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Interactive comment on "Evaluation of atmospheric nitrogen inputs into marine ecosystems of the North Sea and Baltic Sea – part B: contribution by shipping and agricultural emissions" *by* Daniel Neumann et al.

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Response to review comment #3 by Oleg Savchuk

We thank the reviewer, namely Oleg Savchuk, very much for reading this and the companion manuscript. We agree with most of the four major critical aspects mentioned and cannot satisfy/disprove them completely.

Below, the reviewer's comments are printed in bold letters and our answers in non-bold

letters.

[. . .] Instead, I recommend to [. . .] advice against using this version of HBM-ERGOM model, made for operational purposes (perhaps, with the data assimilation), for the long-term studies.

> We will not use it again.

A) "Iron reduction and release of phosphate under anoxic conditions in the sediment are not represented in this ERGOM version" (Part A, L 15/8). Fixing sediment N:P ratio and ignoring redox alterations of the P cycle implausibly affects phosphate dynamics, hence, distorts such important flux as nitrogen fixation and the following cycling of fixed nitrogen. The necessity of Si restarting for every year indicates that its dynamics even during the first iteration is erroneous with corresponding consequences for phytoplankton seasonal succession and nutrient uptake. [. . .]

> *no objection*

[. . .] Finally, many important features and phenomena, for instance, nutrient limitation, nutrient residence times, species composition, tides and oceanic impacts, etc., are rather different between the North and Baltic seas. That makes combining them into a single domain questionable, if not harmful for the objectives of this study.

> Yes.

> The ERGOM was developed for the Baltic Sea and this version was adapted to also work in the North Sea. Particularly the dynamics of the biogeochemical system of the

North Sea are not properly reproduced by this ERGOM as one sees in the validation. As the Baltic Sea dynamics are reproduced considerably better then the North Sea dynamics – as one might expect because ERGOM was develop for the Baltic Sea –, we consider to remove the North Sea from this manuscript. There are still issues in the Baltic Sea model dynamics – silicate decline, simple sediment, and deep water oxygen – but we hope that the model quality in the Baltic Sea is sufficient for a publication of this study.

B) Overestimated deep layers oxygen concentration and underestimated denitrification DIN distribution and dynamics (see comparisons in Figs. 7-10). Together with questionably reproduced nitrogen fixation, such underestimation indicates a wrong balance between nitrogen sources and sinks, hence, biases evaluation of atmospheric N contribution to unknown degree.

> We think that the time scale of five years prevents badly predicted deep-layer oxygen concentrations to feed back to the surface layer nitrogen concentrations.

> We consider to remove the North Sea from the manuscript and only evaluated the Baltic Sea.

C) "Therefore, a detailed validation of the nitrogen deposition data sets is not possible and it is not clear whether the CMAQ nitrogen deposition is actually too low over sea."(Part A, L16-18/13). Already this statement makes studies of the RELATIVE contributions rather uncertain. Further uncertainty (due to possible non-linear effects in the biogeochemical cycling) is introduced by the repetitive implementation of deposition computed only for one year (i.e. 2012) over all five years, forcing a possible deficit accumulation.

> We agree that a bias in the nitrogen deposition will amplify further and further in the marine biogeochemical model in each annual iteration. However, the bias will also

amplify in a model study over five or ten consecutive years if the bias in the nitrogen deposition is systematic and not only present in the year 2012.

> Based on Vivanco et al. (2017), Karl et al. (2018), and companion paper part A, one can assume that EMEP nitrogen deposition is closer to reality than the CMAQ data of this study and that the used CMAQ setup for Europe (nearly the same in Vivanco et al. (2017) and here) tends to systematically underestimate nitrogen deposition. However, we are not aware of any nitrogen deposition data set, which is validated above the ocean in detail because (operational) continuous nitrogen deposition measurements are missing (or other nitrogen deposition measurements with sufficient temporal or spatial coverage).

> Most marine biogeochemical model studies do not mention the uncertainty of the driving nitrogen deposition data at all. Moreover, some studies use monthly or annual mean nitrogen deposition as input and re-distribute it onto individual days.

D) The model set-up and simulated dynamics contain many features that are "typical within order of magnitude" rather than year-specific. Therefore a comparison of the "first" iteration with observations during concrete 2012 year looks very optimistic, even naïve. Perhaps, such choice partly explains why most patterns of seasonal dynamics are very poorly reproduced either in timing or by the levels, or both (Figs. 7-10). Never mind the plausible oxygen dynamics in the surface layer, where it is mainly driven by air-sea gas exchange. [. . .]

> *no objection*

[. . .] Moreover, the focusing of analysis at the surface layer is unwarranted because the nitrogen biogeochemical cycle must be evaluated for the entire ecosystem, including sediments.

> The sediment is represented by one layer in our model setup. The processes in the sediment and at the sediment-water interface are considerably simplified (see item (A) above). Hence, the sediment representation in the model is far to simple than one should take the modeled sediment concentrations for granted. Therefore, we prefer to evaluated the surface layer concentrations only (top 12 m).

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