

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

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I thank referee 1 for his/her constructive and helpful comments. Below are my answers to the comments, indicated by "IK:"

"I found the comparison interesting and informative, although the main conclusion from the comparison does not differ much from an earlier work by Kriest et al., 2010."

IK: Indeed the results obtained in this study point into the same direction as those of Kriest et al. (2010), the exception being that here via optimisation I use a more thorough and objective scan of the parameter space, with many different combinations of parameters. I will try to point this out in a revised version of the paper.

"The choice of the parameter sets to be optimised vs to be retained from earlier studies

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seems arbitrary. Is there any particular reason or criteria by which the parameter sets are chosen for the optimization?"

IK Yes, there is - I am sorry that this did not come across properly: The first optimisation (MOPS^oS; Kriest et al., 2017) was basically meant as a test for the framework, and a proof-of-concept. I therefore chose a very wide range of parameter types, across all trophic levels, and acting on different time and space scales (see also Kriest et al., 2012, for the large scale effect of "b"). As it turned out, mainly the surface parameters couldn't be constrained by the present misfit function; so I exchanged those by parameters related to deeper processes (MOPS^oD). The selection of those was motivated by the large uncertainty regarding extent and expansion of oxygen minimum zones in models (Cocco et al., 2013; Cabre et al, 2013), and by the fact, that very little knowledge exists about these parameters (or even parameterisations). As it turned out, some of these were quite difficult to constrain, probably owing to the small volume of OMZs, relative to total ocean volume. Given that in RetroMOPS especially the parameters for DOP production and decay have to act as a shortcut for the entire complex surface ecosystem, I chose to optimise those, in addition to "b" and the -O₂:P stoichiometry (as control). I will try to make this approach clearer in a revised version of the paper.

"How would the results differ if the author optimized all of the parameters presented in Table 1 simultaneously? Remineralized nutrients are eventually transported to the euphotic layer and become available to support primary production. Therefore all parameters are interrelated with each other. In other words, optimal values for I_c , KPHY, μ_{zoo} , and κ_{zoo} would be different with the newly optimized values for b^* , KO₂, KDIN, etc. This could affect the model evaluations as well."

IK: I agree that it would be most desirable to optimise all parameters at once. However, given what was found by Ward et al. (2010), and the possible interdependence of some of these parameters, this did not seem feasible to me. I will explain the choice of parameters in a revised version of the manuscript.

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"Evaluation of DOP simulated from RetroMOPS remains qualitative. Although it is not sufficient, global datasets of DOM were presented in some previous studies including Letscher and Moore, 2015. How does the simulated DOP from the two models compare with the observations in terms of its distributions and concentrations? Why can't DOP be used as an extra constraint for the optimization in this study?"

IK: It can, and should, to my opinion, be part of the misfit function. However, as the global DOP data distribution is very sparse (compared to nutrients and oxygen), this may require a special treatment or weighting in the misfit function. We are currently working on different ways and methods to account for DOP in model calibration. A thorough treatment and discussion of this would, I fear, exceed the scope of this paper, which is already quite diverse.

"In many parts of the manuscript, statements are quite qualitative. For example, in conclusions, the author wrote "results in a slightly better fit to observed tracers, and in a much better fit to observed estimates of ..." The author should provide some quantitative assessment. How good is it compared to other experiments within this study and also compared with other studies?"

IK: I will try to be more specific in the conclusion section of the revised version of this paper.

"Table 3: I don't understand what $r\Theta(\Omega)$ represents. How is a priori range determined? How should I interpret the values? Is it discussed in the text?"

IK: This is the range of parameter values of "very good" individuals, i.e. those model solutions, whose misfit is not worse than 1 permil of the best individual, divided by the "allowed" parameter range. For those parameters with a very wide range (or a priori uncertainty), the chance of having a huge spread in final solutions might just be larger - this is why I showed this normalised value of $R\Theta(\Omega)$. I will try to explain this better in a revised version of the manuscript.

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"Fig. 6: It is interesting that the six panels are all different in terms of the pairs of the parameter values corresponding to the lowest misfit and bias. Does it mean that the optimal values for the two parameters (as presented in Table 3) are not optimal anymore if only subsets of the tracers are used as a constraint or if the bias is used as a cost function?"

IK: Yes: e.g., if we chose only oxygen RMSE for the misfit function, we would probably end up with a different best set of parameters than when only using nitrate.

Technical corrections

"Equation (2): This formulation does not look like the Martin curve. Is the term $z(j+1)$ right?"

IK: No, it wasn't. It should indeed be $z(k+1)$. Thank you for spotting this!

"Equation (11): Please check the last term"

IK: The $1/\bar{o}_j$ was in the wrong place: it should have been after the first sum of the second term. Thank you for drawing my attention to this.

"Page #8, line #24: typo"

IK: Will be corrected.

"Page #12, line #1: There is no such a term like preformed waters. Perhaps change it to "reduces preformed DOP in subducted waters"?"

IK: Yes, thank you!

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