

Interactive comment on “Non-linear aspects of the tidal dynamics in the Sylt-Rømø Bight, south-eastern North Sea” by Vera Fofonova et al.

Anonymous Referee #1

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This paper focuses on tidal dynamics in the Sylt-Rømø bight as modeled by the unstructured grid FESOM-C model. The model has a high spatial resolution and bathymetry. The paper discusses effects of model spatial resolution on the circulation results and outlines characteristics of tidal dynamics in the basin.

The application and results are interesting and as such I would recommend the paper to be published. However, the manuscript needs to be improved; most notable the model setup and used methodology should be described more accurately to allow reproducibility.

Specific comments

Section 2.2: It would be useful to include a figure of the meshes to a) see the exact

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location of open boundary and b) details of the mesh resolution in crucial areas (e.g. Lister Deep, shallow areas). As the grid plays an important role in such studies, the authors should elaborate on the criteria used to choose the mesh resolution.

line 103: Which turbulence closure model are you using? E.g. k-epsilon, or one of the Generic Length Scale models that GOTM provides?

Section 2.3: The reference to the NEMO model is inaccurate. I presume that in this study the European north-west shelf model is used and it should be cited appropriately. Why do you compute tidal harmonics from the shelf model results? This potentially introduces an error source; it would be better to use the elevation time series itself as it includes atmospheric effects.

Open data: Are the bathymetric and ADCP data sets introduced in this manuscript publicly available?

Section 2: The model configuration should be elaborated. What were the calibration and analysis period(s)? What were the initial conditions? Was there a spin-up period? It appears that open boundary conditions were calibrated with a 2D model using bottom drag parametrization (lines 224-226), while subsequent results were carried out with a 3D model. For clarity, please define these configurations.

Harmonic analysis: The observations of velocity and water elevation observations do include atmospheric effects which are absent in the tidal models. What were the atmospheric conditions during the calibration and validation periods? Can you estimate their impact on the error metrics? Are the tides in this small-scale system really stationary so that harmonic analysis is well defined?

line 195: Is C_d constant in space? If so, is that a realistic configuration for the bight?

Tide gauge comparisons: It would be useful to have example time series comparing the observations and the model to give a better idea of the model's performance. A Taylor diagram could also be used.

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line 219: Do you use a sponge layer? If so, please describe it in Section 2.

Baroclinicity: The tidal dynamics, e.g. tidal ellipses, are probably affected by density and stratification effects which in the present study is neglected. Can you argue that baroclinic effects are negligible in this system?

Section 5.2: Larger dissipation in the unstructured grid could also be due to better resolved intertidal dynamics that are inherently dissipative. Presumably also the bathymetric features are quite different in these two grids.

The authors conclude that the model results "converge" to a realistic solution (line 81, 478, abstract), based on the presented simulations with two different grids. The authors also conclude that the curvilinear grid has lower dissipation making it thus better suited for baroclinic studies. I find these conclusions somewhat premature: Only two different grids were used, which at the same time had different element types (triangles and quads), resolution and topology (unstructured and curvilinear), as well as (I presume) bathymetry. As such, it is really quite hard to infer what grid properties cause the observed change in model performance. The grid sensitivity study should be extended to better address the effects.

Technical corrections

line 123: $1/30$ *degree*

line 182: this paragraph is duplicate of the previous one.

Figure 3 a: The ratio being thus defined it would be more appropriate to call it "weight of linearity" instead. The unit (m) is wrong.

Table 3: Add units. What do "RMSD", " $\text{amp}(\text{cm})$ ", " $\text{ph}(\circ)$ " stand for?

Figure 9: Add units.

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