Dear Reviewer:

Thank you sincerely for your careful review and insightful comments on my manuscript. Your valuable feedback has significantly helped improve the quality of the work. I have carefully addressed all your concerns and revised the manuscript accordingly. Please find below the detailed responses to your comments. I believe these revisions have substantially improved the clarity, precision, and scientific rigor of the manuscript.

Once again, I deeply appreciate your constructive feedback and the time you have invested in reviewing our work. I hope the revised manuscript now meets the journal's standards and look forward to your further guidance.

Sincerely,

The author: Yaokun Li

After going over the revised paper, as well as the response, I better understand the main points of the paper. The most practical novelty for WCD readers, in my mind, is the detailed "dissection" of the range of possible wave geometry configurations that a barotropic jet can have, in the wavenumber-phase speed-jet parameter space, and the idea that this is significant for the spatial variations of wave energy. In particular, I found it interesting that the waveguide configuration is quite rare.

A main remaining criticism I have is that there is something confusing and maybe a bit misleading in the way the topic is contextualized in the introduction and discussion.

Response: I sincerely appreciate your critical insights regarding the broad statements in the introduction and discussion. I fully agree that the broad statements are confusing and potentially misleading.

The rationale of the original first paragraph in the introduction section was as follows: Rossby waves represent one of the most significant phenomena in Earth's atmosphere, influencing diverse meteorological processes across high, mid, and low latitudes; thus, studying Rossby waves is of substantial scientific importance. When Rossby waves reach a certain stage of development, they may induce dynamic instability, which in turn makes research on this specific instability particularly essential.

Following your comment, I have revised the introduction and discussion sections to refine the contextualization for greater precision. Below are the key revisions implemented point by point.

Specifically, the very broad statements about the central role of Rossby waves for weather and extremes in lines 27-30 almost seem to suggest that barotropic instability is the primary source of weather and extreme events. Also, the statement on line 31-32 suggests wave breaking excites instability, while typically it is the other way around (I think the Read paper referenced in this context also states that wave breaking acts to wipe out PV gradients and stabilize).

Response: In response to your comment, I have retained the macro-level description of "the widespread impacts of Rossby waves" while incorporating precise qualifiers when discussing extreme events to avoid oversimplified attribution. For example, rather than claiming "Rossby waves cause extreme events," we now state that: "In several of these cases, amplified Rossby waves have been associated with extreme events through atmospheric teleconnection mechanisms, although the precise pathways involve multiple interacting processes and Rossby waves are only one possible contributing factor among many." This revision maintains scientific accuracy while avoiding the implication that instability is the primary cause of extreme events.

Besides, as Rossby waves intensify without constraint within the linear theoretical framework, classic instability occurs. During this process, Rossby waves break. However, it does not mean wave breaking excites instability. As you mentioned, Read's paper indeed states that wave breaking acts to wipe out PV gradient and stabilize. The contrast comes from that the conclusion of infinite amplitude increase in classical barotropic instability stems from the linear theoretical assumption of ideal non-viscous fluids, whereas the instability discussed in Read's is based on the nonlinear dynamics of actual fluids. Therefore, I am sorry for the improper expression here. To be more accurate, this sentence has been revised to: "As Rossby waves continue to intensify, classic linear instabilities can arise."

Now the revised first paragraph becomes: “Rossby waves are among the most fundamental dynamical features in the upper atmosphere. A key characteristic of Rossby waves, which has profound implications for their modulation of weather systems, is their propagation (Segalini et al., 2024). Accompanied by transfers of heat, moisture, and momentum during propagation and amplification, they play critical roles in shaping the weather and climate of polar regions (Woollings et al., 2023), in the occurrence of extreme weather events in mid–latitudes (e.g., Ali et al., 2022; Jiménez-Esteve et al., 2022; Kornhuber et al., 2019), and in tropical cyclone activity (e.g., Aiyyer and Wade, 2021; Riboldi et al., 2019). In several of these cases, amplified Rossby waves have been associated with extreme events through atmospheric teleconnection mechanisms, although the precise pathways involve multiple interacting processes and Rossby waves are only one possible contributing factor among many. As Rossby waves continue to intensify, classic linear instabilities can arise. Consequently, understanding the development and instability of Rossby waves during their propagation and evolution is crucial.”

To address your “impression that the author thinks the major instability of Rossby waves is barotropic”, I have added some brief and explicit introduction in the second paragraph to stress that baroclinic instability acts as a basic mechanism for eddy growth. Now the first several sentences in the second paragraph becomes: “Baroclinic and barotropic instabilities are well－known mechanisms responsible for the generation of dominant energy－containing eddies in the atmosphere (Read et al., 2020). For large－scale mid－latitude weather systems, baroclinic instability, which extracts energy from the mean temperature gradient, is generally the primary source of eddy growth. Barotropic instability, in contrast, feeds primarily on the horizontal shear of a basic current and is often secondary in magnitude, though it can become important in regions of strong horizontal shear such as the subtropics and the atmospheric jet flanks.”

