In this paper, authors present a new 3D unstructured-grid global ocean model to study both tidal and non-tidal processes, with a focus on the total water elevation. Unlike existing global ocean models, the new model resolves estuaries and rivers down to ~8m without the need for grid nesting. The model is validated with both satellite and in-situ observations for elevation, temperature and salinity. The authors demonstrate the potential for seamless simulation, on a single mesh, from the global ocean into several estuaries along the US west coast. The model is able to accurately capture the total elevation, and qualitatively capture the challenging salinity intrusion processes in the Columbia River. The model may be potentially serve as the backbone in a global tide-surge and compound flooding forecasting framework. There is no doubt about the importance, innovation and value of this work. However, in the writing and presentation, some places still need further explanation. I think this paper satisfies the scope and standard of Geoscientific Model Development, but I also have some concerns. Therefore, I recommend a major revision.

Major concerns:

- The usual numerical simulation of multi-constituents of tides 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.

- 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.

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.

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?

- 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 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.

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.

- 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.

Minor concerns:

Abstract, Line 14-15, Tidal elevation solutions have a mean complex RMSE of 4.2 cm for M2 and 5.4 cm for combined 5 other major frequencies in the deep ocean. I think the evaluation indexes of all sub-tidal should be given in detail. Where frequencies should be constituents

Page 8, Line 197, the remaining frequencies should be the remaining constituents

Page 16, Line 326, sstems should be systems

Page 17, Line 356, The large stratification should be The strong stratification

Page 20, Line 391-392, The simulated tide showed good skill, with a mean complex RMSE of 4.2cm for M2 and 5.4cm for the 5 other major frequencies in deeper depths. What is deeper depths mean?