A linear assessment of barotropic Rossby wave propagation in different background flow configurations
by
A. Segalini, J. Riboldi, V. Wirth & G. Messori

Comments to Reviewer #1:
(the text of the reviewer is in italic)

We are extremely grateful for the detailed review, which we genuinely believe has helped to improve the explanations of what are a number of technically or theoretically complex passages of our study. In the following we address the reviewer's suggestions for improvement, and point out the changes compared to the original manuscript. Parts that have been rewritten or added due to comments by the referees have been highlighted in red in the revised version of the manuscript.

> *In the current version, the motivation is clearer, and the connection between the new numerical method and the concept of waveguidability is clearer. I think the manuscript could fit for publication in WCD, after another round of revision.*

We thank the reviewer for the support to our work.

> *1) Consistency of terms. (a). There are 3 types of models used in this study. Their names, according to the legend in figure 2a are: Linear Chebyshev, Linear SHT and Nonlinear SHT, where "SHT" stands for spherical harmonics transform. However, these names are not used consistently throughout the paper. The linear Chebyshev method is often called "linear" (e.g., line 186, caption of figures 1 and 3), which may confuse it with the linear SHT method. The nonlinear SHT method is simply called "nonlinear". The authors should choose a name for each method and use it consistently.*

This is a good suggestion. To avoid weighing the text with repeated references to SHT and Chebyshev, we now clarify explicitly at the end of Sect. 3 that we only discuss the linear Chebyshev and nonlinear SHT in the main text, and that these are referred to as "linear" and "nonlinear" simulations throughout.

1) Consistency of terms. (b). Waveguidability is sometimes called "normalized meridional enstrophy density", though it is not made clear if these are two terms for the same physical variable. Specifically, is the expression in equation (17) the same as the waveguidability presented in figure 2?

We thank the reviewer for pointing out this important point. We define the normalized meridional enstrophy density E (Equation (19) in the current manuscript version) so that it can be used to obtain a waveguidability metric (Equation (20)), which is apt to compare the waveguidability of jets located at different latitudes. In these steps, it is important to separate the waveguidability metric (the tool) from the waveguidability (the concept), in the sense of the capability of the large-scale flow to zonally duct Rossby waves. Thus, the long name given to E is a precise way to describe the latter quantity, but it is not the new waveguidability metric we propose.

The assessment of waveguidability shown in Fig. 2b employs the metric proposed by Wirth (2020), and stands as a proof of concept of the capability of the Linear Chebyshev method to reproduce previous results. The definition by Wirth (2020), however, becomes problematic when the waveguidability has to be assessed for jet streams located near the pole, since the smaller and smaller physical distance between the source and the receiver artificially inflates the value of the metric. We deem the metric in Equation (20) as a better assessment of the waveguidability property, since it is a number between 0 and 1, ranging between the two extrema of no waveguide and of a "perfect" waveguide.

1) Consistency of terms. (c). The variable φ_0 is sometimes used to denote the latitude of the forcing (line 209) and sometimes – the latitude of the jet (caption of figure 3). The latitude of the forcing is also called "φ_F" (equation 15) and the latitude of the jet is also called "φ_J" (equation 16).

As correctly noticed, there are three latitudes involved in our analysis:

1. the forcing latitude, indicated as φ_F in equation (16);
2. the jet latitude, indicated as φ_J in equation (17);
3. the latitude where we compute the normalised meridional enstrophy anomaly, indicated as φ_0 in equation (19).

In principle one can compute the enstrophy anomaly at every latitude with or without a jet ($\varphi_J \neq \varphi_0$ or $U_J = 0$) and with or without forcing at the same latitude, $\varphi_F \neq \varphi_0$. However, the idea with equation (19) was to introduce a perturbation at the same latitude $\varphi_F = \varphi_0$ to create an enstrophy source and use equation (19) to assess how much enstrophy remained at that latitude (within a small latitude band) and how much escaped. Since we focus on jet streams and expect that these might act as waveguides, we also chose the jet latitude as $\varphi_J = \varphi_0$. Therefore, jet latitude, forcing latitude and latitude where we compute the enstrophy anomaly coincide almost all the time, since we are trying to assess how strong a jet stream is by evaluating its capacity to hold the enstrophy (injected at the same latitude of the jet stream) within itself. An exception is for the double jet configuration, where there are two φ_J that are different from φ_F. We have added a sentence to clarify this delicate point in the revised version of the manuscript in section 5.1 and explicitly state that for a single jet the three latitudes (φ_0, φ_J, φ_F) coincide.

> *1) Consistency of terms. (d). The term "temporal coefficient" (e.g, caption of figure 5, line 412) is used interchangeably with the term "principal component" (line 263) or simply "amplitude" (caption of figure 6). Please explain if these are all words to describe the same thing. If so – please be consistent with the terminology. If not – please explain what "temporal coefficient" means.*

Thank you for noticing this inconsistency, the two terms indeed refer to the same thing and we have uniformed our terminology to only use "temporal coefficients" in the revised version of the manuscript.

