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 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.

> *I think the method and results presented in this manuscript are interesting and significant, and could be useful for future studies.*

We thank the referee for the support to our work.

> *I personally had to read through the manuscript carefully twice before I had a sense of what it is about.*

We have extended the introduction and extensively modified Sect. 4–7 to better motivate the work (both in terms of introducing the methodology and of highlighting which new scientific insights result from our analysis) as both referees pointed out that the previous manuscript had a structure that hindered readability.

> *1) In my opinion, the introduction, the abstract and perhaps even the title do not express clearly what the paper is about. They give the impression that the paper is more about the physics of waveguides, whereas the main contribution of the paper, as I see it, is the computational method. The single and double jet cases shown in the paper are used as test cases for the computational method, rather than going deep into the dynamics of waveguides.*

We agree with the referee that part of the novelty of the study lies in the new computational approach. However, we would also like to highlight that the implementation of such approach provides novel physical insights (see e.g. the analysis of the double jet in Sect. 5). We thus believe that the contents of the study cannot be fully appreciated without an understanding of the current state of research on Rossby waveguides. The intertwining between methodology, theory and physical insights is particularly relevant for this topic, as a universally accepted definition of waveguide is still lacking. White et al. (2022) and Wirth and Polster (2021) showed that our capability to "see" waveguides critically depends on the methodological approach chosen to diagnose them. We perhaps described this problem in excessive detail in the Introduction, with the effect of de-emphasizing the methodological novelty of our approach. We have now added a new paragraph to the introduction focusing on methodologies for waveguide analysis in the literature. In parallel with this, we have extensively modified the results and concluding sections (including a new Sect. 6) to better highlight the physical insights gained from the proposed linear analysis. Finally, we have revised the title to better reflect the focus of the paper, which is on wave propagation rather than waveguidability.

> *In the last paragraph of the paper the authors write that "This study should be regarded as an introduction and explanation of the techniques, but possible applications of this approach could include systematic waveguidability assessments for different forcings and background zonal wind profiles." I think this sentence should appear in the introduction, and the abstract should emphasize the main point of the paper.*

The paper has been partially re-written to better address the final outcomes of the paper. In particular:

- The waveguidability has been discussed more by means of the definition proposed by Wirth (2020) and an extension of this definition has been proposed to be able to assess jets at different latitudes;
- The improvements compared to ray-tracing theory (limited by slowly-varying background flow, steady wave propagation, Mercator projection) have been described in the introduction, in section 2 and in the manuscript concluding section;
- The difference between the classic barotropic stability criterion, linear neutral curve and locus where the nonlinear simulations become unstable has been extended in section 4.3.

2) Perhaps the authors could add some more motivation for specific choices in the derivation of the mathematical model, that could add to the clarity of the paper. One choice that wasn't immediately clear to me was including a damping term in equation (1). It wasn't clear what this damping term represents physically. It was also not clear to me what the motivation was for looking at the stationary solution in equation (13). Only after I finished reading it became clearer. I think that in section 2 it could help to explain better what the model is supposed to represent, before the derivation of the equations.

We thank the reviewer for this suggestion. It is quite common to introduce this kind of damping term (see for instance Hoskins and Karoly, 1981, Hoskins and Ambrizzi, 1993, Wirth, 2020). Yet, we agree that its use should be explained and that we did not elaborate this important term adequately in the previous version of the manuscript. The latter has been updated according to the suggestions of the referee around equation (1) and (13) to clarify the meaning of the attenuation term and of the equilibrium solution. Furthermore, following the suggestion of the other referee, we have changed the symbol of the attenuation parameter from λ_r to χ.

3) The authors present stationary solutions and time-dependent solutions, but it is not mentioned explicitly at which section each type of solution is examined, what each type of solution represents physically, what is the motivation for looking at each type of solution and what are the different methods for solving for a stationary or time-dependent solution. Each of these are explained somewhere, often after presenting the results, but it is not explained in an organized manner.

Our aim in the results section was to highlight that the linearised methodology provides a simple and reasonably good assessment of the temporal evolution of the waves, while the majority of the analyses in the literature were focused on the equilibrium solution (for the linearised system) or on the temporally averaged nonlinear numerical solution. We nonetheless understand the confusion our previous framing of the results caused. We have now rearranged the structure of the results to separate the discussions of the stationary solution from the time-dependent solutions. In particular, we have moved the discussion of the unsteady case at the end of section 5 as a new section about time-dependent analyses. While the procedure to obtain the stationary solution is described in Sec. 2 [Eq. 12], the derivation of the temporal analytical solution (in the sense that no temporal discretisation is needed) is provided in the paper appendix with both steady and unsteady forcing.

