Response to Referee #2's comments on Manuscript ID egusphere-2025-2529

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The comments of Referee #2 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 2

This manuscript explores two one-dimensonal wave theories for inertia-gravity waves on a mid-latitude beta-plane and consider both geostrophic and Ekman adjustments. They focus mainly on linear theories but also present a few nonlinear simulations. They define a metric to quantify the differences between linear and nonlinear theories and show how this varies in different regimes.

This research is of interesting to the community and well written but I strongly recommend that the following concerns are addressed before it is published.

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

Please give citations for the particular scalings you are using in the two cases.
Done (see L120 and L125).

The authors work very hard to have one set of equations that allows for the two types of adjustments. This makes things mathematically complicated and I'm not sure how much is gained. If there is a big benefit to this, please emphasize this as it's not entirely clear to me.
We thank the referee for pointing out this issue. We agree that the unified formulation of the geostrophic and Ekman adjustment problems introduces additional algebraic complexity. However, the benefit is that both problems are now cast into a single nondimensional system, and the only difference between the problems appears only in the Kronecker delta δ_{i0} function on the RHS of Eq. (9). This has two advantages: (i) it allows a direct comparison of the two classical problems in the same mathematical framework, which clearly distinguishes between aspects of the dynamics are problem-specific and those common to both problems; and (ii) it reduces redundancy by avoiding two parallel derivations. To make this benefit clear, we have revised the manuscript and added an explanatory remark at the end of Sec. 2.4.

In section 2.5, going from equation 14 to 15, the quadratic term is ignored. This is because the authors want to get the equation that has a known special function a solution. However, this is an approximation, and introduces more error into the equation. Since the eigenvalue problem is solved numerically, you can easily keep in this term and that should yield a more accurate theory. Please work on doing this or give a very good reason why not.
We thank the referee for this suggestion. The neglect of the quadratic term b^2y^2 in Eq. (14) is justified for the following reasons:

1. As pointed out by Paldor and Sigalov (2008), neglecting b^2y^2 is consistent with the standard β-plane approximation, which retains only terms linear in y in the expansion of the Coriolis frequency (Eq. 4) and ignores the quadratic (and higher) terms. To maintain the $O(y^2)$ terms in $f(y)^2$ (e.g. in Eq. (14) where $(1 + by)^2$ is the scaled form of $f(y)^2$) requires that the same terms are also maintained in the expansion of $f(y)$.

2. The errors introduced by this approximation are indeed negligible. This is evident in Fig. 2, where the trapped wave solutions (blue curves) and the numerical solutions (black curves) are almost identical for $t < 30$. After $t = 30$, discrepancies arise from reflections at the domain boundaries, not from the neglect of the quadratic term.

3. The main aim of the paper is to evaluate the accuracy of the two existing wave theories and not to develop a new theory. Retaining the quadratic term would complicate the analytical treatment without improving the conceptual comparison of the known solutions (which were derived based on the procedure described in the present MS).

Thus, the neglect of the b^2y^2 term is justified and appropriate for the goals of the present study.

Section 3 discusses harmonic, trapped and semi-infinite domains. These have all been previously studied in the context of the shallow water model. In reality, waves will not be purely harmonic or trapped, but some hybrid of the two. If that's the case, then why spend so much time focusing on each of these limits? Again, with numerical solutions you can study any of these waves and you need not restrict yourselves to these relatively simple cases.
Our focus on harmonic, trapped, and semi-infinite domains is not meant to imply that real waves fall only into these categories. Rather, these limiting cases provide a clear framework for understanding fundamental dynamics, isolating the effects of individual physical processes and for benchmarking numerical solutions by comparing them with analytical results. These limits also have clear physical relevance: waves with harmonic characteristics are often observed in narrow meridional domains, such as lakes (e.g., Simons, 1978), whereas waves with trapped characteristics appear in wide domains, for example in the Indian Ocean (De-Leon and Paldor, 2017). Comparing these basic wave theories with simple, idealized simulations provides a necessary foundation for studying wave dynamics in more complex numerical setups or in real-world circumstances.

Solving the inhomogeneous eigenvalue problem is interesting. But when you do this you are essentially decomposing the inhomogeneous part of the equation in terms of your eigenfunctions. These don't change at all and don't change the eigenfunctions. This is not something that we see very much in the literature and is worthy of further discussion.
We thank the referee for raising this point. We agree that it is useful to emphasize how the eigenfunction decomposition naturally arises in the solution of the initial-value problem. To address this, we have added a short discussion in Subsection 2.5 [following Eq. (14)]. There, we explicitly state that the general solution of the homogeneous problem can be expressed as a superposition of the eigenfunctions of Eq. (15), which ensures that any initial condition can be represented consistently using this basis. This added note clarifies the role of the eigenvalue problem in linking the initial-value formulation with the spectral solutions.

