations (Fig. 6h). The cold spell over Northern Siberia (R09) primarily aligns with a deep trough downstream over northwest Asia, as depicted in the supplementary Fig. S3i and Fig. 6i. Despite the high latitude of this region, it is sufficiently far from the pole to be affected by cold advection from higher latitudes during cold spells (Tuel and Martius, 2024). For R09 (Fig. 6i), ridges form over the western flank of the Ural Mountains, and the EAsia trough at high latitudes is located in the west of its climatological location. These findings indicate that a distinct positioning pattern of ridges and troughs on a hemispheric scale occurs during cold spells over each region. These patterns can also be observed at 500 hPa (not shown) and 850 hPa (not shown), indicating that during cold spells the entire troposphere is located in a rather barotropic manner. These findings suggest an interaction between atmospheric dynamics and surface temperatures.”

Lines 232-234: which processes governs those barotropic shifts? Could it be that what is being diagnosed here is atmospheric blocking, that features an equivalent barotropic structure and is associated with cold spells and other types of extreme weather during winter?

Yes, that is likely the case. However, our study did not specifically investigate the presence of atmospheric blocking during cold spells, so we cannot include this information in the text. Our discussion is limited to the observations we made; however, readers may infer the potential causes and explore the locations of ridges and troughs during blocking episodes.

Lines 234-235: is this formulation a convoluted way to mention the process of "advection", or is there more to it?

We did not directly calculate advection; instead, we analyzed the changing positions of the ridge and trough, along with the MCI index. The configuration of these features suggests an increased likelihood of cold air being advected to the location of the cold spell. However, without direct calculation, we cannot definitively conclude that advection will occur.

Lines 259-260: It is very difficult to portray how such a propagation of the ridge at low latitudes is supposed to happen... could the authors provide for review a case study, among the chosen cold spells, exemplifying this behaviour?

Our objective is to examine the general behavior of upstream ridges and downstream troughs in the formation of cold spells. This aspect is detailed for the region and requires a separate study, which falls outside the scope of our current research. We have already noted in line 261 that “The underlying reasons for this pattern can be explored in future research.”

Caption of Fig. 6: do the authors mean 0.2, 0.2% or 20%?

At most time steps, there are three ridges and troughs. Since we are utilizing the ERA5 data with a horizontal resolution of 0.25 degrees, the probability of a ridge occurring at one time step and one longitude is approximately (3/1440 (number of grid points in longitude))*100, which is about 0.2%.

Line 270-273: I think the motivation provided here by the authors is a bit weak. By which process could a surface cold spell could cause the amplification and slowdown of an upper-level Rossby wave? The opposite chain of processes, on the other hand, is much more obvious through the process of cold air advection. Maybe what the analysis contributes to is to evaluate separately the different contribution of ridges and troughs? Please explain or consider reformulating for clarity.

Preece et al. (2023) showed that dry summer soil moisture in Northeast America can induce a stationary Rossby wave, leading to amplification of a ridge over Greenland. Therefore, the linkage between the upper-level Rossby wave and surface weather is two-way. We changed this sentence to:

“In the previous section, it was shown that cold spells are associated with higher-amplitude and slowing ridges and troughs in the vicinity of the cold spells. This raises the question of the timing of these changes in relation to the occurrence of cold spells.”

Line 280-281: Has something like that already been discussed before in the context of cold spells? Otherwise this might sound a bit hand-wavy...

Thank you for your comment. As far as we are aware, no other study has computed phase speed at various levels within the context of cold spells. Consequently, we must depend on our observation (Fig. 9), which logically supports our conclusion.

Lines 283-284: This theoretical connection seems a bit hand-wavy, as it cannot be tested. Why the need of disturbing finite-amplitude wave-activity flux theory, when one can rely on simpler explanations such as the PV-view of baroclinic instability as in Hoskins et al. (1986) that also involves a deceleration of the upper-level wave pattern?

We add the PV-view of the baroclinic instability explanation to the text:

“Both amplified ridge and trough are also encountered prior to the cold spell, but the speed reduction precedes the amplitude increase (Fig. 9a and b). This could potentially be attributed to the breaking of waves associated with exceeding the wave for the wave activity flux (Nakamura and Huang, 2018). In addition, this pattern can also be explained by the poleward excursion of low-potential vorticity subtropical air ahead of a slowly moving elongated trough (Hoskins et al., 1985).”

Lines 285-286: I am still not convinced about the novelty of this statement and encourage the authors to be more precise: if the added value of the approach is to separate the contribution of ridges and troughs, then make this explicit here rather than generally talking about "waves".

We explicitly mentioned “the upstream ridge and the downstream trough” at the beginning of the sentence. Therefore, the reader knows that the “local upper-atmospheric waves” at the end of the sentence refer to “the upstream ridge and the downstream trough.” We do not see any reason to change our sentence.

Lines 290-295: I would be very cautious with such statements about the 850hPa propagation, because low-level flow evolution might be substantially affected by orography and many of the study regions involve large-scale orography (e.g., the first three regions are all very close to the Rocky Mountains) that can divert the low-level flow and locally alter the propagation of waves. How can the authors be sure that these effects are not due to orographical features? Could they separate regions with orography reaching 850hPa from the others and re-do the analysis separately?

Thank you for your valuable comment regarding the influence of orography on the 850 hPa propagation. We conducted additional tests by separating regions with orography reaching 850 hPa from those without. The results did not support the original conclusions presented in this paragraph. Therefore, to maintain the accuracy and integrity of the analysis, we have decided to remove this paragraph from the manuscript.

Line 293: It might be inappropriate to use "somewhat baroclinic" just by judging from a 1-day lag. Also, what would be the physical meaning and the implications of such a difference in wave character between upper and lower levels?

It is removed from the manuscript.

Lines 296-310: these two paragraphs are not organized and overly speculative, with little references to previous research to back up the hypotheses. Please consider rewrite and delete parts that are too speculative (e.g., the connection with stratospheric activity).

The paragraph including the discussion regarding “the connection with stratospheric activity” has been removed from the text.

In the second paragraph, we primarily describe our observations based on the results obtained. To the best of our knowledge, no existing research addresses this specific observation, so we are unable to provide any citations. At the end of this paragraph, we state that “This indicates that the ridge at lower latitudes tends to move zonally, which may weaken the mixing of warm tropical air with colder midlatitude air long in advance of the cold spell formation.” This conclusion follows the fundamental logic of heat and cold exchange through waves and does not require any citation.

Lines 315-316: the authors should pinpoint in the Introduction previous studies that raise such a question about the direction of causality between these two, so that the novelty of this work can be easily appreciated.

The novelty of our work lies in demonstrating the contributions of ridges and troughs individually, as thoroughly documented in the Introduction.

Line 319: cold spells in Europe (missing "in")

Done

Lines 323-324: the sentence "shifting the location of waves relative to climatology can result in a cold air advection to the cold region." suggests that the waves considered here are the "climatological" waves, stationary waves due to orography, land/sea contrast, storm track activity. Please reframe also considering the major comments above.

Lines 323-324 are changed to:

“Additionally, we discuss the importance of wave location in cold spell development, as different configurations of ridges and troughs, compared to their typical winter climatological positions, can result in a cold air advection to the cold region.”

Please also see the responses to your major comments.

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