> *1) Consistency of terms. (e). The bar (over-line) is used sometimes to denote the background flow (equation 1) and sometimes to demote the amplitude of a wave mode (line 124).*

That's correct, thank you for noticing it. We have now resolved this issue in the revised version of the manuscript. The overline indicates now the background flow vorticity and background streamfunction only, while the amplitude of the wave mode is indicated by a tilde.

2) Missing details. (a). For most of the results presented (at least figures 3, 4, 5 and 8), it seems that the authors used the same latitude for the jet and for the forcing, however this is not mentioned explicitly and the reader is left to guess what the latitude of the forcing is. Specifically, it is confusing the φ_0 is initially used for denoting the forcing latitude, while the jet latitude is sometimes called φ_0 and sometimes φ_J (see comment 1c above). Also, it would help if the authors explain why they chose to use the same latitude for the jet and for the forcing (when this is the case).

We have added a discussion about this point in the revised version of the manuscript in section 5.1.

2) Missing details. (b). It is not described how the EOF analysis is done exactly. Specifically, I would expect to find an exact explanation for how the "temporal coefficients" shown in the bottom panels of figure 5 were calculated. If these are the same as principle component time series of the EOFs, then it is surprising that the principle component of the first EOF oscillates around 1 and not around 0. Usually, an EOF analysis is performed after removing the trend from the time series. Is that the case here or not? Please explain.

No de-trending was applied to the present simulations. The time average of the meridional velocity field, V, was not removed and therefore the first EOF mode is approximately given by the time average of the solution. The EOF analysis was performed by considering the simulation results after 10 days from the start to exclude the initial transient (the wave generation and spreading) and the subsequent 90 days were collected and analysed by means of the singular-value decomposition (SVD) algorithm.

The inclusion of the time average in the SVD process affects only weakly the subsequent modes since the first vector of the decomposition is approximately provided by the arithmetic mean, while the second mode must be orthogonal to the first mode and so forth. This orthogonality constraint can influence the shape of the second mode (and subsequent ones), but the dimension of the space is sufficiently large to not have a noticeable consequence. On the other hand, the advantage of including the mean is that the field we reconstruct is complete and the time average is also projected into orthogonal directions, facilitating the construction of a reduced-order model. We have now specified this methodology in section 4.2.

> *2) Missing details. (c). According to the caption of figure 8a, the green line marks the "locus beyond which the temporal vorticity variance of the nonlinear simulation becomes 10 times larger than in the stable regimes". While this is explained in the figure caption, this criterion is not mentioned explicitly in the text, when the "stability" of the nonlinear solution is considered (lines 291-294). I would expect a more detailed description and explanation for this criterion in the text. How is the temporal vorticity variance calculated? Which stable regime is it compared to?*

The green line is a result of several nonlinear simulations performed for many jet latitudes and jet strengths. We expected that all the points to the left of the solid black line in Fig. 8a (the linear neutral curve) should be characterized by a flow that just converges towards the equilibrium solution (i.e., all transient Rossby waves decay in time in this region), while all points to the right correspond to an exponential deviation from the equilibrium solution (i.e., Rossby waves amplify). This is what we see from the linear eigenvalue analysis with the linear Chebyshev method, but also by running linear time simulations with spherical harmonics. However, the nonlinear simulations do not behave like that: the jet must actually be stronger than the value indicated by the solid black line, in order to see the unsteady waves grow indefinitely.

The temporal variance of the meridional velocity was computed from the nonlinear time simulations to diagnose such a growth. If the simulation converges in time, the temporal variance remains low, while the latter increases significantly when an unsteady behavior is present that is not damped. This is indicated in figure 1 of the present response where the variance is reported for different jet conditions. It can be clearly seen that nonlinear simulations are more stable than linear ones. The threshold has been decided arbitrarily to $\langle (v - \langle v \rangle)^2 \rangle = 2\ \mathrm{m}^2/\mathrm{s}^2$ to mark a sufficiently strong growth. This has been clarified in the revised version of the manuscript at lines 238-239.

> *3) Interpretation of the nonlinear simulations. (b). The authors interpret the behavior of the nonlinear solution presented in figures 5 and 6 as evidence for a limit cycle (e.g. line 265, 294).*

