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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 #4 by referee #1

We thank the reviewer for the feedback to this manuscript.

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

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Discussion paper

1. Bioavailable PON has been mentioned at several places (such as line 21, page 1). This is a misleading term unless further qualified. Only inorganic nitrogen (dissolved nutrients form) and up to certain extent, DON is considered bioavailable. I have not seen any (oceanographic) study, where PON is proposed to be bioavailable. I am unsure if the authors wanted to convey the availability of nitrogen to heterotrophs (such as fish), then PON can be bioavailable. But in traditional view, we do not present the definition of bioavailable in this way.

> We did not properly introduce the meaning of PON as we use it in the study. We denoted the sum of phytoplankton, zooplankton and detritus that is further available for the model processes as bioavailable PON. We will include this description.

2. Authors must specify how does their model is able to differentiate between different components of anthropogenic (for that matter natural as well) inputs?

> An established method to tag individual nutrient sources was used to trace *total nitrogen deposition*, *shipping-related nitrogen deposition*, and *agricultural-related nitrogen deposition* (p.7, l.14–16). The methods has originally been published by Ménesguen et al. (2006).

> When we write about *atmospheric nitrogen deposition* (without prefix) and *total atmospheric nitrogen deposition* we mean the sum of natural and anthropogenic nitrogen deposition. Hence, we do not differentiate between anthropogenic and natural inputs at all in this context.

> The shipping-related nitrogen deposition was calculated by the difference between atmospheric chemistry transport simulations with and without shipping emissions (p.5, l.28–29). We are aware that this approach is not the first choice for our use case – tagging shipping-nitrogen in the atmosphere would have been more appropriate – because non-linear interactions between shipping imissions and other atmospheric com-

pounds are neglected (*imissions* = atmospheric compounds that were *emitted* previously).

> The agricultural-related nitrogen deposition was estimated to be $95\,\%$ of reduced nitrogen deposition (only ammonia and ammonium; p.5, l.30–32). This assumption is roughly valid because $95\,\%$ to $100\,\%$ of the ammonia emissions originate from agricultural activities and animal husbandry. We are aware that we ignore agricultural-related oxidized nitrogen emissions into the atmosphere, e.g.: nitrate from wind blown dust, nitrogen oxide and dinitrogen oxide emissions from plants, and nitrogen oxide emissions from combustion-driven agricultural vehicles. Therefore, we state that our study underestimates the agricultural contribution (p.5, l.32–33).

3. No uncertainties are provided in the estimates. It is important to provide uncertainties (in all tables and texts, wherever an estimate is quoted) anyways but here it more important as % contribution difference of different processes in the two basins is not much.

> We agree with the reviewer that it is important to provide uncertainties. Unfortunately, we simulated only one year with one model and are not able to provide the variation between different years or within a model ensemble. Hence, we are not able to provide uncertainty estimates. We see no possibilty to sufficiently reply to this criticism.

> Due to the high (unknown) uncertainty we do no statistical evaluation of the model results.

4. What deposition velocities are used in the model to estimate deposition rates? These must also have large uncertainties.

> We are not sure if understood the question correctly. If we miss-understood the

question and answered it not correct, we kindly ask the reviewer to reformulate the question.

> The manuscript is quite long, which was critizied by several other reviewers. We were aware of that fact prior to submission and tried to keep some parts of the material and methods section as short as possible (it is still very long). Hence, we provided only brief references to publications, which explain the parameterization of the wet and dry deposition processes in CMAQ (p.5, l.12–14). We wrote a bit more about it in companion paper part A of this study (Neumann et al., 2018b; p.4, l.33 to p.6, l.2).

> A general overview of the processes and uncertainties: The wet deposition velocities (and the scavanging rates) are substance specific and they are subject to uncertainty. The nitrogen wet deposition considerably depends on the used meteorological forcing (see Neumann et al. (2018a) and Karl et al. (2018, previously *in prep.*) for details). In the case of particulate matter, the dry and wet deposition depends on the indiviual particle size distribution at each time step and location. The dry deposition depends on the surface roughness, which is not necessarily correctly represented in the model. There is no bi-directional flux of gas phase species between atmosphere and ocean or atmosphere and land surface included.

> We assessed the uncertainty introduced by the nitrogen deposition by using three atmospheric nitrogen deposition data sets as forcing for HBM-ERGOM simulations in anouther study (Neumann et al., 2018a). The differences between EMEP and our CMAQ data were very high. Moreover, the resultion of the meteorological forcing also impacted the nitrogen deposition pattern. Vivanco et al. (2017) evaluated the nitrogen deposition of several atmospheric chemistry transport models and found a wide spread in the results. In summary, yes, there are huge uncertainties in the atmospheric deposition. However, we are not aware of any marine biogeochemical study, which properly deals with the uncertainty in its atmospheric nutrient forcing.

5. Chlorophyll is a pigment so how does one estimate relative contribution of

shipping etc to chlorophyll and what does it signify (Fig. 6)? Perhaps an estimate to primary production instead of chlorophyll would have been meaningful.

> We did not describe the usage of chloropyll properly. Chlorophyll is calculated as diagnostic variable from the phyoplankton concentration. The relative contribution of nitrogen source XY to chlorophyll actually equals the quotient of *nitrogen of source XY in phytoplankton* divided by *total nitrogen in phytoplankton*. We have choosen chlorophyll for the evaluation because we validated chlorophyll and did not want to introduce another parameter. Considering primary production would have introduced additional uncertainty. We will include this description.

6. Why a particular year (2012) is chosen (line 5, page 20)? Will the conclusions change for another year? How does one specify a particular year in model (unless there is some time-series analysis involved, which is not the case here)?

> We used nitrogen deposition data that was obtained by atmospheric chemistry transport model simulations performed within the EU Bonus project SHEBA. In SHEBA, only the year 2012 was modelled because calculating high resolution emission data sets and removing artifacts from these data sets is very time consuming. We have choosen this nitrogen deposition data set and not e.g. EMEP data because the shipping contribution to the nitrogen deposition was provided by SHEBA. If we would have choosen EMEP data, we would have been able to simulate longer time periods. However, we would not have be able to properly quantify the shipping contribution. We are aware of the fact that considering only one year introduces a considerable bias.

> We plan to perform a decadal simulation with a more recent ERGOM version (coupled to MOM) and with EMEP deposition in future in order to deal with the question asked by the reviewer.

7. Baltic Sea is also zone of nitrogen inputs through N2 fixation. Is this component taken into account in the model?

> Yes, nitrogen fixation is included in the model. We forgot to mention it in the Material and Methods section. We will include it in the manuscript.

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