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Interactive Comment

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

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Response to anonymous Referee 2'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 referee changes to the revised manuscript have been indicated using `latexdiff`.

- Overview

  This article presents a detailed analysis of a penalization technique to represent “vertical" coastlines in shallow-water models. The technique is then applied to an exist- ing wavelet-adaptive finite-difference/finite-volume discretization of the shallow-water equations and used to simulate tsunami and global oceanic barotropic circulation. I found the manuscript clear and well-presented. The model derivation and error analysis are thorough and useful in practice. I have two major reservations however:

1. The issue of representation of coastlines (or complex boundaries) in ocean models (or more general PDE systems) discretized on fixed grids (i.e. non-boundary con- forming grids) has been studied very extensively in the past. The authors do not give sufficient credit and context for their own contribution. The introduction should do a much better job of summarizing this field, besides the few references already given for penalization techniques. One could mention in particular [...]

   Response: We agree that the introduction has a bias towards penalization-based handling of coastlines, which is far from being the mainstream approach. We thank the referee for providing a broader sample of references, which are now referred to in the introduction.

   I also note that both Dupont, 2001 and Popinet and Rickard, 2007 both present (semi)-analytical test cases of the accuracy of boundary representation which are more stringent than the practical examples used by the authors (as well as very relevant for the type of applications envisaged). Moreover better than first-order in space accuracy is obtained. This need at least to be mentioned in the introduction.

   Response:

   The issue of accuracy is now raised in the introduction. We also include a new case proposed by Adcroft and Marshall (1998) that tests the sensitivity of the penalization to rotations of the physical coastline with respect to the computational grid in section 5.1.

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2. The need for “vertical" coastlines (i.e. “side walls") in ocean models is not obvious at all. As with most the Earth’s topography, coastlines are usually not steep at all (aside from the very few areas where sheer cliffs fall into the deep ocean). In most cases assuming vertical coastlines is done to circumvent dealing with “wetting/drying" at coastlines. In itself wetting and drying is not a major theoretical difficulty for shallow-water models: fullly-nonlinear shallow-water models have been shown to be theoretically well-posed in the limit where the water depth tends to zero. Indeed for applications such as tsunamis, the non-linear shallow-water system has been shown to describe very well the shoaling and flooding properties of long waves on coastlines. Assuming “side walls" for such applications (as is done here for the 2004 tsunami) will essentially mean giving up any results regarding the extent of flooding on the coastline, which is of course one of the main reason to do such tsunami simulations. This point needs to be discussed by the authors both in the introduction and for the tsunami example.

   Response: The issue of vertical walls vs wetting and drying is now highlighted in the introduction and in section 5.3. We would like to stress however that our perspective is to progress towards a three-dimensional global ocean model. As far as we are aware such models do not handle wetting and drying at the shoreline.

   Also, the authors need to credit previous adaptive simulations of tsunamis, such as:

   Response: Yes, we have included this reference this work and another one (Harig et al., 2008)

- Some minor comments follow:

1. line 15: “Smaller-scale features, such as vortices and jet meandering, are predominantly generated in the real ocean by baroclinic mechanisms which

cannot be captured by a single-layer model." I find this comment too general. On the scales the author consider (i.e. less than 1km) and close to coastlines (which is the point of the article), barotropic flows are often the main cause of vortices and jets.

Response:

We have narrowed this statement to make it more specific. We now refer to mesoscale and sub-mesoscale ocean eddies which are, as far as we are aware, always baroclinic except possibly in very shallow waters (exciting the barotropic mode in the open ocean requires a lot of energy).

2. line 10 p 5291: Giving clear indications of computational speed, both relative and absolute, is important since the point of adaptivity is computational efficiency. Besides the approximate runtimes already mentioned, it would be good to give the absolute speed of computation, for example using number of (degrees of freedom/grid points) advanced / computation time / number of cores.

   Response:

   We now give absolute computation speeds for the tsunami case at the end of section 5.3. For the 475 m local resolution, the average wall-clock time on 256 cores is 9.1 s for 1 s of physical time. We note that since the code has 94 % strong parallel scaling efficiency it should be possible to achieve operational forecasting with several thousand cores (we didn't have access to this number of cores for our runs).

Interactive comment on Geosci. Model Dev. Discuss., 8, 5265, 2015.