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

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

We appreciate the new 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 referee have been highlighted in red in the revised version of the manuscript.

> *Following my comments, the authors added an appendix (B) to show that a limit cycle exists in the nonlinear simulations. This helped me understand what limit cycle they referred to in the previous version, but I see it as an over-complicated way of looking at a simple phenomenon. Traveling waves and the limit cycle are in fact the same thing, as I elaborate in major comment 4 below. In any case, I think appendix B is not necessary, and this discussion deviates from the main theme of the paper, at least as I see it. I also still think that the authors are missing a much simpler interpretation of the results of the nonlinear simulations, based on the role of wave-mean flow interactions. I elaborate on this in major comment 2 below.*

Since this limit cycle analysis has been quite controversial and it leads to a description similar to traveling waves, we have decided to follow the suggestion of the reviewer and we have removed appendix B and dropped the discussion about limit cycles.

> *In the bottom line, even though the paper does present a method that could be useful for future studies, and demonstrates its usefulness, I think that some parts of it are written in a way that will not be understandable to the potential readers, and are presented in an over-complicated way.*

We hope that the revised version of the manuscript is more understandable since we have eliminated some of the more technical discussions.

Major comments

> *1) A general comment on section 4: It is hard to follow how this section is connected to the main theme of the paper. If I understand correctly, the purpose of this section is to compare the prediction for the steady state solution (and its stability properties) based on the linear model with the "real" solution of the nonlinear system. If this is indeed the case, that should be stated explicitly. I think that the analysis of the limit cycle in subsection 4.2 and appendix B is not necessary, because I don't see how it contributes to the goal of this study. Perhaps all of subsection 4.2 is unnecessary. In any case, its title "dynamics of the unstable modes" does not describe the content well, because the modes of the nonlinear simulation are not unstable. If the authors choose to keep subsection 4.2, I think they can shorten it and remove the limit cycle analysis, for the reasons elaborated in comment 4 below.*

A linear analysis is useful as long as all the modes decay since waves initiated by a perturbation from the background state (or the topographic forcing in our case) vanish in time and a steady solution can be obtained (in our case equation (11)). If at least one mode is unstable, the linear analysis becomes much less useful for the assessment of the steady state. Beyond the neutral curve equation (11) should be of no use since the field should be time dependent and the traveling waves could orbit around another equilibrium state. Nevertheless, the good agreement between equation (11) and the time average of the nonlinear simulation continues to be the case and this required a deeper investigation into the dynamics observed in the nonlinear simulations, the only reliable information beyond the neutral curve. Section 4 is planned by following this red thread. We have now highlighted this aspect in the revised version of the manuscript and changed the title in section 4.2 to "Waves evolution in the linearly unstable regime" as pointed out the the Reviewer.

> *2) The authors mention the role of wave-mean flow interactions only briefly and refer to it as a dissipative effect (lines 211-212). In other places it is ignored (lines 242, 282, 388-389). I commented on the previous version that a nonlinear simulation is*

expected to reach a statistically steady state with a mean flow that is stabilized (or neutralized) by the effect of the waves on the mean flow. It's not a dissipative effect on the waves, it's a stabilizing effect on the mean flow. In their response, the authors mentioned that "the linear model is linearized around the background state and not around the equilibrium state". They say that "the remedy is to linearize around the time average instead" but claim that "the linear analysis with a nonzonal background flow is more computationally expensive". I agree, but I don't see why not take the time average of the zonal mean flow in the nonlinear simulation and perform the linear analysis on that. I would expect to see that it is more stable than the imposed background flow. If that is the case, it would show that the nonlinear solution does not diverge because the mean flow is stabilized by wave-mean flow interaction.

We have reworded the sentence at line 212 mentioning the stabilizing effect of nonlinear terms instead. Whenever the linear simulation is analysed, these effects are ignored since they are not accounted for and they start to be taken into consideration whenever we observe significant deviations between linear and nonlinear simulations.

Regarding the linear analysis, it is not a problem to take the mean flow from the nonlinear simulation and perform a linear simulation around that to see what happens. However, it is harder to perform a linear stability analysis where the modes are obtained by means of an eigenvalue problem. This happens because we cannot anymore decompose the modes as independent waves with a fixed wavenumber in the zonal direction since zonal convolutions happen in the equations. We are currently working on a 2D eigenvalue problem to assess the Rossby waves on non-zonal background flows and we plan for a follow-up publication on the topic.