Thank you for pointing out the confusing and potentially misleading contextualization of Rossby waves in the introduction. The revised text now better reflects current scientific consensus.

Also, lines 549-551 in the discussion: "For example, it provides a systematic assessment for Rossby wave evolution and instability in different basic flow profiles, especially in real－world atmospheric flows."

Response: To address your concern: “confusing and maybe a bit misleading” and “impression that the author thinks the major instability of Rossby waves is barotropic” in the discussion section. I reorganize the discussion section. The logical chain now becomes: The analysis approach presented in this paper can be extended to baroclinic systems, which then can be better applied to real atmospheric conditions.

I also list the revised paragraph here: “The proposed analysis approach in this investigation has potential applications in the future. It not only provides a systematic assessment of Rossby wave evolution and instability for a range of prescribed basic flow profiles within a barotropic framework. More importantly, the methodology permits a natural extension to baroclinic systems. Such extensions would allow the current barotropic insights to be bridged with more realistic atmospheric basic flows, where vertical shear and stratification often dominate Rossby wave evolution. Correspondingly, by specifying realistic atmospheric basic flows, practitioners can systematically identify turning points governing wave reflection, critical points regulating phase speed matching, and inflection points controlling modal instability onset. This diagnostic capability enables two practical applications: (1) optimization of disturbance parameters through precise determination of wavelength/wavenumber combinations that maximize transient amplification potential, and (2) quantitative tracking of energy – amplitude co – evolution via closed – form analytical expressions. This theoretical foundation ultimately provides a pathway towards a unified understanding of instability (whether modal or nonmodal) in both barotropic and baroclinic flows, and thereby offers a framework that, after appropriate extension, can be applied to real- world atmospheric contexts. In addition, the current version lacks topography, which may be insufficient when investigating the influence of large – scale topography on wave evolution along rays in the real – world atmosphere. Moreover, in giant gas planets such as Jupiter and Saturn, where a deep – layer jet, serving as a dynamical topography, also plays a significant role in modulating the weather layer above it. All these deserve further investigation."

Reading through the paper one can get the impression that the author thinks the major instability of Rossby waves is barotropic. There is a mention in the discussion that the work can be extended to baroclinic flows, but its minor place also contributes to the above impression. The relevance of the theory presented, in light of the fact that baroclinic instability is the major instability for Rossby waves, should be stated much explicitly.

I agree that barotropic wave dynamics is central for understanding realistic atmospheric flows, but barotropic instability is less commonly considered a main driver of weather.

I think the author should be a bit more explicit in how the understanding of barotropic amplification is relevant.

Response: I fully agree that baroclinic instability is a more effective mechanism in eddy growth in the atmosphere. Following your comment, I have added some explicit descriptions in the introduction and discussion sections, as I have mentioned above.

The present study focuses on barotropic instability not because it can perform a dominant role in influencing weather and extreme events, but because it provides a clean theoretical framework for investigating the instability in a controlled dynamical setting. Correspondingly, we have also added an explicit description in the conclusion section. The relative sentences are: "This study re–examines classical barotropic instability through the lens of Rossby wave ray–tracing theory and wave action conservation. This focus on barotropic instability is not motivated by overemphasizing its role; rather, it is chosen because it offers a clean theoretical framework for investigating instability in a controlled dynamical setting. "

These revisions have addressed potential misunderstandings arising from imprecise or overly broad expressions in the manuscript. Thank you for your constructive feedback, which has significantly enhanced the quality of the manuscript.

Looking at the papers mentioned in the first paragraph, Segalini et al (2024) discuss waveguidability of stationary waves (Segalini et al 2024)- these waves are expluced from the current analysis. In fact, stationary waves are maybe quite unique in that they are in a WG state, especially if the winds are westerly and there are no critical surfaces. The uniqueness of the wave geometry of these waves is maybe one of the interesting easy to understand implications of this paper. The paper by Woolinges et al discusses the turbulent vs wavy nature of the flow as a function of latitude with an emphasis on polar regions which this paper avoids discussing. The papers by Ali et al., 2022; Jiménez-Esteve et al., 2022; Kornhuber et al., 2019 on recurrent Rossby wave packets - these are short lived propagating waves which are phase locked. I am curios to hear how the author thinks the dynamics of this paper are relevant. An explicit statement on this will help the reader place this paper in context and understand possible uses of this methodology and thinking. The same goes for the tropical cyclone phase locking papers referenced - Aiyyer and Wade, 2021; Riboldi et al., 2019. Do you think your theory can illuminate some of this dynamics? These are papers where a subtropical anomaly is introduced - it is more relevant to the latitudes which you decided to look at.

