Our response is shown in red italic texts below. The page and line numbers refer to those in the track-change version.

Response to Reviewer 1

The paper has made some progress after the authors revised it. For the questions raised by reviewers, some answers are satisfactory, but there are other parts that the author needs to continue to answer and give convincing evidence.

1.In the reviewer's opinion, it is still very difficult to understand that the bottom friction coefficient is set to 0 in deep water. The following is the corresponding expression in the revision text, the reviewer still really difficult to understand the bold part of the expression.

The vertical high resolution is focused on the near-surface zone at the expense of the bottom in order to conserve computational cost. As a result, the near-bottom vertical layers can be as thick as 1km in the deep ocean; in other words, the logarithmic bottom boundary layer at deep depths is not well resolved and therefore, we apply zero friction in the deep depths. Alternatively, using a small friction coefficient (10-4) gave similar results. To ensure adequate energy dissipation toward shallows, we use a simple depthdependent bottom friction coefficient (used in the quadratic drag formulation) that linearly increases from 0 at depth 200m to 0.0025 at 50m (i.e., 0 friction is used at depths deeper than 200m and 0.0025 is used at depths shallower than 50m, with a linear transition in between the two depths). For the sake of simplicity, no attempt has been made to optimize the friction in each region yet, and this is left for future work.

I don't think the author has fully explained it clearly here. There are still some questions: Are the authors clear about the specific expression of friction coefficient and its physical meaning? According to the authors, the bottom friction coefficient is 0.0025 when the water depth is less than 50 meters, while the bottom friction coefficient is 0 or 0.0001 when the water depth is more than 200 meters, which is very inconsistent with our understanding of the bottom friction coefficient in the tidal model. The author gives the impression here that the bottom friction coefficient is assigned different values at different depths in a water column. The reviewer really think it is difficult to understand. Please confirm and give convincing evidences.

Sorry for the confusion about 1.e-4. We have revised the texts on pg 5. Basically, we specify the drag coefficient C_d based on the local depth:

$$C_d = max\{C_{d2}, \min[C_{d1}, C_{d1} + (C_{d2} - C_{d1}) * (h - h_1)/(h_2 - h_1)]\}$$

where h is the local depth, $h_1 = 50\text{m}$ and $h_2 = 200\text{m}$ are the two transition depths with corresponding friction coefficients of $C_{d1} = 0.0025$ and C_{d2} respectively. In the baseline setup we used $C_{d2} = 0$, but we have also tried $C_{d2} = 0.0001$. There is a unique value for C_d along each water column.

So, we do in fact use a 0 or 0.0001 friction coefficient in deep water, but this is justified by the large thickness of the bottom layer in deep water (~1 km). This doesn't mean we think that the actual boundary layer in deep water is frictionless; this is more a numerical treatment.

2.In the text, The averaged complex RMSE for M2 is 4.2cm for depths greater than 1km, and 14.3cm for shallower depths. The averaged total RMSE for all constituents) is 5.4cm / 16.6cm or depths greater/less than 1km. The breakdown of RMSEs for the other 4 constituents (S2, N2, K1, O1) is: 2.05cm, 0.93cm, 2.08cm, and 1.34cm for depths greater than 1km; 6.07cm, 2.60cm, 4.71cm, and 2.84cm for depths shallower than 1km. These results are slightly better than the previous best 3D model results without data assimilation (Schindelegger et al. 2018) but slightly worse than those in Pringle et al. (2021); e.g., the total RMSE from their model is 3.9 cm / 17.2 cm in the deep/shallow ocean respectively. But in Table 2, RMSE M2=32cm, RMSE S2=11cm. What's wrong with the inconsistency between the above two? I feel that the results in Table 2 may be problematic.

The inconsistency in RMSE is because the comparison was done against different datasets: TPXOv9 or GESLA. The latter consist of world-wide tide gauges, many of which are located in sheltered coastal areas that have larger uncertainties in DEMs used or have not been resolved yet in the current mesh. The GESLA comparison is therefore inherently much more challenging. As you can see from Table 2, even FES (which assimilates tide gauges) has larger errors. We have added some explanation on pg 11.

3. In Table 2, unit is missing. What does the formula (5) have to do with Table 2?

About Formula (5), how do you calculate the area of the corresponding part?

Units are now added in Table 2. We are not sure about your question on Eq. (5) and Table 2; they are not related. The numbers shown in Table 2 are calculated from Eq. (6), which uses Eq. (3). Note that the averaging here is done over all tide gauges, not over areas.

The area in Eq. (5) is the union of all mesh elements. The RMSEs are first calculated in each element before area-integration.

4.The color bar in Figure 3 should be corrected. In Figure 3, I think the differences of a and b, a and d, a and e, a and f, may be a good choice. Where a stands SCHISM3D, b stands TPXOv9, d stands Antarctica Shelf, e stands Shallow removed, and f stands 8 constituents.

We have added color bars for some subplots. The differences between (a) and (b) is (c). The differences for some other subplots are tricky to compute. Computing complex differences between (a) and (e) is problematic because the area in (e) is reduced from (a). In any case, the qualitative differences as described in the text are much more telling than the quantitative differences.

Response to Reviewer 2

I thank the authors for their revisions. My previous comments have been addressed. Specifically, I think it is more clear that the model is not just similarly good at simulation of tides as other models, but does so with less calibration effort. Proof of this calibration effort relies somewhat on experience of the authors, but I think it is now sufficiently motivated. I'm still not convinced that the coastal and estuarine small scale processes beyond tides are sufficiently well resolved/calibrated so that they can be studied in isolation in this model, but the authors also discuss this properly. I think therefore the demonstration of the estuarine scale is now appropriate.

Thank you for your support.

I have some minor suggestions and otherwise am happy to accept this manuscript for publication.

Comments

- The link to the 3D model set-up is now included under 'code-availability'. This refers to a VIMS website. I'm not sure what the rules of the journal are with respect to code repositories and I recommend the authors (or editorial team) to check if this website satisfies the criteria.

We have now uploaded the zipped files to Zenodo with a DOI (see Code Availability section). The model input file size (127GB) is large so we had to split it into 4 pieces.

- Near line 150 (or elsewhere): could you refer to documentation of SCHISM, to refer readers to all features of the model?

We have added a sentence on pg 6 to refer readers to continuously updated online manual on SCHISM web site (schism.wiki) that explains details of all features.

- Ln 150-153: in point 2) you claim that your use of 'scribe' cores significantly improves parrallelisation and scaling. If available, please refer to documentation that shows this.

The details are explained in the continuously updated online manual. In addition, this claim has been corroborated by many community users.

- Ln 360: you cannot technically conclude that your results are consistent with 70-75% dissipation of energy on shallows as you did not show an analysis of the energy budget. Please weaken the statement that it is consistent with earlier findings that dissipation on shallows is important for the global tidal amplitude.

Revised on pg 18.