

Referee comment to:

*Importance of high resolution nitrogen deposition data for biogeochemical modeling in the western Baltic Sea and the contribution of the shipping sector*

**General comments:**

The manuscript (hereafter referred to as MS) attempts to answer two relevant ocean research questions. What resolution of atmospheric nitrogen deposition is necessary to properly evaluate its' impact and how high is the contribution from shipping? The main problem with the manuscript is, as the authors seem to be aware, that the method may not allow them to fully answer the first question.

The evaluation of different spatial resolution of the nitrogen deposition is based on model results from two years, one year spin up and one year to evaluate. A typical residence time for the evaluated region is not given, but the residence time for the Baltic Sea (which is given in the MS) is stated to be significantly higher than two years. It seems reasonable to assume that a one year spin-up is too short for the biogeochemical system to reach a steady state ready to be evaluated.

However, the manuscript reads in a clear, concise, and well-structured way, although a bit repetitious. The scientific approach is transparent and the methods and results are discussed in an appropriate way. The problem is that the overall experiment design is flawed, which is a shame.

**Specific comments:**

The title of the manuscript, and the abstract, represents the manuscript's content well and only some bits of information are lacking to clearly present the methods (see the technical comments below).

One of the significant pieces of information that are missing is a statement of the quality of the physical model, HBM. This can be done by stating findings in previous publication since it is not the main focus of this MS, but it needs to be stated and the relevant publication referred to. It is not necessarily so that an operational model gives good results when used to hindcast.

Another thing missing is the lack of focus on the biogeochemical processes in the model and how they are affected by changes in the resolution of nitrogen deposition. Also, a discussion of the general nutrient dynamics of the study area should be an important part of the section "2.3 Study region". Both these additions would aid in understanding the results. Especially as the validation shows large discrepancies.

Since the study uses a constant phosphorous deposition, and aims evaluate the effect of nitrogen deposition, I assume/hope that the area is generally nitrogen limited? However, this should be stated.

The validation section does not really state that the model results are good enough to answer the research question. Are they? Is there to little data? The validation section should have a clear and well-argued conclusion.

The authors also leave too much important information to be available in other publications/submissions only. The reader should be given the most important info for this MS in a sentence or two and not only the statement that it is elsewhere. See more specific comments about this in the technical corrections section.

The conclusions section is too long and contains too much discussion and outlook.

Finally I'm skeptical to be given a residence time valid for the Baltic Sea. Is it applicable also for the shallow Belt Sea, the southern Kattegat ect. ? If so, please state it.

**Technical corrections:**

Page 2. Line 6: Change the "Å" to lower case.

Page 2, line 21: Remove "the" before "land".

Page 2, line 31: Change "it" to "it's" or "it is".

Page 2, line 31: "Example of a region", not "for".

Page 3, line 17: The sentence feels unfinished.

Page 4, line 1: Consider removing the commas around "this is the first study". At least the second comma seems out of place.

Page 4, line 13-14: "Based on .... is performed". The formulation is out of place. Please, consider rewriting it.

Page 5, line 26: Please add the relevant information from the validation. I.e. are the cmaq04 simulation results sufficiently good? Are there any shortcomings the reader needs to know about?

Page 6, line 3-4: And what do Brian et al (2017) say? Since the EMEP model results are used in this MS you should shortly mention the essence of the comparison. If it is of no importance, why mention it at all?

Page 6, line 16-17: Does HBM work well in hindcast mode? Please state and/or refer to some info about the model skill.

Page 9, line 7: I think it would be beneficial to state in the "2.4 Model validation" section that the model validation will be done for all three runs.

Page 11, line 11: Remove "the" before "land".

Page 12, line 20: Replace "few" with "little" or any other word meaning "not much". "Few" is used for "not many".

Page 13, line 3: Replace "few" with "little" or any other word meaning "not much". "Few" is used for "not many".

Page 13, line 3-4: Since the winter DIP levels seem more correct than the winter DIN levels, I'd say that the high summer DIP concentrations are caused by too little DIN, not too much DIP, or possibly that there are cyanobacteria in reality, but not in the model. Something similar is stated a few lines down, but the argument is sort of started in the wrong way. Please rewrite.

Page 14, line 3: Replace "few" with "little" or any other word meaning "not much". "Few" is used for "not many".

Page 14, line 31: It is unclear if PON includes living biomass, i.e. phyto and zooplankton, or only dead particulate organic nitrogen. Does it? I suppose living matter is bio available, but it reads strange.

Page 15, line 9-10: The sentence: " This is reasonable ... and vertical stratification" makes me think PON includes living organic particulates, otherwise it makes no sense?

Page 17, line 13-15: I'm skeptical to be a given only a residence time valid for the Baltic Sea. Is it applicable also for the shallow Belt Sea, the southern Kattegat ect. ?

Page 17, line 30-31: Why is the short term impact lower?

Page 18, line 8: Mai=May ?

Page 19, fig 10: Figure it too small.

Page 20, line 8-12: But what did those publications find? Even if the evaluation work in itself is not part of this study the result is certainly important. Please use a few sentences to state the main findings instead of using two to just say that the information is elsewhere. E.g. :

"Karl et al (in prep ... issue a) found that ..... and the same study also showed that.... "

Page 20, line 12-13: Repeated sentence.

Page 20, line 28-29: Isn't EMEP better/less bad (Fig 6)?

Page 20, line 30: Why do you use a time period (April - September) when the DIN levels are likely to be depleted and thus depend to a large degree on the biogeochemical model, not so much the input? Wouldn't it be better to use winter months for DIN? If it is changes in phytoplankton growth/production you are after I suggest you show assimilation by the phytoplankton or maybe the export production or such?