Answers to Referee 2

There are many major issues to this submission:

We thank the referee for his/her comments. In the following we respond to the individual points. Reviewer's comments are written in italics, while authors' answers are in kept in plain font.

1) There is no/little hydrodynamic calibration. Authors need consider to submit two manuscripts: one for hydrodynamic and one for water quality dynamics.

Authors: The Elbe set-up has been derived from a larger set-up of the German Bight (Stanev et al. 2019). The model area of the recent set-up is completely contained in the already published set-up, they share the same topography and – most important – the Elbe set-up is hydro-dynamically driven by the German Bight set-up. A separate publication on the physical estuarine dynamics would be necessary and justified in case the processes involved or parametrization have changed, which is not the case. However, the model integration period has been extended from several months (01.01.2012 to 31.08.2012) to two years, 01.01.2012 to 31.12.2013. Therefore, more observational data has become available for the time of model integration. Following the reviewer's advice we have performed additional validation of water levels, salinity and temperature. Please find tables giving basic statistical assessment in our finalized answers to referee 1 (Tables 1 and 2).

2) Why authors didn't calibrate the water quality for the bottom part, particularly to the oxygen? Ammonia simulation is a little different from the observed one, any justification?

Authors: We find the model performance regarding water quality, such as oxygen saturation, is very convincing (please see Fig. 6 of the initial submission, Fig. D-5d and table 3 in the finalized answers to referee 1). In particular the good agreement of simulated oxygen saturation with the observed values particularly at Hamburg station during summertime (Fig. 6c of the initial submission) demonstrates that the predicted by the model increased bottom respiration in this area (shown in Fig. 13b of the initial submission) contributes to the realism of the simulation.

Regarding the ammonia simulation it would be good to know exactly to which "little difference" the reviewer refers to. Fig. 5c, d, f show a mismatch between model and observations. The model underestimating ammonia levels is likely to be due to underestimated water temperature. Therefore the most likely explanation is non-optimal boundary forcing. The ammonia variability is however very similar in model and observations (Fig. 5c, d). The most important dynamical feature is an almost stationary peak of ammonia at the downstream end of the Elbe inland delta. Our model reproduces the shape, magnitude and position of this pattern well in comparison with previous modelling effort in the area (Schroeder, 1997; Holzwarth and Wirtz, 2018). To better illustrate and specify the agreement between model and observations we have performed basic statistical assessment for the stations measuring biogeochemical variables in Figs. 4, 5, 6 and 7 of the intial submission (please find statistic assessment of simulation regarding ammonia and other nutrients in Table 3 added to the finalized answers to referee 1).

3) The model set up and data description is very weak, and need a lot of work to this part. Again, authors need consider to split this manuscript into two manuscripts. Why choose year 2012 and 2013?

Authors: We have written in model description (pages 4-5, lines 99-108 and lines 122-141), that the model framework combines two established models, SCHISM (Zhang et al. 2016, Stanev et al. 2019) and ECOSMO2 (Daewel and Schrum, 2013). These models have been described in details in the previous works. However, there is a novel aspect to this particular set-up which is the coupling through the FABM (Bruggeman and Bolding, 2014). The coupler routine also calls the subroutines prescribing the fluxes between the water column and the organic sedimentary layer. To follow the reviewer's advice, we will add a decent description of the coupling through the FABM:

"The biogeochemical model is implemented in the FABM framework (Bruggeman & Bolding 2014), which is used within the hydrodynamical model SCHISM. The ecosystem model tracers are defined in volume concentration in the grid elements and are transported with the baroclinic simulation. Their internal dynamics of matter cycling and biological production is integrated for each model timestep. The light climate is updated each timestep based on the photoynthetically active radiation at the surface of the water column, the model layers' depth, and the linear combination of exponential extinction of constant estuarine background attenuation and extinction due to organic compounds in the water column. A set of bottom variables at the sea floor is used for a pool of particulate organic matter, which gets filled with settling from the lowermost model layer, resuspended for critical shear stresses, and exchanges oxygen and nutrients during remineralization with the water column above. This bottom layer dynamics and exchange is integrated together with the transport and the pelagic ecosystem dynamics each model timestep. At the open boundaries, the state variables are prescribed for the grid elements at the open boundary. The ecosystem coupler is part of the official SCHISM repository. Both, the coupled SCHISM model as well as the ecosystem model library are part of the GCOAST model system of the Helmholtz-Zentrum Geesthacht."