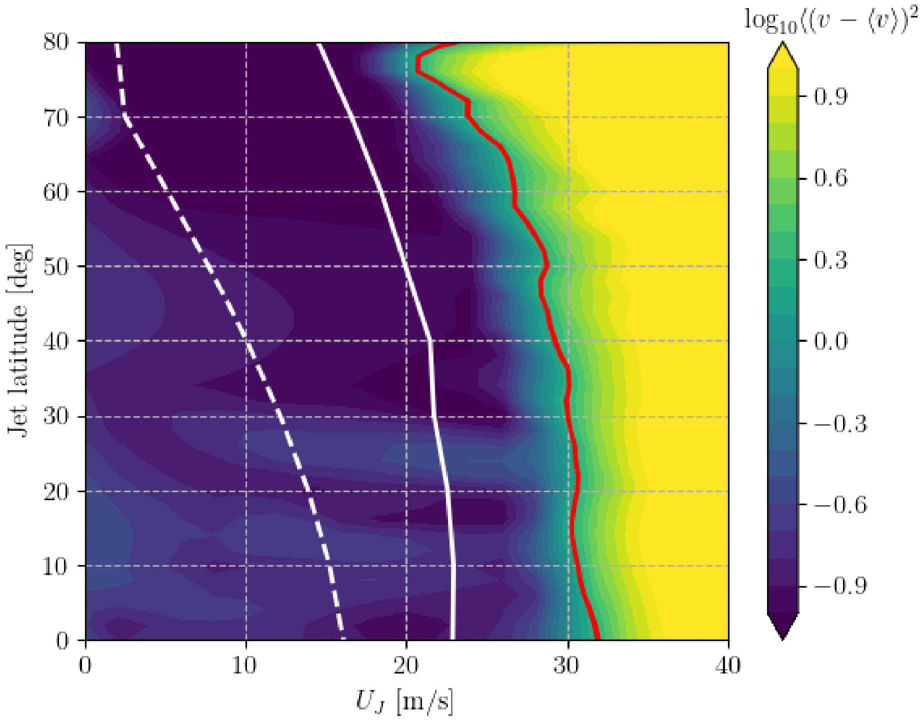

Figure 1: Meridional velocity standard deviation for different jet latitudes and strengths from nonlinear simulations. A logarithmic scale is used in the color scale. The white dashed line and white solid line indicate the Rayleigh criterion and the linear neutral curve, respectively, while the red solid line is the locus where $\langle(v - \langle v\rangle)^2\rangle = 2\ \mathrm{m}^2/\mathrm{s}^2$.

I am skeptic about this interpretation. First, because in order to identify a limit cycle, a phase space should be defined and the existence of the limit cycle and its stability need to be shown in this phase space. I don't see what is the phase space in which the authors find a limit cycle. Second, I disagree that the wave modes shown in figure 5 are not traveling waves. In lines 263-265 the authors argue that "The trajectories... behave as traveling waves: however, the linear stability analysis does not support this interpretation as the waves should grow exponentially in magnitude because they are unstable". In this argument the authors ignore the fact that in the nonlinear simulation the mean flow is modified by the wave fluxes and therefore it can be stabilized. Additionally, the wave-wave interactions introduce a dissipative effect. So there is no reason to believe that these are not traveling waves (actually figure 6 shows exactly that these are traveling waves).

We thank the reviewer for this comment, which gave us the opportunity to verify the presence of a limit cycle and to fortify the results we draw from our analysis.

By traveling wave we meant a neutral solution of the equations where its amplitude does not change in time. The linearised analysis did indicate the presence of unstable modes for jet speed 40 m/s, that however did not amplify exponentially indefinitely. The reviewer is right about the fact that a nonlinear traveling wave that was neutral with respect to the new equilibrium point could have, in principle, emerged. Only the nonlinear system can be used to sort out this matter.

In order to assess whether a limit cycle or a traveling wave takes place, it is important to define a state space – as the reviewer points out. We ran a long-time (200 days) simulation starting from the background state (this is referred as the reference simulation in the following analysis). From this simulation we calculate the EOF modes of the streamfunction, namely the variable used to solve the barotropic vorticity equation. As in Fig. 5 of the manuscript, the first mode $\Psi_0(\mathbf{x})$ is close to the time average of the forced system, while the successive modes $\Psi_1(\mathbf{x})$, $\Psi_2(\mathbf{x})$, ... are interpreted as Rossby waves. The projection of the instantaneous streamfunction field $\Psi(\mathbf{x}, t)$ into the n^{th} EOF mode provides the mode amplitude as

$$a_n = \int \Psi(\mathbf{x}, t)\Psi_n(\mathbf{x})\mathrm{d}\mathbf{x}\,. \qquad (1)$$

The instantaneous values of the coefficients can be collected into a vector

$$(a_0,\, a_1,\, a_2,\, a_3,\, \ldots)$$

that provides our state space. The EOF analysis of the reference simulation provided typical amplitudes of the EOF temporal coefficients $\sqrt{\overline{a^2_{n,ref}}}$ and the EOF spatial basis, that is from now on kept constant.

We ran several nonlinear simulations with different initial conditions given by

$$\Psi(\mathbf{x}, 0) = \sqrt{\overline{a^2_{0,ref}}}\Psi_0 + \alpha\sqrt{\overline{a^2_{1,ref}}}\Psi_1 + \beta\sqrt{\overline{a^2_{2,ref}}}\Psi_2 + \ldots\,, \tag{2}$$

namely by initiating our simulation from the time average field plus a set of Rossby waves with arbitrary amplitudes. If the state space evolution from different initial conditions will lead to the same orbit, a limit cycle is present, while if the orbit depends on the initial condition a traveling wave is present. Figure 2 shows the evolution of the $a_1 - a_2$ and $a_3 - a_4$ coefficients for four different initial conditions, that nevertheless lead to a state evolution that spirals towards the same orbit, supporting our claim about the presence of a limit cycle.

It appears that, if the modes have a too high amplitude, they will be damped, while for too small amplitude they will amplify as predicted by linear theory. This is particularly clear for the $a_1 - a_2$ coefficients, while the $a_3 - a_4$ coefficients undergo a transient growth first and decay afterward.

