Reply to Referee #1:

Following reviewers' comments, Figs. 6 and 11 are added for the revision, and Figs. 4, 8 and Table 2 are modified. Suggestions by the reviewer are included as below. The page and line numbers in parentheses indicate the revised places in the new manuscript.

Specific comments

1. We decided not to execute a comparison using a model with the Arbic et al. (2010) scheme, which the reviewer proposed, because of following reasons:
   (a) We want to propose a different framework to introduce tides into OGCMs with less numerical resources, and do not intend to improve upon their scheme.
   (b) As the reviewer wrote, we try to show that the assumption of our scheme (i.e. decomposition of the linear tidal component and the basic component as indicated by Eq. (22)) is valid to some extent, using results of our test experiments. However, we do not think that a comparison of the reproducibility of tides with a model in which another tidal scheme is incorporated is good for it. This is because a score of skill assessment of tides does not directly prove validity of the assumption of the tidal scheme. The reproducibility of tides rather depends on implementation specification of the scheme, along with selection of the framework to introduce tides into an OGCM. (One of the main features of our tidal scheme (framework) is turning specification independently from the OGCM configurations.) Even if our low-resolution model with our new scheme achieved a root-mean-squared error less than the model with the Arbic et al. (2010) scheme, it could not be concluded that the assumption of our tidal scheme is more valid than Arbic et al. (2010). It just means that implementation specification of the tidal scheme is selected more appropriately for the former than for the latter.

   Therefore, we think that the reviewer's proposal, incorporation of another tidal scheme to the model and a comparison of the reproducibility of the tides, is not worth effort for the paper.
2. Following the comment, we additionally analyzed the density field around Hawaii to examine how the internal tides arise in our model (Fig. 11 in the new manuscript). The results show vertical undulation of isopycnals with a period of half a day and with an amplitude of 50 m, and also horizontal propagation of density anomaly signals. These suggest that excitement and propagation of the internal tides were represented to some extent, though the model meridional resolution of 0.5 °was insufficient to represent the spatial structure of the internal tides. These results are added to Sect. 3.3. (Pg. 23, Lines 12-27)

   On the other hand, we could not run our model at a different resolution, but we have a plan as written in Sect. 4.
3. As the reviewer pointed out, the wind stress (included in $\boldsymbol{X}$) and the freshwater flux F_w were not explained sufficiently in the original manuscript, as compared to the explanation of the advection term. We think that these terms should be included in the basic equations (100 %), not in the linear tidal equations, because of following reasons.
   (a) The wind-induced circulations and the thermohaline circulations resulting from these terms

should be represented in the OGCM framework. If these terms were moved to the linear tidal equations, the dynamical balance would be violated due to tidal parameterizations such as the SAL term. In other words, these terms should not be included in the linear tidal equations, since they are not specific for a barotropic tide model, $\boldsymbol{X}_{IT}$ in Eq. (20).

(b) It has not been reported that these terms change the primary response of the ocean to the tidal forcing, as far as we know. As for the studies the reviewer cited, Lee et al. (2006) studied influence of tidal mixing on a thermohaline circulation and Xing and Davies (1997) studied modification of internal tides induced by winds. This kind of processes is secondary interactions between tides and basic fields, and intended to be represented under the OGCM equations, Eqs. (13) and (14). (Even secondary modification of the barotropic tidal currents can be represented by the OGCM equations, as explained in Appendix.)

A new paragraph is added to explain the above discussion. (Pg. 10, Para. 1)

Supplementarily, we executed two additional cases, where the wind stress and F_w are moved from the basic equations to the linear tidal equations, to show the sensitivity, following the reviewer's suggestion (*Alternatively, you could perform...*).

- case RA1: move the wind stress from Eq. (13) to Eq. (11)
- case RA2: move F_w from Eq. (14) to Eq. (12)

The other configurations are the same as case TIDE. We show the results here.

