A linear assessment of barotropic Rossby wave propagation in different large-scale flow configurations
by
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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.

> *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

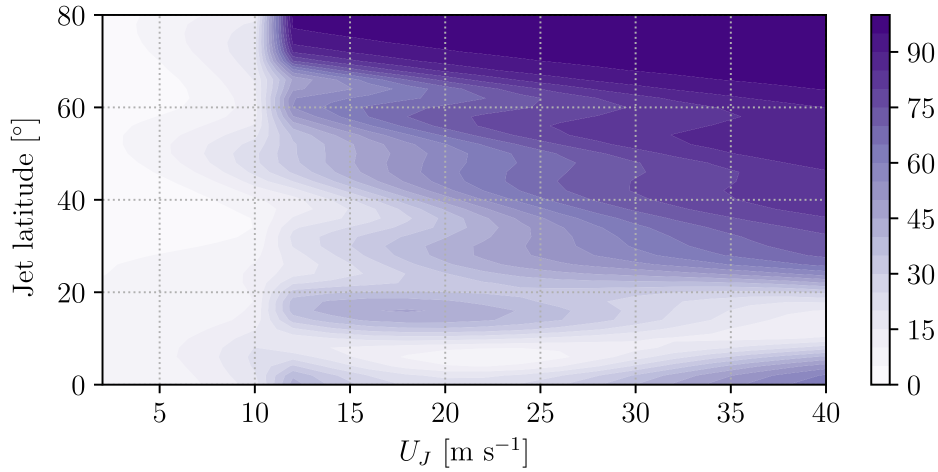

Figure 1: Waveguidability of the single jet zonal profiles for different jet latitudes and strength. Only narrow jets with $\sigma_J = 5°$ have been considered.

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 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 large-scale 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 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 matching 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. 4.1.

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. 4.1.

> *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 lin-*

ear 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.

> *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.