We thank again the reviewer for pointing out that we did not provide sufficient evidence to support our claim about the limit cycle in the previous version of the manuscript. We have now added an appendix to discuss this matter.

> *3) Interpretation of the nonlinear simulations. (a). As far as I could understand, the nonlinear simulations solve equation (3). This equation includes wave-mean flow interactions and wave-wave interactions. In contrast, the linear equation (equation 7) includes the effect of the mean flow on the waves, but does not include the effect of the waves on the mean flow, therefore it neglects both the wave-mean flow interactions and the wave-wave interactions. In the discussions in the paper, where the nonlinear simulations are compared with the linear method results, the authors assume that the differences between the solutions arise from*

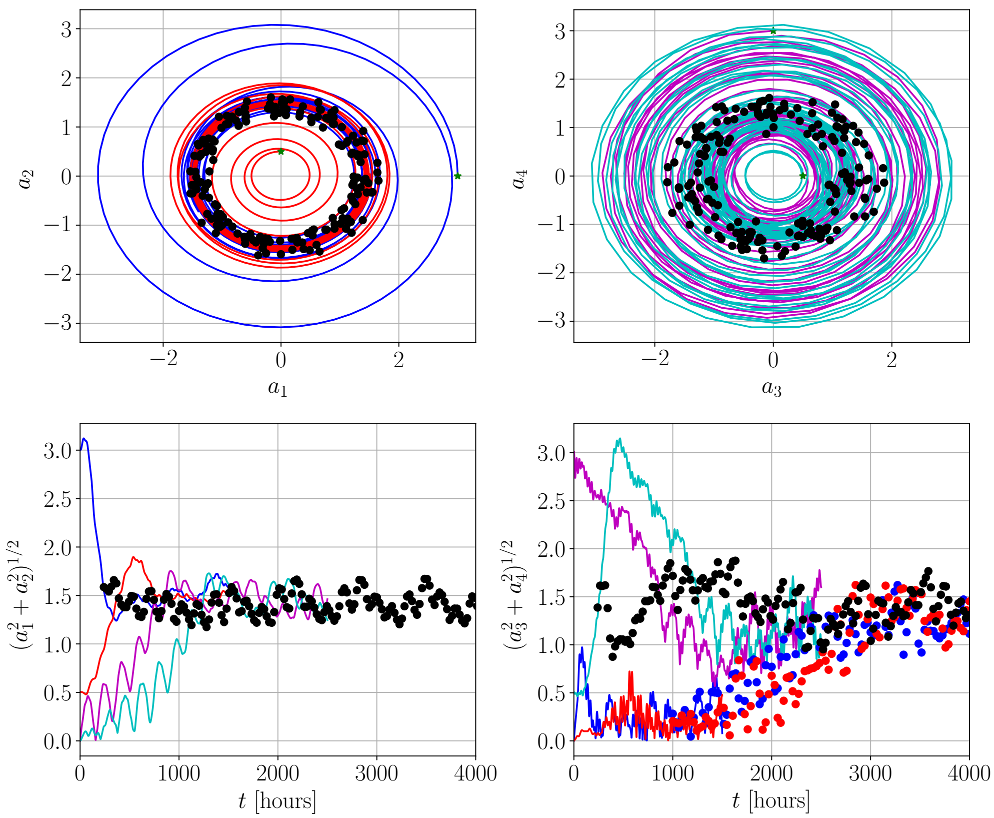

Figure 2: State space sections. (Top left) $a_1 - a_2$ coefficients, (Top right) $a_3 - a_4$ coefficients. (Bottom left) $(a_1^2 + a_2^2)^{1/2}$ temporal evolution, (Bottom right) $(a_3^2 + a_4^2)^{1/2}$ temporal evolution. Black circles: state space evolution over 4000 hours with initial condition $\Psi(\mathbf{x}, t = 0) = 0$. Blue: initial condition $\Psi(\mathbf{x}, t = 0) = \Psi_0 + 3\Psi_1$. Red: $\Psi(\mathbf{x}, t = 0) = \Psi_0 + 0.5\Psi_2$. Magenta: $\Psi(\mathbf{x}, t = 0) = \Psi_0 + 3\Psi_3$. Cyan: $\Psi(\mathbf{x}, t = 0) = \Psi_0 + 0.5\Psi_3$. The starting point of the lines is highlighted by a green asterisk.

the inclusion of wave-wave interactions in the nonlinear model, while ignoring the wave-mean flow interactions (e.g., lines 187-188, 295-296, 387-389). I think this is a very fundamental issue. When wave-mean flow and wave-wave interactions are included, the equilibration occurs due to the combination of stabilization of the mean flow profile by the wave fluxes, and the dissipation by wave-wave interactions. While the authors mention the latter, they ignore the former, which could be very important.

We now also briefly discuss the potential role of wave-mean flow interactions in explaining the differences between the simulations in Sect. 4.1 and 4.2, and acknowledge that we cannot separate the role of wave-wave and wave-mean flow interactions when comparing the simulations.