3) Lines 226-231: Even though the authors agreed with my comment on the relevance of the Rayleigh criterion for the interpretation of the stability analysis, they have not changed anything in this paragraph. It stayed the same as in the previous version. This paragraph is difficult to read, as it is phrased in a very confusing way. Actually, a much simpler interpretation can be given, as I explained in my previous review. The Rayleigh criterion predicts *the borders of the stability region in the absence of dissipation. In order to adjust for the inclusion of dissipation, all you need to do is to subtract the dissipation time scale from the growth rate. The results in Figure 4 are consistent with this idea. Therefore, I don't understand why the authors chose not to mention this simple explanation. This comment applies also to lines 385-386 in section 6.*

We have re-written the above mentioned lines in the revised version of the manuscript. It is true that the linear dissipation just shifts the neutral curve towards higher velocities. However, our statements remain correct: the change in PV gradient (the non-dissipative Rayleigh criterion) is not immediately associated to the onset of barotropic instability.

4) Here is why I think the limit cycle interpretation is an over-complicated way of representing traveling waves: Equation B1 defines the coefficients of the EOF modes (that are called "temporal coefficients" in other places). The authors claim in their response that these coefficients represent the amplitudes of the Rossby waves in the nonlinear simulation. I disagree, and I will try to explain why. Looking, for example, at modes 2 and 3 in figure 5, I see that each one of them represents a different phase of the same mode with wavenumber 5. Let's assume that this is a traveling wave. What would you expect the time trajectory to look like in the phase space defined by a2 and a3? It would look like a circle (as it does in figures 6 and B1a). This is because the traveling wave is equal to: a2(t)sin(m(x-x0))+a3(t)cos(m(x-x0)), where a2(t) = sin(omega(t-t0)) and a3(t) = cos(omega*(t-t0)). Here I described, for simplicity, the wave shown in figure 52 as sin(m(x-x0)) and the wave in figure 53 as cos(m(x-x0)). The authors wrote in the response that "If the orbit depends on the initial condition a traveling wave is present". But actually, what happens is that during the simulation (no matter with what initial conditions) the flow adjusts toward a statistically steady state that supports specific traveling waves. In any case, this discussion does not add much to the main argument of the paper, so I don't think it is worth going into so much detail about the flow evolution. The way I see it, the nonlinear statistically steady state represents a* *mean flow that was neutralized by wave-mean flow interactions, with traveling waves that, on time average, do not grow or decay. This interpretation is consistent with the results presented in this paper and with previous studies (I mentioned two of them in my previous review, and you can add to that a few papers by Brian Farrell). Lines 275-282 should be revised according to this comment.*

We agree with the reviewer that the limit cycle complicates the paper unnecessarily and we have dropped the limit cycle analysis in the revised version of the paper. We mention now those waves as traveling waves since, at regime, there is no difference between the two concepts. However, the limit cycle analysis provides an explanation about why the nonlinear simulation is not diverging, while the linear simulation diverges (in both the stability analysis and linear simulation approach with spherical harmonics). Following the Reviewer's suggestion, the papers from Hou & Farrell (1986) and Lachmy & Harnik (2016) have been included in the revised manuscript as they support the role of wave-wave interactions in maintaining an equilibrium mean flow.

5) A general comment on section 5: It is not clear why the authors chose to compare between the waveguidability in the linear and nonlinear case only for the double-jet case (subsection 5.3), whereas for the single jet case (subsection 5.2) only the linear analysis is considered. The authors should at least provide a motivation for these choices.

We have not performed any assessment of the waveguidability on nonlinear simulations in section 5. The only waveguidability assessment based on nonlinear simulations was shown in figure 2 just to compare with Wirth (2020).

6) A general comment on section 6: I think that organizing the paragraphs as a list ("Firstly... secondly... thirdly") does not aid the reader to understand the overall theme of the paper. It would help to explain instead how each part is connected to the main goal and how all the parts combine to one story. For example, in line 382, instead of writing "Secondly, we elucidate some features of Rossby waves...", it would be better to motivate it by *explaining that the linear analysis is compared with the more realistic results of the fully nonlinear simulations, in order to assess its ability to capture what happens in the nonlinear simulations. The paragraph that starts with "Thirdly" does not connect to the subject of the previous paragraph – the comparison between the linear analysis and the nonlinear solution.*

We have reworded the conclusion paragraphs of the revised version of the manuscript.

