Biogeosciences Discussions

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Reply on RC2

Jenny Hieronymus et al.

Author comment on "Modeling cyanobacteria life cycle dynamics and historical nitrogen fixation in the Baltic Proper" by Jenny Hieronymus et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-156-AC2, 2021

Author comments in italics.

Major comments:

The paper conducts a 3-D modeling of Baltic Proper to simulate filamentous diazotrophic cyanobacteria and associated N2 fixation. The two major points that the authors appear to emphasize is that, the incorporation of cyanobacteria life cycle (CLC) dynamics and phosphorus dependence in the model greatly improve model performance in correctly simulating seasonality of the cyanobacteria biomass and N2 fixation. However, I found that the paper lacks clear focus, introduction, and thorough analyses. I was also puzzled by some results particularly from P dependence schemes.

Authors: Thank you for this observation, we will revise the manuscript to become more focused and streamlined to the aims of the paper (to include CLC to a 3D model and perform phosphorus sensitivity runs to optimize specific to the Baltic Sea). Please see more detailed replies to the comments regarding the mentioned issues here below.

(1) CLC appears to be one of the key issues the paper aims to resolve. Although the lead author and others have published a series of papers of CLC simulations, which making me not quite clear if CLC is still one of the key schemes that this paper would focus, at least both in the abstract (line 17-18) and conclusion (Line 301-304) the CLC is described as the main point of the paper. However, scientific background of life cycle of filamentous cyanobacteria is not sufficiently introduced in the paper, making it difficult to understand the different stages set in Method Section 2.3. Even in this section, CLC model part is not clearly described. There is Fig. 2 included in the paper which appears to be the structure of CLC and the model, but this figure is not referred and described. More importantly, model results of CLC (of different stages) are not shown. How the CLC improve the model is not analyzed and compared (such as to a model version without CLC or to the previous studies), except only two conclusing sentences (line 244-245).

Authors: It is true that CLC is the main focus of the paper since this is the first 3D modelling effort that includes the CLC in the Baltic Sea. Although the CLC model has been published before by two of the co-authors (Hense and Beckman 2006; 2010, Hense et al. 2013) this was neither combined with a 3D model nor specifically validated to

observations from the Baltic Sea. In these previous publications there was free P availability, which is not true for the Baltic Sea, where P is very limiting for the filamentous cyanobacteria during the summer blooms together with weather conditions (e.g., Klawonn et al. 2016; Olofsson et al. 2016; Degerholm et al. 2006). This will be included in the revised version of the manuscript starting with the background of cyanobacteria life stages and P limitation in the introduction. We will clarify both that this is the first time CLC and 3D model is used as a combination in the Baltic Sea and why we need to evaluate P dependency before estimating biomass and nitrogen fixation in the revised version of the manuscript.

We will also more thoroughly compare the model results of biomass and timing of bloom with a run that excludes CLC.

(2) P dependence and weak P limitation (wPlim). I have problem to follow the key scheme (wPlim) that chosen as the main scheme. Half saturation concentration is very low (10^-6 nM), how it can be effective? Naturally there should be no complete absence of phosphate. The current half saturation of 10^-6 nM is extremely low, several order of magnitude lower below the detection limit. Indeed, Fig. 6. shows the lowest phosphate concentration is still much higher than 10^-6. How it can make substantial different results from noP as shown in such as Figs. 3 and 4? Even in these two Figures, Fig.3 and 4, wPlim results are not consistent (compared to other setups). For example, in Fig. 3 seasonal experiments, wPlim produce lower biomass for three of four sites (BY2\5\15). How in the Fig. 4 interannual variation, wPlim gives higher biomass?

Authors: The difference between noP and wPlim is that noP requires no P at all to bloom while wPlim blooms as long as P is present, even in tiny (~10^-6) concentrations. This means that when all P is consumed, the cyanobacteria can no longer grow.

In the noP case, the end of bloom is completely dependent on the temperature and light availability (cf. Eq. (1), (2) and (6) in Table S3 and Table S5). Furthermore, there is no uptake or release of phosphorus by the cyanobacteria in this case which means that they do not affect the nutrient composition of the water column.