We will also provide a table with the parametrisations used for the biogeochemical model

About choice of period of simulation: To be consistent with the earlier works including the Elbe estuary (Stanev et al., 2019), we start the simulation in the same year (2012). We integrate it for another year (2013) in order to establish a data-set embracing the seasonal to inter-annual variability of the biogeochemical processes. Furthermore we have observations available for these two years which allowed us to perform the necessary biogeochemical validation. Therefore we find the chosen period well-justified.

3) This study is very local, and there is no linkage to broad area? What is the contribution of this work the research community? The questions is pretty local, and not novel? Authors even didn't fully answer the questions of introduction part.

Answers: We agree with the reviewer that linkage with other estuarine studies needs to be improved. Deepened comparison with other biogeochemical studies on estuarine ecosystem and the relevance of our study for the global situation of estuaries will be provided in the revised manuscript.

The novelty of our study is that the unstructured mesh has been used to resolve the 3D coupled physical-biogeochemical processes in the narrow, curved channels and small basins of the Hamburg port area. In order to achieve this in a computationally feasible manor, an unstructured model is a decent tool. Previous modelling studies of the Elbe ecosystem have not aimed at spatially resolving the nitrogen cycle using a realistic geometry (Schroeder, 1997; Holzwarth and Wirtz, 2018). Another new and important feature of our model is the sedimentation and resuspension of nutrients which plays a crucial role in this estuary. Our simulation reveals the hot spots of sedimentation, hypoxia and remineralisation. These are located in particular in the side basins and channels of the harbor area which is why our study reveals a novelty compared to previous modelling studies. In the revised manuscript we will better clarify the novel aspects of our study. We will also provide its relevance for similar estuarine configurations worldwide.

We agree that the answers to research questions need to be more complete. We find this comment linked to the next one “*however authors want to cover everything”.* Therefore, we propose to better streamline the manuscript focusing on the impact of biogeochemical processes in area of the Elbe inland delta onto the estuarine nitrogen cycling.

4) The mixing diagram was used by Jiang and Xia, 2018 and isn’t new. This study is mainly for nitrogen dynamics, however authors want to cover everything. It is a little bit difficult to follow, and authors need think how to make a nice flowchart to this manuscript. Overall, it reads like a modeling or technical report. There are many minor issues, however I would like authors to take care of major issues now.

We use the mixing diagram to a) validate the along-channel distribution of the inorganic nitrogen species over the estuarine salinity gradient (in isohaline coordinates), b) characterize the mixing behavior of these species along the Elbe River. The method is of course not new (see our references), however there is no such extensive demonstration of mixing behavior of nitrogen species for the Elbe system. As the paper focusses on the nitrogen cycling we consider its mixing behavior a central aspect.

The referee is right that we need better streamlining of the manuscript. As written in the answer to the previous comment, we plan to focus the study onto heterotrophic decay confined to the harbor basins and side channels and its impact onto the estuarine nitrogen cycling. This will lead to a more balanced manuscript, where new scientific inside arising from the more complete representation of processes in the port area will be set in relation to better known spatio-temporal organisation of nutrient cycling in the main channel downstream from the port area.

Jiang, L. & Xia, M. (2018), Modeling investigation of the nutrient and phytoplankton variability in the Chesapeake Bay outflow plume. Progress in Oceanography, 162, 290-302. doi: https://doi.org/10.1016/j.pocean.2018.03.004

We thank the referee for recommending this useful reference.