Referee 1

Dear Referee 1,

I am very grateful to you for the valuable and useful comments and remarks! Please, find below the answers to the comments and remarks.

Kind regards, Vera on behalf of the co-authors

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

Thank you for the comment! Please, find an answer below.

The minimum grid cell size (2-14 meters) was chosen based on available bathymetry data resolution. In case of the first-curvilinear grid the cell sizes are determinate by the square of the subarea. The whole domain was divided into 6 subareas (Fig. 1a). The smallest among them are those, which are covered the Königshaven area and Lister zone. The division of the domain into subareas was dictated by the bathymetry information and area of particular interests - Königshaven, Lister Deep and ets. Figure 1 shows the subdivisions, grid quality and final nodal areas for the curvilinear grid.

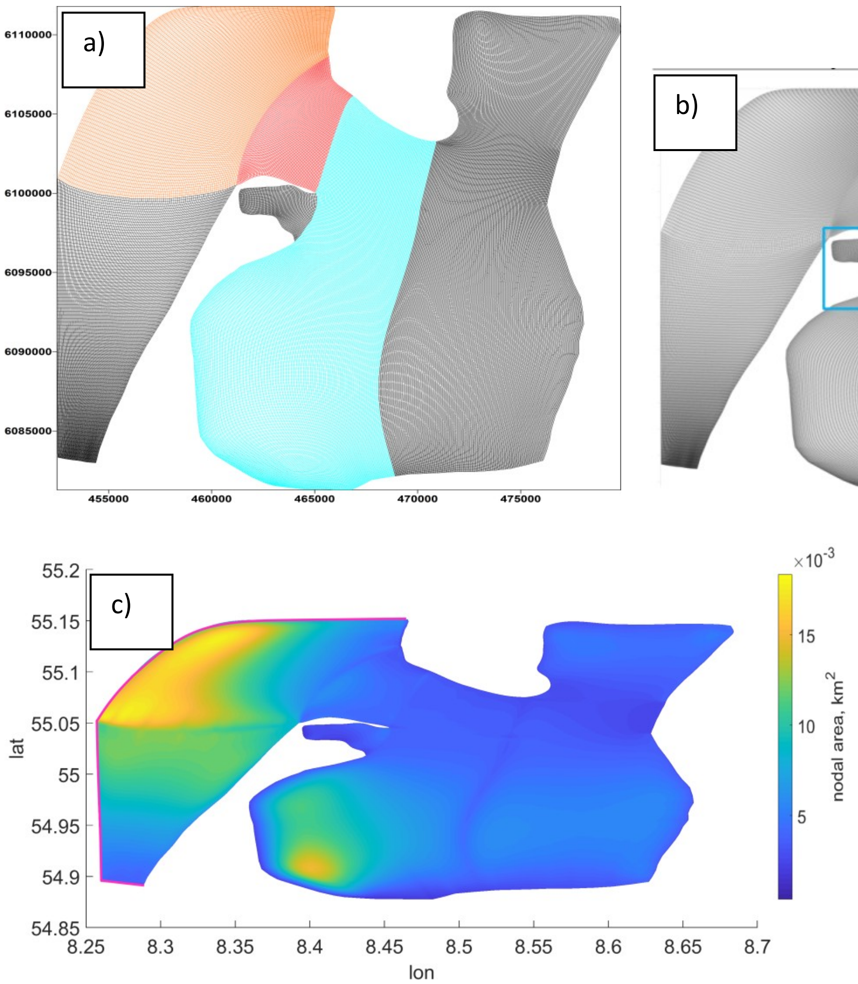


Fig. 1. Demonstration of the curvilinear grid: a) subdivision into subareas; b) grid quality; c) nodal area, pink color indicates open boundary position.

