

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

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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 ecosystem models as well as a thorough and very useful review on the available literature on cyanobacteria. However, by now I cannot unreserved 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 to the model developers prior to sub-

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mission to verify the model descriptions and implementations. One correction has already 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.

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 were the model parameters and the observational finding 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.

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 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 the ms.

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 cyanobac-

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teria 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.

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.

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?

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

p2, l 22: comma

p2, l 30: reference from 2006: can you add a more up to date reference

p3, l 30 remove “so-called” as prognostic is a clearly defined concept

p4, l 7 I doubt that any modeler really does “ad-hoc” choices on parameters, please rephrase also see comment above

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 relate to models or observations?

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.

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P5, l 6 remove “they”

P5, l15-16 please rephrase, its not clear which model does what.

P5, l20 comma

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.

P10 l23-24 Grammar, please rephrase the two sentences. (& comma)

P11,L8 “Similar to this, . . .”

P15,L28-29 Please explain this sentence.

P18,L27 Reference?

P19,L5 “. . .,they are not able to. . . “

P22, l14-15 revise sentence

p23, l27: “Sect. 4.2 debates the impact of the oceanic processes to the Baltic Sea, respectively” In respect to what?

p24, l9: “Another potentially problematic assumption [. . .] is the fixed Redfield-ratio [. . .]” Why and how is this problematic? Enhance

Specific corrections for ECOSMO:

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  $S > 11.5$

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-positive buoyancy : sinking vel=-0.1 m/d

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