In section 5, when introducing the MITgcm, be explicit as to what equations you are solving. It does not solve the shallow water equations you have focused on up to this point.
We note that the MITgcm was configured to solve the same linear shallow water equations that are the focus of this study. The setup closely follows the procedure described in Section 4.1.1 ("Equations Solved") of the MITgcm barotropic gyre example (see `https://mitgcm.readthedocs.io/en/latest/examples/barotropic_gyre/barotropic_gyre.html`). Additionally, we removed the nonlinear terms in the material derivative and viscous dissipation from the MITgcm, leaving only the linear shallow water dynamics. This ensures that the MITgcm simulations are directly comparable to the analytical solutions of the linearized RSWE. Following the referee's comment, this clarification was explicitly added in the revised version of the manuscript (see P10).

In section 5, you now start to consider averages of the numerical solutions. But the theory does not mention temporal averages at all. If you want to have a good comparison, you should consider temporal averages in the linear theory.
The wave theories presented in the manuscript focus solely on the time-dependent component, v', which is governed by the homogeneous part of Eq. (12), i.e., Eq. (14). In contrast, the MITgcm solves the RSWE, and therefore the simulations also include the time-independent, mean component, $\bar{v}(y)$, which corresponds to the solution of the inhomogeneous part of Eq. (12), i.e., Eq. (13). The comparison between the numerical simulations and the analytical wave solutions is carried out by subtracting $\bar{v}$ from the total velocity $v(y, t)$ of the numerical simulations as an estimate of the time-dependent component

v'. To explicitly clarify this point, we have revised the 1st paragraph on P11.

Page 11, you say the number of modes summed is 10^4, then later its reduced to 500. How sensitive is your result to this number?

In the revised version, we clarify that in the calculation of the trapped wave solutions, the number of modes summed (N) was set to 10^4, while for the harmonic wave solutions only, N was reduced to 500. Multiplying N by 2 or 0.5 has a negligible effect on the results. Figs. 1–2 below show v' (for $L = 60$) in the geostrophic adjustment problem and the Ekman adjustment problem, respectively. In these figures, the harmonic wave solutions are shown by red curves, while the trapped wave solutions are shown by blue curves. The solid lines correspond to $N = 10^4$ (trapped waves) and $N = 500$ (harmonic waves). Dashed lines represent N multiplied by 0.5, and dotted lines represent N multiplied by 2. The overlap of the solid, dashed, and dotted curves in the figures clearly indicates that the insensitivity of the results to these changes in N. Reducing N further to 50 has a more significant effect on the results (not shown).

Lots of emphasis is put on this epsilon parameter, the so called error estimator, for many different cases. I'm not convinced this is physically interesting. If you feel it is, please give some better justification for why you are considering it so intently.

We agree that the error measure $\epsilon(t)$, defined as the spatially averaged absolute difference between the theoretical and numerical fields, is not intended to represent a physically meaningful quantity on its own. Rather, its purpose is to provide a simple, systematic, and reproducible **quantitative** diagnostic that complements the visual comparisons presented in Figs. 1-4. The inclusion of $\epsilon(t)$ allows us to quantify the overall mismatch between theory and simulation in a way that is both concise and uniform for all cases. This helps us highlight the trends and systematic differences that might otherwise be overlooked in purely visual inspection.

To clarify this motivation, we revised the manuscript by adding a short subsection entitled *Error estimates* (Sec. 4.2), where the role of $\epsilon(t)$ is explicitly explained as a diagnostic tool rather than a physically interpretable parameter. We believe that this addition strengthens the justification for the use of epsilon as a single quantitative measure of accuracy. The entire discussion of the error measure in Sec. 4.2 is significantly shorter in the revised version.

Section 6 redoes everything for two layers. Why not do it all together? It was reduce what is a rather long manuscript.

We agree that the manuscript is rather long. Following the referee's comments, we moved the entire discussion of the two-layer and continuously stratified ocean models to the appendices so the discussion is now split into two appendices: Appendix C presents the analytical two-layer ocean model, while Appendix D describes the numerical simulations of a stratified ocean. Appendix C serves as an introduction and motivation for Appendix D.

Section 6.2 is called numerical simulations of a multilayered ocean and the authors use the MITgcm. This is not a layered model. Please be vary careful in your descriptions.