Response: I sincerely appreciate your positive and valuable feedback. I acknowledge that the papers by Ali et al. (2022), Jiménez-Esteve et al. (2022), Kornhuber et al. (2019), Aiyyer and Wade (2021), and Riboldi et al. (2019) are not directly relevant to the present study. However, these works collectively address Rossby wave propagation and influences. As noted in my previous response, the logical framework of the first paragraph is as follows: Rossby waves represent one of the most significant phenomena in Earth's atmosphere, influencing diverse meteorological processes across high, mid, and low latitudes; thus, studying Rossby waves is of substantial scientific importance. These references therefore serve to illustrate the broader recognition of Rossby wave propagation and development in various meteorological contexts, rather than implying direct relevance to the current investigation.

I agree with your observation that these references might inadvertently suggest a closer connection than intended, potentially causing confusion for readers. To address this concern alongside previous feedback, I have revised the first paragraph accordingly. Should the revised version still not fully address your concerns, I have prepared a simplified alternative for the first paragraph, which removes these references and replaces them with a general statement. This revised sentence now reads: "Accompanied by transfers of heat, moisture, and momentum during propagation and amplification, they play critical roles in shaping weather and climate across different latitudes and in influencing a wide range of atmospheric phenomena." Should this approach be preferred, I can implement this revision to the paragraph.

More specific comments:

lines 97-98: this sentence confused me on two accounts- first, I think the fact that the atmosphere is baroclinic and baroclinic instability if the main unstable Rossby-wave process should also be mentioned if you are listing caveats. Second, for Earth, the main influence of topography is maybe setting the phase speed to be zero, no? This should also be discussed maybe. Also, the relevance of topography in the context of Venus and Jupiter can at first be confusing- I assume you mean that convection can act as a dynamic topography.

Response: Thank you for this constructive comment. First, I will address your concern regarding the role of topography, which exerts a significant influence on Rossby waves. In the shallow-water model, topography can generate an equivalent potential vorticity gradient through the "stretching" or "compression" of the air or water column, which acts as a restoring mechanism analogous to the beta effect, thereby markedly altering the frequency, propagation speed, and direction of Rossby waves. Not only can topography amplify the waves, but under certain conditions, it can also induce wave instability and even serve as a key mechanism for exciting a distinct wave type—topographic Rossby waves.

Second, your understanding that deep convection can act as a dynamical topography aligns precisely with the point I intended to convey. I apologize for the imprecise wording that may have caused confusion. Third, the linearized barotropic vorticity equations have been presented prior to lines 97-98. Adding caveats about baroclinic instability at this juncture would disrupt the narrative flow. Instead, I have explicitly acknowledged that the real atmosphere is baroclinic and that baroclinic instability constitutes the primary Rossby wave instability process for large-scale mid-latitude dynamics. This clarification has been inserted before introducing the barotropic equation.

Now the revised contents are: "The present study focuses on barotropic instability, which provides a fundamental theoretical framework for understanding baroclinic instability, the primary process driving unstable Rossby waves in Earth's atmosphere. The linearized vorticity equation for an incompressible barotropic atmosphere is typically written as

$$\left(\frac{\partial}{\partial t}+\bar{u}\frac{\partial}{\partial x}\right)\nabla^2\psi+\beta^*\frac{\partial \psi}{\partial x}=0, \qquad (1)$$

where ψ denotes the perturbation stream function, $\bar{u}=\bar{u}\left(y\right)$ represents the zonal basic flow, $\nabla^2=\frac{\partial^2}{\partial x^2}+\frac{\partial^2}{\partial y^2}$ is the Laplace operator, and $\beta^*=\beta-\frac{\mathrm{d}^2\bar{u}}{\mathrm{d}y^2}$ signifies the meridional gradient of absolute vorticity. $\beta=\frac{\mathrm{d}f}{\mathrm{d}y}$ denotes the Rossby parameter (assumed constant herein), and f is the Coriolis parameter. Notably, this study does not incorporate topographic effects, thus excluding a class of marginally stable cases arising from dynamic topography, a phenomenon particularly relevant to gas giants such as Jupiter and Saturn (e.g., Deguchi et al., 2024)."

Thank you for helping us improve the precision of these statements.

line 109 Read and Dowling (2026) are missing from the reference list.

Response: I apologize for omitting this reference from the list. It has now been included in the revised manuscript, and I have provided the full citation below for your convenience:

Read, P., and T. Dowling, 2026: Barotropic Instability. Encyclopedia of Atmospheric Sciences (Third Edition), W.A. Robinson, Ed., Academic Press, 263 – 283, https://doi.org/10.1016/B978-0-323-96026-7.00211-3.