> *4) Section 4.3 analyzes the stability of the problem for different parameters of the jet profile. I was missing a discussion on the connection of this instability to linear barotropic instability, in the sense of the necessary conditions for instability including a change of sign of the PV gradient. Some more physical context would be useful.*

We thank the referee for this comment. By repeating the derivation of the Rayleigh stability criterion it is found that a necessary condition for the instability is that the PV gradient becomes zero for the spherical case as well. However, this condition is not sufficient, since it enables the imaginary part of ω to be different from $-\lambda_r$ ($-\chi$ in our current notation), so that there is still a large stability margin until the imaginary part of ω becomes positive and the mode unstable. This is actually what we observe from the present analysis since the PV gradient changes sign when $U_J = 9$ m/s (for a jet at 45° N and $\sigma_J = 5°$), while the flow becomes linearly unstable at around $U_J \approx 20$ m/s. In the stability analysis part we have now discussed this and updated the figure with the eigenvalue and the neutral curve to show the curve where the PV gradient changes sign: as visible in the new figure 8 (figure 1 of the present reply), the Rayleigh criterion is violated in a region that is still stable. If the damping was absent, then the Rayleigh criterion will become a sufficient condition for the instability. We have added this discussion in both Sect. 4.3 and Sect. 7 of the revised version of the paper.

> *5) The analysis of the double jet case shown is much shorter than that of the single jet case and does not include a calculation of waveguidability. Based on the last sentence of the abstract ("Examples using single- and double-jet configurations are discussed to illustrate the method and study how the background flow can act as a waveguide for Rossby waves") I was expecting a comparison between the waveguidability of the two cases. If the authors choose*

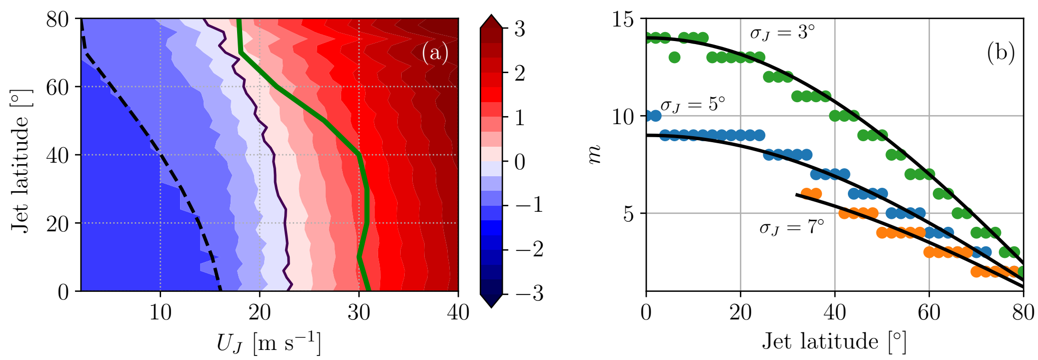

Figure 1: (a) Maximum of the imaginary part of the eigenvalues for a given jet velocity (normalised by χ) when $\sigma_J = 5°$. The black line indicates the neutral curve, the dashed line is the locus where the absolute vorticity gradient changes sign (Rayleigh stability criterion), while the green thick line is the locus beyond which the temporal vorticity variance becomes 10 times larger than in the stable regimes (approximating the neutral curve in the nonlinear case). (b) Azimuthal wavenumber of the most unstable eigenvalue for $U_J = 40$ m/s for different jet widths. The black lines are curves $m \propto \cos\varphi_J$ fitting the linear stability results.

not to include a calculation of waveguidability for the double jet case, they should otherwise motivate the choice for the analysis that is presented.

We have followed the suggestion of the reviewer and have now assessed the waveguidability of both jets in the revised version of the manuscript, always using a new diagnostic that is a modified version of the one proposed by Wirth (2020) and able to account for jets at different latitudes. These results have been added to Sect. 4.1 as a new figure 3 (figure 2 of the present reply). The waveguidability of the two jets taken separately is around 84% for the southern jet and 92% for the northern one, so that the northern jet has similar waveguidability than the southern one. When two jets are simultaneously present, the northern jet shows again a higher waveguidability (82%) compared to the southern jet (70%), although both values are lower than the isolated case due to additional leakage of enstrophy from one jet to the other and the surrounding. This is not necessarily an intuitive result and highlights the type of physical insight that our approach can provide.

