Review on "Importance of high resolution nitrogen deposition data for biogeochemical modeling in the western Baltic Sea and the contribution of the shipping sector" by Neumann et al.

This study presents an analysis of the impact of atmospheric nitrogen (N) deposition, and in particular the contribution from ship-borne emissions, to the biogeochemistry in the western Baltic Sea. This region is affected strongly by atmospheric N deposition from land sources as well as from ships in the North Sea and Baltic Sea.

The authors use three different datasets (high to low spatial resolution) provided by two different atmospheric models (CMAQ and EMEP) to force their physical-biogeochemical model (HBM-ERGOM) to investigate the impact of different resolution and different data sources on the marine environment. They furthermore apply an element tracing method to the atmospheric inputs from the shipping and non-shipping sectors to quantify the contribution of the former to biogeochemical processes in the study region. A 2-year simulation (incl. a 1-year spin-up) is conducted for each of the different deposition datasets.

The authors find that the effect of different data products (i.e. atmospheric model outputs) is much larger than the effect of high vs. low resolution of the atmospheric deposition data. In addition, the contribution to the shipping sector to dissolved inorganic N (DIN) exceeds 5% in large parts of the study regions, with maximum values up to 10%. The contribution to particulate organic N (PON) is found to be slightly lower. Hence, the shipping sector plays a small, nevertheless relevant role for the biogeochemistry in the western Baltic Sea.

The manuscript is generally well written and easy to read. However, I think in its current form the manuscript lacks a strong conclusion/scientific finding owed to the study setup. In addition, some of the analyses could be more explicitly address the research questions defined by the authors.

My strongest criticism – as also mentioned by the authors themselves – is that the current study setup, in particular the comparably short simulation period, is likely not sufficient to draw general conclusions on the impacts of both model resolution and the shipping sector on the biogeochemistry in the study region. Instead of referring to future studies, the authors should address at least one of the aspects they outline in their introduction to a sufficient level in order to provide a strong study and conclusion. Besides this, I have a few minor points, which I would like to see addressed before considering the manuscript for publication (outlined in the comments below).

Therefore, I recommend reconsidering the manuscript for publication after major revision.

1. Major points/General comments

Study setup and findings

From my perspective, the current study does not allow for a strong conclusion on any of the points raised by the authors (model resolution, different datasets, shipping sector), which leads to Conclusions/Outlook, which rather read as a discussion of the limitations of the setup and a referral to future studies to overcome these. As discussed by the authors, this is due to the simulation period of only two years relative to the long residence times of N (several years) in the Baltic Sea. The former is owed to the fact that the applied marine model is not designed for multi-year simulations, as it is not sufficiently parallelized. Consequently, the model is not run to steady state.

I understand this technical constraint. However, there needs to be (at least) one strong and important finding that is sufficiently supported by the present study. This could be either the importance of resolution vs. different datasets or the impact of the shipping sector. The former might be easier to do, as there is already a strong indication that the different datasets overrule the effect of the different resolutions. However, it would be worthwhile to conduct this analysis based on a simulation that reached steady state, and thus provide not only qualitative indications but also a quantitative assessment of the impact. This would make the study much more relevant than it currently is.

Providing a comparable analysis (i.e. for a steady state) for the shipping sector's contribution would, of course, be very interesting as well, and even more relevant from a scientific point of view. However, I suppose it is computationally more expensive due to the element tagging, and the authors may want to choose only one of the two topics.

If neither of the two is feasible, the authors should state clearly in the introduction and conclusions that this study is only a proof of concept and more comprehensive analyses are required and left to future studies. However, this would significantly reduce the impact of the study. Hence, I recommend addressing one of the two topics as described above.

I further wonder why the authors chose the coarse (50km x 50km) resolution EMEP dataset although there is finer resolution (0.1° x 0.1°) EMEP data available? The resolution of the latter would be much closer to the CMAQ16 case, and thus would allow for a better analysis of the effects of resolution vs. different datasets. The results may not change significantly (Karl et al., in prep. same spec. issue b), nevertheless using this data also in the present study, would make it more consistent with the study goal.

