

Interactive comment on “Microbial diversity-informed modelling of polar marine ecosystem functions” by Hyewon Heather Kim et al.

Anonymous Referee #1

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This ms presents a bacteria-oriented ecosystem model, calibrated with a data-assimilation scheme, with two explicit bacteria types, fast growing (HNA) and slow growing (LNA). The authors find that properties of the bacterial community are strong predictors of bacterial carbon (C) demand, primary production (PP), and export (EP). The calibrated model is used to make predictions for a warming ocean.

At first I was quite intrigued by the approach of this study. But after going through the ms, it now appears fraught with too many problems to make it worthwhile. The problems start already with the title. I consider "microbial diversity-informed modelling ..." a gross overstatement of the authors' approach, which is more correctly describes

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in the abstract as "bacteria-oriented". Below I will outline why I consider this a failed attempt and how it might be modified into a useful contribution. Because I have the strong impression that essential information about the model and the data-assimilation method is missing, I will not go into much detail, though.

My first major problem was understanding the design of the model. The authors refer to one published work (Luo et al., 2010) regarding the model equations (besides unpublished manuscripts, which may or may not eventually be published), and present only the equations for the two bacteria groups. The model of Luo et al. (2010) is much more complex, totalling 30 state variables, than this one (with 12 states), so this reference does not really help much. Without access to the model equations, any attempt to understand the model code will be futile. In consequence, it also remains unclear what the model currency is. According to Fig. 1 and the description in the text (which is not very clear in this respect, except that the number of states is 12) the model employs a fixed stoichiometry approach but it remains unclear whether the fluxes are based on nitrogen (N) or phosphorus (P). Also according to Fig. 1, it appears that inorganic nutrient have no effect on and are not utilised by phytoplankton, leaving open the question what drives PP in this model. Since only very little information about the model is provided in the text of the ms and the supplement, the model design remains very much opaque. From what little information is presented I can see clearly only that the model is 0-D and employs a rather simplistic physiology (fixed stoichiometry).

The model has 84 parameters, of which 22 (inferred from l. 219 of the ms) are calibrated via data assimilation. What is missing here is a description of how these 22 parameters have been selected in the first place. For example, was the selection based on a preliminary sensitivity analysis or a-priori knowledge or assumptions of the model equations? Also, 22 is, in my experience, a very large number of parameters to constrain given the kinds and amount of data employed here. Thus, it is not very surprising that only a subset of 7–10 of these could be constrained well.

Related to this, the next problem is the description regarding overfitting and portability.

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I agree that these are essential concepts all too often neglected in modelling studies and so was happy to see that these are addressed here. Nevertheless, I question the quantification of overfitting (lines 175–179) by comparing the residual error with the (undefined in the ms) "target error" of the observations. Overfitting has very little to do with the noisiness of the observations. It is a consequence of the fact that every model is a simplification of the system it describes, and it is also tightly related to portability. The connection is that overfitting can compromise portability, and this is a good way of assessing overfitting. Overfitting often results from attempting to constrain too many parameters, which is revealed here by several parameters being not well constrained (Tables S2–S6). The different estimates of portability for the different year are another indication of overfitting.

I must admit that the concept of the bacterial modes was new to me, so I was happy to see the clear definition in Section 2.2 (first para). However, I could not figure out the main characteristic of these modes, since only very cursory information is presented in the text and Fig. 6. A table listing the modes and their properties and composition might be very helpful here. As it stands, the concept remains rather confusing. For example, the authors state that (l. 276) each mode is dominated by unique bacterial taxa. But considering Fig. 6, it appears that *Candidatus Pelagibacter* dominates both modes 6 and 1, although it appears that mode 1 is supposed to be dominated by *Candidatus Thioglobus*.

The above may be viewed as more technical problems, which could possibly be dealt with by, e.g., a detailed model description with all equations, or a recalibration of the model etc. However, I also see a major conceptual problem regarding the design of the study.

The problem lies in the way the authors use the model to make predictions for a warmer ocean. The main assumption behind the presented approach is that bacterial community composition is strongly correlated ("strong predictor", Abstract) to PP and EP. The functional bacterial community composition is represented in the model and its calibra-

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tion by assigning higher growth rates to HNA than LNA. Nevertheless, bacteria process the DOM produced during PP, so the behaviour of the bacterial community must be viewed as a response, not a driver, of PP. If bacterial community composition is in fact strongly correlated with PP and EP, that is in itself a very significant finding and I would very much like to see this substantiated. It could become a very useful diagnostic tool. However, here the authors treat the bacteria as the driving force determining PP and EP, which is wrong for several reasons. First, it reverses the cause-effect relation between bacterial activity and PP. Second, even if the cause-effect relation was OK, the data do not cover sufficient interannual temperature variability to allow predicting the response to a warmer ocean.

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