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Discussions

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 #1 by referee #5

We thank the reviewer for the constructive comments on the manuscript.

Below, the reviewers comments are written in bold letters and our answers in non-bold letters.

Major comments

While I think it is important to investigate the impact nutrient inputs related to shipping and agriculture on the Baltic and North Sea ecosystems, I must admit that I got lost in the description of many details and had problems to identify a clear aim.

In the given context, I would mostly be interested in ecosystem changes due to atmospheric nutrient deposition and thus rather expect something like sensitivity experiments with and without this extra nutrient supply. I am not sure what to gain from tagging the fraction of atmospheric nitrogen shares in % to DIN, PON, and chlorophyll-a after five years.

> We agree that it is reasonable to turn individual source sectors like shipping or agricultural activities on and off if one wanted to see how the system reacts on the reduction of anthropogenic emissions. This approach is reasonable for assessing the impact of emission reduction legislation. However, this studies deals with one step earlier: does (total/shipping-related/agricultural-related) nitrogen deposition contribute significant amounts of nitrogen to the marine biogeochemical processes and, if yes, where is it the case? Correspondingly, we formulated our research questions, i.e. "*What contribution do shipping- and agricultural-related nitrogen emissions [into the atmosphere] have [. . .] to the marine nitrogen concentrations?*". Hence, we considered % of total/shipping-related/agricultural-related atmospheric nitrogen to DIN, PON and chlorophyll. After identifying source sectors with relevant impacts, the next step would be to turn off these sectors in model studies.

> We will try to formulate the research questions and the introduction more concise in a revised version of the manuscript.

Another major point of criticism is the negligence of the strong impact of phos-

phorus. In the presence of nitrogen fixers, I regard the availability of phosphate as key. As I understood it, the phosphate input was set to a fixed value of unknown origin.

> We are pleased that the reviewer points to this aspect and we strongly agree with the reviewer that correct atmospheric phosphorus deposition is important.

> The fixed value for the phosphorus deposition was set to a value close to the currently by HELCOM suggested value. Someone, whom we could not identify, tuned the value of the phosphorus deposition. We know that this does not comply with the idea of good scientific practice and we are not happy with this situation.

> We are not aware of validated deposition fields of bioavailable phosphorus for the North Sea and Baltic Sea region. OSPAR does not consider this topic and HELCOM suggested one spatio-temporally constant value. We would have liked to stick to the approaches chosen in other model studies but phosphorus deposition commonly is not documented in the Material and Methods sections of respective publication.

> Most main-stream atmospheric chemistry transport models do not consider phosphorus compounds. First, phosphorus is not of high interest for atmospheric chemistry processes. Second, emissions of some source sectors are difficult to estimate: i.e. detailled soil information is necessary to properly calculate wind-blown dust emissions of phosphorus (soil type, phosphorus content, humidity, plant cover). Third, emitted phosphorus compounds are processed during their atmospheric transport and the fraction of bioavailable phosphorus to the time of deposition is difficult to assess.

Specific comments

2.1 Atmosphere: I repeatedly lost overview. I would find it helpful if there was a more clear separation of the model assumptions, the input and the outcome.

> We will restructure the section.

Also I would expect at least some evaluation of the results (apart from a non-published reference).

> We included a brief comparison with EMEP nitrogen deposition data in companion paper part A. The validation paper has recently been published as discussion paper in Atmospheric Chemistry and Physics as Karl et al. (2018, "*Impact of a nitrogen emission control area (NECA) on the future air quality and nitrogen deposition to seawater in the Baltic Sea region*", doi: 10.5194/acp-2018-1107).

While the authors state that everything is rather uncertain, they do not put this uncertainty into perspective. How do the modelled numbers compare the official estimates by HELCOM and OSPAR?

> We will restructure the section.

> To the best of out knowledge, the estimates of HELCOM and OSPAR are based on EMEP data. We compare EMEP and CMAQ nitrogen deposition in the companion paper part A. CMAQ nitrogen deposition is lower than EMEP nitrogen deposition according to recent comparison study of atmospheric chemistry transport models (Vivanco et al., 2017, doi: 10.1016/j.atmosenv.2016.11.042). Moreover, EMEP nitrogen deposition was closer to land-based nitrogen deposition measurements. Nitrogen deposition measurements above the North Sea and Baltic Sea are not available in reasonable spatial

or temporal resolution. Hence, one might assume that CMAQ underestimates the nitrogen deposition. We mentioned this in the manuscript (p.19, l.23–26) but should have put this information to another place in the manuscript. We will do it if we are allowed to submit a revised version.