Figure 13a,b show the linear tidal field η_{lt}(RA1) and the anomaly from η'_{lt}(TIDE), η'_{lt}(RA1),

$$\eta'_{lt}(\text{RA1}) = \eta_{\text{lt}}(\text{RA1}) - \eta_{\text{lt}}(\text{TIDE}), \tag{0.1}$$

at the end of the integration (40 days). Comparison between Fig. 13a and Fig. 2c in the paper shows that the η_{lt}(RA1) distribution is basically the same as η_{lt}(TIDE). However, the barotropic wind-driven circulation began to be generated in response to addition of the wind stress, e.g. decrease of 20 cm in the Antarctic Ocean, where the wind is especially strong (Fig. 13b). Next, to show the results of sum of the linear tidal component and the basic component, the tidal heights $\eta_t(\text{RA1}) = \eta(\text{RA1}) - \eta(\text{NOTIDE})$ and its anomaly from TIDE, η'_t(RA1), are calculated. Since η'_t(RA1) is deformed as

$$\eta'_t(\text{RA1}) = \eta_{\text{t}}(\text{RA1}) - \eta_{\text{t}}(\text{TIDE}) = \eta(\text{RA1}) - \eta(\text{TIDE}), \tag{0.2}$$

it represents the total SSH variation due to the movement of the wind stress term. In the results, η_t was almost identical to TIDE (Fig. 13c), and the anomaly η'_t decreased to less than 5 cm (Fig. 13d). This is because the basic component η_b changed in the opposite direction of η_{lt} in response to removal of the wind sress term from the basic equations, and therefore the two changes are cancelled out (i.e. $\eta'_t = \eta'_{lt} + \eta'_b$). Thus, as far as seeing RA1 results, systematic differences were not found in the tidal heights.

The results of case RA2 are shown in Fig. 14. In this case, η'_{lt} is noticeable in the Arctic Ocean, where the freshwater flux is large due to river runoff (10 cm at most, Fig. 14b). However, $\eta'_t = \eta'_{lt} + \eta'_b$ was as small as less than 1 cm almost over the entire region (Fig. 14d).

The results of RA1 and RA2 mean that the movement of the terms has very limited influence on

