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Ocean Science
Discussions

Interactive comment on "A practical scheme to introduce explicit tidal forcing into OGCM" *by* K. Sakamoto et al.

Anonymous Referee #1

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General comments:

This explicit introduction of tides into an OGCM is a good idea and appears to be promising as a way to simulate tides and the non-tidal circulation together. According to the way you described your new scheme, it appears that your explicit tidal scheme, when simulated concurrently with the "basic" component of the circulation, does not violate the dynamical balances in the OGCM. Your root-mean-squared error is impressively small for such a coarse resolution model without several pieces of physics that are known to improve tidal simulations (e.g., the full self-attraction and loading computation and a topographic internal lee wave drag scheme). You focused on showing that the self-attraction and loading scalar approximation decreases the errors in tidal phasing, as previous studies have shown using other schemes to implement tides, and

on showing similar-looking tidal energy flux maps with those from previous studies.

Specific comments:

1) Do a comparison of your scheme using your manuscript's Eq. (22) with use of your scheme using your manuscript's Eq. (21). That is, I would like to see a comparison of your scheme with that of Arbic et al. (2010) since the latter is the scheme you are trying to improve upon. Your reported root-mean-squared error values with your scheme in your model are compared against the NAO.99b dataset. Your results can only be fairly compared with root-mean-squared error values using the Arbic et al. (2010) scheme in your model against the NAO.99b dataset, not the Arbic et al. (2004) results. It is probably correct to argue that the all of the SSH deviation is not always the tidal height, as you do, but you have the opportunity to show evidence that you are correct in this assertion here. I understand that the computational expense to implement the Arbic et al. (2010) is greater than that of your scheme, but because you are using a coarse-resolution model, it should not be prohibitive. Local machines where I am, for example, can perform this computation without asking for additional computer resources, and I doubt that's specific to my institution.

2) You argue that the differences in the vertical velocities between your full tidal simulation, using your new scheme, and your simulation without tides are due to the presence of internal tides caused by the interaction of tides and the bottom topography. It is unclear how the internal tides would be generated from the interaction of tides and the bottom topography when you're using a relatively coarse resolution (particularly in the horizontal). (Also, use of a topographic internal lee wave drag parameterization would influence your results.) You need to explain how the internal tides arise in your simulations with tides. At the very least, you should show a time series of the isopycnal vertical displacements (not just the vertical velocities) to demonstrate the presence of greater undulations in the internal wave field when tides are included. Also, if you were to run your model at a different resolution, you would expect that there would be an effect on the vertical velocities (Niwa and Hibiya, 2011, J. Oceanogr.) in your model,

which would be best to show explicitly. Since you're implementing tides differently from previous studies, you need to show this.

3) It's unclear whether the X term, which includes the winds, in the momentum equations and the freshwater flux in the continuity equations should be absent from the differential equations you derive, your Eqs. (11) and (12), for the primary barotropic response of the ocean to the equilibrium tidal potential. It's clear that those two terms should be present in the "basic" circulation equations, your Eqs. (13) and (14), and that the sum of those equations, your Eqs. (11) + (13) as well as (12) + (14), should yield X and the freshwater flux term. However, studies such as Xing and Davies (1997, J. Phys. Oceanogr.) have shown that the winds have an influence on the tides and studies such as Lee (2006, Ocean Modelling) have shown that the freshwater flux is not independent of the tides. Your scheme allows for a connection between these variables through coupling of the "basic" and tidal flows, but I think you need to further justify your 100% separation of X and/or the freshwater flux term out of the aforementioned tidal equations. Alternatively, you could perform another simulation showing the sensitivity of this assumption by putting in one/two new parameters: one/two different fractions between zero and one on X and/or the freshwater flux term in the "basic" equations and one minus each of those fractions in the tidal equations.

Technical corrections are listed in the attached document.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/10/C123/2013/osd-10-C123-2013-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., 10, 473, 2013.