Influence on biogeochemical processes

My second major point is that the study claims to analyze the impact of resolution, datasets and shipping sector on the "biomass production" (Introduction, page 3, first research question) or "biogeochemical processes" (Conclusions, page 22, lines 6-7). However, they do not show results of actual processes, but only DIN and PON (and DIP). Instead of presenting results for PON, an analysis of the effects/contributions to primary production (PP) would be much more insightful and would explicitly address the formulated research question. In this context, I would also suggest to not only analyze surface values (like in Fig. 9), but to consider water column averages (or water column integrals for PP). The differences between the shipping contributions to DIN and PON might simply be related to the sinking of (detritus as a part of) PON into deeper layers.

2. Minor points/Specific comments

Page 3, line 27: What is "this"? Biomass production? Please specify in the question.

Page 3, lines 28-34: It should be mentioned that a marine model is used in combination with different atmospheric deposition data (question 1) and element tracing (question 2).

Although, there is a well outlined motivation for this study, it gets a bit lost in the last paragraph of the introduction (page 4, lines 2-9), which reads like a rather long summary of existing research related to the present study. The authors could simply move the first sentence (page 4, lines 1-2) to the end of the paragraph to finish the introduction with a highlight statement.

Page 4, lines 11-21: This paragraph should be part of section 2.1, as it only refers to the atmospheric modeling. Also, it would help to have a short introduction at the very beginning of the methods section, in which the different methods that are combined (atmospheric models, marine model, element tracing) are briefly mentioned.

Throughout the manuscript (incl. figure labels), use acronyms/abbreviations consistently, i.e. do not use the same acronym for two different things (e.g. CMAQ04 for the atmospheric model and the setup of the marine model) and do not change between upper- and lowercase letters (e.g. emep and EMEP, dmu547 and DMU547)

Page 6, lines 2-4: The authors later state that EMEP data with 0.1° x 0.1° resolution are available (Karl et al., in prep. same spec. issue b). Some of the key findings of Bian et al. (2017) regarding the quality of EMEP could be included here.

Page 6, line 12 – page 7, line 16: It might make sense to first present the marine model (i.e. before the atmospheric models), as it constitutes the basis for all aspects of the study. What boundary conditions were used for the outermost model domain?

Figure 2: Are nitrification and (benthic) denitrification not included in the model? N2 fixation is also not included in the figure. I suggest including arrows for all processes that are involved in the N cycle. Also, include the description of the abbreviations in the figure caption.

Page 7, lines 13-17: It would be helpful for readers not knowing the element tracing method to at least add a short sentence how the tracing is technically done, i.e. by introducing additional model state variables and corresponding processes to the model.

Figures 3 and 4: I would suggest to merge these figures into one (my preferred choice), or at least show them as panels A and B in one figure.

Figures 5 and 7-10: The panels of the figures are too small and many features discussed in-text are barely visible.
What does "surface" in Figures 7, 9 and 10 refer to? Is it the uppermost model layer or is it a surface layer of defined thickness? Please specify in the text.
Does Figure 8 also show surface values? Please specify in the caption.
You could add the 5% and 10% isolines to the panels in the right column of Figure 9 for an easier link to the text description.

Page 13, lines 2-3: The simulated fall increase in DIN seems fine to me, considering the data variability in October/November.

Page 14, lines 29-31: What exactly do you mean with bioavailable PON? Is it detrital N only? Please add how exactly PON is calculated. Also mention that DIP is shown in the bottom row of Figure 7.

Page 17, lines 6-10: I would suggest describing the differences between the two CMAQ cases and between CMAQ and EMEP, before summarizing the section. At the moment, the latter is

not addressed and only mentioned in the last sentence of the paragraph, without any text basis for the reader.

