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Discussions

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 #2

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General comments:

The article is a detailed study of the nonlinear dynamics of the tide in the shallow bay of the North Sea, which is characterized by a significant area of the intertidal zone. As a research tool, a relatively new FESOM-c model is used, which approximates the governing equations by the finite volume method and is able to work on hybrid unstructured computational grids. New data on the bathymetry of the bay and tidal currents are also presented. The authors of the study set a rather difficult goal, analyzing the results of observations and modeling for the summary tide instead of effects of nonlinearity for the single harmonic tide. In the latter case, it would be easier to estimate the contribution of the main mechanisms of nonlinearity (shallow depth, advection, and quadratic friction) on the structure of tidal asymmetries. Nevertheless, the results pre-

sented in the article are of real interest, since they provide a serious basis for analyzing the features of sediment dynamics and the formation of a variable bottom relief. I would recommend this paper to be published after a little revision. I hope the comments below help improve the manuscript.

Specific comments:

Line 100: Are only 10 sigma vertical layers enough for 3D simulations? In other words, is the numerical solution dependent on the number of vertical layers?

Section 2.3 (Open boundary conditions) Please specify the period of model calculations (dates).

Line 165 (Data. 3.2 Tide gauge (TG) data): VidaTG station is located in the intertidal zone, and so during low tide (when the seabed is exposed) the continuous (quasi harmonic) time series is greatly distorted. (see for example, https://www.emodnet-physics.eu/map/platinfo/piroosplot.aspx?platformid=9015&60days=true). In fact, this is time series with data gaps. Nevertheless, the authors used classical harmonic analysis for this station in the validation of the model. Did authors take into account the peculiarity of the tide in this station when analyzing the results? (see also the comment below on Section 4.5.2).

Table 2. The results of the inter comparison show rather large RMSD values (compared with the velocity values themselves). Apparently, this is due to the neglect of wind-induced fluctuations in the simulation. The question arises: Is inter comparison in Table_2 appropriate?

Line 330: It seems to me that the use of the term "seiche" is not entirely appropriate in this context, since we are dealing with forced fluctuations. It would be more correct to speak about oscillations as near standing wave.

Line 350: It is desirable to immediately emphasize that the results of Fig. 8 relate to the sea level (not currents).

Line 359: Frankly, I do not see indication on degenerate amphidromic point in external area. Yes, there is a slight closeness of phase contours lines (caused by proximity to the strait and refraction of the tidal wave), but there is no decrease in the tide amplitude as it is usually in nodal (amphidromic) zone. The effect of the capture of a Poincare wave is interesting, but requires explanation or reference.

Line 365-370: It is interesting how the authors distinguish between the different duration of the ebb and tide caused by the nonlinearity from the effect of the sum of harmonics of various periods. The last effect is called the Diurnal Inequality of tide, and it is not connected with non-linearity.

Line 405-410: To explain the effect of the dominance of tidt velocities over ebb ones, it is not necessary to use phase speed inequality (SQRT (gh)). There is a simple explanation for this effect: the bottom friction dampens the flow more efficiently in smaller depth (during low ebb).

Section 4.5.2 (Line 412). Again about the accuracy of harmonic analysis in the intertidal zone. Indeed, in this zone, at low tide, the bottom is exposed, and this means that data gaps appear in the model time series. In this case, the classical harmonic analysis can give inaccurate results for the amplitudes and phases of the waves. It seems that it would be more logical to exclude intertidal zones from the analysis of results (at least in terms of the results of harmonic analysis). How was this fact taken into account? Otherwise it's necessary apply special procedures to avoid mistakes.

Section 6 (Summary). Unfortunately, the question of the sensitivity of the numerical simulation to the accuracy of bathymetric data remained outside the discussion. In particular, how important is the effect of new bathymetric data in the strait on simulation results. A comparison of the solutions with the old and new bathymetry would answer this question, and perhaps provide a serious justification for the need for new good bathymetry for the whole area.

Technical corrections:

Lines 175 and 182: Two paragraphs contain the same information.

Line 245: 29.5 days is rather a lunar (synodic) month than a tidal cycle

Line 335: Direction of rotation (not orientation) of the tidal ellipse is determined by the sign of the ellipticity. (Orientation is rather the inclination of its main axis of the ellipse).

Line 372: Replace "(level panel)" by "(left panel)"

Figure 5. Please explain what the following sentence means: "The length of vectors on the maps is scaled based on the square root method."

Figure 8 .: "The dark blue color indicates zone, where topography features are above sea level." ..... above the highest sea level ???

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