A linear assessment of barotropic Rossby wave propagation in different large-scale flow configurations
by
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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);
- 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 4.2.

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. 4.2 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*

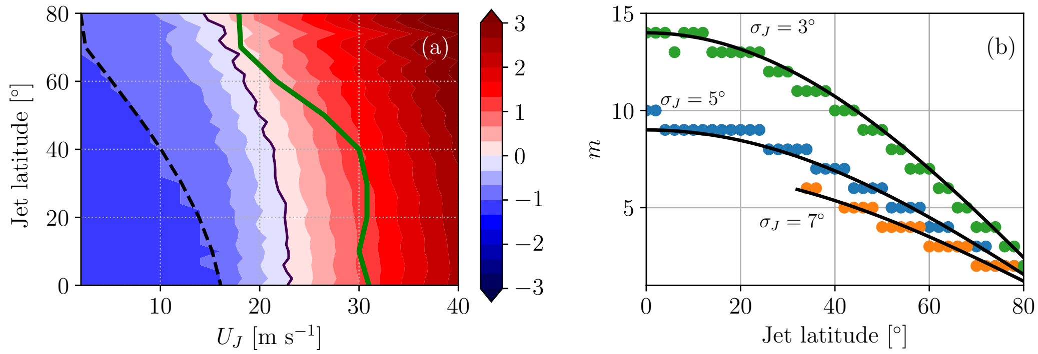

Figure 1: (a) Maximum of the imaginary part of the eigenvalues for a given jet velocity (normalised by χ) when $\sigma_J = 5°$. The black line indicates the neutral curve, the dashed line is the locus where the absolute vorticity gradient changes sign (Rayleigh stability criterion), while the green thick 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 the diagnostic by Wirth (2020). These results have been added to Sect. 5 as a new figure 3 (figure 2 of the present reply). The waveguidability of the two jets taken separately is around 37% for the southern jet and 58% for the northern one, so that the northern jet has more waveguidability than the southern one. When two jets are simultaneously present, the northern jet shows again an even higher waveguidability (98%) compared to the southern jet (71%). 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 introduction was confusing for me. I expected to see a solution expressed*

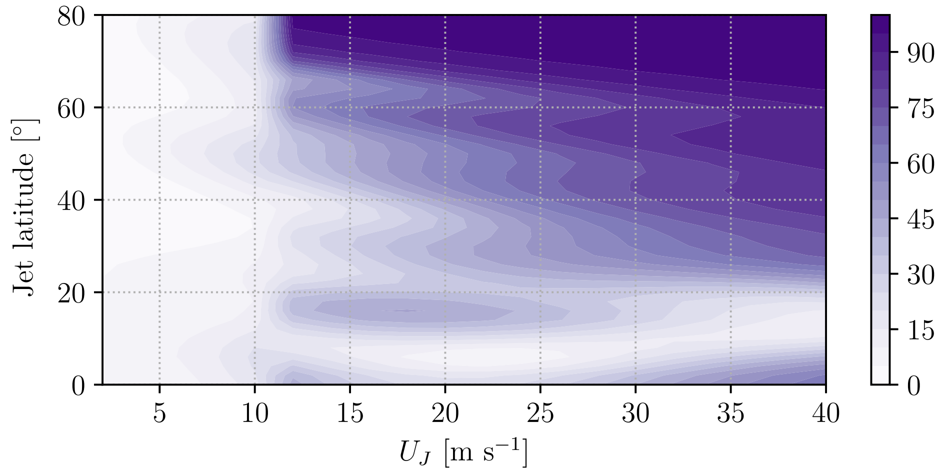

Figure 2: Waveguidability of the single jet zonal profiles for different jet latitudes and strength. Only narrow jets with $\sigma_J = 5°$ have been considered.

> *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 preprocessing 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.