Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2022-151

Anonymous Referee #2

Referee comment on "A nonhydrostatic oceanic regional model, ORCTM v1, for internal solitary wave simulation" by Hao Huang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-151-RC2, 2022

Review of the manuscript A nonhydrostatic oceanic regional model ORCTM v1 for internal solitary wave simulation

The paper describes the non-hydrostatic Oceanic Circulation and Tide Model (ORCTM) and numerical experiments on reproducing internal solitary wave dynamics with different types of analytical topography. The authors concluded that the developed model adequately simulates the behavior and evolution of the non-linear internal waves and tide-topography interaction. However, I found that the manuscript in its present form includes numerous flaws and needs to be substantially revised.

General comments:

1) The model description is inaccurate in many places and requires deep revision. Specifically,

- Eqs. (4) and (5) are not independent and (4) is just the form of the continuity equation (3) integrated over the vertical coordinate with a kinematic boundary condition on top.

- The authors use the splitting of the total pressure for components, however it is unclear what components they use and how this splitting was performed. It becomes more clear only with eqs. (15)-(16) in the middle of Sect. 2.2, which is confusing.

- The Boussinesq approximation does not assume the reference density in (3).

- Eq.(8) describes water density which is not used somewhere in (1)-(7).

- The authors should explicitly formulate what boundary conditions they use at the open boundaries of the model domain. The same goes for all numerical experiments described in Section 3.

2) Despite a fair description of the model, the benefits of using ORCTM compared to other non-hydrostatic models cited in the ms are entirely unclear. What is the novelty of this model? I would love to see an additional paragraph or two where the authors would specify the novelty in the physical formulation of the model or boundary conditions used and/or in numerical implementation. In their present form, both these sides look quite trivial, which raises the question about the target for this paper. Do we need just one more non-hydrostatic model?

3) It was nice to see in Section 3 that the model can adequately reproduce the physics of the solitary waves under conditions of different topography. The authors did a good job presenting these results. However, a direct comparison of the model results with analytical solutions for the most uncomplicated idealized cases is missing. In the case of lack of comparison with direct observations, this is the only way how we can attest the robustness of the model. I believe that this addition would benefit the manuscript.

Additionally, over the entire manuscript, I found lots of language mistakes and incorrect terms that make the text hard to read and understand. As a result, in many places, I cannot follow what the authors are trying to say. Therefore, I would strongly suggest the authors do professional proofreading of their text before the next iteration.

Specific comments:

L10: How could the boundary conditions support the regional simulations?

L12: The equations cannot consider something. Check the language.

L37-40: Unclear sentence. Reformulate, please.

L44-47: How could the hydrostatic approximation result in the inapplicability of the non-hydrostatic dynamics? Clarify.

L89-95: The sentence is hard to understand. Try to split it to make clearer.

L103: The Cartesian coordinate system does not require the eastward and northward velocity components.

L111: Check the components of the forcing vector.

L112: Usually, the river runoff and boundary inflow are specified through the boundary conditions but not in the model equations. How are the authors supposed to adjust such a forcing with the boundary conditions?

L115: Check the components of the viscosity vector.

L125-127: Provide exact equations for boundary conditions.

Eq.(16). Not sure that the reference density is correct here. Otherwise, it needs more details.

L191-193: Unclear sentence. Please, reformulate.

Figure 1a: This is a density anomaly, not just density.

L210-213: Unclear meaning.

L241: You already introduced the free surface elevation in L104.

L266: Put both these densities in Fig. 4

Figure.6 : Use (a), (b) and (c) notation in the title.

L311: Check the language.

L332: What does the “rear effect” mean?

Figure 16: The left panels are unreadable.

L466: How can the horizontally uniform stratification drive the model?

Appendix A: Check the correctness of indices in A.2-A.4. It is unclear how were Au, Av, and Aw calculated. Please, reformulate using DX, DY, and DZ notation.

L678: What does “harmony” mean here? Please, clarify.

In summary, based on my evaluation of the current version of the manuscript, I would suggest major revision.