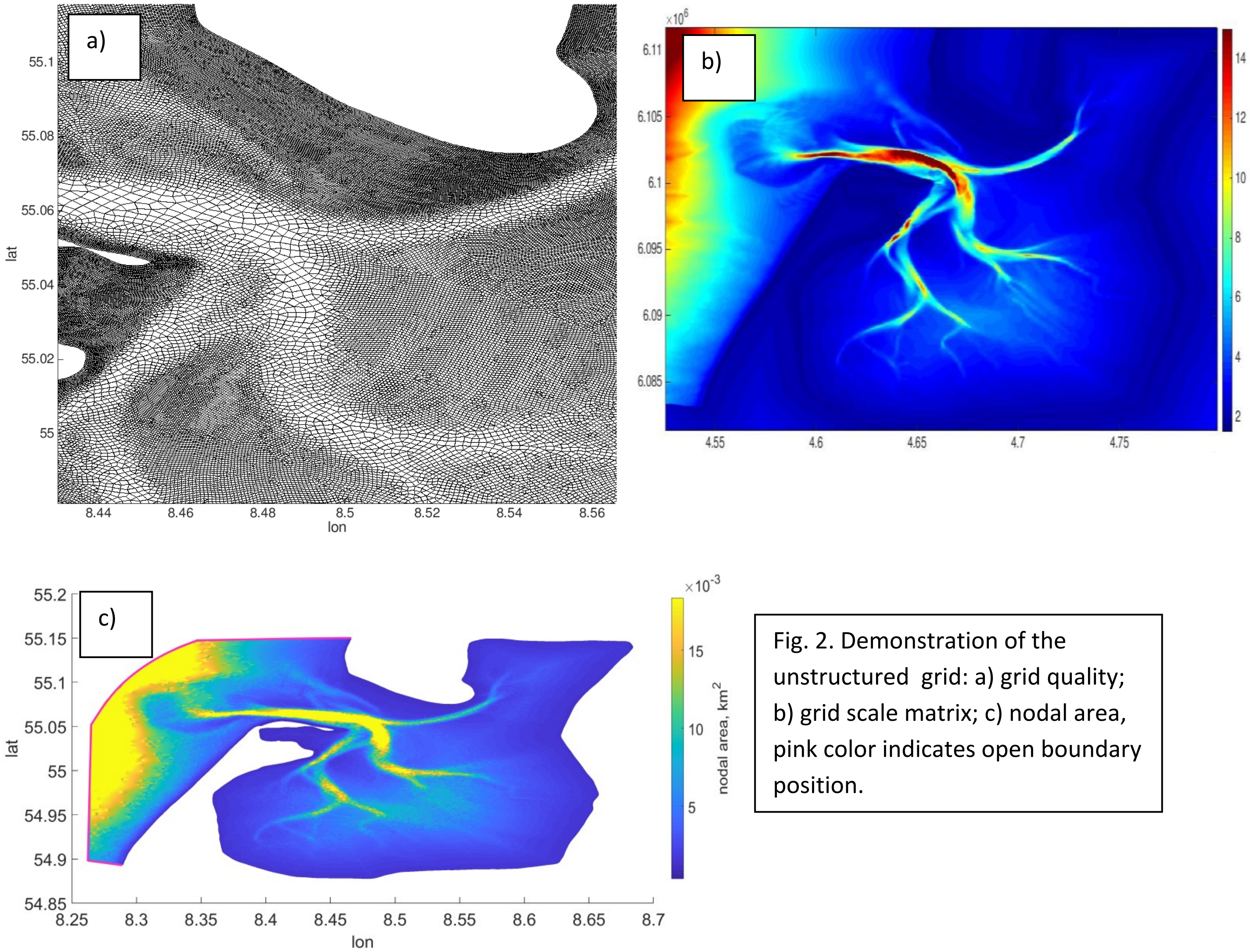


Fig. 2. Demonstration of the unstructured grid: a) grid quality; b) grid scale matrix; c) nodal area, pink color indicates open boundary position.

In case of the mixed mesh (second grid) we were more flexible in choosing of the grid sizes. The matrix of element sizes was constructed based on the information about the bathymetry, the bathymetry gradient and the zones of particular interest (Lister Deep; main draining channels). Figure 2 shows the grid quality and nodal area.

Both grids have nearly the same open boundary position.

The additional figures have been added to Appendix, the additional notes have been added to the text of the manuscript.

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

The k-epsilon closure was used. The information has been added to the text.

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?

Yes, the European north-west shelf model results were used, the ocean part of which is based on NEMO. We have completed the reference by citation of the web-source with full description of the setup: http://marine.copernicus.eu/services-portfolio/access-to-products/?option=com_csw&view=details&product_id=NORTHWESTSHELF_ANALYSIS_FORECAST_PHY_004_013

We did not take the elevation directly, because in this study we did not include the wind forcing. We took for the analysis the elevation time series for the whole year, therefore we hope that the analysis mistake is sufficiently small.

