

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-441

Anonymous Referee #2

Referee comment on "Formulation of a new explicit tidal scheme in revised LICOM2.0" by
Jiangbo Jin et al., Geosci. Model Dev. Discuss.,
<https://doi.org/10.5194/gmd-2021-441-RC2>, 2022

The authors introduced the direct effect of the gravitation of the Sun and Moon for the external forcing in the tidal model, i.e. the equilibrium tidal potential. This is different from a traditional method which uses a sum of tidal constituents, obtained by spherical harmonics expansion. The authors ran an OGCM by this new tidal forcing, and showed that reproducibility of sea level variations was improved. Although this result could be an important achievement in tidal modeling, the current manuscript is insufficient for explanation, model experiments, and analysis. I encourage the authors to resubmit the paper after a drastic improvement, at least in terms of the points listed in "Major comments".

Major comments

#1

The advantages of the new scheme are not explained clearly. Normally, we treat a tidal potential individually for each tidal constituent specified by the so-called Doodson number, but I think that this paper deals with all the tidal constituents directly from the first principle. In the paper, the advantages of the new method are not theoretically discussed, so the purpose of its introduction is unclear. The traditional method also has advantages, for example, changing the tide parameters such as the love number for each tide, targeting specified tidal constituents, and so on.

#2

I think that the verification approach using an OGCM is not suitable for the purpose of this paper, which is to propose a new tidal scheme. The authors should first verify it by a barotropic tide model. In addition, as the authors wrote, tide models have various tuning parameters, so the accuracy should be compared after tuning the parameters for the two schemes. Alternatively, the authors may explain theoretically the errors inherent in the traditional method, and show that they would be eliminated.

#3

If the authors still want to introduce the new tidal forcing into an OGCM, the introduction method should be reconsidered. As discussed in detail in Arbic et al. (2010) and Sakamoto et al. (2013, DOI:10.5194/os-9-1089-2013), replacing the barotropic equation in an OGCM by Eq.(10) in the paper leads to disrupt the dynamical balance of the ocean circulation in the original OGCM. There is no point in verifying the model results in such a situation.

Minor comments

#4 Abstract

Quantitative evaluations are necessary when discussing accuracy.

#5 Section 2.

Add some appropriate references for the gravitation of celestial bodies (a textbook?). Description of variables is also insufficient.

#6 Eq. (4)

I cannot follow derivation of Eq.(4).
Please give a supplement or reference for readers.

#7 Eq. (9)

There is no need to separate cases, since the value of " $\cos T_m$ " is the same.

#8 L.187 "the negative regions of the spring tide..."

The meaning is not clear. What part of Schwiderski (1980) do you refer to?

#9 L.199-201 "The different distribution..."

Enrich arguments to support the conclusion.

#10 Section 4.3

The definition of "Dynamic sea level" is required.

#11 L.313-315 "Therefore, compared to Exp 1..."

Why does the Exp 2 improve? The reason should be discussed.

