Final author comments

We thank the reviewers for carefully reading our manuscript and making insightful comments. We agree with most of their items and will be happy to implement their suggestions. In this comment we focus on the major issues that require some discussion.

1 Restricted choice of basic states

Both reviewers suggest that we could consider a more general class of basic state wind profiles. This is a fair comment, especially in light of the fact that observed wind profiles show large variability and usually include a critical line. By contrast, in our manuscript we restricted attention to rather idealized wind profiles and excluded the occurrence of critical lines. These are the reasons why we prefer to stick to our restricted choice of basic states:

- The main point of our paper is not an exhaustive investigation of different basic states. Rather, the main point is to systematically compare the two diagnostic methods, and to show with the help of a few selected basic states that the refractive index method comes with a number of issues. At the same time we aim to improve our understanding of these "issues" by connecting our results with results from earlier papers. To reach this goal we consider it sufficient to restrict attention in this paper to a few carefully selected basic states.
- The paper is already rather long (as pointed out by one of the reviewers). The discussion of more general basic states, and in particular the (thorny) issue of critical lines warrant a separate publication, allowing one to discuss the relevant points in some detail. In fact, we are currently preparing a forthcoming publication that is going to do just that. In our revision, we plan to point the reader to this forthcoming publication and give a very brief preview to its main result.

2 Use of WKB with matched asymptotics

The reviewer I. Held comments our section on the use of WKB with matched asymptotics. He points out that (i) the analysis should better been done after a transformation to Mercator coordinates, and (ii) our analysis does not account for the phase change that one obtains when a wave tunnels through a forbidden region. We agree with both items. Indeed, a similar but less approximate analysis in Mercator coordinates (performed by us in the meantime) indicates quantitative differences regarding the change of phase in Fig. 12b. Regarding an additional phase change at a leaky boundary, such a phase change will occur if there is some back reflection into the waveguide from the region outside the waveguide, as would occur in a narrow tunneling region with weak damping. In the paper of Harnik and Wirth (2025) we assumed that all the wave activity that leakes out is absorbed (none reflects back) and thus there is no additional phase change. This, we now realize, is not the most general form of a leaky boundary condition. Such a change in phase at a formal boundary may have a considerable impact on the conditions for resonance.

We will say more about these important issues in our upcoming point-by-point reply, and we are tempted to write a short note or a correction to our Harnik and Wirth (2025) paper by generalizing the boundary condition to include phase shifts.

Given this state of affairs, we decided to drop the section on the use of WKB with matched asymptotics, because a thorough discussion is way beyond the scope of the present paper. At the same time, none of our results depend on this section. Therefore, this change will make the paper shorter and more focused, which we think is an improvement.

3 Sponge design

We agree with the reviewer that the sponge design implies "a bit of art". In fact, we have elaborated quite extensively on the sponge design in section 4a of Harnik and Wirth (2025). In that section, we experimented with two types of sponges in our desire to obtain a sponge that is both strong enough and slowly-varying enough such that it absorbs any wave activity that impinges on the sponge without leading to any wave reflection.

Nevertheless, there is always the danger of becoming overconfident, and it is worth checking the functionality of the sponge in each individual application. For this reason we performed, in the meantime, a separate analysis with the aim to test the sensitivity of our current results with respect to the sponge design.

As it turns out, our key parameters s_{res} and Q do not depend at all on the strength of the sponge. We decided to mention this important fact in our revised text, however without adding a comprehensive description of the whole sensitivity analysis. More details about this sensitivity analysis will be given in our forthcoming point-by-point reply.

4 Mention the singular peak from the Cartesian version?

In our original manuscript we mentioned the fact that the current spherical version of our method does not feature the very sharp resonant peak that we found earlier in Wirth and Harnik (2026) under the use of Cartesian geometry. We plan to follow the reviewer's suggestion and omit this section in the revised version. To be sure, this is a rather technical detail, which does not have any implication on the results of the paper. However, we also plan to discuss this issue in our upcoming point-by-point reply as part of our internal discussion with the editor and the reviewer.