The term "analytical solution" used in the abstract and introduc-

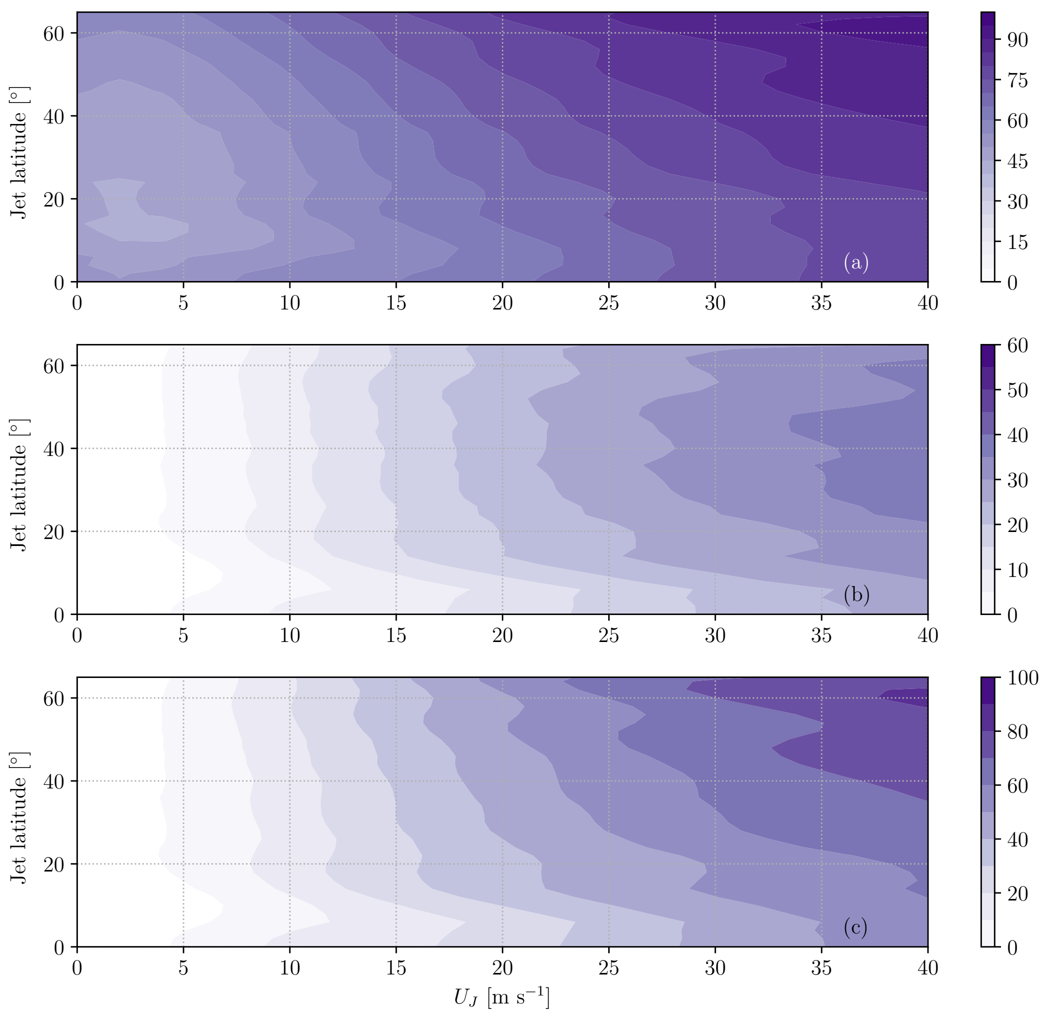

Figure 2: (a) Enstrophy meridional density of the single jet zonal profiles for different jet latitudes and strengths, estimated according to Eq. (17) of the revised manuscript. The enstrophy field has been computed from the equilibrium state obtained from Eq. (12), namely from the linear method. (b) Difference $E(U_J,\, \varphi_0) - E(0,\, \varphi_0)$ used to highlight the increment in enstrophy density with the jet speed with respect to the solid-body case. (c) Calculated value of waveguidability according to Eq. (18) of the revised manuscript.

tion was confusing for me. I expected to see a solution expressed as a mathematical function. It is true that the Chebyshev polynomials are analytical functions and in that sense the solution is analytical, but eventually there is a numerical calculation that leads to the solution, so perhaps a different terminology would describe the method more clearly. As I see it, the main difference between what is called a "numerical solution" and "analytical solution" by the authors is that the former is a time-integrated solution and the latter is an eigenvalue problem.

We agree and we have removed the adjective "analytic" from the majority of the manuscript where a numerical assessment was involved. As the referee acknowledges, Chebyshev polynomials are just a basis for the projection of the streamfunction and they are characterised by an exponential convergence as typical of spectral methods. However, the analytical form of the solution (obtained with pen and paper) is absent even if the solution is nearly exact.

Lines 181-182: In what sense is it counterintuitive that the nonlinear solution is more dissipative?

The presence of nonlinearities is associated with turbulent effects by the onset of an energy transfer towards other scales. This energy transfer across scales often leads to the divergence of the solution and to the onset of turbulence in fluid dynamics problems (e.g., flow in a tube, Kundu & Cohen, Fluid Mechanics, 2013), but instead here the nonlinear terms appear to smooth the solution. This is also pointed out by the stability analysis. In the linear case with narrow jet ($\sigma_J = 5°$ and jet latitude at 45° N) the flow becomes linearly unstable with a jet intensity of $U_J \approx 20$ m/s, while the nonlinear simulation becomes unstable at $U_J \approx 28$ m/s, highlighting an unexpected stabilizing effect of the nonlinear terms. We have removed the adjective counterintuitive in the revised version of the manuscript since the damping/amplifying effect of nonlinearities is probably an heuristic feature that requires more investigation.

Perhaps the appendix can include some more details of the computational method, such as the matrices D(1) and D(2), and how the time-dependent solution is calculated.