Additionally, I have conducted a thorough review of the manuscript to verify that all cited works are properly listed in the reference section.

line 134-135: "In the case of an almost plane Rossby wave." - I am not sure this is always the case- if there is meridional reflection, the local wave is a superposition of poleward and equatorward waves. The wave is only a pure plane wave initially just after you turn on the wave source and before it reflects back from the turning surface (this is a the main point of Harnik, 2002).

Response: The concept of the "almost plane wave" comes from Harniki (2002). In the appendix A of his paper, he wrote: We assume linear quasigeostrophic Rossby waves, that satisfy WKB conditions, and an almost-plane Rossby wave of the form:

$$\phi \sim \exp\left[i\left(kx-\omega t+\int l dy+\int m dz\right)\right]. \qquad \text{(A1)''}$$

With this form of solution, he then derived that wave activity velocity equals group velocity. He then wrote "In general, however, Eq. (A1) does not hold, rather, we have a superposition of waves, for example

$$\phi \sim \left[\alpha_+ \exp\left(i\int l dy\right)+\alpha_- \exp\left(-i\int l dy\right)\right]\exp\left[i\left(kx-\omega t+\int m dz\right)\right]. \qquad \text{(A6)''}$$

For solution like Eq. (A6), he then suggested that wave action velocity differs from group velocity.

Therefore, we may conclude that an almost plane wave has the form defined by Eq. (A1). As you pointed out, when an almost plane wave propagates toward a turning point (or turning surface), the wave action velocity equals the group velocity. However, after reflection at the turning point, the local wave becomes a superposition of poleward and equatorward waves. This implies that Eq. (A6) may come into play, causing the wave action density to deviate from the group velocity.

To address your concern, I considered adding a more precise expression: "For the almost plane Rossby wave analyzed in this investigation, the wave action velocity coincides with the group velocity before being reflected at the turning point, indicating the equivalence of the two methods." However, I have opted to remove this sentence as its inclusion does not substantially enhance the argument.

Thank you for your thorough and insightful review.

line 164 - *background* absolute vorticity

Response: Thank you for pointing out this. I have added the "background" in the revised manuscript.

line 176 - Harnik (2002) reference does not state this- remove. I would add some of Lindzen's over-reflection papers.

Response: I apologize for this mistake. Harnik (2002) did not analyze or discuss the overreflection theory; instead, Harnik and Heifetz (2007) related overreflection to the Counterpropagating Rossby Wave Perspective.

Following your comment, I have removed the citation of Harnik (2002) from this sentence and added key references by Lindzen on overreflection. The revised sentence now reads: "In overreflection dynamics, incident waves can propagate through the evanescent region (where the index of refraction is negative) to undergo overreflection (e.g., Lindzen and Tung, 1978; Lindzen, 1988; Harnik and Heifetz, 2007)."

Thank you for your careful review.

Figure 1- this figure helps but it is not clear what subplot b shows.

Response: Panel b illustrates the trajectory of a northward-propagating ray originating from an initial location (y_0), denoted by a filled black circle in the figure. Prior to reaching the turning point (y_t), the trajectory is represented by the solid black curve. Following reflection at the turning point and subsequent southward propagation, the trajectory is indicated by the dashed black curve.

Following your comment, I have revised the caption to“the trajectory of a ray propagating northward from an initial location marked by the black point. The solid (dashed) black curve denotes the trajectory prior to (after) the turning point (b).”

Thanks for yours helpful comment.

Figure 3- I assume you chose a specific value of k - maybe state what it is?

Response: Your understanding is correct; I chose specific values for k in each panel: k=2, u_M=1.5 for (a); k=3.5, u_M=1.5 for (b); k=2, u_M=4 for (c); and k=1, u_M=4 for (d), where u_M denotes the nondimensional magnitude of the westerly jet. For (a) and (b), u_M=1.5 (corresponding to a westerly jet magnitude of 15 m/s) was selected to represent a relatively weak jet, where the meridional gradient of the background absolute vorticity has no zero points. For (c) and (d), u_M=4 (corresponding to a westerly jet magnitude of 40 m/s) was chosen for a strong jet, where the meridional gradient of the background absolute vorticity contains zero points. These values were selected to illustrate the typical propagative regimes of rays.

Following your comment, I have included the specific zonal wavenumber values in the revised figure caption. Thank you for your insightful feedback.

Reference

Read, P., and T. Dowling, 2026: Barotropic Instability. *Encyclopedia of Atmospheric Sciences (Third Edition)*, W.A. Robinson, Ed., Academic Press, 263–283, https://doi.org/10.1016/B978-0-323-96026-7.00211-3.