Answer to reviewer 1: Interactive comment on Validation of the coupled physical-biogeochemical ocean model NEMO-SCOBI for the North Sea-Baltic Sea system by Ruvalcaba Baroni et al.

Note: Authors answers are given in bold

Reviewer #1

The paper of Ruvalcaba-Baroni et al. represents an important step forward in coupled physical-biogeochemical modeling of the North Sea and Baltic Sea regions as a single domain. The combination of NEMO with SCOBI is logical and according to the authors knowledge is only the third such effort from the modeling community after the papers of Maar et al. (2011) and Daewel & Schrum (2013), based on DMI-BSHcmod/ERGOM and ECOSMO, respectively. Given the need for model ensembles to improve overall understanding of biogeochemical functioning of marine systems, this study is very relevant, even more so with current efforts to better integrate management of the two seas under study.

The paper is generally of very high quality and builds upon the base of previous work in the same group with respect to both the development of NEMO-Nordic and SCOBI. Therefore I do not see any fundamental problems with the research and support its publication. That said, it is interesting to observe the discrepancies between SCOBI-modeled parameters and observational data in certain regions, which show the current limitations in knowledge and hopefully will guide the authors towards future improved iterations of the model. Some of these discrepancies deserve a bit more elucidation in the text, or at least better structuring of the sections, so as not to leave the most important discussion points to the end of the paper. There are also a small number of technical clarifications I would draw attention to, and some suggestions for alternative phrasing and setting the context of the study. These may all be considered minor revisions so I give them as a Line-by-Line list below.

Kind regards,

Tom Jilbert University of Helsinki, Finland

We thank Tom Jilbert for nice words and accurate comment/suggestions, which we address in details below and will improve our manuscript.

1) Line 45-50: Not clear why primary producer assemblages are mentioned here for NS but no equivalent description for BS. I suggest to introduce the physical aspects first, then biogeochemical and finally the plankton assemblages.

We indeed only mention cyanobacteria in the Baltic Sea, as these are the most problematic species there. However, we will restructure the paragraph and add more details on primary producers in the Baltic Sea.

2) Line 66: Replace ”recycling of benthic phosphorus minerals” with ”recycling of phosphorus from sediments”

We will change the wording as suggested.

3) Line 70-71: Please update this setting of the context with a citation of the following publication and potentially references therein: ”The Baltic and North Sea Strategic Research and Innovation Agenda BANOS SRIA 2021 The final BANOS SRIA draft of the proposed, new, joint Baltic and North Sea Research and Innovation Programme -BANOS BANOS CSA Deliverable 1.5”.

We will adjust the context to include this relevant work (Koho et al., 2021).

4) Caption of Fig. 1: Capitalize SHARK

We will capitalize this.

5) Table 1: Modify mg CHL m-3” to mg Chl-a m-3”

We will change this.

6) Figure 2 legend: Typo in atmosphere

We will correct this.

7) Line 183: Typo 100 kton N/yr

We will correct this.

8) Line 195: Does this mean ”reduced to 0.3 x and 0.75 x the original value, respectively”? Please clarify. Also clarify (as implied in Fig. 2) that the resulting nutrient flux to the sea is entirely in the dissolved fractions and comment briefly on simplifications with respect to e.g. fluxes of TP, that in reality are largely particulate.

Yes, the reduction factors are: 0.3 x detritus P, and 0.75 x detritus N. This means that we have reduced the riverine input of particulate organic phosphorus and nitrogen (in our model assumed to be detritus of nitrogen and phosphorus) by 75% and 30%, respectively, to account for coastal retention and bioavailability. This is based on values at the Swedish coast (Edman and Anderson, 2014; Eilola et al., 2009). These factors are largely unknown for most of other coastal areas in the model domain and therefore the same reduction factors are used throughout the domain. We will ensure that this is clear in the text. Regarding the resulting nutrient flux, figure 2 shows the total

phosphorus and nitrogen (so the sum of dissolved inorganic and detritus) that enters the model domain. We will ensure this becomes clear in the text. Please also see our reply to comment 30.