The ADCP data used in this study are available in PANGAEA: https://doi.pangaea.de/10.1594/PANGAEA.894070

The bathymetry data will be made publicly available this year or in the beginning of the next year and also will be available in PANGAEA database.

The additional information has been added to the text.

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.

All results demonstrated in the manuscript are obtained based on barotropic simulation with tidal forcing only. To find optimal tidal solution at the open boundary we performed series of 2D experiments, mainly, because the intercomparion with observations was performed based on depth-averaged velocities. After that we moved to multi-layer task. The optimal roughness height was set to 0.001 m for the TPXO9 solution; this value agreed with one estimated from observations in a similar region (Werner et al., 2003) and, in terms of the mean, with the Cd equaled to 0.0025 for the 2D-scenario (we made series of sensitivity experiments to find this value). Table 1 presents the comparison of the RMSD and Correlation coefficient with the available ADCP data for 2D and 3D cases on 1st grid with Cd=0.0025 and r=0.001 m. Table 1 shows that 2D and 3D cases show similar results in terms of intercomparison with observations.

N of obs.	**Date in May**		**TPX09, first grid, 3D case, z_0=0.001**	**TPX09, first grid, 2D case, C_d=0.0025**
655	**22**	**RMSD**	0.28	0.28
		C.C. u, v	0.87, 0.86	0.87, 0.85
637	**23**	**RMSD**	0.31	0.31
		C.C. u, v	0.82, 0.82	0.82 0.82
618	**24**	**RMSD**	0.31	0.31

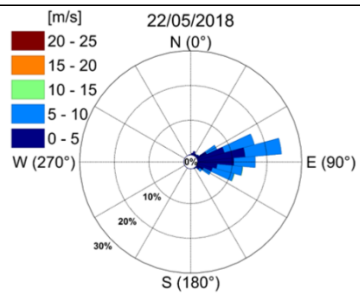
		C.C. u, v	0.84, 0.7	0.84, 0.66
764	**29**	**RMSD**	0.29	0.3
		C.C. u, v	0.89, 0.89	0.89, 0.87
1259	**30**	**RMSD**	0.25	0.26
		C.C. u, v	0.97, 0.78	0.97, 0.76

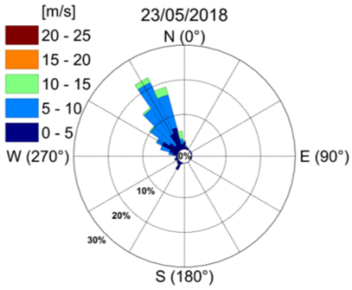
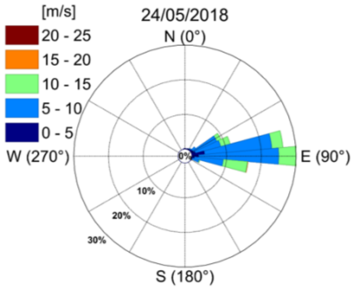
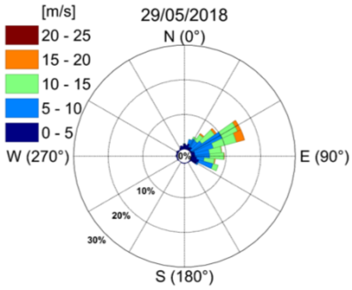
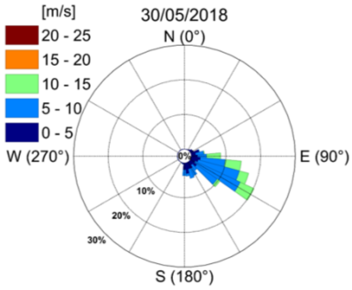
The spin-up period for all simulations was three months with a criteria of the stabilization of the total energy behavior. Due to the fact that paper considers only the tidal dynamics for the analysis we took last two full tidal cycles - 59 days. We simulated the tidal dynamics in 2018, which is expressed in Doodsen correction of the prescribed amplitudes and phases, therefore we were able to compare velocities second to second.

The additional setup details have been added to the manuscript.

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?

