

## ***Interactive comment on “Calibration of a simple and a complex model of global marine biogeochemistry” by Iris Kriest***

### **Anonymous Referee #4**

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#### General comments

The author presents results from optimisation experiments with two relatively simple global biogeochemistry models that are notable for their representation of oxidant-limited remineralisation. The second model is a simplified version of the first, but performs similarly well to the first with regard to reproducing global  $\text{PO}_4$ ,  $\text{NO}_3$  and  $\text{O}_2$  concentrations as the model approaches a steady state. This finding is the main focus of the paper in the title and abstract.

Despite this focus, much of the paper is given over to comparing a previous optimisation of the more complex model to surface observations, to a new optimisation to deep observations. The author finds that parameters relating to remineralisation, stoichiometry and the oxidant limitation of remineralisation are well constrained by deep-ocean

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observations of  $\text{PO}_4$ ,  $\text{NO}_3$  and  $\text{O}_2$ .

My overall feeling is that the paper is badly lacking in focus. Reading through I was always struggling to understand what major point the author was hoping to make. Is it that the simple model is nearly as good as the complex model, or is it that different parts of the model are better constrained by different kinds of observations? At the moment the article reads as if two separate (and somewhat poorly developed) stories have been combined into one, with very little thought as to what connects them. I think that the author either needs to pick one theme, and develop it better, or needs to do a much better job of finding a narrative thread linking the two themes together. It is up to the author to identify how that might be achieved.

#### Specific comments

##### Abstract

Line 7: "a complex seven-component model (MOPS), and a very simple two-component model (RetroMOPS)" and "The simple model, which contains only nutrients and dissolved organic phosphorus (DOP)". RetroMOPS clearly has four components:  $\text{PO}_4$ ,  $\text{NO}_3$ ,  $\text{O}_2$  and POM.

Line 13: Please do a better job of explaining what is "the global bias".

##### 1 Introduction

Line 29: "[Kriest et al. (2017)] showed that annual mean tracer concentrations do not provide much information on parameters related to the dynamic biological processes taking place in the euphotic zone". Should be "annual mean tracer concentrations did not provide much information", as I am not convinced this is a general result for all models.

##### 2.2.1 Primary production

Equation 1: Why use the mean phytoplankton concentration at all? It would be more

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consistent with the rest of the model (i.e. Equation 5) to convolve the specific growth rate and the phytoplankton concentration into a single growth rate of the phytoplankton population ( $\text{mmol P m}^{-3} \text{d}^{-1}$ ).

## 2.2.2 The fate of primary production: Export, DOP production and remineralisation

Line 19: "DOP then decays to phosphate and nitrate". To me it would make sense to call it POM.

Line 19: "To allow for a potential, fast recycling loop at the surface, RetroMOPS parameterises an additional decay rate". Presumably this is inspired by (Oschlies 2001), but why would this be necessary in the absence of assimilated primary production observations?

Equation 4: I think a bit more could be said about the interdependence of  $s_{O_2(j)}$  and  $s_{DIN(j)}$ . For example, their sum forms a coefficient for remineralisation, so it is important to note that their sum is constrained between 0 and 1.

## 2.5 Misfit function

Equation 11: I am a bit confused by how the misfit function and its components are defined. In particular, I cannot see how  $\bar{\delta}_j$  (the global average observed concentration of the respective tracer) is included in the RHS.

Also, it seems that the model is being compared to gridded observations, instead of observational equivalents being extracted at the spatiotemporal locations of the observations. As the gridding process will introduce its own set of errors, this choice needs some justification.

## 2.6 Optimisation of MOPS

Line 15: I don't think including results from the hand-tuned model brings anything of value to the paper.

## 3.1 Optimisation of MOPS

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Line 4: Fig 1 (rather than Fig S1)?

Line 4: 10% and 1% (rather than 10% and 1°/∞)?

Line 12 (and throughout the text): "reduced denitrification". It is probably safer to avoid the word reduced except with regard to the chemical process.

## 3.3 Optimisation of RetroMOPS

Line 21: "The misfit to phosphate (Fig. 8, lower left panel) shows an elongated valley in the two-dimensional projection". I do not see a valley in this Figure. The misfit slopes down from the top-right towards the bottom left, but there is nothing to suggest it slopes back up again after reaching a minimum.

## References

Oschlies, A. (2001) Model-derived estimates of new production: New results point to lower values. *Deep-Sea Res. II.* 48, 2173–2197.

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