We thank the referee for the comment. To clarify, the MITgcm is not inherently a layered model. In our simulations, we employ a 3D configuration with 38 vertical grid cells (numerical layers) to represent a simplified two-layer physical ocean. The upper and lower physical layers correspond to groups of numerical layers, allowing a meaningful comparison with the two-layer analytical model. To make this distinction explicit, we have revised the opening paragraph of Appendix D to clearly emphasize the difference between physical layers (representing the conceptual two-layer ocean) and numerical layers (used for the model's vertical resolution).

In general, this paper seems to be a lot of different things thrown together and they don't necessarily flow very cleanly. Please work harder to make this a coherent story, not just a series of interesting results.

We greatly appreciated the referee's comment regarding the manuscript's coherence. In response, we have substantially shortened the main text and reorganized the material to present a more coherent narrative. The main text now focuses on the core problems, the analytical solutions, and comparisons with idealized single-layer simulations, while all technical derivations, the two-layer model analysis, multilayered simulations, and connections to observations have been moved to the appendices. This restructuring ensures that the manuscript now reads as a unified story, with the appendices providing all necessary details without disrupting the flow of the main discussion.

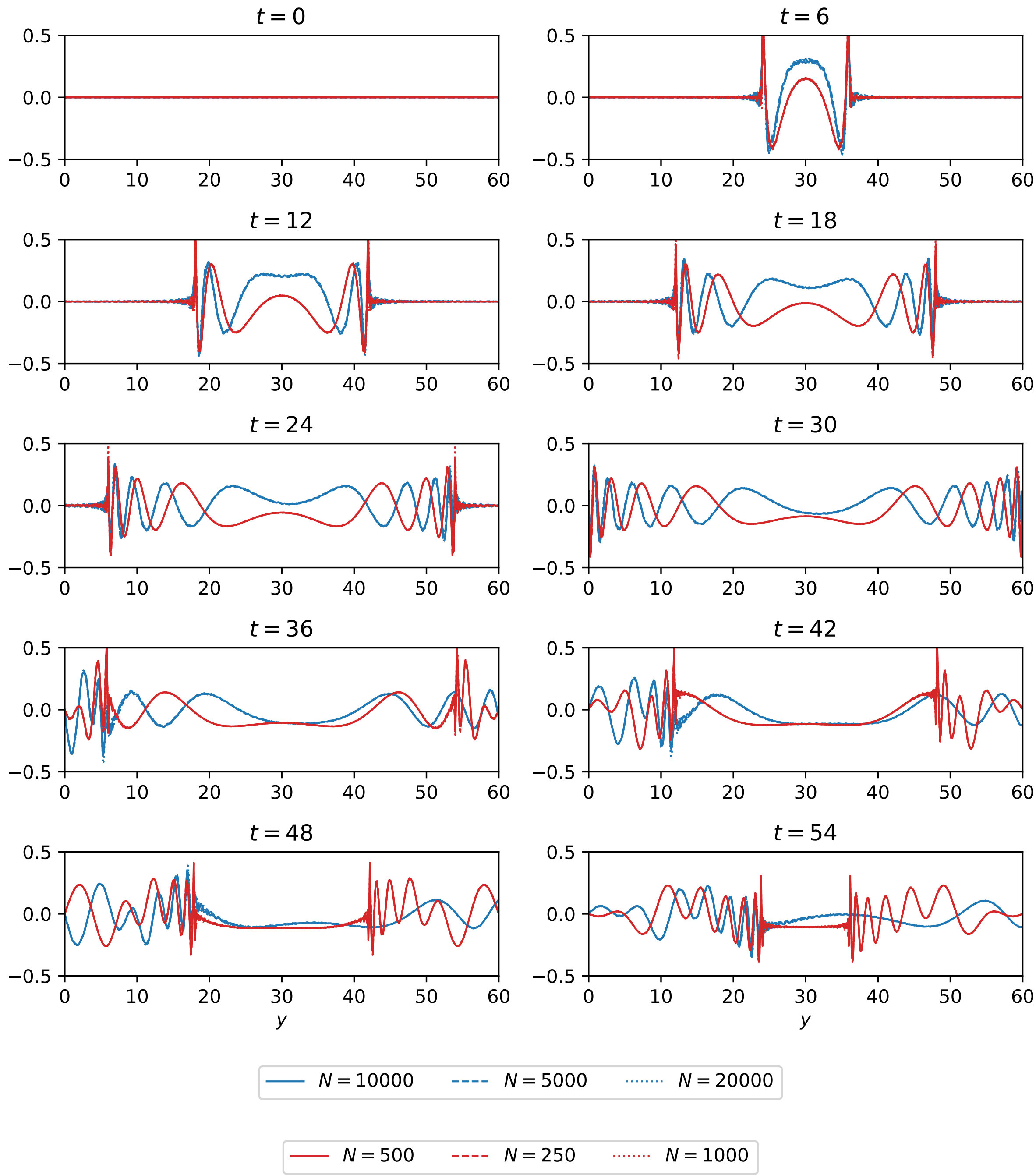

Figure 1: Geostrophic adjustment for $L = 60$. Red: harmonic wave solutions; blue: trapped wave solutions. Solid lines: $N = 500$ (harmonic) and $N = 10^4$ (trapped); dashed/dotted lines: N multiplied by 0.5 and 2, respectively. Overlapping curves show negligible sensitivity to N.

