A linear assessment of barotropic Rossby wave propagation in different background flow configurations
by

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

We appreciate the new feedback regarding our manuscript and the careful scrutiny of the reviewer. 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 referee have been highlighted in red in the revised version of the manuscript.

> *1) Lines 230-233: This sentence was added following my comment, that it is not a coincidence that the Rayleigh criterion curve in figure 4a coincides with the growth rate of -1 (normalized by the damping rate, $\xi$). I think this point should be written in a more explicit way. The current text says that "... whenever the absolute vorticity gradient changes sign, the imaginary part of omega can be different than $-\xi$...". But it is not mentioned that the dashed line in figure 4a shows exactly that the absolute vorticity gradient changes sign when the growth rate equals $-\xi$. This means that the Rayleigh criterion is relevant for the growth rate, since it gives exactly a zero growth rate if no damping is added, and the correction for the growth rate in the presence of damping is exactly the damping rate.*

Thanks for suggesting this clarification: we have now pointed this aspect out explicitly and slightly modified the wording to make the paragraph even more understandable. Now it reads:

"At low $U_J$, the growth rate of the most unstable eigenvalue corresponds to the dissipation rate $\chi$, which is negative (meaning that perturbations are dampened with the same rate). This assertion is valid as long as the absolute vorticity gradient has the same sign throughout the domain: as the sign starts to flip (dashed line in Fig. 4a), a transitional regime is reached where the eigenvalue growth rate is still negative, but not as low as $\chi$. This is due to the fact that, in presence of dissipation, the simple change in sign of the vorticity gradient (i.e., the Rayleigh-Kuo criterion) is not sufficient to ensure the onset of barotropic instability. As $U_J$ increases, the stability margin decreases: above 15-22 $\mathrm{m\,s^{-1}}$ (depending on the latitude) the growth rate of at least one eigenvalue becomes positive, leading to the divergence of the linear solution (bold black line in Fig. 4a). The nonlinear simulations, on the other hand, approach divergence for larger jet velocities than for the linear method: this is marked by the green line in Fig. 4a, denoting the locus where the meridional velocity time variance equals 2 $\mathrm{m^2\,s^{-2}}$ as an arbitrary threshold. Above such a threshold, the velocity variance increases drastically and it is concentrated at the jet location irrespective of the forcing, indicating the presence of barotropic instability. The neutral curve from the linear stability analysis provides then only a conservative estimate of the onset of instability: this underlines the stabilizing role of nonlinear terms (such as wave-wave and wave-mean flow interactions) not considered by the linear method."

*2) Line 234: "this results pinpoints" -> "this result pinpoints" (or "these results pinpoint").*

Done.

*3) Line 246: "such as wave-wave interactions" – I suggest to add also wave-mean flow interactions, as explained in my previous review. The same comment applies to line 395, where I suggest to change to "...pointing to potential damping or stabilizing effects operated by nonlinear terms (e.g. wave-wave and wave-mean flow interactions)".*

We agree and it has been changed in the revised version of the manuscript.

*4) Line 258: Following my comment the authors replaced "indeed" with "this is verified in...", but the problem remains that the previous sentence is not connected to the next sentence, i.e. looking at the wavenumber of the most unstable mode does not verify the analysis of Gill (1982) that relates to the growth rate rather than the wavenumber.*

Correct. We have rewritten the sentence with minor modifications to highlight that broad jets become stable while narrow jets are unstable at all jet latitudes.

*5) Line 284-286: "The nonlinearity of the waves was further confirmed..." – I didn't understand what this sentence is saying. Perhaps consider removing it.*

We have removed the sentence in the revised version of the manuscript. Our purpose was to highlight to the reader that we tested even different initial conditions but the regime wave amplitude remained the same.

*6) Line 290: "similarly to what suggested by..." -¿ "similarly to what is suggested by".*

Done.

*7) Lines 350-351: Is waveguidability assessed here around latitude 45? That's what it sounds like here, but later in lines 366-369 the waveguidability is compared with its values calculated around the jet latitude. This sounds like a strange comparison, where in one case the jet latitude and the forcing latitude are different and waveguidability is calculated around the forcing latitude, while in the other case the forcing latitude and the jet latitude are the same. If I misunderstood it, then perhaps it should be explained more clearly in the text. Otherwise, please explain the choice to calculate the waveguidability around the forcing latitude (when it is different from the jet latitude) and to compare it with a case where the forcing latitude is at the jet latitude.*

For the single jet case, the waveguidability is always computed at the jet latitude (where we located also the forcing). In the case of two jets, when we assess the waveguidability of one jet, we put the forcing at the same latitude of the jet. We added a sentence at line 357 to highlight that the waveguidability is computed at the jet latitude.