The matrices D_1 and D_2 are obtained by considering the first-order derivative of the Chebyshev polynomials (grouped as a matrix T where the columns are the polynomials and the rows are the colatitude locations). The derivative polynomials can be collected by means of a matrix T_1. The matrix D_1 can be written as $D_1 = T_1 \cdot T^{-1}$ so that the matrix T^{-1} converts the streamfunction distribution into the associated polynomial coefficients and T_1 provides the derivative in physical space. This discussion is not strictly necessary in the article and we fear that including it would shift the focus on the numerical details rather than on the methodology, so we prefer to omit that. However, we provide a reference to Peyret that goes sufficiently in detail about estimates of these matrices and on how round-off errors can be reduced.

Regarding the time integration, we provide details in equation (A7): The matrices A, Λ and P (together with their inverses) are computed in the pre-processing stage once and for all, together with the Rossby waves features. At any arbitrary time t only a few matrix multiplications are needed to obtain the solution evolution in the stable case (in the unstable case it works well too until the unstable eigenmodes become too large). No time integration (or time stepping) is needed in the linear stable case, and we now explicitly state this in the Appendix.

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Comments to Reviewer #2:
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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.

> *While the manuscript is well written and the figures well prepared, it is not fully clear what this paper is about. The title and introduction suggest that the manuscript is about an assessment of waveguidability for different background flows, whereas the results mainly focus on introducing a solution technique for Rossby waves, which is not entirely novel in its design. The discussion and conclusions leave the reader wondering how the introduced methodology is aiding the overall question on waveguidability for different flow configurations, as only very few highly idealized setups are tested. Hence, given the more technical character of the manuscript and lack of presentation of direct scientific usage of the method, this manuscript is not suited for Weather and Climate Dynamics in its current form and a resubmission to a more technical journal, such as Geoscientific Model Development, should be considered after major revisions have been implemented.*

Although we agree with the reviewer's critique that the structure and the aims of the paper can be made more intelligible, we believe that the novelty of our contribution is not exclusively methodological (see for example the double jet results presented in Sect. 5). Indeed, what we seek to do is propose a new paradigm to study the flow evolution of forced Rossby waves that rapidly enables to investigate in principle any background flow and forcing combination. As far as we are aware, the "very few idealised setups" we present are currently the largest systematic collection of background flows investigated in any single paper in the literature (see e.g. the new Fig. 3

of the manuscript, namely Fig 1 of the present reply). Moreover, since our study specifically speaks to the meteorological community, we believe that Weather and Climate Dynamics is the best-suited outlet for its publication. The consideration of idealised setups should not be an issue per se, as WCD's scope explicitly includes "idealized numerical studies", by the journal's own description.

To better understand the innovative character of our approach, we compare it to the classic ray-tracing approach proposed 40 years ago by Hoskins and Karoly (1981), and still used in the literature today. The ray-tracing technique requires the tracking of the wave during its evolution. However, a wave written in the form

$$\psi = \widehat{\psi} \exp\left[i\left(kx + ly - \omega t\right)\right] , \tag{1}$$

is correct only when the background flow U is constant in the β plane. This requires several approximations to apply this simple theory to general flow over a sphere:

1. A Mercator projection of the flow field
2. The background flow must be slowly varying compared to the spatial scale given by the wavelength of the Rossby wave
3. Although not an intrinsic limitation, in practice the analysis is typically conducted on stationary waves

Under these assumptions we can use WKB theory and extend the classical Rossby solution to more realistic cases with jet streams, for instance. However, we lose information about how the waves are evolving in time or whether the flow will ever approach a steady state: Wirth (2020) noted that, when a strong jet stream was present, the Hovmöller diagram did not approach a steady state and filtering was required to get it.

The approach that we propose is completely different from previous works since any generic zonal background flow can be considered and no ray tracing is required. The waves are obtained as a result of the equations independently from the forcing so that general conclusions can be obtained. No assumption of scale-separation is required nor do we need to project the flow in a Mercator plane, with the consequent deformation at the poles. Furthermore, a stability analysis can now be performed systematically, enabling an estimate of the flow evolution. Even the interpretation of the nonlinear simulation results is