3) Interpretation of the nonlinear simulations. (c). The comparison between the stationary solution of the linear equation and the time-mean solution of the nonlinear equation assumes that we should expect them to be similar (e.g. lines 250-253, 371-373). I think these two solutions capture a fundamentally different phenomenon. The linear stationary solution captures a stationary (i.e. zero-phase speed) wave, forced by the topography. The nonlinear time-mean solution captures a statistically steady state. The paragraph in lines 253-268 (as well as parts of the conclusions section) tries to explain the similarity between the stationary solution of the linear equation and the time-mean solution of the nonlinear equation in an unstable case. They argue that one should expect the nonlinear solution to diverge in the unstable conditions (e.g. lines 236-237). Their interpretation for the lack of instability in the nonlinear case is that the system reaches a limit cycle. I would argue instead that the system reaches a nonlinear statistically steady state, where the mean flow is stabilized by the waves (in the time-mean sense), and the waves are equilibrated in the sense that their life cycles give a net zero growth, when averaged over time. I would definitely expect to find traveling waves in such a solution. These traveling waves don't necessarily need to be identical to the most unstable modes of the mean flow. They could be neutral modes in the linear sense, but they need to be able to maintain the mean flow in a profile that *enables them to go through cycles of growth and decay (see for example DelSole 2004, Lachmy and Harnik 2016).*

The linear equilibrium solution is a result of a linearised analysis while the time average of the nonlinear solution is another entity, as the reviewer points out. In both the paragraphs indicated by the reviewer our statement is that "the averaged flow field in the nonlinear simulation resembles ... the unstable equilibrium state calculated from the linear method". We did not expect them to be the same, but the qualitative similarity is only a result of empirical evidence. The discrepancies can arise in the linearised approximation because

1. the linear model is linearised around the background state and not around the equilibrium state: this error is enhanced when the topographic forcing has large amplitude.

2. the linearised model is unstable: in this case the perturbation growth leads to some wave fluxes that transfer energy from the wave to the mean flow

The remedy to the first issue is to linearize around the time average instead, an approach done by one of the authors in (Matsubara M, Alfredsson PH, Segalini A. Linear modes in a planar turbulent jet. Journal of Fluid Mechanics. 2020 Apr;888:A26.). However, the linear analysis with a nonzonal background flow is more computationally expensive since the Fourier decomposition is not separating the individual waves anymore.

In principle we were expecting that, if the background flow is linearly stable, perturbations should decay and the flow should approach the equilibrium point again, with minor discrepancy due to the linearisation approximation, as in the case with no jet. However, we were surprised to see that this good resemblance kept being the case even beyond the onset of linear instability, probably because the wave-mean flow terms in the time-averaged vorticity equation $\nabla \cdot \langle \mathbf{u}'\zeta' \rangle$ remains bounded by our damping term (see our previous comment on the limit cycle).

Our aim while discussing the limit cycle in the conclusions was more oriented towards the perturbation evolution, since we were expecting the onset of turbulence motion rather than a quasi-periodic wave activity. The scenario depicted by the reviewer is correct. Our statement about the unstable modes from the linear analysis resembling the EOF modes was also speculative: however, it is true that the linearly unstable modes are the infinitesimal waves expected to grow the most, although this does not automatically exclude the possibility on non-normal growth (transient growth) of other waves.

Once again the description of the waves dynamics around the statistical equilibrium point requires a more accurate analysis linearised around the statistical steady state with a model of the wave-mean flow terms, an approach that we have not yet attempted and that goes beyond the scope of the present work.

> *4) Interpretation of the stability analysis. Figure 8a shows the maximum of the imaginary part of the linear eigenvalues (i.e., the linear growth rate) normalized by the damping time scale. The dashed line marks the locus where the absolute vorticity gradient changes sign (the Rayleigh stability criterion). The authors argue that the distance between the line where the linear growth rate is zero and the dashed line in figure 8a shows that "the Rayleigh criterion provides a necessary but not sufficient condition for the onset of instability" (lines 282-285). In the conclusions section (lines 382-384) they argue that the Rayleigh criterion was not capable of detecting the onset of barotropic instability. I disagree with this interpretation, because the linear stability criterion could easily be adapted to incorporate the effect of the damping term, by examining the line where the growth rate is equal to minus the damping time scale (i.e. where the growth rate is equal to -1 in figure 8a). Note that this line corresponds to the dashed line, meaning that it is consistent with the Rayleigh criterion of instability. This is not a coincidence. When the wave equation includes a linear damping term, the growth rate is expected to be the same as the linear growth rate of a model without damping, minus the damping time scale. Therefore, the results are consistent with the theory of barotropic instability, where the Rayleigh criterion marks the state where the linear growth rate of a model without damping is zero, and when linear damping is added, the growth rate is reduced by the damping time scale.*

We agree with the reviewer on this point. However, it is still true that the change in sign of the potential vorticity gradient is not associated to an unstable regime, but rather to a less stable one. The analysis is reported here below, although it follows the arguments of the reviewer.