Table 1. Summary of the five cruises on board of Research Vessel Mya II, profiling three main transects: Inner Tidal Inlet (ITI), Main Tidal Channels (MTC) and Outer Tidal Inlet (OTI). Main Tidal Channels (MTC) show the initial, turning and ending points, since it covers three sections in one transect. The Wind Roses characterize the wind conditions during cruise times, with the legend colors representing the wind velocity in m/s and the circles representing the frequency percentage of the direction from where the wind blows.

Transect Name	Date	Lon / Lat (°) (Start – End)	Tidal Period	Duration (h)	Wind conditions (Wind Rose)
ITI	22/05/2018	8.464 / 55.047 8.489 / 55.062	neap	6:28	

MTC	23/05/2018	8.461 / 55.039 8.474 / 55.035 8.499 / 55.046	neap+1 day	6:19	
	24/05/2018	8.505 / 55.056	neap+2 days	5:56	
ITI	29/05/2018	8.464 / 55.047 8.489 / 55.062	spring	7:11	
OTI	30/05/2018	8.418 / 55.059 8.428 / 55.071	spring+1 day	12:24	

The main wind direction during the cruises was around the 90° (East) sextant, with the most frequent intensities in the range of 5 to 10 m/s. During 24th of May the wind was blowing strongly from the east nearly during the whole cruise. Exceptions were the cruise on May 23 concerning the direction, when were observed winds from the NNW (330°), and the cruise on May 29 with mostly frequent intensity ranging between 10 to 15 m/s.

Table 2 in the manuscript shows that for all solutions except FES, the correlation coefficients are higher during spring tides as well as in the deepest part of the domain despite the quite strong winds often ranging from 10 to 15 m/s 29th and 30th of May. The largest velocity errors for all solutions occur when tidal velocities are small as well as during the tidal state change, so we can guess that at these moments wind, wave and baroclinicity effect become much more pronounced. On May 23 and May 24, the measurements were performed on nearly the same side; but the May 24 correlation coefficient for the 'v' component is relatively small. In this case we can really say that the reason is permanent wind from the east.

However in this zone, tides seem to be explained more than 80 % (or 90 % or more in case of a spring tide) of the dynamics in case of absent storm (more than 20 m/s) and blowing continuously in one direction winds.

We believe that the wind forcing will add approximately the same contribution to error for all boundary conditions used based on very high correlation coefficients.

The FFT analysis of the modeling results was done after the stabilization of the total energy behavior. The elevation signal from TG was analyzed based on the whole year time-series. Due to the fact that the frequencies induced by wind are very high, we believe that this is high quality analysis. However, of course, some errors are there, which are hard to correct.
The 'wind' table has been added to the Appendix.

line 195: Is Cd 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.

Yes, we took Cd as a constant for the region due to the fact that the seabed habitat map is not ready yet for the area, there are nearly no observations in the shallow part of the region and considered area is relatively small. Therefore we think that such a decision is justified.
The reconstructed tidal elevation from observations based on FFT and elevation based on model runs visually are nearly identical. The intercomparison example of the observed and modeled amplitudes at the gauging station + Taylor diagrams for amplitude and phase are given below. The diagrams were added to supplementary materials.