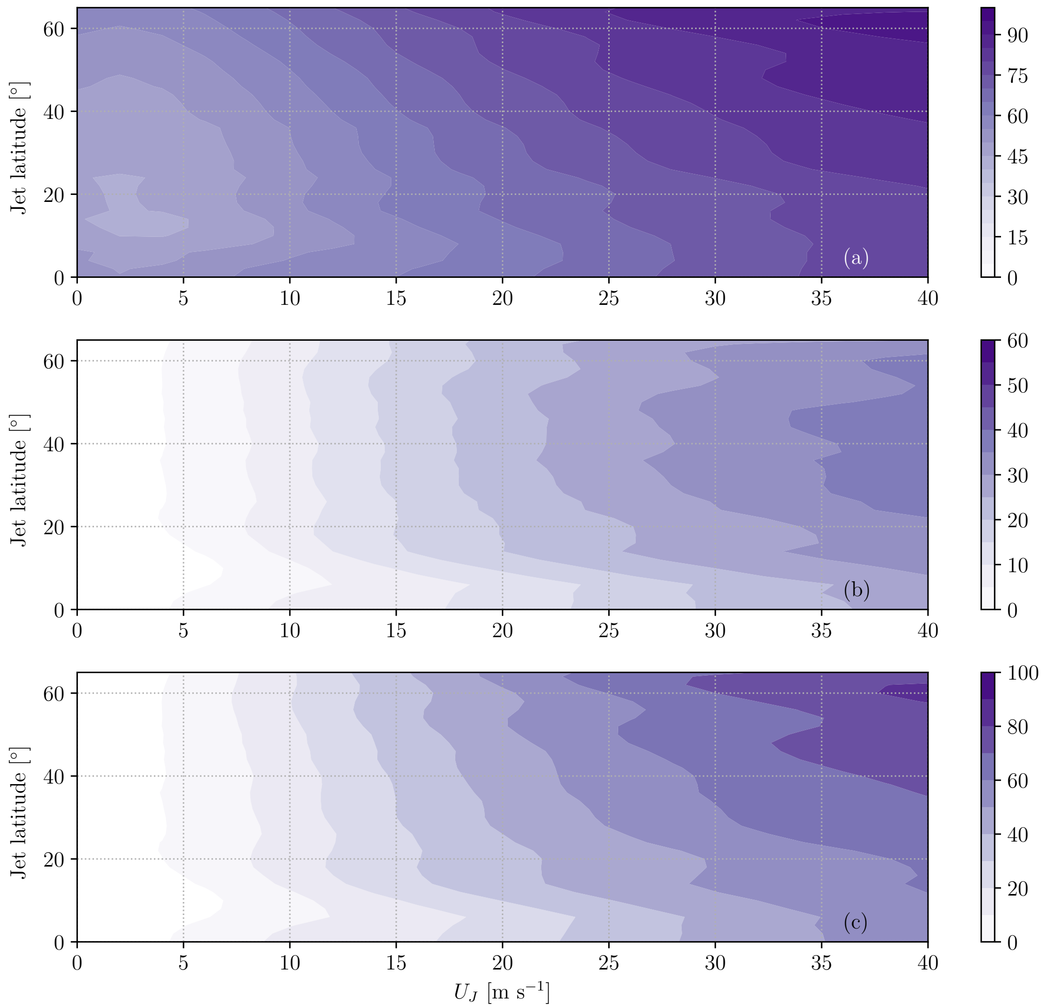

Figure 1: (a) Enstrophy meridional density of the single jet zonal profiles for different jet latitudes and strengths, estimated according to Eq. (17) of the revised manuscript. The enstrophy field has been computed from the equilibrium state obtained from Eq. (12), namely from the linear method. (b) Difference $E(U_J,\, \varphi_0) - E(0,\, \varphi_0)$ used to highlight the increment in enstrophy density with the jet speed with respect to the solid-body case. (c) Calculated value of waveguidability according to Eq. (18) of the revised manuscript.

much facilitated by the proposed linear framework, for example in terms of obtaining a lower bound for (nonlinear) instability onset. Therefore, we see the added value of this work for the atmospheric dynamics community as a paradigm shift in wave propagation analysis. While the technical details of our approach are essential to ensure reproducibility of the study, they should be viewed as functional to the proposed new wave analysis paradigm, and not as a key result in themselves. To address the Reviewer's concerns, we have now reworded the final two paragraphs of the introduction to frame more clearly the motivation for our work. We have further modified the title to shift the focus from waveguidability to wave propagation, which is the core of our results. The new title now reads: "A linear assessment of barotropic Rossby wave propagation in different background flow configurations". Finally, we have clarified earlier on in the introduction the key limitations of ray-tracing that our approach overcomes.

> *The very notion of waveguidability defined as in line 31 demands an a priori philosophical choice about the separability of the atmospheric state into a "basic state" and "wave perturbations". In the beginning of the second paragraph, the authors first emphasize the relevance of waveguidability for extreme weather events but at the end of the same paragraph the authors state that the assumption of separating the perturbation from the basic state is violated during extreme events. What should the reader take home from this obvious contradiction? Are there particular limits that the authors would like to point the reader to?*

As the reviewer notices, we are indeed trying to point the reader to a contradiction in how previous research on the topic has been conducted. The first paragraph introduces Rossby waveguides and discusses how they have been invoked by several studies as decisive contributors to recent extreme weather events. The second paragraph, on the other hand, cites research work that has pointed out the limitations of available diagnostics and theoretical frameworks to diagnose Rossby waveguides. For instance, Wirth and Polster (2021) noticed how the methods used in the previous studies can lead to misleading conclusions, pinpointing to the presence of waveguides even if there aren't any. This critical review of the literature aims at highlighting this contradiction to the reader and at justifying attempts to move forward, towards an improved understanding and a shared definition of Rossby waveguides.

