Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-254

Anonymous Referee #1

Referee comment on "Development and validation of a global 1/32° surface wave-tide-circulation coupled ocean model: FIO-COM32" by Bin Xiao et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-254-RC1, 2022

This manuscript describes the implementation and initial results of simulations using a very high-resolution (1/32°) global ocean model, including waves and tides. This is an exceptional effort and adds to a small handful of similar very high-resolution simulations of the ocean which have been undertaken to date. As such, it is suitable for publication in GMD and deserves to be eventually published.

However, the manuscript is essentially the same as their previous version, gmd-2022-52, which I have reviewed twice and was ultimately rejected. The main problem with their previous version, and the current submission, is their persistent belief that the Bv parameterisation represents mixing by non-breaking waves, which it does not. Although they have removed three of the previous references to Bv as representing mixing by non-breaking waves, there are still several places in which this belief is retained (which I will detail below). Unfortunately, therefore, the paper must be rejected again.

Firstly, though, I include again my response to the authors' previous comments about Bv: "I thank the authors for their response on the Bv parameterisation. However, no good reason has still been provided to support their assumption that w' and l' are in phase for the waves (to leading order). Even though, yes, the fluid motion is not fully irrotational, to first order, for a monochromatic wave train, w' and l' are in quadrature, so that <w'l'> = 0. However, in Qiao et al. (2010, Ocean Dynamics 60: 1339-1355), a single monochromatic wave is considered, and the key underlying assumption for Bv is made between equations 34 and 35 that w' and l' are in phase, so that <w'l'> is NON-ZERO. There is no justification given for this, either in the paper, or the authors' response to my original point on this. Regarding the wave tank observations which are purported to support Bv, I would need to look at these very closely as a separate exercise, but would make the observation that mixing effects will result from the sides of the tank which may be difficult to allow for. And their third point that Bv has already been used in a range of leading models and can dramatically improve their mixed layers is irrelevant to the point in question: of course, the addition of a (possibly large) near-surface mixing term will result in the reduction of over-heating in the ocean surface through additional downward mixing." The overall effect of Bv is to add an arbitrary, unphysical mixing term (which

could be quite large) to the upper ocean.

The places which need to be corrected in the present manuscript, concerning Bv, are now:

l. 16. "The non-breaking surface wave-induced mixing (Bv) is proven to still be" should read "A previously described upper ocean mixing scheme (Bv) is proven to still be"

l. 252-270. Why is the discussion of the Stokes shear force introduced here, what impact does it have on anything being discussed? i.e. what does the epsilon parameter compare the Stokes shear force to? Is this intended to justify the inclusion of Bv in the model (ie by saying this will be important when the Stokes shear force is important)? Note that Bv does NOT represent the effect of mixing by non-breaking waves. Therefore, I cannot see the point of this discussion about the Stokes shear force, and the discussion in lines 252-270 should be deleted.

l. 305. "Prior to examining the effects of surface wave-induced mixing in the …" we are NOT examining the effects of surface-wave induced mixing here, only of the Bv parameterisation which does NOT represent breaking by non-breaking waves. This sentence must be changed to "Prior to examining the effects of the Bv mixing scheme in the …"

l. 411. Bv does NOT represent mixing by non-breaking waves, so change "the non-breaking wave induced mixing (Bv)" to "the mixing induced by Bv"

Other comments which should be addressed are as follows:

l. 22. Need to explain what are the "unbalanced motions" referred to here? Are they the motions induced by internal tides for instance?

l. 121-125: In the high resolution case, the wave and ocean circulation model are coupled offline, so the wave field cannot interact with the ocean circulation fields as they change (ie because the wave fields are previously saved as fixed data files). Need to explain this fully here.

l. 138-139. The Bv field applied to the high-resolution (1/32°) model is calculated from an online-coupled lower resolution model (1/4°). The lower resolution model will have different circulation fields (i.e. the currents will be slower and broader, and probably in

different places), so the Bv field applied to the high-resolution model will not be appropriate. What difference will this make to the high-resolution results?

l. 160 The wave-tide-circulation model is NOT fully coupled since the waves are run offline – change this to "In EXP2, wave-tide-circulation **coupling** is enabled".

p. 8 and fig.s 4 and 5. What longitudes are the ICRE diagnostics defined over (presumably those in the figures)?

p. 8 and fig. 5. How is the ICRE defined for the Gulf Stream in the 1/10° model, since the contour used for its definition does not exist eastwards of about 63°W?

l. 293-294: how do you justify the claim that "the global tide accuracy is sufficient to support" …. " the investigation of tide-circulation coupled processes" given that the errors in fig 8g are in excess of 25cm over large regions of the ocean?

Fig. 12 (d) shows the L_{SML} not the MLD as specified in the caption.

Fig.s 13 and 14 and discussion of the inclusion of internal tides. This was nice to see and the most useful part of the paper. The inclusion of the internal tides appears to add SSH variability between 70-250 km and increase the amount of energy in the spectra (fig. 14) in the more quiescent tropical regions, so that the spectral slope is reduced and more in agreement with the observations. However, fig. 13 clearly shows that the internal tidal field at the surface is too strong, probably because of the lack of bottom dissipation. Can the authors comment on how to reconcile these two aspects, ie if the internal tidal field was realistic, what would the effect on the spectral slopes be (e.g. in fig. 14(e)).

l. 390. Replace "it displaces by clear discrete beams" with "these are shown by clear discrete beams"

l.395: what are the unbalanced motions – presumably the internal tides, IGWs etc?

Fig. 15. Please say which solid black line is the tenth normal mode, and which is the first?

Fig. 15 caption is wrong: e.g the box centred at 138°E, 26°N is shown in panels (a), (b) and (c) and not in panels (a) and (b) as in the caption, with similar comments for the

other rows of panels.

l. 402. This is NOT a fully-coupled model as the surface waves are computed offline and do not interact with the ocean circulation fields as they evolve, so change "surface wave-tide-circulation **fully** coupled model" to "**surface wave-tide-circulation coupled model**".

l. 438-439: "we **clearly** show surface wave-tide-circulation coupling can dramatically improve our simulations" is not true since the surface waves are not fully coupled. So delete the word "clearly".

Minor corrections to the English (up to line 147) are as follows (there are many more such corrections which could be made, so a thorough read-through by a native English speaker would be of benefit here):

l. 33 Further improved resolution has a significant **impact**

l. 48 The most **uncertain** term

l. 51 proposed **an** upper ocean mixing scheme of Bv

l. 61 in many **coarse** resolution

l. 64 **coarse** and high resolution

l. 97 configurations and **design**

l. 147 baroclinic experiments **so** that