2.2 Ocean: Again, I find the model description confusing. Specifically, it did not get clear to me why the simulation time was five years only (while the model is drifting?) [. . .]

> We had two major reasons for considering five years. It is explained in companion paper part A. First, we were limited in the computing time when we performed the model simulations for the study. The convergence of the tagged atmospheric nitrogen towards a steady-state seemed to be sufficient after five years in most regions of the model domain except for the Gothland Basin. Second, we are aware of shortingcomings in our model setup with respect to the oxygen cycle and the denitrification in deep layers of the Gothland Basin (companion paper, Fig. 8). We hoped to avoid a feedback of these deep layers on the surface layer concentrations by keeping the simulation time as short as necessary.

[. . .] and, also, it should, at least briefly, be mentioned how the key processes which impact the distribution of nutrients are implemented.

> We will add information on the transport processes if we are allowed to submit a revised version.

Also the initial conditions of the model need to be clarified and I had problems to see why the physical model was restarted from its initial conditions (which?) each year.

> The physical model is restarted each annual iteration from spun-up initial contions for the year 2012. We did this to be able to assess the biogeochemical model results without interferences by the physics: the physics receive no feedback from the biogeochemistry. Thus, the processes are the same in each iteration. Variations in the biogeochemistry from one iteration to the next are only due to biogeochemical processes and not due to variations in the ocean physics. We described it only in companion paper part A (p.9, l.17–20) and forgot to mention it in this part B.

In addition, the model description would strongly gain from a comprehensive, clearly arranged list of nutrient sources and sinks in the model (e.g., is there a sediment model and burial? how large is the riverine input?).

> The sediment is represented by one model layer at the bottom of the sea. It is mentioned in companion paper part A (p.8, l.16–17) but we forgot to describe it in this part B. We will add this information if we are allowed to submit a revised version.

> A detailled list of all processes is included in the Supplementary Material (pdf file starting with bm_ergom_2017_model_description*.

How did the authors determine the nitrogen fraction of chlorophyll a?

> We did not describe the usage of chloropyll properly. Chlorophyll is calculated as diagnostic variable from the phyoplankton concentration. Hence, the relative contribution of nitrogen source XY to chlorophyll actually means the quotient of *nitrogen of source XY in phytoplankton* divided by *total nitrogen in phytoplankton*. We decided to show and discuss chlorophyll because chlorophyll concentrations were validated.

Why did the authors chose to show atmospheric nitrogen shares in % to DIN, PON, and chlorophyll-a . . . ?

> Our major aim was to assess how relevant the contribution of total/shipping-related/agricultural-related atmospheric nitrogen deposition is for nitrogen compounds in the marine water compared to other sources. We found it reasonable to compare %-ages instead of absolute amounts.

. . . and which depth level do they consider, why?

> We presented the averaged over the top five model layers (approximately 12 m; p.9, l.30–31). We know that the model has issues in the Gothland Basin (see our reply further above). We considered the upper 12 m because we expect that this depth range is not impacted by deep water processes in the considered time frame.

Most important, however, I am not even sure what exactly was tagged - was the atmospheric deposition marked continuously or did the authors follow a pulse? In both cases there ratio behind the approach needs to be clarified.

> Atmospheric nitrogen was continuously tagged. It takes several years until atmospheric nitrogen reaches an equilibrium in the Baltic Sea. We wanted to assess how much atmospheric nitrogen deposition contributes to the North Sea and Baltic Sea in the long run. If we wanted to calculate the residence time of atmospheric nitrogen in the North Sea and Baltic Sea, an individual pulse would have been reasonable. We will add this information if we are allowed to submit a revised version.

This section consists mainly of a list of numbers in % showing atmospheric nitrogen shares in % to DIN, PON, and chlorophyll-a (without providing any absolute values). Often I was not sure which region/depth levels the authors exactly refer to. Also, I lack explanations about reasons and ecological consequences (e.g., which paths did the nutrients take?). The few explanation provided did not be

come clear to me (e.g., why should offshore and coastal differences in the Baltic be explained by high DIN loads at the coast and P limitation?).

Conclusions: Also this Section would benefit from some guidance by the authors what the results mean for the ecosystem. As I see it now, it's mainly a repetition of the foregoing.

> We will clarify this and shorten the results section in a revised version of the manuscript if we are allowed to submit a revised version.

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