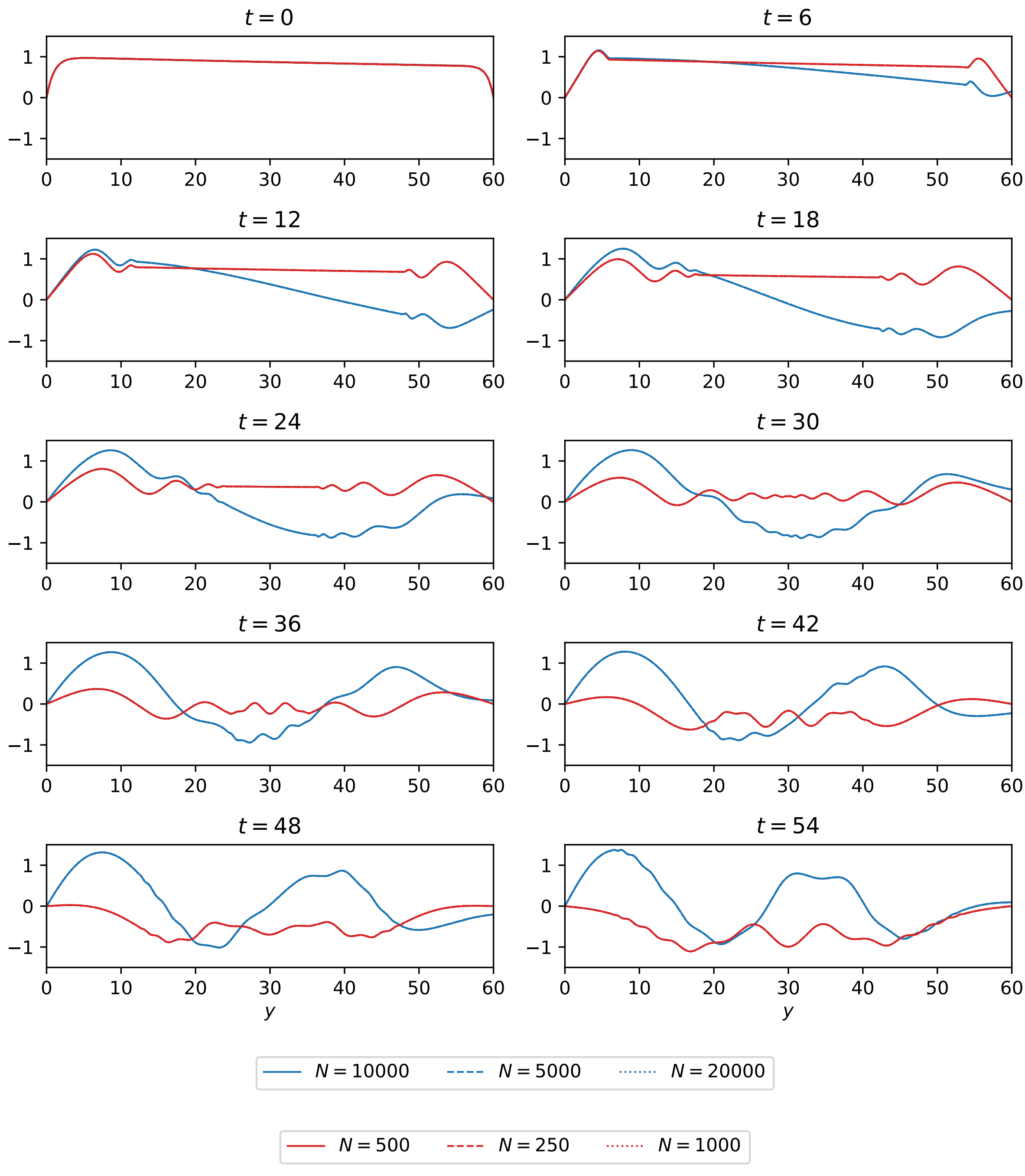

Figure 2: Ekman adjustment for $L = 60$, using the same color and line conventions as Fig. 1. Curves overlap, indicating minimal sensitivity to variations in N.