We agree with the reviewer that any wave analysis requires the separation of the flow into a background component (usually steady or slowly changing in time and in space) and a wave component (as a perturbation that is faster in time and characterized by smaller spatial scales). However, the question about how we can identify the background flow remains unsolved and it is not our aim to address this very important task. Wirth and Polster (2021) have recently investigated this problem by applying various spatial and temporal filters to artificially perturbed potential vorticity fields to determine the ability of these methods to identify the (known) background flow. It was noted by Wirth and Polster that standard average methods work well when disturbances have small amplitudes and are not persistent, while large-amplitude waves influence very much the identified background flow. If the background flow is erroneous, the associated waves (obtained from the present analysis for instance) are erroneous too, underlining the high importance of this task. Once again, it is not our aim to identify the actual background flow from a snapshot of ERA5 data, for instance. Instead, the goal of this work is to assess the wave properties given the background flow where they operate. In this respect, our paragraph in the introduction is informative and provides a note of caution about a task that should not be underestimated.

> *In the final paragraph of the introduction, the authors state the actual content of the paper, though the very aspects that they test are not really motivated in the previous paragraphs of the introduction. What is the actual question at hand? What is the context of this study? If this study is only about the linear solution for various basic states, one won't be able to address the conundrum pointed out in the second paragraph, and in a way most of the wave refraction arguments have already been put forward in previous publications two to three decades ago. The comparison to the non-linear simulation can provide an assessment to the limits of the analytic solution, though it is not assessed in greater detail in the manuscript vis-à-vis the limitations of philosophical choices to thinking in a framework of waveguidability.*

As outlined in our first reply, the approaches currently used in the literature for the study of waveguidability have a number of conceptual and practical limitations (we highlight in particular three main ones on p. 2 of this reply document). One way to phrase the scientific question behind our work could be: is it possible to overcome these limitations? In this study, we set out to develop a new paradigm for the study of flow evolution of forced Rossby waves, which provides a computationally efficient, forcing-independent wave solution applicable in principle to any background flow. Crucially, this also overcomes the above-mentioned limitations present in the literature. The benefits of such an approach compared to the state of the art on the topic are multiple, as again outlined in our first reply. Concerning the link to the introduction, 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. We have nonetheless shortened the introduction and limited the discussion of background flow derivation, to provide a more focused text in the spirit of the Reviewer's comment. Concerning the Reviewer's comment on the linear solution, we argue that even without invoking the comparison to the nonlinear case, our results provide new understanding compared to the literature. Specifically, we can understand how one approaches the steady-state solution and even infer physical information about the temporal evolution of the waves when there is no steady state (e.g. in the strong jet case). To our knowledge this is not something presented in previous analyses. The linear simulation additionally facilitates the interpretation of the nonlinear case by providing a lower bound for the onset of (nonlinear) instability. We see the above, and in particular the inferences we make on the nonlinear (in)stability from the linear analysis, as independent of a waveguidability assessment. Indeed, the very concept of waveguidability is complementary to having a full wave solution.

> *From line 109 onwards the authors state that the question arises as to how the equilibrium state is obtained and subsequently mainly address the homogenous time-evolving part of the solution that does not project onto the forcing and thereby the equilibrium state. This is rather confusing, as these transient modes, stable or not, will not project onto the stationary forcing and the equilibrium state. It is thus unclear what the authors try to achieve and construct. In the ensuing section, indeed only the forced response is focused on again. The authors need to more clearly outline the rationale of their work, as it is currently difficult to follow what they aim to achieve.*

We agree with the reviewer's comment and we modified the structure of the results to address it. In particular, we now discuss separately the equilibrium state and the transients (now shifted to section 6), avoiding to jump back and forth between the two. Hoping to further clarify the results, we discuss now why the stability analysis and the analysis of the transients are important elements in the rationale of our work. The equilibrium point can be conveniently obtained by eliminating the time derivative and therefore looking only at the stationary solution. This was already done by Hoskins and Karoly (1981) for instance. However, if the steady-state solution is linearly unstable, then this state will not be achieved (within the linear formulation). Therefore, inspired by dynamical system theory, we named this solution an equilibrium state where the temporal derivative is zero. If this state is stable, the system will evolve to approach the equilibrium solution regardless of the initial condition (for a linear system there is only one equilibrium state). Otherwise, the linear system should diverge from that. Our interest is not the determination of this state but rather if it is ever possible to achieve and how. One of the most surprising discoveries in this work has been the observation that, even if the equilibrium state is unstable, the state of the nonlinear system oscillates around this equilibrium condition.

The comment of the referee about the homogeneous/forced part is partly correct. It is true that the modal basis is obtained for a homogeneous system, but it is also true that the forced system response is obtained by means of the variation of constants method where the same functions are exploited to get the solution (A6). Equation (A7) provides the full solution of the steadily-forced system. When looking at the time-dependent forced response, this is once again known analytically for the linear system once the eigenfunction and eigenvectors are known. We intended to show this by including the new figure 12 (figure 3 in the previous version of the manuscript) where the temporal evolution of the solution is shown. While most of the present work is still focused on the equilibrium state, we felt that it was important to highlight that our approach also enables a study of the temporal evolution.