The relationship between the biomass and the P limitation scheme is not straight forward. It is not possible to say that wPlim will always generate the highest biomass compared to the other setups. The choice of P limitation not only affects the biomass but also the nutrient composition of ambient water which in turn affects the biomass of all functional types. Fig. 6 shows that during the early period, the phosphate concentrations are higher and the DIN lower compared to the other experiments generating higher cyanobacteria biomass. During the later period, DIN is completely depleted after the spring bloom in all experiments, while the phosphate is lowest in wPlim generating the lowest cyanobacteria biomass. Note that Fig 6 shows the mean seasonal cycle of phosphate over two different time periods at monitoring station BY15 only and says nothing about the overall minimum concentration.

We will deepen this discussion in the revised manuscript.

Overall, the authors emphasize both CLC and wPlim schemes give better bloom timing (or seasonal pattern). However, even the "timing" of bloom is not quantitatively defined, and therefore the comparison and the conclusion they give better bloom timing is not supportive. For example, in Fig. 3, I cannot directly identify the difference of the start and end time of the bloom in each experiment. The authors should quantitatively define and

show in numbers the start and termination of the blooms both in observations and the four experiments.

Authors: In the revised version of the manuscript we will include a table with mean peak Cyanobacteria biomass values and timing (date of the peak) of both model runs and observations and also the seasonal span of the blooms (between which dates) of the different model runs and observations, in order to more easily compare their differences. This comparison will also be more thoroughly discussed in the revised version.

We will also compare the model results of biomass, date of bloom peak, dates between the bloom span with a run that excludes the CLC.

Specific comments:

Section 2.2. Model structure Fig 2 should be referred and basic structure of the model sufficiently described.

Authors: We will include a modified version of Fig 2 in the revised manuscript that is clearer and also include a table to explain abbreviations. It will also be more clearly referred to in the text. Furthermore, we will include a schematic that more readily describes CLC.

Section 2.3. The text of this section should be reorganized to logically describe the stages and transitions.

Authors: We will include a new schematic of the CLC (see enclosed New Figure 2 for a fist version) parallel to the current figure 2 in the revised version of the manuscript. We will also revise the text of this section using the chronological order of the new CLC figure to help the reader understand the text of this section better and in a more logical way.

Line 117-119: Two versions? Or is it the “simplified version” of the "modified Version"?

Authors: It is a mix of the original model of Hense and Beckmann (2006) and the simplified model of Hense and Beckmann (2010). We agree that this is confusing and need to explain and revise this sentence.

Line 125-126, logically it is not justified why the differences among the species cannot influence the main patterns?

Authors: Since we are using an average salinity and temperature preference range we can not look at specific regions where for example one of the taxa dominates because they might be outside the mean range. We will clarify this in the revised version of the manuscript. It might for example be difficult to apply our settings in a low salinity region of the Baltic Sea where we may have cyanobacteria that can grow in salinities down to 0, while the model has the lower range set to salinity 3.

Line 132, the difference between AKIW and AKIB is not described.

Authors: Akinetes are pelagic (AKIW) or benthic (AKIB) and can be transferred between these reservoirs through sinking and resuspension. This is described in lines 132-133, but all abbreviations will be included in a new table in the revised version of the manuscript.

Line 139-140: "For the transition between AKI (AKIB and AKIW) and REC we prescribe a fixed germination window - from April 20 to the end of April": It is unclear. How the germination window defined? So, there is only one full life cycle each year? Before April 20, it is HET to AKI; and after end of April, it is always REC?

Authors: The germination is defined in Eq. (30) in Table S.3. Between April 20 and the end of April, germination occurs at a constant rate times the AKI concentration. The transition from HET to AKIW is defined in Eq. (11) in Table S.3. It is dependent on the temperature and occurs when the growth of HET has fallen below a critical value. There is thus only one full cycle each year. This will be clarified in the revised version of the manuscript and more easy to follow when referred to the new figure which will show the seasonal CLC.

Line 146, "Growth of HET and REC are inhibited under anoxic conditions." Why? Normally diazotrophs prefer anoxic conditions, right?

Authors: We do not understand why they would prefer anoxic conditions. As they spend their active life stage in surface waters where there is enough light for them to perform photosynthesis there is plenty of oxygen around all the time, as they also produce oxygen themselves. Heterotrophic diazotrophs might prefer other conditions but the organisms in this paper are all photoautotrophs.

