Review of egusphere-2023-316

"A linear assessment of waveguidability for barotropic Rossby waves in different large-scale flow configurations"
by
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Recommendation: Reject - consider resubmission to a more technical journal after major revisions

General Comments:

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.

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?

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.

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.

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.

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 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.
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.

Specific Comments: (Reference to line numbers in the manuscript)

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

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.

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.

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

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?

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 Lambda_r as a dampening parameter. The authors are encouraged to change the naming of the dampening parameter to avoid confusion.

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.

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.