Page 17, lines 11-16: So, we cannot draw any clear conclusion here. See my first major point

Page 17, lines 29-31: What about the effect of sinking of PON out of the surface layer? Also see my major point #2.

Page 18, line 13 – page 19, line 1: "Evaluating the total atmospheric contribution to DIN might have probably provided a qualitatively similar picture than this shipping evaluation." I do not see the relevance of this statement – maybe delete it?

Page 19, line 3: The Conclusions are too long and should rather be called Discussion. It is currently not possible to get, what the main conclusions are. The authors could further rename their Outlook to "Conclusions and Outlook" and state their most important conclusions there, followed by the suggestions for future studies.

Page 20, lines 17-18: What are the potential causes for this mismatch in the bloom timing? Please specify in the text.

Page 20, lines 28-29: This statement is very general and its validity depends strongly on the purpose and time scale of the simulation. I would argue that the high-resolution data, which resolve short-term precipitation events (see page 18, lines 4-6), are better suited for the impact analysis of such events on the local biogeochemistry.
In fact, it could be very interesting to compare station time series of, e.g. DIN and PP, produced by the different model setups at the location and time of this precipitation event, to analyze the effect of the improved resolution on local biogeochemistry.

Page 21, lines 4-5: Please explain why the relative differences in DIN and PON are much lower than those in the deposition, despite the high contribution of atmospheric deposition to nitrate at station DB2 (Figure S1)? Is the atmospheric contribution much lower in most other regions?

Page 21, lines 6-12: Please be specific what processes/interactions may not be fully covered by the model, and what "not fully covered" means in this context? Are they not included in the model or "just" under-/overestimated. Also provide references if the latter is the case.
What exactly do you mean with "improving the quality"? Is it the resolution or the magnitude of the data (considering the strong differences between CMAQ and EMEP deposition)?

Page 21, lines 19-20: The evaluation of the different deposition data is not really the goal of this study. However, considering that N deposition (from land sources) is largest in the coastal regions, I partly disagree with the statement of the authors that regions with less coastline should be considered to evaluate CMAQ and EMEP deposition data. Reliable data are especially needed in the regions with the largest atmospheric influence. These can be offshore regions as shown, e.g. for the southern North Sea (Troost et al., 2013; Große et al., 2017). However, this can also be the case in near-shore regions in the absence of major rivers (see Figure S1).

Page 21, lines 24-34: In this paragraph, the authors basically admit that the present study setup does not allow for a clear conclusion on the different study goals. From my perspective, this questions the value of the study in its current form (see also my first major point).

Page 22, lines 6-7: No actual processes are analyzed (see my second major point).

Page 22, line 15: Please state the key finding of Karl et al. (in prep. same spec. issue a).

3. Technical corrections

If possible within the journal manuscript guidelines, the authors could consider providing a list of abbreviations/acronyms as part of the supplement. There are a lot of these in this study, especially in the methods section. Introducing all of them in-text could be tedious.

Page 2, line 6: "StÅlnacke"

Page 2, line 18: "because a higher"

Page 3, line 24: "questions" instead of "topics"?

Page 3, lines 31-32: "to get a feeling" sounds a bit informal

Page 3, line 33: "question" instead of "topic"?

Page 4, Table 1: use uppercase acronyms (CMAQ, EMEP); use "16km x 16km" or simply "16km" for the spatial resolution (and analogous for the other resolutions; also in the text); use "Meteorology" instead of "Meteo"

Page 4, line 11: please introduce the acronym "HBM-ERGOM" here by first stating the full name

Page 5 lines 2-5: "For this study, we used CMAQ version 5.0.1 …"; remove "was used for this study"

Page 5, line 11: "online" instead of "inline"?

Page 5, line 14: "and 30 vertical z-layers each"

Page 5, line 17: There is no cmaq64 in this study

Page 5, line 33: What is "AIS"?