> *Also in the results sections the focus is more on the actual method and its performance when compared to other solution techniques. The authors also include a discussion on the influence of model resolution on the performance of their method. While this is all interesting and relevant, it again emphasizes the more technical character of this manuscript with an absent focus on actual applications to more general background flows.*

We hope that the modifications we are bringing to the revised manuscript will further emphasize the physical aspects behind Rossby waveguides, rather than the technical details. The grid-convergence analysis as well as the comparison with the solid-body velocity distribution $U_\lambda = 15 \sin\theta$ (where the analytic solution of the linear problem is known) is placed at the beginning of section 3 "Model validation" to validate the model, namely to assess its consistency with what known already from the literature. The comparison of the waveguidability estimated by Wirth (2020) by means of nonlinear simulations is a warning to the reader to keep in mind that linear and nonlinear estimations are not always the same and, in particular, to highlight that linear simulations provide qualitative trends that are easy to interpret in light of the system's linearity.

We have tried to limit the technical details within the manuscript as much as we could,moving some more technical parts in the paper appendix. However, we are keen to ensure full reproducibility of our results, and thus believe that it is important to have a detailed description of the methodology, even if this may partly dilute the focus from the physical insights obtained by its implementation. Once the methodology is introduced, it provides a new way to interpret linear and nonlinear simulations and to understand the wave evolution in a more general and systematic framework that we believe to be valuable.

We recognize that we did not discuss sufficiently waveguidability in a general context and we have now included the new figure 3 in the revised manuscript (also provided as figure 1 of the present reply document) that provides the waveguidability (calculated by means of a new formula that extends the one proposed by Wirth (2020) and able to account for jets at different latitudes) for a variety of single jet latitudes and strengths. We highlight that such a figure would have been very demanding to produce without the new approach we propose. Furthermore, we have now reported the waveguidability values in the double jet case in Sect. 5. We hope that these improvements highlight the physical insights brought by our analysis, and the fact that our study goes beyond the simple description of a new method.

> *For the strong single jet case, the authors discuss unstable solutions and even perform an EOF analysis on the non-linear simulation. The relevance of this to the presented solution technique is unclear. The authors discuss some of the linear unstable modes in the light of the identified EOFs, though indicate that the match-*

ing is not convincing. The ensuing subsection on stability analysis is therefore also difficult to contextualize with the rest of the manuscript. In particular, it is unclear if the authors present the unstable modes to discuss instability, or if they present the unstable modes to assess the validity of their linear method. This confusion relates back to the general comment further above about the general topic of the manuscript being unclear.

The EOF analysis is presented to give information about the temporal evolution of the flow. Wirth (2020) noted that the strong jet case exhibited oscillations that did not decay even after a long time. He solved this point by taking the temporal average of the simulation, as we also did. The comparison between the temporal average and the equilibrium state was judged reasonably good and is reported in the manuscript. At the same time, it was strange to admit that, according to our analysis, the strong jet case was linearly unstable and therefore it should diverge from the equilibrium state, which it did not in the nonlinear case (what we consider as the ground truth). In order to explain this paradox, we started looking at the EOFs as a way to simplify the temporal variation now summarized by the modal coefficients. The first mode was provided by the time-averaged solution, while all the other modes were traveling waves centered at the jet location (irrespective of the forcing location), and their topology is strikingly similar to the most unstable modes. A discussion about this is now reported in the revised version of the manuscript in Sect. 4.2.

The characteristic of these traveling waves is actually not relevant if only the time-average state is of interest (as in ray-tracing theory). However, at any instant the unstable linear modes should be the ones growing the most and receiving the highest energy from the background flow. In a linear system they should just grow unbounded, while in a nonlinear system their growth should not be unbounded and their energy is transferred to other wave components by nonlinear interactions. Most likely we were expecting that the linear unstable modes will keep competing to receive the transferred energy and start growing again. Therefore, we wanted to point out that the linear analysis, much less useful in the unstable regime, was still providing useful insight since the first EOF modes (the first is just the mean in our analysis) were actually very similar to the most unstable waves identified in the linear analysis. Practically, this implies that the most energetic EOF modes are approximately known from the linear analysis and one does not need to compute them a posteriori with the EOF methodology, enabling the development of a reduced-order model. We now mention this possibility at the end of Sect. 4.2.

> *The double jet discussion is interesting and in fact one of the parts of the manuscript that also makes a scientific contribution beyond the technical aspects. However, most of the findings there are not necessarily new or unexpected and should thus be put in context to existing literature on wave refraction, ducting, and tunneling.*

Following the referee's suggestion, we decided to extend this section of the result: we calculated waveguidability also for double-jet configurations and included a new figure for the single-jet case (new Fig. 3). As far as we are aware, neither of these results has previously been presented in the literature. For example, despite the large interest in the subject (e.g., Rousi et al. 2022), to our knowledge there hasn't been any quantitative assessment of waveguidability for double jet configurations. The resulting waveguidability values are, at least for the authors of this manuscript, not necessarily intuitive. We thus believe that the results in the revised manuscript are indeed new and partly unexpected.