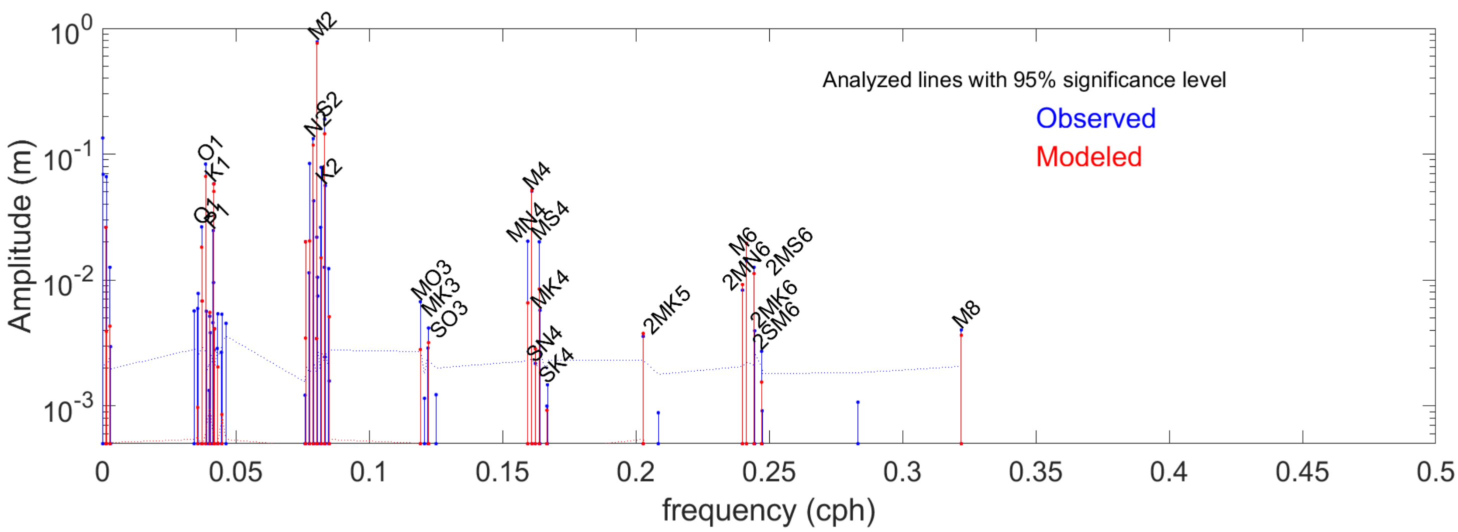
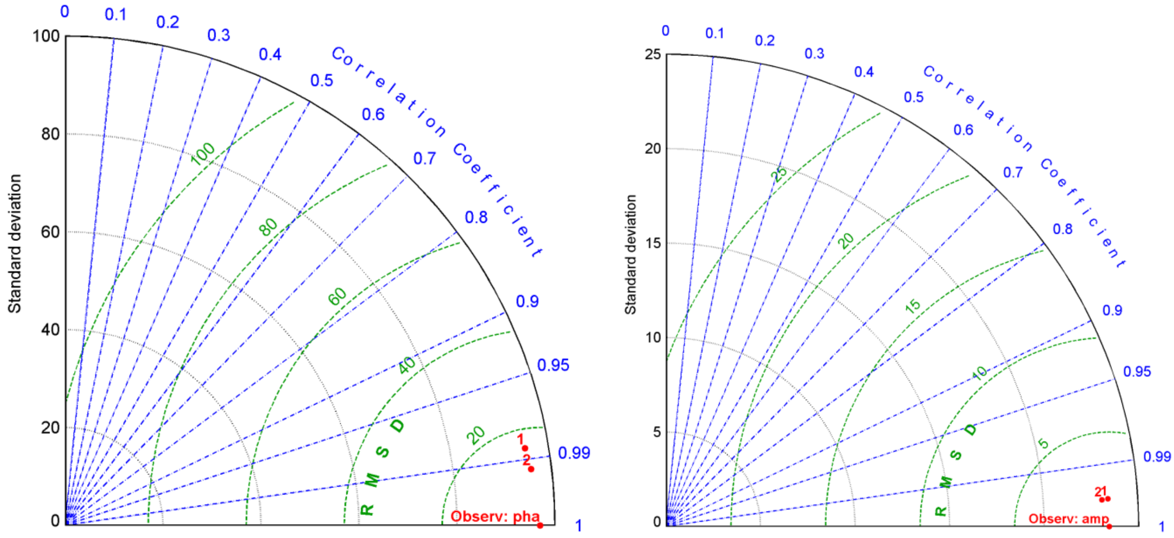


Fig. Demonstration of the FFT analysis results at List TG.



Taylor diagrams based on observed and modelled data at 3 gauging stations, '1' and '2' indicate simulation results on first and second grids respectively: a) for amplitude; b) for phase.

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

To avoid errors due to the inconsistency between the character of equations and the specified open boundary conditions (prescription of tidal elevation only), a 3-km sponge layer has been introduced. It gradually turns off the advection of momentum and viscosity in the vicinity of the open boundary (Androsov et al., 1995). The appropriate information is added to the manuscript.