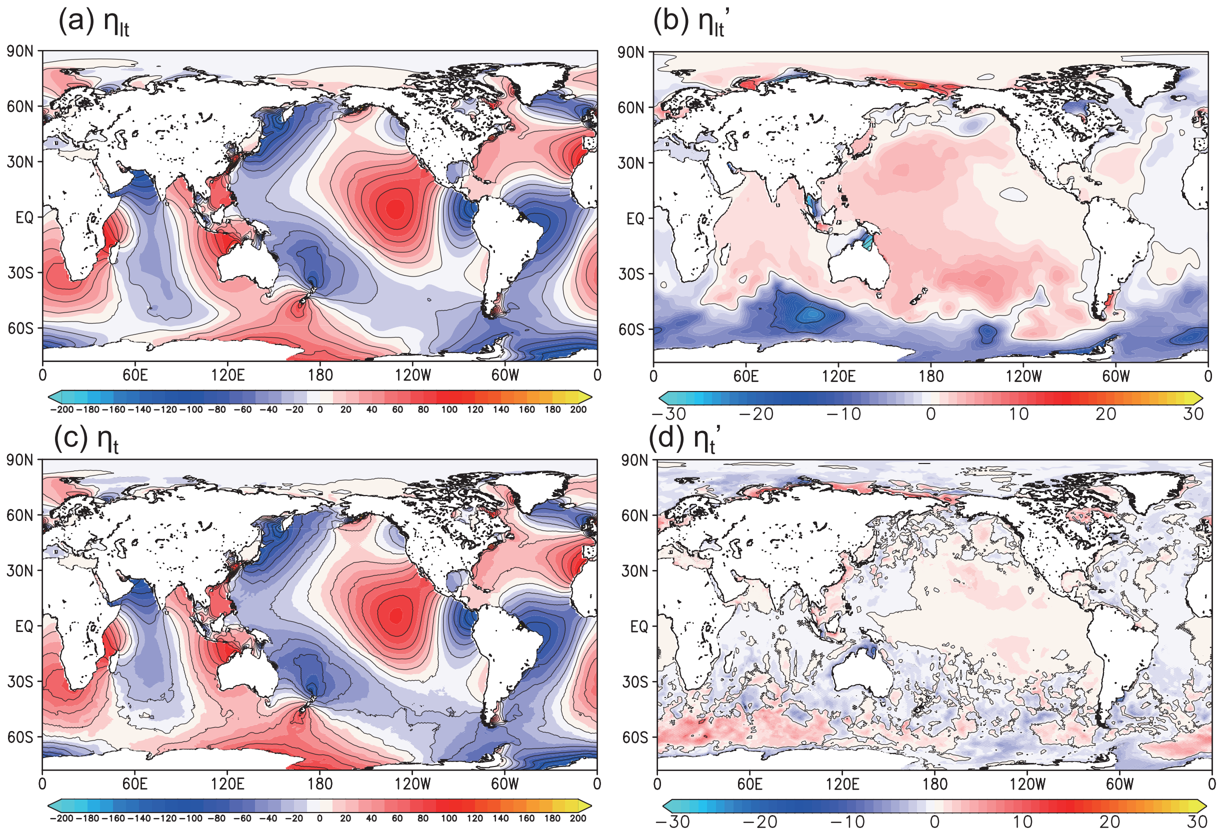

Fig. 13 (a) η_{lt}, (b) η'_{lt}, (c)η_t, (d)η'_t in case RA1. The panels (a) and (c) are the same as Fig. 2c and Fig. 2b in the manuscript, respectively. The color shades are different: from -200 cm to 200 cm in (a) and (c), while from -30 cm to 30 cm in (b) and (d).

the tides in the model, though the experimental period of 40 days is not sufficiently long. We did not write about RA1 and RA2 in the manuscript for brevity.

Technical corrections:

The manuscript is revised following all the comments. We really appreciate the advices.

- Pg. 8, Line 25 or so: *You need to specify the units of each of the variables in Eqs. (7)-(10) because it is not conventional for modelers to write the momentum equations using the depth-integrated velocities in units m$\hat{2}$/s.*

  The units are added to the explanation of Eqs. (3) and (4).
- Pg. 8, Line 25 or so:

  See the reply of *Specific comments 3*.
- Pg. 11, Line 20, 21-22, Pg. 15, Line 6:

  We removed the mention to C_d, and replaced the explanation about $\boldsymbol{\tau}_{lt}^{\mathrm{btm}}$ by a simple sentence giving a reason to use a different parameterization for the bottom friction of the linear tidal currents: "the Wetherly et al. (1980) scheme used for $\boldsymbol{\tau}_b^{\mathrm{btm}}$ is designed for the turbulent Ekman

