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Interactive comment on "Nitrogen cycling in the Elbe estuary from a joint 3D-modelling and observational perspective" *by* Johannes Pein et al.

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General comments

My overall impression is that the manuscript is a draft with a collection of several interesting topics (importance of a 3D model to capture estuarine biogeochemistry, nitrogen cycling, hydro-dynamic transport times, grazing, influence of river discharge, small-scale hydrodynamic pat-terns influencing the system-wide biogeochemistry) but no consistent story line and no clearly expressed conclusion in the end. My general advice is to focus on one aspect from the collection of interesting aspects above and present it in a clear and concise way.

The manuscript presents a work very specifically done for the Elbe Estuary without

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discussing the global situation or the situation in other similar, or different, estuarine systems. Almost no literature apart from specific Elbe Estuary literature is included.

Though I agree on the necessity to use a 3D model for analyzing estuarine biogeochemistry, this point is not worked out well: (1) The argumentation for the necessity of a 3D model fails. (2) The authors make little use of the additional information they get from their 3D model compared to information from measurements or a 1D/2D model.

Apart from the issues above, the model (physical and biogeochemical) is not sufficiently validat-ed for the argumentation later on. While I see nitrogen and nitrogen species well-validated, all other quantities are not. In any case, the physical model need better validation, and – given the poor agreement shown for salinity – probably some more effort in setting up a better physical model is required.

Specific comments Title It is not clear what the authors mean with "joint perspective". The manuscript presents a model-ling study with the use of observational data for model steering and validation, which is the usual way to do.

Abstract Line 7-8: observations are only used for steering & validation here. Line 9: "a coupled physical-biogeochemical 3D unstructured model, applied for the first time to the estuarine environment". It is not clear, what the authors mean: if the sentence relates to the specific model they use (Schism – ECOSMO) it might be true but is nothing new and remarkable in the abstract. However, the expression could also be misleading: I'm not sure if the authors mean that it was the first time ever that a coupled model has been applied to an estuary. In this case, the authors are referred to Cerco et al. (1993), Wool et al. (2003), Wild-Allen & An-drewartha (2016), Lajaunie-Salla et al. (2017) – to name only some. Line 13 – 21: - Apart from the validation for nitrogen, model validation for all other physical and biogeo-chemical quantities is not sufficient to support these findings. - What is the new& important finding and what are its consequences?

Page 2 Line 31: The reference Jickells et al. 2014 is not appropriate for the statement.

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Line 38-39: If oxygen depletion is mentioned later on, it should be mentioned that depletion was most severe during this period. Line 61-62: Sentence unclear: in x-direction (main flow direction), all of the above mentioned models describe the gradient between the weir and the German Bight. Entire last paragraph: the models mentioned before all focus on oxygen, the authors focus on nitrogen (or meso-zooplankton?). The authors should consider that this might be a completely different story.

Page 3 Line 70-74: limiting to one or two questions would probably help to give a more concise and focused article Line 75-81: the stations of WGMN Hamburg (Blankenese, Seemannshöft, Bunthaus) should also be taken into account. They provide high-quality data in an area where later on important con-clusions are drawn from model results. And they give a differentiation of phytoplankton between diatoms, green algae and blue-green algae. Line 80: Use boundary "values". At this point I miss the information about the location of the model boundaries. Line 85: it should be mentioned that measurements are taken approx. 1m below the water sur-face. No vertical (and no lateral) gradients available. Line 92-95: references missing

Page 4 Line 96-97: unclear: were climatologic averages used? Daily values? Is the same method used for all nutrient species? Line 108: What is GCOAST? Is it important information?

Page 5 Line 128: What exactly are the state variables? What is their functional relation? The authors say that organic matter decomposition was very important (which is true), so was the ECOSMO2 setting changed from the references given before? If it is not changed, this information should be clearly given here. In case it is changed (also in case certain parameters are changed), the model functions and their parameter values have to be described in more detail. Line 129: Assumes Redfield ratio in phytoplankton, I guess? Line 133: What kind of feedback is meant here? Line 134: Unclear: first, the authors say that the sediment is a mere storage. Then they say, min-eralization and nitrogen release was possible. Line 140-141: Holzwarth and Wirtz 2018 say: turbidity is so high that depth plays a secondary role. What is the value for the background attenuation coefficient used here? In case it is the same value used for the North Sea it will certainly be too low. See comment above: biogeochemical model description is missing. Line 156: Which station exactly? What is the temporal resolution of the observational data? Line 159: Is the conversion factor in line with the conversion used in other Elbe studies (some of them were already cited)? If not, why different values were used? Entire paragraph with Geesthacht boundary values: please provide a plot containing the time series of the used boundary values.

