

Interactive comment on “Cyanobacteria Blooms in the Baltic Sea: A Review of Models and Facts” by Britta Munkes et al.

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Reviewer #1:

Dear Ute Daewel,

we thank you for your time and effort and constructive comments. Here is a first reply to your comments:

Ute Daewel: As the title already indicates, the study collects information about the processes related to cyanobacterial blooms in the Baltic Sea and how these are implemented in currently used marine ecosystem models. In that context the authors address processes related to growth, limitation processes as well as mortality. Overall the authors provide a good overview about the cyanobacteria implementation in marine

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ecosystem models as well as a thorough and very useful review on the available literature on cyanobacteria. However, by now I cannot unreservedly recommend the study for publication, as there are some, partly major, points that need to be addressed first.

First: The authors should have gotten in contact with the model developers prior to submission to verify the model descriptions and implementations. One correction has already been issued by Oleg Savchuk, and being one of the developers myself, I have to add some points as well that were not correctly described for the model I use (corrections listed below). This could have easily been avoided by one short communication beforehand. However, I encourage the authors to do so now and correct the section on models accordingly.

The authors: We are grateful for your corrections and will acknowledge them accordingly.

Ute Daewel: Second: The authors give a very detailed review on the processes and impacts on cyanobacteria, in functional group type ecosystem models, however, the basic principle is simplification of the ecosystem. I would appreciate if the authors could more clearly connect the modelling section and the experimental section. The discussion section should be rewritten in a way where the model parameters and the observational findings are related to each other to actually address the “key differences between model approaches and observational evidence” (p. 23, l. 26f) and, if possible, provide recommendations for model improvements.

The authors: Thank you for these suggestions. We will improve the link between the modeling and the experimental section and also revise section 4.1 carefully.

Ute Daewel: Third: The manuscript gives the impression that parameterization and choice of functional groups happen at random or according to the modelers' needs. While specific model parameters can indeed differ widely (“Somewhat disconcerting, the respective parameter choices differ substantially from one model to another” p. 24, l. 6f), so does observational evidence on which the parameter choices indeed

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are based. In most cases the developers actually based their parameters on previous experimental publications or at least have a good reason for their assumptions. As this is usually explained in the corresponding publications, it would be more helpful to refer to these underlying reasons for choosing the parameters and revise the impression given in them.

The authors: We apologize in case anyone who has written cyanobacteria code feels offended by our text. Our goal was to establish an understanding of the remaining problems, which are associated with simulating cyanobacteria. Please note that we do not rank one model approach over another and we do not deny that there are good reasons for any of the underlying model assumptions used to date.

The problem we see is a dilemma: On one hand, the models are often developed to improve process understanding - as a means to make sense of observational data, which can be contradicting or inherently "differ widely". On the other hand, a comprehensive process understanding appears to be the pre-requisite for developing a mathematical model. The fact that parameter choices differ substantially from one model to another is an indication that these are still subject to discussion. The aim of this review is to identify such knowledge gaps.

We agree that our choice of wording "somewhat disconcerting" is unnecessarily provocative. We will delete this in the revised version of the manuscript.

Ute Daewel: The general biological model structures are explained well, but I would consider a brief foray into the physics. This might be too extensive for a paper focusing solely on biological aspects, but some basic explanation of the most important physical variables could be useful for establishing context. Especially as the relationships between cyanobacteria and physics such as temperature are not only directly related as indicated by figure 1 & 2, but also indirectly through the ecosystem interactions as well as physical processes such as transport and upwelling.

The authors: Agreed. We will add these aspects to the revised version of the

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manuscript.

Ute Daewel: Even though I found the ms well readable, the English needs substantial improvement. There are several occasions with wrong grammar, typos and somewhat odd expressions. Some are listed below, but, not being a native speaker myself, I would suggest the authors to edit the language thoroughly.

The authors: We will follow this suggestion. Being non-native speakers we are especially thankful that you took the time for the specific issues listed below.