> *Overall, it is not clear how the presented approach is novel or how it yields additional information compared to more traditional linear approaches to assess wave propagation, such as the method the authors compare their results to (spectral harmonical method). If their method is arguable superior to existing methods, this should be made clearer in the manuscript.*

We have already mentioned that the proposed methodology goes beyond limitations of the ray-tracing theory, namely that

1. steady and unsteady waves can be discussed

2. waves without scale separation from the background flow can be analyzed

3. no need to perform Mercator projections without the consequent deformation at the poles

We feel indeed that new physics can be investigated with more confidence since the framework enables for a systematic assessment of the wave propagation in both steady and unsteady conditions, namely to understand how we approach the steady-state solution (if it exists).

The reviewer also raises a point concerning spherical harmonics. In our analysis, we opt for Chebyshev polynomials (compared to spherical harmonics that use Legendre polynomials) as mentioned in the manuscript appendix. The choice of Chebyshev polynomials was motivated by their mathematical properties. Legendre polynomials are used in spherical harmonics but they necessitate numerical integration to assess the spectral coefficients, while in Chebyshev polynomials the spectral coefficients can be computed analytically or even by means of the FFT algorithm. This choice influences only the meridional derivative operators. In the zonal direction we also use the Fourier transform, so spherical harmonics and the proposed approach are equivalent in the zonal direction. We now clarify this point in the Appendix.

> *L15: Hoskins and Ambrizzi (1993) should be stated, as it is probably the most classical reference in this context.*

Good suggestion. It is a very clear paper that we have now included in the revised version of the manuscript.

> *L16: Wave guiding also goes back to the early work on refraction of Rossby waves, so this sentence reads a bit redundant in the light of the previous sentence.*

We agree with the referee that redundancies should be avoided but at the same time we think that the referred lines are necessary for readers that may not be acquainted with the waveguidability concept.

> *L10-29: The first paragraph is rather long and the main topic is not clear. The paragraph might benefit from splitting it and more clearly addressing the context for this manuscript.*

We agree and we will rework the paragraph accordingly. The idea here was to motivate why Rossby wave propagation is important to understand extreme events as the latter are often occurring under special circumstances associated with amplified Rossby wave propagation.

L31: Waveguidability is explained here for the first time, while the reader is left wondering during the first paragraph about its meaning.

We now specify in the first paragraph that waveguidability relates to the "capability of jet streams to promote Rossby wave propagation". We then provide further details in the subsequent paragraphs. Section 4.1 has been added to analyse the waveguidability and to propose an extended definition able to cope for jets at different latitudes.

L61: Do the authors really mean "stability" in the sense of a wave instability or in the sense of applicability of the linear analytic solutions?

We thank the reviewer for their remark. We are indeed referring here to stability in the sense of temporal wave (in)stability. The fact that we have an analytical solution is not related to wave instability. As briefly mentioned in the manuscript, we developed an additional linear code based on spherical harmonics and we solved it similarly to what we did with the nonlinear code. With both spherical harmonics solvers the analytical solution was not at hand and only a numerical assessment was done. For certain conditions (for instance, when the single-jet speed exceeded 20 m/s), the solution of the linear system diverged to infinity, exactly as and when predicted by the proposed model. However, by means of a modal analysis we are aware of all the possible unstable modes (if more than one are present) and therefore we have more understanding about the flow evolution. However, this stability feature and the corresponding divergence, can be even investigated numerically (for instance by simulating sufficiently long in time until the instability grows), although here we preferred an analytical tool since it was at hand.

L74: It is confusing to refer to Lambda as longitude, which is not even used in the equations thus far, while at the same time using λ_r as a dampening parameter. The authors are encouraged to change the naming of the dampening parameter to avoid confusion.

We agree, we have now changed λ_r to χ in the revised version of the manuscript .

L147-153: Almost everything stated here is not new, even though the authors make it sound like a new discovery. Previous findings should be clearly stated and referenced.

As the reviewer points out, the facts stated in these sentences are not new. We strongly disagree about the comment that we are making them "sound like a new discovery", and indeed in the original text we stated that these are "well-known analytical results" and further underscore this by stating that "it is already known ...". What we do is to use this well-assessed material to validate our model, namely to quantify the discrepancy between the analytic solution (known in the case of solid-body velocity, without jets) and the proposed model estimation of the dispersion relationship. For instance, the error in the dispersion relationship (namely the value of the computed ω for a given m) was within machine precision (namely $O(10^{-11})$), while the waves were coincident with spherical harmonics with similar accuracy. Everything included in the validation section follows the same spirit. To avoid misunderstandings, we removed the word "Interestingly" from the sentence at line 147.

L163-173: It is not made clear to the reader why this resolution sensitivity study is performed and its relevance to the assessment of waveguidability.

A numerical algorithm is convergent when it approaches a constant value for sufficiently high resolution. The quantification of "sufficiently-high" is not determinable a priori, and requires numerical experiments. Thanks to this convergence study, we could state that a resolution of at least 128 latitudes is required to achieve good results. Smaller waves should require even higher resolution.