The analysis starts by considering the barotropic vorticity equation in perturbation form (Eq. 10 in the manuscript)

$$\frac{\partial \widehat{\zeta}}{\partial t} + \frac{imU}{\sin\theta}\widehat{\zeta} - \frac{im}{\sin\theta}\frac{\partial Q}{\partial \theta}\,\widehat{\psi} + \chi\widehat{\zeta} = 0 \quad \text{with} \quad Q = \overline{\zeta} + \frac{f}{\cos\theta}\,. \tag{3}$$

By introducing the modal ansatz $\widehat{\zeta} = \widetilde{\psi}e^{-i\omega t}$ it is possible to simplify (3) into

$$-\left(i\chi + \omega\right)\widetilde{\zeta} + \frac{mU}{\sin\theta}\widetilde{\zeta} - \frac{m}{\sin\theta}\frac{\partial Q}{\partial\theta}\,\widetilde{\psi} = 0\,. \tag{4}$$

By multiplying all terms with $\sin^2\theta\widetilde{\zeta}^*/\left(\partial Q/\partial\theta\right)$ and integrating in colatitude one obtains

$$-\left(i\chi + \omega\right)\int_0^\pi \frac{\sin^2\theta}{\partial Q/\partial\theta}\left|\widetilde{\zeta}\right|^2 \mathrm{d}\theta + m\int_0^\pi \frac{\sin\theta U}{\partial Q/\partial\theta}\left|\widetilde{\zeta}\right|^2 \mathrm{d}\theta - m\int_0^\pi \sin\theta\,\widetilde{\zeta}^*\widetilde{\psi}\mathrm{d}\theta = 0\,, \tag{5}$$

where the last integral can be rewritten since $\zeta = \nabla^2\psi$ leading to

$$\int_0^\pi \sin\theta\,\widetilde{\zeta}^*\widetilde{\psi}\mathrm{d}\theta = -\int_0^\pi \sin\theta\left|\frac{\partial\widetilde{\psi}}{\partial\theta}\right|^2 \mathrm{d}\theta - m^2\int_0^\pi \frac{1}{\sin\theta}\left|\widetilde{\psi}\right|^2 \mathrm{d}\theta\,. \tag{6}$$

The last two terms of Eq. (5) are indeed real and do not contribute to the instability (i.e., the imaginary part of ω). The only imaginary term is

$$-\left(\chi + \omega_i\right)\int_0^\pi \frac{\sin^2\theta}{\partial Q/\partial\theta}\left|\widetilde{\zeta}\right|^2 \mathrm{d}\theta = 0\,, \tag{7}$$

Equation implies that either $\omega_i = -\chi$ or the integral must be zero, the latter happening only when $\partial Q/\partial\theta$ changes sign, namely the Rayleigh criterion. We have described this aspect in the paper in Sect. 4.1 without providing the equations to avoid shifting the focus on this aspect. However, as mentioned in the manuscript, the inclusion of the damping term does not inhibit the application of the Rayleigh criterion, but it limits that to the fact that when the absolute vorticity gradient changes sign the imaginary part of the eigenvalue can be different than $-\chi$, therefore still stable.

> *5) Section 6 This section doesn't include a discussion of the implications of the results. I couldn't understand the motivation for looking at the time-dependent solution and what the conclusions from this analysis are.*

We have decided to remove this section since it was interesting to us only as an analytical solution of the linearized equations, without however providing additional insight into the atmospheric dynamics than what already obtained from the stability analysis.

1) Line 84: Lambda is defined, but it is not used in equation (1).

Since we are defining the colatitude θ and the latitude φ, it is natural to introduce the notation for the longitude there too, rather than later on at equation (4).

2) Line 105: Since equation (9) includes variables with a "hat", denoting the amplitude of the Fourier components, it would be better to define the Fourier components (Psi-hat(theta, t)exp(imx)) here, before the equation, or at least mention what the hat symbol means.

We have now fixed this point in the revised version of the manuscript by providing the Fourier transform definition (Eq.- (9)).

3) Line 118: Something in the wording is not correct, "achieved at regime" doesn't sound right. What does it mean?

We meant that equation (13) is the streamfunction field after sufficiently long time. Initially we denoted this as the infinite-time solution. However, when this equilibrium state is unstable, an exponential divergence from the equilibrium state is expected and equation (13) should never be observed: in the nonlinear simulations we do observe anyhow a similar field in the time averaged field instead. We have reworded this sentence to clarify the above interpretation.

4) Line 124: The bar (over-line) was used before to denote the time-mean background solution, here it is the amplitude of the Fourier component in time.

We have now fixed these inconsistencies in the revised version of the manuscript. The overbar denotes the background flow while the tilde is used to indicate the Fourier transform of the perturbation.

5) Lines 137-138: This would be a good place to refer again to the appendix.

Thank you for the good suggestion; we have now added a reference to the Appendix there.

6) Line 141-142: The first sentence of this paragraph seems to belong to the previous paragraph.

Thank you for spotting this error. We have now corrected and moved the sentence to the previous paragraph.

7) Line 144: Please mention exactly which linear and nonlinear equations the SHT package solves. Are these equations 7 and 3?