Minor comments

1) Lines 218-224: It's hard to follow this paragraph, because the text sometimes refers to the nonlinear simulations and sometimes to the linear solution, and it is not clear which is which. The nonlinear solution is mentioned and then it points to figure 3a, which shows the linear solution.

We apologize for the lack of clarity. We have now rephrased the paragraph to highlight the various parts of figure 3 and motivate the linear analysis.

2) Line 225: Is this sentence referring to the nonlinear simulations or to the linear solution?

To both actually. One can perform an analysis by looking at the temporal solution of the linear/nonlinear problem, or by looking at the eigenvalues. The former approach is typical of complex systems that do not allow for a modal analysis at a reasonable cost. We have now specified this in the main text of the manuscript.

3) Line 234: "the imaginary part of the most unstable eigenvalue" – why not use the term "maximal growth rate" here and in other places in the paper?

This is a good suggestion that enhances the readability of the paper. We have now replaced imaginary part with growth rate throughout most of the manuscript.

4) Line 235: "...has the same sign" – add after this "throughout the domain", or change to "...has a uniform sign".

We have now changed the sentence to "a uniform sign throughout the domain".

5) Caption of figure 4: "(equivalent to the neutral curve in the nonlinear case)" – this is not the appropriate phrasing. You are using an arbitrary threshold for defining a "neutral curve", so "equivalent" is not the right word. You could replace it with "(the chosen threshold for defining the neutral curve in the nonlinear case)".

We have now replaced the sentence to "the chosen threshold for defining the neutral curve in the nonlinear case".

6) Line 236: What do you mean by "stability margin"? You mention the range 15-22 m s^{-1}, but it's not clear what feature in the figure the reader should look at to see this.

According to Cambridge dictionary a margin is "the amount by which one thing is different from another". We were mentioning that, while the PV gradient changes sign for some jet velocities, the onset of linear instability happens for larger velocities. We have now clarified what we meant with the velocity range by rewording the sentence to "above 15-22 m s^{-1} (depending on the latitude) the growth rate of at least one eigenvalue becomes positive".

7) Lines 238-242: One gets the impression that the threshold of 2 m^2 s^{-2} represents an abrupt transition of the nonlinear simulations from low velocity variance to very high velocity variance (it says "increases drastically"). Is this really the case? If it is indeed an abrupt transition at this specific value, it would be good to show that in the figure. If not, then the phrasing should be changed to clarify that the threshold was chosen arbitrarily.

The figure 1 below shows the meridional velocity variance from several nonlinear simulations for different jet positions/strength. The red line, indicating the locus with variance 2 m^2 s^{-2}, is also shown. Two color scales are used, one logarithmic when one sees the rapid increase in the variance (and