5 Style of language in the sections containing critique

Both reviewers mention our style of language in those section that contain critique with the refractive index method. By no means we want to be rude, dismissive, or offensive, although we think that objective criticism is allowed and even warranted in a scientific article when necessary. We plan to locate all places where we run into the danger of using blunt language and tone down our formulations without watering down our intended statements.

6 Our section on the "physics of resonance"

One reviewer suggested that we could omit our section on the "physics of resonance". We agree that this section contained mostly textbook material from theoretical physics. On the other hand, it seems to us that some proponents of the refractive index approach use descriptions of the underlying mechanism which in our opinion do not hit the key point. For this reason we think that a short section to review the physics of resonance may not be completely out of place. In the revised version we plan to (i) shorten this section, and (ii) say more explicitly why we think that it is useful for a deeper understanding of our diagnostic method.

7 Novelty of our contribution

One reviewer pointed out that the original manuscript did not make it entirely clear what was new in this analysis, given that we had already quite a number of precurser papers on similar topics.

To be sure, issues with the refractive index approach have been mentioned in several previous publications. However, there has never been a paper in which this approach has been directly confronted with an alternative approach that uses less approximations and is, therefore, considered more trustworthy. Given the popularity of the refractive index approach, we think that such a direct comparison is important, and this is what we do in the current paper.

In our revision we plan to rewrite the introduction with a two-fold aim:

- give a short discussion of our previous papers on related issues (as suggested by the other reviewer), and
- point more explicitly to the novelty of the current work in the light of those previous papers.

8 Waveguide criterion vs. further criteria

The diagnostic method of Petoukhov and Kornhuber consists of a number of criteria, all of which are connected with a logical "and". In the main part of our paper we only address the so-called "waveguide criterion", but towards the end of the manuscript we also mention the other criteria. One of the reviewers wonders whether we shouldn't confine ourselves to the waveguide criterion altogether, and leave the other criteria out of consideration.

In our revision we decided to not follow this suggestions for the following reason. The main goal of our paper is the comparison between two methods, both of which are meant to diagnose the occurrence Rossby wave resonance. Our direct numerical method does this job rather directly. By contrast, the refractive index method implies a combination of several criteria, and only the combination of these criteria give the full diagnostic. Therefore, it seems impossible to compare just one of these criteria with our comprehensive method, because that

would be like comparing apples with pears. For this reason we think it absolutely necessary to complement our main argument regarding the waveguide criterion by considering the other criteria, too, because only this way we arrive at a comparison between apples and apples. In the end this puts the entire Petoukhov-Kornhuber method to a test (not just their waveguide criterion), and this is the goal of the paper. Omiting part of the criteria and comparing only an incomplete version would be doing an injustice to their method. For this reason we also think that the Venn diagram in Fig. 14 is useful, and we plan to leave it in place.

At the same time we became aware of a misunderstanding of one of the reviewers regarding our schematic in Fig. 14, alerting us that its description needs to be improved. We are planning to improve its description in the revised version to avoid any such misunderstandings.

9 When is a method trustworthy?

We only considered a rather limited class of basic state profiles in our analysis, and for some of these basic states we found issues with the refractive index method. One of the reviewers raises the question whether the refractive index method may actually yield better results when applied to other (classes of) basic states.

We do not want to exclude this possibility (as we say explicitly in the manuscript), but we think that this would be a lucky coincidence. In our analysis we show that using a restricted class of basic states, which allow a fairly deep understanding, yields critical issues with the refractive index method. In our view this undermines the trustworthiness of the method. If the same method happens to yield "correct" results for some other basic states, we would not consider this as a proof of its trusworthiness, but rather as fortuitous. We, at least, cannot think of a class of basic states (with relevance to realistic episodes) where none of our critical issues apply and where the method can be used with confidence.

References

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Wirth, V. and N. Harnik, 2026: Rossby wave resonance for idealized jets on a beta-plane: towards a better understanding of the meridional wave structure. *Weather and Climate Dynamics*, **7**, 297–316, https://doi.org/10.5194/wcd–7–297–2026.