Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-51

Anonymous Referee #2

Referee comment on "Australian tidal currents – assessment of a barotropic model (COMPAS v1.3.0 rev6631) with an unstructured grid" by David A. Griffin et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-51-RC2, 2021

This paper discusses the comparison of a new tide model for the waters surrounding Australia and both tidal heights and currents observations, with a dedicated focus on future operational tidal currents prediction (from model simulations) added value.

The compilation of tidal observations, especially tidal currents, is rather impressive and will provide a very useful database for further studies and/or model validation. The comparisons between the model's simulations and observations are exhaustive and detailed, with very informative focus on regions of special interest. Currents data processing and inherent limitations are well presented and discussed. The figures where model and observed currents ellipses are very interesting, however the red colored observed ellipses are sometimes hardy distinguishable of the background currents amplitude pixels. I might suggest showing the model grid itself in an additional figure. Same remark about tidal heights vector errors in addition to the modelled/observed amplitude and phase superimposed ones.

The model is based on a new implementation of shallow water dynamics on an unstructured grid. As far as I understood, COMPAS model is a local evolution of the MPAS one, or at least inspired from it. Unlike the work made on the tidal observation compilation and processing, I find the modelling work rather not sufficiently convincing.

My first remarks concern the model grid design and setting. COMPAS developers made the choice of a basically hexagonal grid (and subsequent finite volume discretization). Despite some flexibility to tune the model resolution, it is much less flexible than triangle element grids, especially in following precisely the coastal geometry. Authors may comment on their choice. The model resolution constraints (depth and currents magnitude) are also a bit surprising to me. In tidal applications, coastal geometry complexity, tidal wavelength (theoretically related to square root of depth, but possibly strongly controlled by local coastal geometry/dynamical resonance) and depth's slope related tidal currents variability scales are the most efficient constraints in setting the appropriate local resolution, especially when tidal currents are specifically targeted. I'd like authors to comment on

that. The setting of bathymetry is mostly set from the best available global datasets for Australian Waters, still I wonder about the choice to extend the uncovered areas with DBDB2, which is a rather ancient bathymetry database. Authors may comment on their choice. The setting of the minimum model depth suggests to me that wetting/drying capabilities were not available/used in the tidal simulations. This is by itself an annoying limitation, but also minimum depth settings can significantly change the model results and, in case where the original bathymetry dataset is accurate enough, deteriorate the simulation accuracy (reversely, a 5 to 10 m minimum depth setting can help to partly compensate for bathymetry inaccuracy in nearshore regions). I'd like authors to comment on that.

My second set of remarks concerns the tidal forcing and dissipation. First having the best performances with the tidal potential left off is not a good indicator of the model performances. Also tidal loading and self-attraction forcing terms are not mentioned at all, I guess they are just no considered in COMPAS. If I am right, this is a very annoying omission for accurate tidal modelling. Equally important, the barotropic tides generate internal tides when their energy fluxes propagate across the shelf slope, and then are partly dissipated by the subsequent barotropic to baroclinic energy conversion. This is a quite large contributor to the barotropic tides dissipation, and it must be implemented through a parameterization in depth-averaged tidal models to reach the best accuracy, even at regional scales. Again, this point is not mentioned in the paper, I just can guess that such a convenient parameterization is not available in COMPAS. Many places in the Australian Waters are very challenging in terms of tidal dynamics, and will require raising the COMPAS tidal capabilities to a more comprehensive level, or at least discuss the impact of the missing tidal ingredients. I'd like authors to comment on these critical issues. Last but not least, the open boundary conditions setting can be potentially critical in the overall simulations accuracy,their discussion in section 2 could be complemented with a domain-wide vector difference between the forcing atlas (TPXO) and COMPAS results.

In summary, the observational and comparison sections are very informative and well organized, and I think they are fully suited for publication. Reversely, the modelling part really needs to be augmented/revised/strengthened. Consequently, I encourage the authors to make the necessary changes to the modeling sections to reach the same level of scientific value as for the observational ones. In consequence, I will consider publication after a major revision of the modeling discussion, with no doubt that the authors will be successful in submitting a more appropriate version. I will be happy to review any new submission, and will provide a more detailed review at this occasion as the present version is susceptible to significantly vary in the revised one.