Response to Referee #1's comments on egusphere-2025-2529

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The comments of Referee #1 are quoted below in blue, and the authors' responses are written in black. All references (figures, sections, equation numbers, and line numbers) in the responses refer to the revised manuscript.

Response to Referee 1

In this paper, two ways to treat fluid motions on mid-latitude β-plane are described and compared. One is a conventional way to treat mid-latitude β-plane, which is, when considering inertial gravity waves, essentially the same as f-plane where the Coriolis parameter is represented as a certain constant value f_0, and in this paper, it is called "harmonic wave theory". The other is to treat the Coriolis parameter as linearly changing with y or $f = f_0 + \beta y$ also for gravity waves, and it is called "trapped wave theory".

Some numerical calculations or time integrations of a simple β-plane numerical model are performed, and the accuracies of harmonic wave theory and trapped wave theory are investigated. And the results are summarized such as the error of the harmonic wave theory becomes large when the latitudinal width is large, while the error of the trapped wave theory becoms large when the latitudinal width is small.

We thank the reviewer for the constructive comments. Below we provide a point-by-point response to the particular points of concern.

The comparison of the two theories for simple β-plane calculation is important and the results obtained here are also significant; it is worth publishing, I think. However, when I first read this article, I over-hoped for this trapped wave theory; it must overcome the defects of the harmonic wave theory since the change of Coriolis parameter is exactly treated without approximation in the trapped wave theory. But, actually, the situation where this trapped wave theory can be applied is quite limited; the region should be widely spread in the north direction, the region should be limited by some cost in the south direction, the motion should be relatively large so that the frequecy of the wave is near to the intertial cycle, which assure that the motion is evanescent in the north area but wavy in the south, and so on.
We thank the reviewer for this important comment. We agree that the trapped wave theory is not universally applicable, but we note that it is not more restrictive than the harmonic theory. In fact, the two theories differ fundamentally in their boundary requirements. While the harmonic wave theory requires two rigid meridional boundaries to support standing wave solutions, the trapped wave theory requires only a single meridional boundary, since the solutions decay poleward from this single rigid boundary. This distinction implies that the trapped wave theory applies in configurations where the harmonic solutions cannot exist at all. Thus, the trapped wave theory is not more limited, but rather complementary, as it applies to wide domains with only one rigid boundary (located on the south side of the domain in the northern hemisphere).

On the other hand, in the conventional harmonic theory, although the Coriolis parameter is treated as constant, we are usually able to consider "local dispersion relation" which is often used as WKBJ treatment.
We agree that the harmonic theory, although formulated with a constant Coriolis parameter, can often be applied locally using WKBJ-type approximations that rely on a local dispersion relation. This important aspect of the theory broadens its applicability in geophysical contexts and complements the global standing-wave solutions. Following this comment, we revised the discussion section. In the revised version, at the end of the paragraph discussing the failure of the harmonic wave theory in large domains, we added the following clarification:

"Nevertheless, it should be noted that the harmonic theory can also be extended locally through WKBJ-type approximations, which account for the slow meridional variation of the Coriolis parameter by introducing a local dispersion relation. This local interpretation has been widely applied in geophysical contexts and extends the usefulness of the harmonic framework beyond the global solutions considered here."

In this article some real examples of observations or simulations are described, and most of them are referred to as examples which show the advantage of the trapped wave theory. In some examples, the difference of the phase speed or frequency cannot be explained by harmonic theory where the Coriolis parameter is constant, but if we consider local value of the Coriolis parameter, it may be explained. As another example shown in p.32, the D'Asaro et al's argument is critisized, the presumed form of the wave does not satisfy the wave equation. This becomes a problem when considering exact solution in the entire region, but if we consider approximation as WKBJ treatment and consider local relation, which is possible when considering solutions like harmonic theory, it does not become a problem.

It is true that the harmonic theory has advantage as an exact solution, but harmonic theory has advantage that it can be considered locally like WKBJ treatment. I think that the theoretical and calculation parts in this artilce does not have problems but introduction and discussion parts where the comparison with real observation is desceibed should be reconsidered, since the harmonic theory and earlier theories are unfarily critisized.

We agree that in the original submission, the Introduction and Discussion may have conveyed an overly negative impression of the harmonic wave theory when contrasted with the trapped wave theory. Our intention was not to dismiss the usefulness of harmonic theory, but rather to highlight the limitations of its global form relative to trapped modes. Following the reviewer's constructive suggestion, we revised both sections to present a more balanced view and to explicitly acknowledge the contexts in which harmonic theory remains valid. In particular, we now emphasize that the harmonic framework has two complementary aspects: (i) its role as an exact solution on the f–plane, and (ii) its ability to serve as a local approximation on the β–plane through WKBJ-type methods. These additions clarify that many observational results (including those discussed in relation to D'Asaro et al.) can be reconciled with local interpretations of harmonic theory.

Changes made in the manuscript:

- **Introduction (Sec. 1):** We added a sentence explicitly noting that the harmonic theory, although requiring two rigid boundaries, can also be applied locally through WKBJ-type approximations, which use a local dispersion relation. This addition clarifies that harmonic theory provides valuable insights not only as a global exact solution but also as a practical local approximation in geophysical contexts.
- **Discussion (Sec. 5):**
  1. We softened the wording where the harmonic theory was described as "failing" in wide domains. The revised text now states that "in large domains, the harmonic theory does not reproduce several features of the simulations."
  2. At the end of the paragraph on the harmonic theory in large domains, we added the following clarification:

     > "Nevertheless, it should be noted that the harmonic theory can be extended locally through WKBJ-type approximations, which account for the slow meridional variation of the Coriolis parameter by using a local dispersion relation. This local interpretation has been widely applied in geophysical contexts and provides additional validity to the harmonic framework beyond the strict global solutions considered here."