

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-156

Anonymous Referee #2

Referee comment on "Modeling cyanobacteria life cycle dynamics and historical nitrogen fixation in the Baltic Sea" by Jenny Hieronymus et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-156-RC2>, 2021

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.

(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 concluding sentences (line 244-245).

(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?

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.

Specific comments:

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

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

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

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

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

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?

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

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

Line 150, The range already described two sentences above.

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

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

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

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

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

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

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"?