Androsov, A.A., Klevanny, K.A., Salusti, E.S., Voltzinger, N.E.: Open boundary conditions for horizontal 2-D curvilinear-grid long-wave dynamics of a strait, Advances in Water Resources, 18, 267–276, 10.1016/0309-1708(95)00017-D, 1995.

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?

The water column in the domain of interests is generally well mixed, weak strain induced periodic stratification only occurs at the end of the flood in some subareas (Villarreal et al., 2005; Simpson et al., 1990; Purkiani et al., 2015). So we think that for the **depth-averaged** ellipse maps we can omit baroclinic effect. The presence of the weak density gradient in the system has an influence to the local circulation, especially in the Lister deep zone. But there the tidal residual circulation is large and would dominant the dynamics. We think that our results are valid in case of baroclinic effects are turned on, however further consideration of the SPM budget would be not possible without considering baroclinicity (e.g., Burchard et al., 2008).

Burchard, H., Flöser, G., Staneva, J. V., Riethmüller, R. and Badewien, T.: Impact of density gradients on net sediment transport into the Wadden Sea, J. Phys. Oceanogr., 38, 566 – 587, https://doi.org/10.1175/2007JPO3796.1, 2008.

Purkiani, K., Becherer, J., Flöser, G., Gräwe, U., Mohrholz, V., Schuttelaars, H. M. and Burchard, H.: Numerical analysis of stratification and destratification processes in a tidally energetic inlet with an ebb tidal delta, J. Geophys. Res-Oceans, 120, 225–243, https://doi.org/10.1002/2014JC010325, 2015.

Simpson, J.H., Brown, J., Matthews, J. et al. Estuaries (1990) 13: 125. https://doi.org/10.2307/1351581.

Villarreal, M.R., K. Bolding, Burchard, H., and E. Demirov, 2005. Coupling of the GOTM turbulence module to some three-dimensional ocean models, Marine Turbulence: Theories, Observations and Models, Baumert, H. Z., J. H. Simpson, and J. Sündermann, Eds., Cam-bridge University Press, Cambridge, 225–237.

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.

Thank you for the comments. We agree that better resolved intertidal dynamics can be an additional factor of the larger dissipation. This part of the dissipation can be largely traced in the behavior of the bottom friction and energy change adds of the balance. The conclusion about larger dissipation on the second grid is made based on analysis of the energy imbalance. We should stress that the unstructured grid is more dissipative numerically anyway with our type of discretization, this is well proofed result (e.g. Danilov and Androsov, 2015; Androsov et al., 2019).

The additional comments have been added to the text.

Danilov, S. and Androsov, A.: Cell-vertex discretization of shallow water equations on mixed unstructured meshes, Ocean Dynam., 65, 33 – 47, https://doi.org/10.1007/s10236-014-0790-x, 2015.

Androsov, A., Fofonova, V., Kuznetsov, I., Danilov, S., Rakowsky, N., Harig, S., Brix, H., and Wiltshire, K. H.: FESOM-C v.2: coastal dynamics on hybrid unstructured meshes, Geosci. Model Dev., 12, 1009 – 1028, https://doi.org/10.5194/gmd-12-1009-2019.

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.

We made a study of convergence of the solution on meshes of different configurations (quadrilateral, triangular and mixed) for the studied region in the work of Androsov et al., 2019. The study is based on coarser meshes and bathymetry data and with only M2 forcing. However, it provides a comparative analysis of energy characteristics as well as a histogram of errors of dynamical characteristics on meshes of different configurations. As was mentioned above it is proofed fact that the quadrilateral (curvilinear) has lower numerical dissipation with our desritization. We agree that this a premature to talk about which grid will better in case of baroclinic study, it is actually should be checked in further work. We have decided to write additional small article dedicated to the reproduction of the nonlinear effects on the grids of different structure. The bathymetry matrix was the same for both grids. We have added the reference to the manuscript and additional notes.

Androsov, A., Fofonova, V., Kuznetsov, I., Danilov, S., Rakowsky, N., Harig, S., Brix, H., and Wiltshire, K. H.: FESOM-C v.2: coastal dynamics on hybrid unstructured meshes, Geosci. Model Dev., 12, 1009 – 1028, https://doi.org/10.5194/gmd-12-1009-2019.

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", "amp(cm)", "ph(_)" stand for?
Figure 9: Add units.

Thank you a lot! Done.