Yes. Three solvers were developed in this project. Two based on the SHT transform (one solving nonlinear equation (3) and the other solving the linear equation (7)), and one based on the Chebyshev formulation (solving only the linear equation (7)). Since the two linear solvers gave the same answer, the majority of the paper is based on the analysis of the linear Chebyshev code and the nonlinear SHT code. One could argue that it would have been more consistent to just do the entire work with the SHT method only: however, the Chebyshev methodology provides an analytical form for the derivative matrices (equation A4 in the appendix) so that the eigenvalue problem could be easily formulated. We have specified this more clearly in the revised version of the manuscript at lines 185-190.

8) Line 153: Bar (over-line) was used before to denote the time-mean, but here it is used to denote the zonal wind divided by cosine latitude.

Thank you again for the careful review. This issue has now been fixed by replacing $\overline{U}$ with U_0 or directly with 15 m/s.

9) Line 167: created by -¿ forced by.

Corrected.

10) Line 183: Delete the second "for the".

Corrected.

11) Line 190: "between the forcing the the monitoring sector" – the first "the" after "forcing" should be replaced by "and".

Corrected.

12) Caption of figure 2: It says that "N" is the number of latitude/longitude grid points for the Chebyshev simulations and the truncation number for the SHT method. Why not define N as the number of latitude grid points for all the cases, to be consistent?

We have modified the caption of figure 2 according to the suggestion of the reviewer so that we use the number of latitudes in both simulation setups.

13) Line 239: "...is also not be ruled out" – delete the "be".

Corrected.

14) The authors use expressions related to time when referring to the behavior of the system as a function of the model parameters (the word "after" in line 299 and the words "started" and "later" in lines 387-388).

We indeed used incorrect terms and we have now fixed this in the revised version of the manuscript, replacing for instance "after" with "beyond".

15) Line 300: "...the maximum growth rate in Fig.8b..." – figure 8b shows the wavenumber, not the growth rate.

That is the wavenumber associated to the maximum growth rate. We have now corrected this in the revised version of the manuscript.

16) Line 310: "jets" – change to "jet".

Corrected.

17) Line 325: "...equally efficient waveguides" – why do you say they are equally efficient if their waveguidabilities are not equal?

What we meant was that the waveguidability is high in both cases, but the reviewer is right and we have reformulated the sentence to: "...similarly efficient waveguides".

> *18) Line 338: "...the waveguidability is reduced...". I assume the authors mean that in the double jet case it is reduced compared to the single jet case. However, this should be mentioned explicitly and not left for the reader to guess.*

Correct. We now state this explicitly in the revised manuscript.

> *19) Line 384-385: This sentence is not clear. Specifically, the phrasing of the part: "...the condition of instability corresponds to a first increase of the...".*

Here we wanted to highlight how the application of the Rayleigh criterion leads to a necessary but not sufficient condition for the instability. We have rewritten the concluding section to facilitate the reading of the manuscript so we hope that the overall clarity has improved.

> *20) Line 388: Please explain what were the signs of barotropic instability in the nonlinear simulations? Why do you consider them to be signs of barotropic instability?*

For the adopted extremely simplified simulation setup only a barotropic instability can take place. We noted and discussed in the stability section that the nonlinear simulations remain temporally stable even beyond the neutral curve (namely for a jet strong enough to trigger a linear instability), although beyond the green curve in Figure 4a, even the nonlinear simulations show an unsteady behavior that is not decaying in time and the overall flow field remained time dependent.

> *21) Line 401: Delete the "that" after c).*

Corrected.

> *22) Line 410: "these evidences" – change to "this evidence".*

Corrected.

23) Lines 412-413: The sentence "the temporal coefficients could be determined by solving a small set of nonlinear ordinary differential equations" is not clear. What do you mean by "temporal coefficients"? Are these the same as the principle component time series (see major comment 2b)? Which small set of equations are you referring to?

The EOF temporal coefficients are the main subject of this sentence. As specified in our previous reply, we have now uniformed our terminology throughout the text to always refer to these as "temporal coefficients". If we substitute the EOF decomposition of the form

$$\Psi(\mathbf{x}, t) = \sum_{n=0}^{N} a_n(t)\Psi_n(\mathbf{x})\,, \tag{8}$$

in the barotropic vorticity equation (3), and exploit the orthogonality of the modes $\Psi_n(\mathbf{x})$ it is possible to obtain a nonlinear system of ODEs where the unknowns are the coefficients $a_0, a_1, a_2, \ldots$. This model is extremely cheaper than the original equation (3) since only a small set of ODEs are solved. The book of Holmes et al. provides a detailed description of this methodology as a reduced-order model. We have added a clarification of this methodology in the manuscript.

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Comments to Reviewer #2:
(the text of the reviewer is in italic)

We appreciate the feedback regarding our manuscript. In the following we address the reviewer's suggestions for improvement, and point out the changes compared to the original manuscript. Parts that have been rewritten or added due to comments by the referees have been highlighted in red in the revised version of the manuscript.

> *The reviewers made a significant attempt in responding to my concerns. However, considering the length of the response, which is almost as long as a short manuscript, one wonders why so much clarification was needed for a piece of work that was apparently deemed ready for peer-review. Furthermore, the revisions almost amount to this manuscript becoming a new manuscript and thus a new submission. For future submissions, the authors are encouraged to assess the significance, context, and clarity of the work more carefully before entering the peer-review process.*

The purpose of peer-review is not merely that of a one-way communication from the reviewer to the authors, but also for the authors, reviewer, editor (and in the case of EGU journals the broader scientific community) to engage in a discussion on specific scientific aspects of the submission being reviewed. Indeed, if the sole purpose was one-way communication, the whole concept behind EGU journals publishing both reviewer comments and author replies would be somewhat moot.