Page 6 Line 164: Please clarify: 2012 was used as spin-up period? Line 171: Why can the authors assume, based on M2 tide, that water levels deviate less than 10 %? Besides: - I consider 10 % as far too poor, given a water depth more than 15 m in large parts of the estuary. This would imply a substantial error in water volume. - More information on tidal analysis for observational and modeled data (mean high and low water, tidal range, slack water times) would be helpful to assess the quality of the physical model, essentially including an analysis of current velocities. Line172: “ventilation” might be misleading in this context (could be understood as “reaeration”) Line 174-175: The stationary monitoring data of the water and shipping administration, which are distributed along the salinity gradient, from Hamburg to Cuxhaven should be used for valida-tion. Using these data, a time series comparison for the entire simulation period, including basic statistics, is possible. This would give a much better impression on the quality of model results than comparing 4 single transects. Same for temperature. Line 176: The deviation in salinity is quite high. Given the really high overestimation in the Oc-tober 2012 profile (up to 15 psu, if I understand it correctly) I strongly recommend to include the stationary measurements, see above. Either the ship-based observational data are bad, or the physical model needs improvement. Line 181: I assume the authors refer to model results (please clarify, also in the figure text)? Giv-en the poor agreement between observational data and model results I recommend not to show this figure until better agreement is proven.

Page 7 Entire paragraph on dispersion and tracer: I recommend proving good performance of salt transport (see above) before calculating a dispersion coefficient based on salinity. More: the relevance of the entire section is not clear to me. I recommend leaving it out complete-ly and better spending more effort in the validation of the physical model. Besides, mixing plots for model data are unnecessary from my point of view: having a model, you can extract from your model results the production rates for all your state variables in your entire model domain. Then you clearly see (in numbers!) whether you have a biogeochemical activity or not. You can even see where it comes from. Maybe the authors can make better use of this information which is so far not used in their study.

Page 8 Line 232: where shown?

Page 9 Line 248-249: earlier you say that denitrification is included in the model. Line 271 ff: Regarding the rather poor agreement of model salinity and temperature, the oxygen values should not be validated using saturation. Dissolved oxygen concentration should be used instead to avoid any offset due to incorrect salinity and water temperature. Line 279: What means high accuracy? This comment relates to the entire validation section, both physical and biogeochemical: analysis of the comparison between observational data and model results seems to be done by plotting and “visual” judgement on the goodness of fit.

Page 10 I don’t see an additional benefit for model validation from comparing mixing curves and I there-fore recommend leaving out this section.

Page 11 The entire chapter 5 has a structure which makes it hard to follow a central theme. Line 326: Announcing to explain the “full spacio-temporal dynamics of the estuarine ecosystem” is a very big promise and I strongly recommend being more moderate. Line 348-359: First, the model used in this study has not been validated for grazing or grazers. Second, the reference given (Schöl et al. 2014) is weak because the conclusion presented therein only bases on model results. Therefore, findings regarding grazers have to be validated, also to sustain the conclusions drawn from it later on. Observational data (counts of organisms, for ex-ample) might exist at the WGMN Hamburg or at BfG for the harbor area. Moreover, the model has also not been validated for phytoplankton. High-quality phytoplankton data are available, especially for the area that shows high model diatom concentration in Figure 10 (a).

Page 12 Line 365-367: The authors don't prove this statement. Line 367 ff: The relation to the text in the first section of this paragraph isn't clear. Line 376 and following two sections: When focusing to such a small region compared to the en-tire model domain, the authors should provide better validation for the hydrodynamic model in this specific area. A system-wide M2-comparison, which also shows its highest deviations in the focus region (roughly km 620 in figure 2(a)), is not sufficient. In addition: lacking validation for phytoplankton and grazers, see above.

Page 13 Line 415 – Page 15 Line 476 This section can be shorted substantially be-cause it contains many well-known and well-described processes and relations (con-tained in the references of the paper). The three – zone - description in the last section (Line 470 – 476) has first been described by Schroeder (1997). New aspects worked out by the authors are not clear. Line 458: where is fig 12e?

Page 15 Line 478 – Page 16 Line 512 What is new &relevant in this section?

Page 16 Chapter Conclusions Line 514 –522: Everything that is mentioned here has been shown already several times, for the Elbe and for other estuaries. A 3D model that is able to simulate all these processes is also not new but more or less common (see the comment on the abstract). Line 523 – 528: Validation missing Line 530 – 540: This section is more a description. What is the important conclusion out of it? Line 542 – 543: This has already been shown for different estuarine systems worldwide. Line 543: From my point of view the importance of the harbor is the most interesting aspect that remains. However, I miss: 1. Better validation, 2. more focus, 3. analyzing hydrodynamic aspects as well (e.g. larger water volume in the harbor compared to section upstream), 4. comparison to other similar systems (like Guadalquivir-Sevilla,

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Loire, Humber or other estuaries containing large ports).

Figures There are many figures. Some figures (like 8, 9 or especially 12) are not easy to understand. Some panels (especially Figure 13) are very small. Check for Figure 2(b) to (e) if the description of x and y axis and the conclusion that in the model salinity intrudes too far into the estuary is consistent. There is no white dashed line visible in Figure 10.

Technical correction There is a plenty of typing errors, careless mistakes and wrong word order, some obviously due to copy& paste. I'm not a native English speaker, and therefore I'm not in the position to give suggestions for language corrections. However, I have the impression that language, including grammar and use of uniform tenses, needs substantial improvement.

References

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