9) Line 286: Modify to e.g. ”depending on their proximity to one another”

This will be changed as suggested.

10) Line 301: Typo ”to analyze”

This will be corrected.

11) Line 315: Replace 80s with 1980s

This will be changed as suggested.

12) Line 317: Typo observations are too low

This will be corrected.

13) Line 326: Replace dont with do not

This will be corrected.

14) Line 346: Typo statistically

This will be corrected.

15) Line 347: Remove ”not studied here”, it confusing to state this

This will be removed as suggested.

16) Line 349: Rephrase to e.g. ”There is a lack of observational data for bottom water oxygen during the period 1975-1995”.

There is indeed a lack of observations before the 1995 at ANHOLT and at most stations in the Skagerrak-Kattegat region. However, it is not limited to oxygen. While nitrate observations are totally lacking before $\sim$ 1995, phosphate and oxygen observations are scarcer (with fewer data points per year) before the year around 1995. Note that the year where observations become more abundant is different at each station in the Skagerrak-Kattegat, but fall between 1992 to 2000. However, we agree that the sentence is confusing. This will be rephrased as follows:

“In the Skagerrak-Kattegat region (e.g., at ANHOLT, fig. 6) observational data is largely lacking before 1992-1995, including that for oxygen in bottom waters and therefore model trends may be more representative of the system for historic values.”

17) Line 407: Typo less than

This will be corrected.

18) Line 420: Typo extent

This will be corrected.

19) Line 421: Modify to The main inference of...

This will be corrected.

20) Line 424: Modify to ”Despite such specificities of...”

This will be changed.

21) Line 457: Modify to its (no apostrophe)

This will be corrected.

22) Line 471: Modify to ”bias is” or ”biases are”

This will be corrected.

23) Line 497: Modify to most models

This will be corrected.

24) Line 572: How could low oxygen theoretically inhibit primary production? Not easy to understand what is meant here.

”low oxygen” will be removed from the sentence.

25) Line 574: The model vs. data discrepancy in the timing of the late winter/spring bloom in the Kattegat is one of the key question marks raised in the study, and indeed it is highlighted in Fig. 7. I think it deserves more elucidation at this point in the discussion, for example an assessment of the degree to which light or temperature might be inhibiting the early onset of the bloom in the model. The authors return to this in Section 3.6 Future work and data gaps but in the current version the reader is left hanging for an explanation after it is established that there is no nutrient limitation at the time in question.

26) **We will add, most likely in appendix, plots of the sensitivity of modelled phytoplankton growth to light and temperature. We will then add more information on it's relevance in the text here. Please note that we attribute the bloom delay occurring in the Kattegat-Skagerrak mostly to the fact that the model does not capture the seasonal variations of the light attenuation depth in this area. In addition, the model temperature is underestimates during spring at for example ANHOLT (by approx. two degree, see figure 1 below). This indicates that the stratification in the Skagerrak-Kattegat is not sufficient in the model during spring and may inhibit phytoplankton growth at the exact time of the blooming period. We realize that we have not included the latter in the text and will be added as a discussion point. Please also see our reply to reviewer #3, on pages 4 and 5, comment 0.8.**

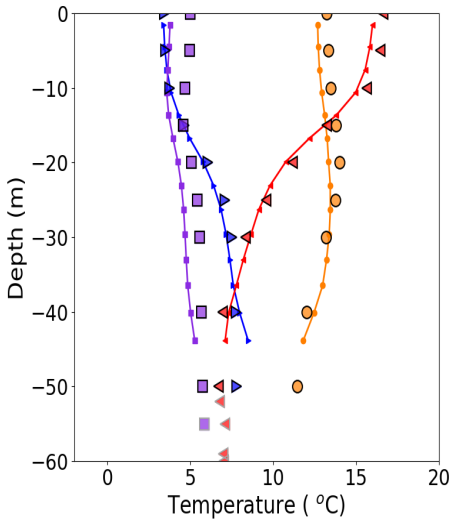

Figure 1: Seasonal profiles averaged over 2001 to 2017 for temperature at ANHOLT, where blue colors are for winter, purple for spring, red for summer and orange for autumn.