Some topics, notably those of a more theoretical nature, may lead to longer written discussions than others, due to the need to clearly explain technical aspects, assumptions and details that may be relevant to the framing of the broader discussion. Moreover, the length of the replies is determined as much by the authors as by the reviewers. Thorough reviews, raising relevant points of discussion, will naturally lead to thorough replies, discussing those points. A superficial review will likely elicit short and simple replies.

Coming to our specific case, we have received two very thorough reviews on our original submission, which we are grateful for. The fact that we disagreed with some of the reviewer's points, further contributed to a lengthy reply. Indeed, to facilitate the editor in their decisional role, we opted to provide particularly detailed replies to those points where we partly or wholly challenged the criticisms of the reviewer. Based on the above, we believe that judging the quality of a submission by the length of the replies to reviewers is a very poor call.

We further stand by our initial judgment that our manuscript is scientifically significant and clearly describes a set of new results. We are grateful for the time you have dedicated to reviewing our paper, as we are well aware it is an entirely voluntary and unremunerated undertaking, but disagree with your stance.

> *Regarding my general comment about the introduction not leading to the actual research question addressed in this manuscript and that it left one wondering what this manuscript is about, the authors responded: "we believe that an introduction should provide context and motivation for the work, beyond a simple list of points that will be addressed in the analysis." It is exactly that what the authors have not provided in their first version, i.e., the context and motivation of their work. The revised introduction is an improvement, but one still wonders about the relevance of, for example, resonance, for which the authors use an entire paragraph. Does their method address this challenge? If so, it should be pointed out in the introduction, otherwise it leaves the reader wondering about the relevance of this discussion on resonance. The authors also discuss extremes and the context to climate change in the introduction, which is not followed up in the rest of the manuscript.*

We wish to keep the discussion of the first version of our manuscript as short as possible, since this is not what is being reviewed here. We nonetheless wish to point out that our original introduction was structured as follows:

1. General background on atmospheric wave propagation (broad topic);

2. Relevance for surface weather (practical implications of studying the topic);

3. More detailed background on waveguidability and existing knowledge gaps;

4. Research question being addressed and structure of the paper.

We see no lack of contextualization in this structure.

Coming to the revised version, the new paragraph on wave amplification and resonance (we note that resonance takes up less than half of the paragraph) was restructured and expanded following the suggestion of this reviewer to split a paragraph in the original text. Splitting a paragraph seldom leads to a shorter text. In the introduction, we explicitly address the relevance of our analysis for wave resonance (lines 25-32). We agree that the single sentence on climate change is superfluous, and have removed it in the new version of the manuscript.

> *My specific comment on L16 was not understood correctly. My point was that there has been extensive previous work on the concept of wave guiding, not only as recent as the last ten years. It was this context that I was missing.*

We cite several studies from the 70s, 80s and 90s, so we still struggle to correctly understand the point the reviewer is making. It would be helpful to a constructive communication if the reviewer were to point to specific bodies of work that they think are lacking from our introduction.

> *The new abstract clearly states that the main thrust of the paper is a novel algorithm, which would imply that my original interpretation that this is piece of work is mainly a technical paper was correct. As indicated in my previous review, for such a more technical manuscript, it would be recommendable to resort to more technical journals, such as GMD. While I find the method and results interesting, the still somewhat confusing presentation of arguments and rather technical character make it not suitable for WCD in my point of view.*

The authors of the study have noticed – albeit without any factual data to support it – a trend towards fewer and fewer technical/theoretical contributions in climate dynamics journals, in favour of papers performing statistical or climatological analyses of large climate datasets. This may have involuntarily led to the notion that more technical analyses are ill-suited for dynamics journals – something that we disagree with.

Concerning the focus of the study, we note that notable previous contributions on algorithms to study wave propagation have been published in WCD (Wirth 2020), J. Geophys. Res.: Atmos (Manola 2013) and J. Atmos. Sci. (Hoskins and Ambrizzi, 1993). A good rule of thumb to judge the relevance of a study for a particular sub-field is to look at its bibliography. In our case, none of the studies we cite has been published in GMD nor in JAMES, currently the two leading Earth System modelling journals. The journal we cite most often is J. Atmos. Sci., whose scope very much overlaps that of WCD.

To further address the concerns of the Reviewer, we emphasized in the manuscript, we emphasized in the manuscript the physical insights that can be gained from the analysis, which leads to an improved understanding of the idealized simulations used to study waveguidability in previous literature (such as Wirth 2020). In particular, we re-arranged the presentation of the results by grouping them in two new sections, one about results concerning barotropic Rossby waves and the other about results concerning the waveguidability problem. The structure of the conclusions was also modified to reflect the novel exposition of the results. We believe the knowledge gained will prove useful in designing future research efforts to study Rossby waves and their waveguides.