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Interactive comment on "Adaptive wavelet simulation of global ocean dynamics" *by* N. K.-R. Kevlahan et al.

N. K.-R. Kevlahan et al.

kevlahan@mcmaster.ca

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Response to anonymous Referee 1's comments

We would like to thank this referee for their detailed and thoughtful comments, which we answer in detail below. They have helped to significantly improve this paper.

For the convenience of the referees modifications are indicated using `latexdiff` in the revised manuscript.

1. Overview

   The authors uses the Euler–Poincaré theory to introduce a new Brinkman penalization for the rotating shallow water equations. An error analysis is performed in the linearized 1-D case and the choice of penalization parameters is discussed. A numerical model based on this new penalization and on an adaptive wavelet method is then used to simulation ocean currents with realistic coastlines and bathymetry. The main input of the paper is the derivation of a penalized formulated that guarantees both mass and energy conservation. In addition this formulation does not modify (increase) the gravity wave speed in the solid region and so is not prone to stability issues to this respect. This formulation is valuable by itself and this paper could be accepted for a GMD publication if the following comments are addressed in a revised version.

2. Major comments

   - Derivation of the new volume penalization

     At several places in the paper, we don't know if the equations are written for flat or non flat bottom:

     – Page 5268, Line 17: h is used instead of η in the case of a non flat bottom.

     – The momentum equation of page 5273 is clearly not consistent with a non flat bottom (a bathymetry gradient is missing at the right hand side) (same at bottom of page 5275). It seems that the partial derivative of the Lagrangian density L (bottom of page 5272) does not take into account the varying bathymetry. States at rest should correspond to constant η and not constant h.

     – The bathymetry b is also missing in the expression of the total energy page 5276.

     Response: As now indicated, Reckinger et al. consider a flat bottom. Thanks for spotting the missing bottom terms in the momentum and energy budgets for our penalized equations. We have corrected them. Notice that the numerics use the vector-invariant form, which is correct.

- Link between the penalization parameters α, ϵ.

  From the beginning, the authors state that these two coefficients are linked by $\epsilon = K/\alpha$, K being the permeability. This is mentioned as an important difference with Reckinger et al. (2012). However at several other places this statement seems to be alleviated. In order to remove confusion, it would be preferable not to assume any dependency between the two coefficients and to mention where needed the advantage (or not) to have these two coefficients linked.

  Response: We have modified this comment to make it clear that for penalization purposes these two parameter may be varied independently.

- Error analysis and choice of penalization parameters

  – In order to make convergence comparison clear, it would be really nice to have the same analysis for the Reckinger et al. (2012) set of equations.

  Response: Our analysis requires deriving jump conditions between the solid and fluid regions because porosity is discontinuous. Unfortunately, since Reckinger et al. (2012)'s method is not conservative we have not been able to derive jump conditions and to obtain similar convergence results for their method. They do, however, present results showing that the method is $O(\alpha)$ with epsilon fixed in figure 5 and show $O(\alpha)$ convergence over a variable range for fixed ratio $\epsilon/\alpha = 10^{-2}$.

  – For clarity, a summary of main convergence results along with main assumptions may be given at end of section 4.1.

  Response: Done.

  In addition, I am not sure that the (dimensional) scaling factor c/L can be dropped from the convergence factor as it is done in the following sections. The error estimates assume that $\epsilon \ll L/c$ so that these numbers are not

independent. It is essentially a question of clarity for readers. No doubt that is is clear in authors's mind. The confusion comes from the fact that the c/L is dropped at the beginning of section 4.2 and is however required for the conclusions of section 4.3: error is $O(\alpha)$ when a) $\epsilon \approx \Delta x/c$ (for stability) and b) $L \approx \Delta x$ for marginally resolved fronts (so that $\epsilon = L/c$). In this case the asymptotic expansion (26) is not valid but the hypergeometric function is bounded.

  Response: We have emphasized that L and c are fixed for the numerical experiments and give the values, as well as the ratio $\sqrt{c/L}$. As you note, expression (25) shows that in the case of minimally resolved waves the asymptotic approximation of the hypergeometric function is not valid, but since it is bounded (actually constant) the error is $O(\alpha)$ as we state in 4.3.

- Numerical 1-D experiments

  – It may appear more natural to have section 4.3 before the numerical experiments of section 4.2. This would allow to understand and to comment the choices made in 4.2.

  Response: We see 4.3 as an interpretation of the various convergence results in 4.2. Seeing the convergence results first in 4.2 is necessary to understand the particular choices we recommend in 4.3.

  – Note that a number of important parameters are missing here: what are the values of $L, H, \Delta t$ and of the Courant number?

  Response: These parameters have now been defined.

  – The influence of the smoothing parameter Δ (or of the ratio Δ/L) is not discussed.

  Response: We have added a new figure and discussion about the effects of smoothing at the end of 4.2. We also clarify the role of smoothing and why it is needed at the beginning of the section.

- Realistic experiments

  – As a general comment, I appreciate the work done by the authors to apply their code to simulations with complex coastlines and bathymetry. In particular, the choice of the indicator function is well explained and makes sense. However, I have to say that, for a first experiment, I would have prefer to see the code in action on a much simpler application that still allows to evaluate the merits of the volume penalization technique and of the grid refinement features. Shallow water numerical experiments on a rotated grid (cf Adcroft (1998)) could have been a good application.

  Response: Thank you for this reference: we were unaware of this proposed test. We have added a new section 5.1 and new figure 7 that show the results of the Adcroft and Marshall (1998) test for four rotation angles. We conclude that the effect of rotating the physical domain with respect to the model domain is negligible.

  – Could the authors detailed their remark on mass conservation ? (lines 4-10 on page 5288)

  Response: We have written a more detailed explanation of why the mass of the mean sea level is not exactly conserved during grid refinement, even though the mass of the perturbation to the mean sea level is. Essentially, this is due to the fact that the bathymetry values interpolated from the smoothed ETOPO data may modify the mean value of the sea level over the refined cell. This mass defect is very small, does not accumulate, and disappears if the grid subsequently coarsens to its original resolution.

  – Concerning the figures illustrating section 5.3, a plot of a well-known region (e.g. Gulf Stream) would be of interest.

  Response: We have added a final figure showing the grid and vorticity for the Gulf Stream region.

3. Minor comments

- The title of the paper is not really reflecting its main content. May be just adding “based on a new Brinkman volume penalization" would be sufficient.

  Response: Changed as suggested.

- The introductory section should be a bit longer with an introduction to other ways of dealing with complex coastlines in ocean modeling (e.g. unstructured meshes, cut cells, other immersed boundary methods. . .)
- Page 5277. Would it be possible to treat the velocity penalization term implicitly to remove the stability constraint?

  Response: It would be possible, but our goal is to provide a technique that that can be used without modifying the underlying numerical scheme. Note also that accurate approximation of the no-slip boundary condition still requires that that the numerical boundary layer be properly resolved so the time step would be constrained by accuracy rather than stability requirements. We mention this in the revised paper.

- Page 5286, lines 19-21. I agree with this remark. However in a 3D simulation, care would have to be taken in order to not remove bathymetry barriers important for the overall circulation.

  Response: Agreed. The actual smooth mask for coastlines will require some manual adjustment of important small scale features.

- Page 5292, lines 19-24. Authors should recall here that the stability is constrained by the smallest grid size in the computational domain. All (fixed or adaptive) refinement methods that do not include local time stepping share this limitation.

  Response: We have added this comment.