Biogeosciences Discuss.,
doi:10.5194/bg-2017-71-RC1, 2017
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Biogeosciences Discussions

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

Anonymous Referee #1

Received and published: 5 April 2017

General comments

The author used a time-efficient technique to optimize biogeochemical model parameters from two global ocean biogeochemistry models of different complexities. A large uncertainty in climate modeling arises from highly parameterized representations of biogeochemical processes. Constraining uncertain model equations and their parameters has been a great challenge in climate modeling. Previous studies, including the ones led by the author, have addressed the challenge by developing an offline modeling approach in which equilibrium solutions of global ocean biogeochemistry models can be obtained in a time-efficient way. The offline model is combined with a time-efficient optimization technique to constrain biogeochemical model parameters of a NPZD type global ocean biogeochemistry model, which is mainly presented in Kriest et al., 2017. This study uses the same model and technique to constrain some of key parameters

controlling subsurface biogeochemical cycles. The author also takes a step forward by evaluating optimization results of two models with different complexity: a simple two-component model and the NPZD type seven-component model. 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. Overall, the paper is well written, but I feel that the author needs to do more to highlight the novel aspects of this study.

Specific comments

The author chose to optimize 6 parameters from the NPZD type model and 4 parameters from the simpler model. The six parameters from the NPZD type model mostly represent remineralization processes of sinking organic particles, especially in suboxic conditions, whereas the four parameters from the simpler model represent remineralization of both dissolved organic matter and particulate organic matter. The choice of the parameter sets to be optimized vs to be retained from earlier studies seems arbitrary. Is there any particular reason or criteria by which the parameter sets are chosen for the optimization? The transport matrix method combined with the optimization technique (i.e., CMA-ES) seems a powerful way to constrain many uncertain biogeochemical model parameters. 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 lc, KPHY, μzoo, and κzoo would be different with the newly optimized values for b*, KO2, KDIN, etc. This could affect the model evaluations as well.

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?

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?

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?

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?

Technical corrections

Equation (2): This formulation does not look like the Martin curve. Is the term z(j+1) right? Equation (11): Please check the last term Page #8, line #24: typo Page #12, line #1: There is no such a term like preformed waters. Perhaps change it to “reduces preformed DOP in subducted waters”?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-71, 2017.