27) Line 590: Again please check the BANOS SRIA for up-to-date statements about the need for integrated modeling of BS-NS system

Yes, we will.

28) Line 595: Citations in brackets

The missing brackets will be added.

29) Line 604: Modify to ”in Ford et al. (2017)...”

Brackets for this citation will be removed.

30) Line 616-623: This is a very important section of the discussion: the model assumes (if I understand correctly) that a certain bioavailable fraction of nutrients enters the sea, directly into the dissolved phase state variables PO4, DSi, NO3, NH4 (Fig. 2). This is of course a large simplification of reality, where there is transfer between bioavailable and non-bioavailable fractions within the coastal filter, as well as heterogeneous removal of nutrients. Different types of coastlines may behave quite differently in this regard, see eg. Asmala et al., L&O 62 (2017). Are there any of the observed model vs. data discrepancies e.g. in near shore nutrient or Chl-a concentrations, that could be affected by this simplification? If so, it would deserve some mention higher up in the discussion for those specific areas.

The bioavailability factors are only used on the particulate organic matter that enters the sea with the rivers. The factors are to one part accounting for the different quality of the organic matter from land, of which some part is refractory and will not decompose fast enough to be a part of the marine nutrient circulation within the model system. In the ocean model, this factor also accounts, to some extent, for the filtering effect of the coastal zone. In SCOBI, the bioavailable detritus (particulate organic material that has been decreased from the rivers) enters the sea into

the detritus pools for the specific nutrient and then go through the transport, grazing, sinking and decomposition processes. Thus, the bioavailable fraction of the organic pool does not directly enter the dissolved nutrient (see figure 1 in the main text). We will better clarify this in the text.

Regarding the filtering effect, the reviewer is correct that different types of coastlines have different response to nutrient removal. The bioavailibilty factors used here are typical values for the Swedish coast and the Baltic Sea Eilola et al. (2011) , which are also not too far from those in Asmala et al. (2017) (16% of nitrogen and 53% of phosphorus removal from land versus 30% and 75% assumed here). These factors are, however, poorly quantified for the North Sea. As a first approach we assumed a homogeneous value for our entire domain following the approach of Eilola et al. (2011). In addition, we did perform a scenario with the exact same settings as that presented here, but without any retention factor. When comparing results from the latter to the hindcast scenario here, the spatial effect of the retention factor was minor, especially for nitrogen which decreased by one or two mmol/m3 almost homogeneously in our domain. The chlorophyll-a was not much affected by this and therefore we dedicated only minor discussions to this in lines 620-624. We think that the major effect of this is confined to specific coastal areas, probably those limited by P. However, we agree with the reviewer that this is relevant and deserves more explanation in the text. This also links to our reply to reviewer #3, comment 12 and 13 where we say that we will look further to better detect which coastal regions are most affected by the rivers. We will also add the reference of Asmala et al., 2017 in line 621.

31) Line 666: typo therefore

This will be corrected.

32) Appendix Line 737: Important: should this read "BOPREM-BIP decreases with increasing salinity and decreasing bottom oxygen concentrations..." ? That is the implication of the equations and following text, and the logical relationship.

Yes, that is correct and 'decreasing' will be added before 'oxygen concentrations'.

33) Appendix Line 771: Typo from

This will be corrected.

34) Appendix Table A2: Comment on the validity of using a single porosity value for entire NS-BS system.

We are fully aware that porosity is not homogeneous in the Baltic Sea and the North Sea - see for example a model study of benthic phosphorus cycling in the Baltic Sea by co-authors of this study, where porosity was region-specific (Almroth-Rosell et al., 2011, 2015). The results presented here are, however, from the first version of NEMO-SCOBI which indeed is simplified and has currently a hard-coded porosity. It is definitely an important point in our list of future development for NEMO-SCOBI, where it will be made regional specific. A comment on this will be added in the text.

References

Almroth-Rosell, E., Eilola, K., Hordoir, R., Meier, H. M., and Hall, P. O. (2011). Transport of fresh and resuspended particulate organic material in the Baltic Sea - a model study. *Journal of Marine Systems*, 87(1):1–12.

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