Ute Daewel: specific comments: p2, l 6: “[...] a reduction of loads will have no net effect on the nutrient budget because cyanobacteria will compensate...” Can you give a citation? The authors: This is a logical consequence in models which assume a fixed Redfield-ratio. We will clarify this point.

Ute Daewel: p2, l 12-16: “Some of the numerous studies on cyanobacteria [...] are motivated by concerns to run into low-oxygen conditions [...]” Please consider reformulation The authors: We will do so.

Ute Daewel: p2, l 22: comma The authors: Thanks.

Ute Daewel: p2, l 30: reference from 2006: can you add a more up to date reference The authors: Agreed. We will add Shimoda et al., 2016 and Taranu et al., 2012 (see below).

Ute Daewel: p3, l 30 remove “so-called” as prognostic is a clearly defined concept A: Agreed.

Ute Daewel: p4, l 7 I doubt that any modeler really does “ad-hoc” choices on parameters, please rephrase also see comment above The authors: We will do so.

Ute Daewel: p4, l 23ff: “[...] cyanobacteria grow more slowly [...] and can in most models only thrive when nitrogen is no longer accessible to ordinary phytoplankton [...]” The citations contain both modelling and observational studies. Does this assumption

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relate to models or observations? The authors: It's rather an assumption in most models, supported by observational evidence. We will clarify this in the revised version of our manuscript.

Ute Daewel: p4, l 29-31 Can you give a reference where this was done. The phytoplankton bloom dynamics is generally determined by nutrient availability, which is the obvious reason for the nitrogen depletion in surface waters. The authors: True – we will add a sentence on bottom-up control.

Ute Daewel: P5, l 6 remove “they” The authors: Thanks.

Ute Daewel: P5, l15-16 please rephrase, its not clear which model does what. The authors: We will make this more clear in the revised version of the manuscript.

Ute Daewel: P5, l20 comma The authors: Thanks

Ute Daewel: P9 Section 3: In this sections there are some paragraphs that basically just list the same numbers that are given in the tables. You might want to consider shortening these paragraphs to what is new and necessary and avoiding listing the same numbers in the text and in a table. The authors: Agreed.

Ute Daewel: P10 l23-24 Grammar, please rephrase the two sentences. (& comma)
The authors: We will do so.

Ute Daewel: P11, l8 “Similar to this,…” The authors: Thanks

Ute Daewel: P15, l28-29 Please explain this sentence. The authors: We will do so.

Ute Daewel: P18, l27 Reference? The authors: s. below: Eglite et al., 2019/ Wasmund et al., 2019

Ute Daewel: P19, l5 “...,they are not able to...” The authors: Thanks

Ute Daewel: P22, l14-15 revise sentence The authors: Thanks

Ute Daewel: p23, l27: “Sect. 4.2 debates the impact of the oceanic processes to the

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Baltic Sea, respectively” In respect to what? The authors: We will delete “,respectively”

Ute Daewel: p24, l9: “Another potentially problematic assumption [...] is the fixed Redfield-ratio[...]” Why and how is this problematic? Enhance The authors: E.g., page 12, line 21ff refers to the cyanobacteria’s storage capacity of DIP which is very difficult to account for when assuming a fixed Redfield-ratio. Not considering this in a model might shrink the ecological niche of cyanobacteria. Further, there is observational evidence that diazotrophs do not obey Redfield (e.g., Larsson et al, 2001). We will clarify this in the revised version of the manuscript.

Ute Daewel: Specific corrections for ECOSMO:

Thanks for pointing this out. We will correct all formulations accordingly.

Temperature dependency for ECOSMO cyanobacteria: $T_{bg}=1/(1+\exp(-T))$ with growth multiplied by T_{bg} -Zooplankton in ECOSMO does not actively feed on itself, but Macrozooplankton feeds on Microzooplankton -Salinity constraints in ECOSMO: no growth for $S > 11.5$ -sinking vel=-0.1 m/

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