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Comment on gmd-2022-189

Anonymous Referee #1

Referee comment on "GULF18, a high-resolution NEMO-based tidal ocean model of the Arabian/Persian Gulf" by Diego Bruciaferri et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-189-RC1, 2022

This is the first review of « GULF18, a high-resolution NEMO-based tidal ocean model of the Arabian/Persian Gulf » by D. Bruciaferri et al. The study introduces two new dynamical ocean models of the Arabian/Persian Gulf. The authors evaluate in particular the effects of modifying the model vertical coordinates and the NEMO codebase against available observations of tides, hydrography and surface currents. They identify an improved representation of the Gulf hydrograpy and surface currents in the GULF18 models, with a slight added value of the GULF18-4.0 configuration. However, due to limitations in the representation of coastal shallow waters, tidal representation at the coast is degraded in the GULF18 models compared to the former PGM4 configuration. Overall, I found the manuscript very well written and illustrated. The authors have a deep insight into the NEMO model, ocean dynamics and the relevant physical processes at stake at the Gulf. They provide a thorough evaluation against a diversity of reference observations, despite the limited available in situ measurements in this basin. They provide overall thoughtful and convincing interpretations of the differences between models. I have one general concern but I would still recommend a minor revision given the quality of this work.

My main remark relates to the evaluation of simulated drifter tracks. The models being uninitialized, only the forced ocean dynamics (e.g. tides, Ekman currents) can be accurately reproduced, whereas internal variability ((sub)mesoscale eddies and filaments) suffers from the "double penalty effect", as mentioned by the authors. I feel that by removing trajectories with a skill score <0.35, the authors force a positive assessment on the ability of uninitialized runs to reproduce observed drifter tracks. I have listed below some suggestions to adapt this section in order to separate more explicitely the evaluation of the forced versus internal variability:

- what are the results when also including trajectories with ss<0.35? Are the conclusions

unchanged regarding the comparison PGM4-GULF18 and the skill for those unassimilated runs?

- instead of a deterministic evaluation, would a statistical analysis (e.g. the surface geostrophic EKE against along-track altimetry, or the ensemble average dispersal rate across all trajectories) be more relevant to evaluate the internal dynamics of those unassimilated runs?

- given the fact that only the forced variability can be accurately represented by those unassimilated runs, wouldn't it be more relevant to evaluate specifically tidal and Ekman currents, instead of the total trajectories?

Detailed comments :

- L.71-72: as shown in Table 1, the two GULF18 models do not only differ in the NEMO version and vertical discretization. I would moderate this statement.

- Table 1: apart from model version and vertical coordinate, a large difference I see between both GULF18 versions is the value for the harmonic diffusivity. I have two questions with that regard: 1. you mention a Smagorinsky-like diffusivity for GULF18-3.6, but in the NEMO 3.6 user book the Smagorinsky formulation concerns momentum viscosity, and not tracer diffusivity. Can you clarify that? 2. What motivated you to switch to a constant value?

- L.104-105: could you calculate the Rossby radius from the model interannual mean density field to confirm that GULF18* models are eddy-resolving? Given the shallow bathymetry and the low stratification, I doubt that even at 1.8km resolution the first Rossby radius is resolved by a few (typically 5-10) model grid points.

- L.162-169: you could mention where the mesh is terrain-following, and where it is geopotential, and relate that to the relevant processes to be resolved. For example, I note that the upper enveloppe is oriented along geopotential surfaces where H>180m,

which I assume is relevant to minimize HPGE while accurately representing mixed layer processes.

- L.197: helps

- L.198: from Fig.2c model levels seem to be terrain-following

- L.213: did you compare with the TEOS10 formulation, which has now been the reference EOS for seawater for over a decade?

- L.237: did you check the occurrence of statistic instabilities upon initialization? Initialization in the winter increases the risk of such spurious initial behaviour due to the reduced stratification, which can degrade water mass properties.

- L.245: for the PGM4 flux forcing, what Bulk formula was used in the forcing atmospheric model to produce the air-sea turbulent fluxes? Also, what is the forcing frequency?

- L.306: is R sampled randomly from a uniform distribution?

- Equation 2: a parenthesis is missing to include the observed values

- L.349-352: even in the cases where ss>0.35, there might be some double penalty effect in the GULF18 simulations compared to PGM4. What about only comparing current statistics instead of simulating trajectories for those uninitialized runs?

- Fig.6: could you add a reference cotidal chart from say the FES2014 reference data?

- L.377-379: if the PGM4 land-sea mask and bathymetry modifications improve tidal representation at the coast, why not apply the exact same procedure to the GULF18 models?

- L.394: it is not clear to me from Fig.1d that the 10m bathymetry approximation is more frequent in GULF18 than in PGM4. It rather seems to apply to different locations.

- L.410: what is the depth difference between the first model level depth and the OSTIA reference depth used to reconstruct the observed SST? Can this difference affect the model evaluation, especially in the summer when near-surface temperature stratification is large?

- L.413: regarding the PGM4 warm anomaly with respect to GULF18 models: are the parameters of the GLS scheme identical to the GULF18 models? What background vertical tracer diffusivity is used, and does it differ from the GULF18 models? Regarding the solar radiation penetration, are the characteristic decay depths of the GULF18 models from the NEMO RGB scheme higher than that of PGM4?

- Fig.9-10: displaying reference observations before showing model biases would help identify the relevant patterns and water masses.

- L.504: could you quantify that by performing the RMSE for those four vertical profiles?

- L.513: there seems to be an overall sub-surface saline bias in the GULF18 runs, more so than for PGM4. did you compare the terms of the freshwater budget (evaporation, precipitation, river runoff)?

- L.517-518: if Fig. 12 is not commented anywhere in the manuscript, I recommend removing it.

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Fig.13: I think it would read better with the bias of each model against observations for Fig.13e-g, and in the same order as the absolute values are shown (PGM4 and then both GULF18 runs)

- L.532: accurate

- Table 6: those large skill scores give the impression that data assimilation is not necessary to predict particle dispersals, however all trajectories with ss<0.35 have been priorly removed. Some suggestions:

  - what are the results when also including trajectories with ss<0.35? Are the conclusions unchanged regarding the comparison PGM4-GULF18 and the skill for those unassimilated runs?

  - instead of a deterministic evaluation, would a statistical analysis (e.g. the surface geostrophic EKE against along-track altimetry, or the ensemble average dispersal rate across all trajectories) be more relevant to evaluate the internal dynamics of those unassimilated runs?

  - given the fact that only the forced variability can be accurately represented by those unassimilated runs, wouldn't it be more relevant to evaluate specifically tidal and Ekman currents, instead of the total trajectories?

- L.546 and Fig.14: I do not see the cyclonic flow. Southward drift demonstrates an overall surface southward advection. The cyclonic flow would be an extrapolation assuming a northward return flow further east and an eastward boundary current along the southern boundary, which is not evident from the drifter trajectories.

- L.551: seem

- L.554: seems

- L.563: present

- L.628: generates

- L.653, 655: ran

- L.10-11: after reading the manuscript and in particular Fig.7, I feel that the sentence on comparable results across models for tides is not faithful to your results. I recommend to acknowledge in the abstract that tidal dynamics are degraded near the coast in the GULF18 simulations, e.g.: "For the surface currents the three models give comparable results. However, for the tides, the PGM4 overperforms the GULF18 models. Our tidal harmonics analysis suggests that..."