Page 6, line 2: "simulation is"

Page 7, line 5: "Reid1990"

Page 7, line 8: remove "Sil" in parentheses and use "SiO4-") in schematic (Fig. 2)

Page 7, line 10: add abbreviations "MiZ" and MeZ" after the two full terms

Page 7, line 15: "all model state variables and processes"

Page 8, caption Fig. 3: "Study region and geographic locations …"

Page 9, Table 2: add units for "Lon" and "Lat"; use "IOW-DB" as in the text

Page 9, line 7: "Validation data" instead of "Model validation"

Page 9, lines 9-11: remove the sentence about PON, as it is not used for validation

Page 10, lines 1-2: "less than 10km" distance from land? Please clarify. "North", "West", "South" now with uppercase first letter, previously with lowercase (page 8, line 13), please be consistent

Page 10, lines 6-11: repetition of lines 1-5

Page 10, line 14: DB2 is not yet introduced

Page 10, line 17: "because of its enclosed location and missing rivers"

Page 11, lines 5-6: This has already been mentioned on page 5, line 25

Page 11, lines 15-17: the explanation for the artifact can be shortened, as it has been done previously (page 2, lines 18-19)

Page 11, line 27: "Fig. 8", references to Figures and Tables should be in the order of their appearance in the manuscript

Page 12: line 9: What do you mean with "boxes" and "less regular patterns"?

Page 12, line 10: "Whether it"

Page 13, Figure 6: use the acronyms in the top left legend (CMAQ16 etc.)

Page 13, lines 11-12: "timing" instead of "temporal occurrence"?

Page 14, line 13: "atmospheric deposition data"

Page 14, line 22: "much lower. At station"

Page 14, line 24: "The highest differences correlate with the vicinity to land"

Page 14, line 26: "coast. Station TF13"

Page 14, line 32: add references to left and center column of Figure 7

Page 15, line 1: "Oder River"

Page 15, line 8: "patterns"; remove "except for the magnitude", it is a repetition

Page 16, Figure 8: "CMAQ04 minus CMAQ16" and "EMEP minus CMAQ16" in figure titles; add to the caption what was used for normalization to calculate the relative changes (CMAQ16?); the units of the absolute values of DIN, PON and DIP should be "µmol N/P m-3", right? Some are "µmol N/P m-2"

Page 16, line 2: "in the two columns"

Page 16, line 3: "the patterns shown for absolute differences are"

Page 17, line 10: "to EMEP nitrogen deposition"; remove "data set – namely EMEP"

Page 17, line 21: "east of Rügen"

Page 17, line 23: "Fig. 9, top right"

Page 18, line 1: "south of Funen"

Page 18, line 3: add "Lolland" label to Fig. 3

Page 18, line 5, "south of Funen"

Page 18, line 6: "due to the averaging"

Page 20, line 1: "to get a feeling" sounds very informal; "Independent of the coarse resolution, …", include reference to Karl et al. (in prep. same spec. issue b)

Page 20, line 4: "attributed" instead of "accounted for"

Page 20, lines 12-13: repetition of the previous sentence

Page 20, line 15: "was presented"

Page 20, line 17: "timing" instead of "temporal occurrence"?

Page 20, line 33: "closer" or remove "than DIN" in next line

Page 21, line 2: "therefore" instead of "wherefore"; "In the open Baltic Sea" instead of "At the open ocean"

Page 21, line 7: "fjords, boddens, and lagoons"

Page 21, line 29: "this study"

Page 22, line 15: "from a NECA scenario"

Page 22, line 24: "spatio-temporal"

Page 22, lines 31-33: shorter, because it repeats the last sentence of the previous section

Page 23, line 9: "The data is available"

Page 23, line 14: "was created"

Page 23, line 15: "publish the data, because"

Page 23, lines 16, 18, 21, 22: "data is available"

Page 23, line 22: "upon request"

Page 23, line 30: "Introduction section, and to the development of the research questions"

Page 24, lines 22-23: remove ".cdo) …", looks like a copy-paste error