

Interactive comment on “A novel acclimative biogeochemical model and its implementation to the southern North Sea” by Onur Kerimoglu et al.

Anonymous Referee #3

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The authors present the implementation of a coupled 3D physical-biogeochemical model in the southern North Sea. The model included a detailed description of autotrophic growth explicitly taking into account for photoacclimatation and stoichiometric regulation. Model simulations were validated compared to available data for the period 2000-2010.

The paper is well written and clear and I agree on the importance of correctly understanding and representing physiological mechanisms in biogeochemical models. However, some points need to be clarified and/or added to better support their conclusions.

General comments

1) Model formulation: The authors consider a grazing rate function of prey biomass whatever the phytoplankton species represented. There are potential issues with this

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hypothesis as *Phaeocystis* colonies (that can dominate the spring bloom in some of the coastal stations of the studied area) is not grazed by copepods. This should be modified or/and discussed.

2) Model validation: In general, the model reasonably well reproduced available data. However, it is not clear which criteria is used to determine when observed data are realistically represented or not (e.g. p9 L1). This needs to be clarified.

3) Model exploitation: The mechanistic description of the regulation of phytoplankton composition is pointed as an important process and an improvement compared to other existing models to correctly describe primary producers but also nutrient cycling. However, this is not directly evidenced in the paper based on model results. A comparison of results obtained with and without taking into account for these processes is needed to support this conclusion.

Specific comments:

Figure 4: legend 'T' and 'S' on the dots: not clear

P9 L1: How determine 'realistic' and 'not realistic' results ? (see general comment 2)

P15 L6-8: This is an important result and could be developed and evidenced based on model results (Figure with different parameterization of under-water light climate and sinking rate of phytoplankton for example).

Figure 12: Why N:P variability of model results is always lower than the one observed?

P17 L10-11: This is not so clear for me: Fig 5 also shows an important overestimation of simulated Chl a compared to observation.

P 18 L5: The variability of Chl:C can also partly result from the overestimation of Chl a in the model (see previous comment).

P20 L 3-10: This should be evidenced based on comparison of two simulations (with and without taking account for photoacclimation) (see general comment 3)

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