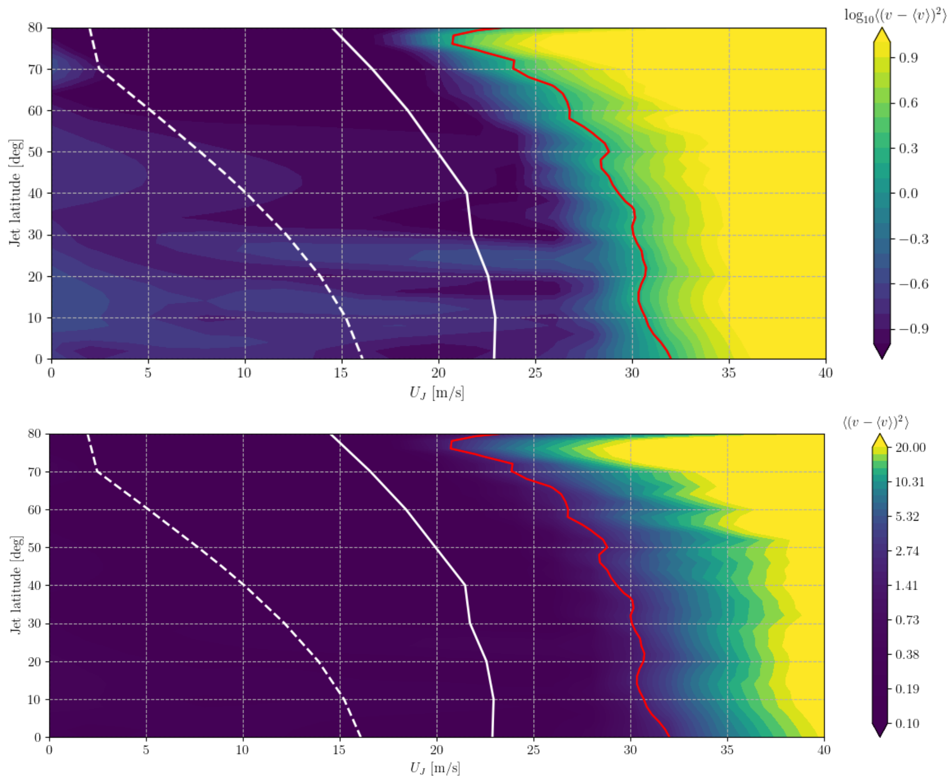

Figure 1: Meridional velocity variance in the nonlinear simulations for different jet velocity and jet latitude plotted. (Upper panel) logarithm of the variance. (Lower panel) linear value of the variance. The red line indicates the locus where the variance is 2 m^2 s^{-2}.

the red line is centered around that), and a linear one where the growth is visible and the red line is indicating more the start of the unstable region. The threshold of 2 m^2 s^{-2} was chosen to sort out small amplitude variances in the stable region and it is arbitrary. We have clarified this aspect in the revised version of the manuscript.

> *8) Line 251: "which is indeed proportional to cos(phi)" – but the equation says that L is constant. I suppose you mean to say that this gives a wavenumber that is proportional to cos(phi).*

Correct. We have corrected the sentence in the revised version of the manuscript.

9) Line 252: "at different latitudes of the zonal jet" – add "of a given width".

Done.

10) Line 253-255: It is not clear how the first sentence, that relates to the degree of instability, is related to the second sentence, that relates to the wavenumber. The word "indeed" seems not appropriate here.

The sentence has been changed to "This is verified in Fig. 4b that shows ...".

11) Line 269: A reduced-order model of what?

We have changed the sentence to "a reduced order model of the velocity field" in the revised version of the manuscript.

12) Line 301: I assume the time averaging refers to the enstrophy and not to the vorticity anomaly. Please change the wording accordingly.

Since the estimation is based on the linear stability results, there is no averaging involved, as done for instance by Wirth (2020). The equilibrium vorticity field is first subtracted by the background vorticity, the result is then squared and processed as described in the manuscript. We have clarified this aspect in the revised version of the manuscript.

13) Line 302: The comment in the parenthesis is not clear. Why not use the time average? The statistically steady state represents fluctuations around a time-mean state, so an instantaneous state is not equal to the time average.

Correct. We have removed the sentence.

14) Line 307: It would be clearer if the words "when assessing how strong the jet stream is" were deleted.

Done.

15) Lines 314-315: It would help the reader if the authors point to the relevant features to look at in figure 8.

We thank the Reviewer for this suggestion. We have added some sentences to clarify the motivation of the analysis. We have constructed figure 8 to show the main steps of our analysis. At the end a gradual waveguidability trend is present but at least now bounded between 0 and 100% providing a better metric than what available from previous definitions.

16) Line 331: "Increases with jet speed from 0 to around 90%" – The jet speed values should also be mentioned, otherwise this statement is meaningless.

There is a gradual increase of W for all the simulated range, so the figure provides quantitative support to the qualitative text.

17) Figure 8: Equation 19 is not expressed in percent, but in a dimensionless number between 0 and 1, but the variables in the figure are in percent. Please clarify this in the caption.

Done.

18) Line 343: Delete "once again". Also, the location of the forcing should be mentioned.

Done.

19) Lines 350-351: It should be clear if this calculation is for the linear analysis or for the nonlinear simulations.

Right. We have clarified this point in the revised manuscript. We only use the linear method for the assessment of W.

20) Lines 360-364: It is not clear where the forcing is located in each of the cases mentioned.

We have clarified that at the beginning of the section in the revised manuscript.

Language/typos

1) Line 75: "...in sections 4 and 5.3." – delete the "3" after "5."

Done.

2) Line 211: "The difference in waveguidability metric" – add "the" before "waveguidability".

Done.

3) Line 220: "some eigenvalues have in fact positive imaginary part" – delete "in fact" and add "a" before "positive.

Done.

4) Line 238: delete the word "time" before "variance".

The variance can be computed in space too, so we feel that it is important to clarify here.

5) Line 245: Change "associated to" -¿ "associated with".

Done.

6) Line 295: the word "metric" is written twice.

Thank you for noting that.

7) Line 351: Change from "jets with same velocity" to "jets with the same velocity, but different latitudes".

Done.