Line 147, What is difference between this salinity dependent window and the above-mentioned time window (April 20-end)?

Authors: The salinity dependence has little to do with the seasonality but is an effect of the observation that the optimum growth conditions of cyanobacteria occur in salinities between approximately 3 and 10 PSU. This span is taken to approximately represent the optimum span of N. spumigena, Aphanizomenon sp. and Dolichospermum spp. (Rakko and Seppälä, 2014). Its effect is more to limit the growth spatially than seasonally.

Line 150, The range already described two sentences above.

Authors: The sentence above line 150 is referred to AKI and REC and the one on line 150 is AKIB. We will revise these sentences so this is more clear in the new version of the manuscript. We will also include a table with all abbreviations so the reader can easily look at them when needed. It is easy to miss that there are different versions of AKI for example.

Line 151-153, Between 11C and 28C, it increases linearly from 10% to 100%?

Authors: The temperature limitation is defined by Eq. (8) in Table S.3 and is shown in the enclosed Figure R1. It is an adaptation to RCO-Scobi from the original model by Beckmann and Hense (2004).

Line 181-182, Nitrogen fixation rates appear to be an important observation parameter. How exactly calculated (estimated) from biomass in this paper?

Authors: Detailed calculations can be found in Olofsson et al. 2021 as we also refer to in the method section, but we will also add a few more explaining sentences on the calculations in the revised version of the manuscript. Biovolume (mm3 L-1) of the three different species were obtained from the SMHI database and mean values of volume-specific measurements of nitrogen fixation were obtained from in situ measurements of thousands of cells of each of the three taxa across two summer seasons (From Klawonn et al. 2016, as referred to in the manuscript). Observed taxa-specific biovolume (mm3 L-1) were multiplied with the taxa-specific nitrogen fixation measurements per day (umol N mm3-1 d-1) to obtain nitrogen fixation rates per volume water per day (mmol N L-1 d-1), and further depth-integrated over 0-10 m (mmol N m-2 d-1) to obtain area-specific nitrogen fixation rates. These rates could then be summarized for the whole year and multiplied with the size of the Baltic Prover (200 000 km2) to provide nitrogen loads via nitrogen fixation by filamentous cyanobacteria (kton N yr-1).

Fig. 3. Unclear the seasonal cycle is for earlier period (1960-1979), later period (1999-2008) or full period (1850-2010)?

Authors: It is for the period 1999-2008. This will be clarified in the caption.

Line 195: Diazotrophs tend to have much higher C:P or N:P ratio than normal phytoplankton.

Authors: It is true that they have a flexible ratio which can be both above and below Redfield, but the difference is not huge. Ploug et al. 2010 and 2011 show a fixation ratio of 6.6 for Baltic Sea filamentous cyanobacteria for example, this is only slightly above Redfield of 6.5. There is a difference between diatoms and dinoflagellates as well (Menden-Deuer and Lessard 2000), but we can not include all differences in this model and have to make some simplifications.

Line 252-253, How the N2 fixation is simulated? That may indicate wrong simulation of biomass-specific N2 fixation rate.

Authors: The N2 fixation is a function of the temperature, light availability, N/P ratio and P. It is fully described in the appendix of Eilola et al. (2009). This section will be clarified with these details in the revised version of the manuscript since nitrogen fixation estimates is a main focus of the paper.

Also, the observed N2 fixation is derived from observed biomass (still unclear to me the method); is that unreliable?

Authors: It is based on many measurements (thousands of cells across two seasons; From Klawonn et al. 2016) and observations over a long period of time (ca. 10 years of monitoring data from biweekly sampling in the Baltic Proper) so we would say it is fairly reliable. We will describe how it was estimated in more detail in the methods of the revised version of the paper (please see a more detailed reply to this comment above).

Some format issue: such as some incorrect parentheses (line 47, 107, 148), missing unit (line 201), incorrect subscripts and superscripts (line 178, 182), inconsistent color codes of experiments across figures 3, 4, 6, 7. "Baltic proper" or "Baltic Proper"?

Authors: Thank you for these observations. We will correct these errors in the revised version of the manuscript.

Author reply references:

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Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-156/bg-2021-156-AC2-supplement.pdf