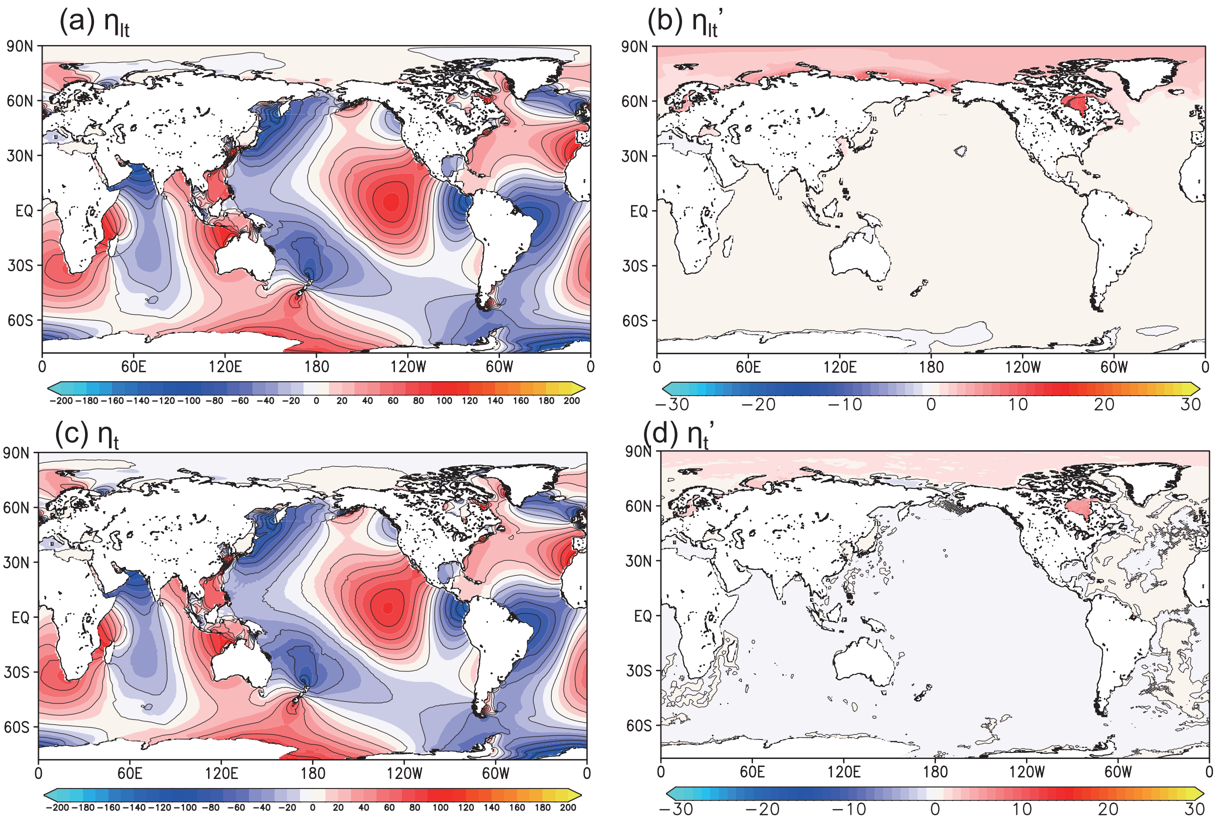

Fig. 14 Same as Fig. 13 except for case RA2.

layer and unsuitable for the tidal currents (Sakamoto and Akitomo, 2008, 2009)." (Pg. 14, Lines 28-)

- Pg. 14, Lines 1-3: *Given this admission, it would be nice to see a computation of the root-mean-square error in coastal areas (within some distance of the coasts) in Table 2, for example, and some discussion about confounding factors (e.g., no wave breaking on shelves) as well as future directions.*

  We calculated the root-mean-squared error in coastal areas (the region shallower than 1000 m) in Table 2. In the results, the reproducibility is apparently worse than in open oceans, as the error is 35.6 cm and the percentage of SSH captured is 42%. This is likely because our low-resolution model cannot represent relatively small physical processes affecting on the coastal tides, such as excitement of internal tides over shelf slopes, wave breaking on shelves and friction of complicated topographies (Xing and Davies 1997, Osborne et al. 2011, Nagai and Hibiya 2012). The results and the discussion are added following the comments. (Pg. 19, Para. 2)

- Pg. 16, Line 6:

  A physical background of usage of different values for the horizontal viscosity of D_{lt} is added after the comment, together with a reference to Polzin (2008). (Pg. 15, Line 23-)

- Pg. 18, Line 22:

The suggestion is not adopted, since we want to discuss that the difference of the reproducibility between TIDE and TIDEa1 is attributed to different representation of the tidal phases, not the tidal amplitudes.

- Pg. 18, Lines 23-24: *This seems like an orphan sentence. Elaborate to save it from being sent to an orphanage.*

  The explanation about neap and spring tides is moved to the new paragraph about long time variation. (Pg. 19, Lines 22-24)
- Pg. 19, Line 6: *Define what A is. Is A the World Ocean?*

  The definition of A is given above, as "the region ranging from 66°S to 66°N with water depth exceeding 1000 m" (This follows Arbic et al. (2004)). The expression is revised to emphasize A. (Pg. 18, Line 22)
- Pg. 21, Lines 12-14: *You should show P and W for experiments M2v2 and M2v10, or at least say that P and W are relatively insensitive, consistent with how insensitive the precision of your model is to horizontal viscosities. Instead of the sentence, "Also in our model experiments,$\cdots$ the horizontal viscosity, nu.)," say something like: "Here, we verify that this is the case using our scheme and demonstrate that P and W are relatively insensitive to the viscosity and friction used in our model. We show this by varying the horizontal viscosity, nu."*

  We cannot see the point of the comment. We used M2v2 and M2v10 to show that "tides significantly depended on the viscosity settings, especially the horizontal viscosity ν". As a result, both of $\boldsymbol{P}$ and W are sensitive to ν.
- Pg. 23, Line 21: *"The SST decrease with the inclusion of tides was especially large in shallow coastal regions; e.g., more than$\cdots$" Non-linear tidal effects?*

  We think that it is mainly attributed to enhancement of the tidal mixing (especially in the BBL). The expression is revised. (Pg. 24, Lines 18-20)
- Pg. 24, Line 10: *Is it apparent that the differences between the currents are getting larger (asymptoting to some values) as your simulation goes on?*

  We ran cases TIDE and NOTIDE for one year and examined the velocity differences. In the results, the differences did not get larger after 2 months. The results are added within parentheses. (Pg. 25, Lines 3-5)
- Pg. 25, Lines 24-25: *"In addition, enhancement of vertical mixing was found in the model,$\cdots$" You didn't show that the internal tides were excited (only found suggestive evidence), nor did you show they were necessarily realistic.*

  We left "excitement of internal tides" since analysis about isopycnal vertical displacement is added, but removed "realistic". (Pg. 26, Line 17)