

## ***Interactive comment on “A perturbed biogeochemistry model ensemble evaluated against in situ and satellite observations” by Prima Anugerahanti et al.***

**Anonymous Referee #3**

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This study investigates the response of a 1D version of the UK MEDUSA model to variations in the structure of equations for phytoplankton growth, zooplankton grazing and mortality of both planktonic groups. The model's response is tested at 5 different stations of varying seasonal cycle and nutrient supply.

I appreciate the attempt to investigate the sensitivity of biogeochemical models that will be, at a later stage, used for projections of climate change and other applied tasks, and I think the setup and design of the study are comprehensive, thorough, and suited to address this. However, I have some serious concerns with the way the results of the study are presented, and discussed.

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Firstly, in the introduction (page three, line 29) the authors state that "It has been demonstrated in conventional sensitivity analyses that only small perturbations are usually produced even with large variations in parameter values, but much larger changes in system dynamics can result from changes in the structural process formulations". I am not quite sure what "conventional" means, but I do think that this statement is misleading, as it neglects previous works that indicate a large sensitivity of marine biogeochemical models to their parameters, when compared to structural sensitivity. These studies have been carried out at a local scale, across different oceanic regimes, or in 3D (see, e.g., Friedrichs et al., 2007, *Jour. Geophys. Res.*, 112, C08001, doi:10.1029/2006JC003852; Ward et al., 2013, *Prog. Oceanog.* 116, 49–65, or Kriest et al., 2012, *Glob. Biogeochem. Cyc.* 26, GB2029, doi:10.1029/2011GB004072, to name just a few examples). Some of them even address the role of different functional forms, or have been applied to the BATS site (e.g., study by Ward et al., 2013). They may be helpful for presenting and discussing this current work in a wider context. Thus, more exploration about what has been found for marine biogeochemical models and their structural and parametric uncertainty can help to improve the discussion, which is currently somehow repetitive, lacks a critical discussion of the results, and how they might relate to other uncertainties (structural, parametric, physical, ...).

Secondly, I miss some discussion about the way the different functional forms have been made "equivalent to each other." (p4 line 17). As it seems, the parameters of the different equations (e.g., half saturation-constants) were fitted against the default function "so that the overall shapes are as similar as possible." (p 4, line 19), by "minimising the sum squared difference between the default and other uptake forms" (line 32ff). Obviously, when looking at Fig 1, this happened across a very wide range of potential nutrient or chlorophyll (in case of zooplankton grazing) concentrations. The upper limits are far outside the range of values for most stations simulated in this study (up to 100  $\mu\text{M}$  nitrate or phytoplankton N will likely never be found at BATS or ALOHA). Thus, it seems that the different functional forms were homogenised for a range that, at many stations, is outside the expected and/or observed range. On the other hand, the

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functions deviate most strongly when nutrients or phytoplankton are scarce (Fig 1a and 1b), and more representative for the simulated regimes. What would have happened, if the test functions (e.g., sigmoidal or Holling III) were made equivalent to the default functions at lower substrate levels, representative for more oligotrophic regimes? Could it be that the effects of switching to alternative forms becomes less important? Again, the paper to my opinion would benefit a lot from a more critical discussion.

Thirdly, as recommended by the second referee, I suggest that the authors read through the manuscript again carefully, revise some sections for clarity, and correct spelling and grammar.

The results section already contains a lot of detail, which is partly repeated in the discussion. I would suggest to shorten and streamline the presentation of results, highlighting those that are common among stations (or differ), as well as the effects of different parameter combinations, and use the discussion to clarify and discuss some of the aspects mentioned above.

Some detailed comments:

p2, line 14ff: "Inclusion of ..." - As mentioned by the other referee, even the spatial variability of light, nutrient availability and mixing already induce a spatial variability of plankton concentrations.

p2, line 34ff: "However, in biogeochemical models, it is rare that a solid mechanistic basis is present, ..." But see e.g., more recent developments of adaptive models based on mechanistic approaches, such as Pahlow, et al. (2008, *Prog.Oceanog.*, 76 (2), 151-191, doi:10.1016/j.pocean.2007.11.001) or Pahlow, and Prowe, F. (2010), *Mar. Ecol. Prog. Ser.*, 403, 129-144, doi:10.3354/meps08466.

p3 line 5: "applying"

p3 line 9: "highly susceptible" - What does this mean?

p3 line 3: "happened"

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p6 line 25: "Oschlies and Garcon, 1999" - a follow-up study by Oschlies and Schartau (2005, *Jour. Mar. Res.*, 63, 335–358) highlighted this even more; see also the study by Friedrichs et al. mentioned above.

p7, section 2.5.1: Physical input: please indicate the vertical grid on which this model was run, including its maximum depth.

p7 section 2.5.2: Biogeochemical input and validation data: I would suggest to list all the details of the different stations (location, max depth, data source, data assimilated) in a table.

p7 section 2.5.2: Do I understand correctly, that the observations were used for initialisation as well as for model validation? If so, then the model is not validated against fully independent data (at least not at depth, given a short simulation time of just 10 years), and I would suggest to mention it here.

p7, line 13: "Simulations are made at 37 depth levels" - This formulation sounds as if simulations were done separately for each depth level.

p15 line 24: "Most current biogeochemical models are run in a deterministic, rather than a probabilistic, manner, even though data from observations contain many uncertainties, eg. in satellite-derived chlorophyll." - I think I can guess what you want to say, but in the current form this sentence is not clear.

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