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Geoscientific Model Development Discussions

Interactive comment on "Global Storm Tide Modeling with ADCIRC v55: Unstructured Mesh Design and Performance" *by* William J. Pringle et al.

Anonymous Referee #1

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This paper by Pringle et al. presents recent developments of the circulation model ADCIRC that allow simulating efficiently tides and storm surges at global scale. The paper is well-written and organized, the figures are clear and the topic addressed fits well the scope of the journal. However, while storm surge predictions are rather good for a global model, tidal predictions are locally weak compared to other well-established global tidal models. Thus, in the Bay of Biscay, the RMSE on M2 reaches 0.12-0.15 m, that is more than 10% once normalized by the amplitude of this constituent. Over the Patagonian Shelf, RMSE on M2 reaches 0.25 m, which again represents errors over 10 %. In these regions, other global models have errors of a few % in these areas, see for instance a paper describing the hydrodynamic version of FES2014 (i.e. without

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Discussion paper

assimilation) under discussion in Ocean Science (Lyard et al., 2020). For this reason, I think that the paper cannot be considered further for publication until the authors explain why the model is locally not reproducing tides correctly or better, improve their results. Indeed, only discussing the improvements compared to the previous version of global ADCIRC is not sufficient as tidal predictions from this version of the model were really bad (i.e. errors on M2 locally > 20%).

I would also have the following along-the-text comments:

-L35: I would indicate somewhere that all these studies neglected the contribution of short waves, although this process can drive a "regional setup" (i.e. a storm surge extending outside surf zone) reaching 0.5 m (e.g. Fortunato et al., 2017).

-L73: as the model is used to compute storm surges, you should explain how Cd is computed/which bulk formula is used.

-L100: please explain how much larger

-L106: "obtain" rather than facilitate?

-L157: Gulf of Mexico rather than Western North Atlantic?

-Table 3: please compare with Figure 12 in Lyard et al. (2020), where FES2014 yields errors on M2 < 0.5 cm in deep water and <4 cm on the shelf, that is about one order of magnitude smaller than here.

-L249: as shown by several studies (e.g. Townend and Pethick, 2002) and synthetized in Idier et al., (2019), representing flooding in storm surge models results in lower water levels seaward compared to simulations where the flooding is not represented. Therefore, I expect that water levels in the present simulations are biased high due to this process, possibly by 0.5 to 1.0 m considering previous studies on the topic.

-L256: please refer to Bricker and Roeber (2015) who showed that Hayan also drove very large infragravity waves, which could explain the large scatter on HWMs observed.

-Figure 11: for Katrina, the model displays a 0.5 m negative bias before the surge peak, could the authors comment on the possible causes? Could it be related to the 2DH approach which only allows for a crude representation of Ekman transport?

-L357: please correct “are be able”

-L376: I m not sure that this conclusion is very robust based on a model that does not represent flooding (see my previous comment).

Cited references: Fortunato, A.B., Freire, P., Bertin, X., Rodrigues, M., Ferreira, J. and Liberato, M.L., 2017. A numerical study of the February 15, 1941 storm in the Tagus estuary. Continental Shelf Research 144, 50-64.

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