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## **Comment on os-2021-98**

Anonymous Referee #2

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Referee comment on "Tide characteristics and tidal wave propagation in the Persian Gulf" by S. Mahya Hoseini and Mohsen Soltanpour, Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-98-RC2>, 2021

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Review of manuscript OS 2021-98 "Tide characteristics and tidal wave propagation in the Persian Gulf" by S. Mahya Hoseini and Mohsen Soltanpour.

The paper presents a study of the tidal characteristics of the Persian Gulf and on the influence of Coriolis forces, bathymetry, and bottom friction on its propagation. The authors present a validation of the model and discuss the numerical model results and those obtained for an idealized case. The authors claim that the "tidal modelling in the Persian Gulf needs improving despite previous efforts with comparisons to new water levels...", however this new data covers the same period that the other studies they cite. The novelty of the study is not clear enough in the current manuscript and it is not clear that this model performs better than the one followed by other studies that they cite e.g. Ranji and Soltanpour since no comparisons are provided. The manuscript will also benefit of clarifications at some points. I cannot recommend the publication of this paper. Below I provide some more comments that hopefully will help in future submissions.

The description of the model can be improved. The information on how the model is forced is buried at the end of the section and the way bottom friction is considered is not very clear. It may be clearer to present the model first and then the sensitivity tests performed on the position of the open boundary and the Manning coefficient.

The period of validation seems too short (i.e. about 9 days). The authors do not show that the spring and tide cycle is accurately reproduced. According to Table 1, the authors should have tide gauge data available to provide a longer validation period.

There are 28 figures, which seems excessive. Some figures could be combined together (e.g. Figure 1 and Figure 6) and others are probably not necessary (e.g. 5). I am a bit confused by the results presented since some figures lack an explanation of how they were derived (i.e. Fig. 4, Fig. 20, Fig. 25, Fig. 26) or about why those stations or data are chosen and not others (e.g. Fig. 9). Some figures lack units on the y axis (Fig 8 and Fig. 11) and others would have increased readability with a larger font.

The authors say that they select Jask-Almasnaeh as the open boundary. This is the larger domain of the two that they have. However, in figures 12-17 they present results for the small domain, in Fig 21-22 they present the results for the large domain, and again in Fig. 23-24 and 27-28 they present the small domain.

Notes:

L110-117 and table 2 I do not think this is necessary in the introduction

L130 "The mangrove forests and salt marshes at the Khuran Channel, located north of Qeshm Island, are excluded from the computational domain to improve the modeling results" I do not understand how excluding them can improve the results, do you mean you do not consider a different friction there? Is this land? Also because afterwards in L137 the manuscript reads "the grid points is also increased near mangrove locations in the Khuran Channel."

Fig. 6, 18, 19, 21, 22 and fig.23, 24,27 and 28 Please change the jet color scheme and a divergent colour scheme helps understanding when negative and positive values in the same figure and so increases and decreases. Jet rainbow colour schemes can distort perceptions of data and alter meaning since they create false boundaries between values (e.g. Thyng et al (2007))

L150-151 "Although the differences of statistical measures are not significant, it is observed that all three indices correspond to the optimum Manning number of  $60m^{1/3}/s^{-1}$ " This sentence needs reformulating, it is not clear what the authors mean. And the authors should also indicate what the underlined values in the table mean.

L180, L189, L417 Please avoid using good when assessing the model those are subjective terms. The model is satisfactory for the purpose of the study or it is not.

L243 "It is also observed that the map fully conforms to the contribution of shallow-water constituents in Fig. 4. As an example, because of the decrease of water depth the contribution of shallow-water constituents increase from 245 3.3% to 8.9% from Larak to

Pohl stations. " Not sure to what the authors refer to here, Fig4 shows the contribution of tidal constituents on the northern coastline. It is not clear how figure 4 was obtained and except for Pohl station, there are no stations on Fig. 4 on areas where the amplitude of shallow water constituents is high.

L 264-270 Please reformulate, I do not follow the reasoning in this paragraph. How are the checkpoints chosen?

L 285-291 It is not clear for the second and third model whether there is Coriolis forcing or not. Why is 36m the depth chosen? From text later on, I understand this value of 36m is the average depth of the PG, but this could be presented here already.

L294-315 provide theoretical information about the development of amphidromic points when constrained bathymetry and influence of Coriolis. However, the information is not presented in relation to the present study.

L426-428 I understood from the description that the bathymetry was considered constant in the second test while the authors refer here to a transverse step that is not shown in the results of the numerical model. A transverse step is shown in Fig. 25 however it is not clear how this figure is produced.

#### References:

Thyng, K.M., C.A. Greene, R.D. Hetland, H.M. Zimmerle, and S.F. DiMarco. 2016. True colors of oceanography: Guidelines for effective and accurate colormap selection. *Oceanography* 29(3):9–13, <http://dx.doi.org/10.5670/oceanog.2016.66>