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Interactive comment on "Evaluation of atmospheric nitrogen inputs into marine ecosystems of the North Sea and Baltic Sea – part A: validation and time scales of nutrient accumulation" *by* Daniel Neumann et al.

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Response to review comment #1 by referee #2

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

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

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Discussion paper

My main criticism is that, to calculate a residence time, a model is not needed. Multiplication of observed nutrient inventories with the inverse of the HELCOM nutrient fluxes directly, at the back of an envelope, yields residence times already. According to the authors (c.f. pg. 1, ln. 16-18) these residence times have been already known. I conclude that their model estimate does not present novel concepts, ideas – nor substantial conclusions.

> We agree with the reviewer that one can estimate the residence times by back-of-the-envelope calculations. However, the residence times of nutrients of individual sources depend on the spatio-temporal input pattern. Nitrogen compounds sourced in flat coastal regions are removed faster by denitrification than nutrients sourced in deep open basins. Inorganic nutrients sourced during summer are faster processed by phytoplankton than nutrients sourced during early winter. Hence, we think that it is important to include the spatio-temporal variability of nitrogen inputs in the estimation of nitrogen residence times.

> This manuscript rather is to see as a basis for the companion paper part B.

The authors state that simulated deep nutrient concentrations in the Baltic are biased and that denitrification in the Wadden Sea is underestimated but that, at the same time, that " ... this did not impact surface layer concentrations" (pg. 1, ln. 13 to 15). Assuming that simulated surface nutrient concentrations were realistic makes me wonder if they are so for realistic reasons.

> Because the model performance was low in the German Bight, we plan to completely remove the North Sea from the manuscript. The evaluation of the model results of the Baltic Sea also revealed issues but these are not as severe as in the North Sea.

As concerns the second aim of the study, to validate a marine ecosystem model, I feel that a more specialized journal like "Geophysical Model Development"

would be more appropriate because the audience addressed by Biogeosciences is rather broad.

> We agree with the referee that this study ("companion paper part A") is clearly focused on the model evaluation/validation. When considered individually, it is more appropriate for journals such as GMD. However, we submitted it in combination with a second discussion paper ("part B"), which is focused on the contribution of nitrogen deposition from different atmospheric emission sources to surface DIN and PON concentrations. Unfortunately, part B of the study (doi: https://dx.doi.org/10.5194/bg-2018-365) was not available online when the review of the referee was performed.

> Originally, both discussion papers were one manuscript, which was very long. Therefore, we decided to split it into two shorter – still long – discussion papers consisting of a *part A* (model validation and first results) and *part B* (detailed results and evaluation). We have the feeling that both discussion papers belong together. Discussion paper part B without validation of the model would be questionable. Hence, we hesitate(d) to submit them to two different journals.

> We are the users but not the developers of the particular model version, which was used for this study. The actual developers should be the ones to published a detailed validation of their model – getting the credits (and a first-author publication) for the development work. Hence, we limited the validation to the year and to the aspects, which are relevant for our evaluation of nitrogen deposition data, leaving the developers the possibility a publish a full validation in an appropriate journal. If such a publication would be available, we would have omitted submitting discussion manuscript part A and would only have submitted part B (without "part B" in the title).

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