

## ***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.***

**Anonymous Referee #5**

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Review of the manuscript “Evaluation of atmospheric nitrogen inputs into marine ecosystems of the North Sea and Baltic Sea - Part B: contribution by shipping and agricultural emissions” by D. Neumann, H. Radtke, M. Karl and T. Neumann

SUMMARY:

The authors investigate the fate and behaviour of atmospheric nitrogen deposition from shipping and agricultural activities in the North and Baltic Sea. The study is based on a tagging method in the coupled physical-biogeochemical model HBM-ERGOM. Re-

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gional fractions of atmospheric nitrogen are provided for inorganic nitrogen, particulate organic nitrogen and chlorophyll-a.

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. Another major point of criticism is the negligence of the strong impact of phosphorus. 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.

I am afraid that, in the present form, I have to reject the manuscript. I must, however, admit that I struggled to keep overview and it might well be that I missed an important point. I might thus change my mind, in case the authors could clarify their aim and the argumentation was more stringent.

SPECIFIC COMMENTS:

2 Materials and Methods

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

2.2 Ocean: Again, I find the model description confusing. Specifically, it did not get

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clear to me why the simulation time was five years only (while the model is drifting?) and, also, it should, at least briefly, be mentioned how the key processes which impact the distribution of nutrients are implemented. 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. 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?). How did the authors determine the nitrogen fraction of chlorophyll a? Why did the authors chose to show atmospheric nitrogen shares in % to DIN, PON, and chlorophyll-a and which depth level do they consider, why?

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.

Results - subsection 3.2: 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 become 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?).

Results - subsections 3.2.2 and 3.2.3: Again, I found it very hard to keep overview. I would suggest to condense these parts considerably. Also I propose to focus more on the results and not to elaborate on the pros and cons of extra nitrogen input in general. Comparisons to the results of other studies could be summarized in a Discussion.

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.

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