

Geosci. Model Dev. Discuss., author comment AC2
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Reply on RC1

Y. Joseph Zhang et al.

Author comment on "Global seamless tidal simulation using a 3D unstructured-grid model (SCHISM v5.10.0)" by Y. Joseph Zhang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-165-AC2>, 2022

My response is shown in bold italic texts below. Thank you so much for your constructive comments.

Major concerns:

- The usual numerical simulation of multi-constituents oftides basically considers four major diurnal and semi-diurnal tides, or eight major diurnal and semi-diurnal tides. Six of the eight main sub-tides are considered. In fact, Q1 is weaker than K2 and P1 tides. The author considers Q1 tides in the study, but does not consider K2 and P1 tides, which gives a strange feeling. Of course, the authors may have made this trade-off from a computational standpoint. The reviewer still insisted that either four major diurnal and semi-diurnal sub-tides or eight major diurnal and semi-diurnal sub-tides should be considered. In view of the fact that the author did not display and analyze the results of N2 and Q1 tides, it is suggested that the author remove these two tides.

>> We have actually tested including 8 constituents but decided to follow Pringle et al. to include only 6 of them in the text. The difference for M2 is negligible. We will add the results with 8 constitutents and also for each constituent in the revision.

- In Page 3, Line 89-91, These features has allowed a single model to be used for challenging compound flooding studies that involve coastal transition zones between hydrodynamic and hydrologic regimes, forced by **ocean, precipitation and watershed rivers** (Ye et al. 2020; Zhang et al. 2020; Huang et al. 2021; Ye et al. 2021). Necessary modifications are required. Where ocean, precipitation and watershed rivers are not a juxtaposition.

>> We agree and will revise the texts. Compound flooding will be future development.

3. In the reviewer's opinion, it is very difficult to understand that the bottom friction coefficient is set to 0 in deep water. Please confirm and give a clear explanation. The following is the corresponding expression in the original text, the reviewer really difficult to understand the bold part of the expression.

Page 4, Line 110, As a result, the near-bottom vertical layers can be as thick as 1km in

the deep ocean; in other words, the logarithmic layer there is not well resolved and therefore, we apply **zero friction in the deep depths**.

Page 4, Line 111-113, To ensure adequate energy dissipation toward shallows, we use a simple depth-dependent bottom friction coefficient (used in the quadratic drag formulation) that linearly increases **from 0 at depth 200m to 0.0025 at 50m**.

>> To save computational cost the vertical grid coarsely resolves the deeper depths such that some bottom layer has thickness of ~1km. Therefore, the assumption of a logarithmic velocity profile in the bottom boundary layer is not appropriate. We could use a very small drag coefficient in lieu of 0 but the effect is similar: the velocity profile inside the bottom layer should not be strongly affected by the friction when the layer is 1km thick.

- Page 4, Line 106-107, The number of sigma layers varies from a maximum of 34 to 1 (i.e. 2DH configuration), with **an average of 32 layers**. What does average mean? How it's calculated?

>> The LSC² vertical grid system in SCHISM allows different numbers of layers to be applied at different horizontal locations, and the average number of layers is simply the arithmetic mean of the number of layers at each node.

- In Page 5, Line 138-139, Relaxation of temperature and salinity near the ocean surface, which is commonly utilized in many global ocean models (Ringler et al. 2013), was not applied here due to the relatively short duration of the simulation. This is an awkward statement. What does the author mean by this expression? Ask the authors to give an explanation.

>> In climate models it's a common practice to relax near-surface T,S to climatological values in order to prevent drift of simulation over a long time. This is not used here. We'll add some explanation in the manuscript.

- In Page 8, Line 196-199, The averaged complex RMSE for M2 is 4.2cm for depths greater than 1km, and 14.3cm for shallower depths. The averaged RMSE for the remaining frequencies (S2, N2, K1, O1, Q1) is 5.4cm / 16.6cm or depths greater/less than 1km. These results are slightly better than the previous best 3D model results without data assimilation (Schindelegger et al. 2018) but worse than those in Pringle et al. (2021). Here the reviewer thinks it must be pointed out that in the numerical simulation of multi-tidal, the evaluation indexes of all sub-tidal should be given in detail. In this paper, the author gives the index of M2 sub-tide alone, and the other five sub-tide indicators are combined.

>> We have those numbers and will add them.

- Page 8, Line 203-206, Compared to other global 3D models, our model seems to be able to obtain satisfactory results without the need for some elaborate drag formulations described in A18, which might be attributed to the fact that the higher resolution used in the coastal ocean has provided adequate energy dissipation. I don't agree with the author here, the higher resolution used in the coastal ocean is not a panacea. Ask the authors to give an explanation.

>> Indeed this is speculative and that's why we used 'might'. Will revised.

- Page 10, Line 245, Table 1. Summary of model performance to reproduce the main

semi-diurnal tidal component for SCHISM and FES2012 models against GESLA. Are the indicators here consistent with those in lines 196-199? I think this table currently lacks a clear interpretation.

>> *The GESLA comparison is complementary to the co-tidal chart because it's focused on nearshore. We will add more explanations for the table. Thank you.*

Minor concerns: **>> *Will correct those issues.***