Ocean Science
Discussions

Interactive comment on "Constraining parameters in state-of-the-art marine pelagic ecosystem models – is it actually feasible with typical observations of standing stocks?" *by* U. Löptien and H. Dietze

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We thank reviewer #2 for his time and effort. #2 has some major concerns, which we address in the following.

A major problem of reviewer #2 is apparently related to language and presentation: Reviewer #2 is concerned that our presentation is too simple (reviewer #2 even uses the word "naive"). On the one hand this is backhanded compliment because we deliberately used simple methods and language in order to address a wider audience. On

the other hand we realize that reviewer #2 misunderstood some of our main points. We will clarify these issues in a revised version of the manuscript (c.f. specific comments).

Another major concern of reviewer #2 is that we do not use "... systematic methods for quantifying parameter dependencies and uncertainty ...". Unfortunately, reviewer #2 is neither specific nor constructive as concerns these systematic methods. Please note, however, that all methods we are aware of have their limitations and that we deliberately decided against applying them (rather than not being aware of them). E.g., a generic method to quantify parameter dependencies is the Hessian matrix (c.f. line 894 in our manuscript). It is a powerful tool but in our specific case it has the following limitations that forestall a reinforcement of our arguments (note that one or more of these limitations apply to all systematic methods we are aware of):

(1) The Hessian matrix is a function of the choice of the underlying parameter set and of the model forcing and boundary condition. Essentially, the Hessian is a local derivative. It is - by definition - not well suited to describe global parameter dependencies and uncertainties in cases of very complex cost topographies. One way to interpret our results is that cost functions become very complex when data is contaminated by red noise. Thus, we find that such traditional methods to systematically quantify parameter dependencies are not well suited for our problem.

(2) We show that higher order dependencies are very important (c.f. chapter 3.3). Higher order dependencies, however, can not be explored with the Hessian matrix and were thus, to our knowledge, not investigated before.

We realize that our decision not to use generic methods to systematically quantify parameter decisions has not been communicated in a comprehensive way. We will add a respective discussion in the revised version of the manuscript.

Specific comments:

- Reviewer #2: Abstract: "The respective MM constants, along with other model parameters, are usually tuned by trial-and-error exercises where the parameters are changed until a "reasonable" similarity with observed standing stocks is achieved." This is a naïve and inaccurate characterization of common practice. Formal parameter optimization is a standard approach in biogeochemical modelling, not trial-and-error.

- A: We apologize, we refer to "coupled ocean-biogeochemical 3D-setups" here. We will state this explicitly in the abstract of the revised version.

- Reviewer #2: (page 233, lines 9 - 20) The attempt to redefine parameters in order to "simplify" and "guide" the optimization algorithm and to make the optimization "computationally more efficient" is complete bogus. As the authors state on line 17- 18, the same number of free parameters is optimized. Hence, nothing is gained by combining the three parameters into the new mu_net (there are still three interrelated parameters being optimized). This is just smoke and mirrors and doesn't serve any purpose other than to potentially confuse the readers. The authors should instead stick to optimizing mu, m and m_PD and be straight about it. The same arguments hold for g_net.

- A: We disagree: Growth and loss of phytoplankton are antagonistically affecting phytoplankton stock. An infinite number of combinations of growth-related and loss-related parameters determines a system in which no phytoplankton will ever emerge. Naturally, we are not interested in this part of the parameter space and, further, we do not want to waste computational resources by exploring this space. One straightforward way to do this (i.e. to stay away from this part of the parameter space which is of no interest to us) is to invent additional rules. In our case the respective rule is the definition of mu_net. We realize that this has not been understood and will add a more comprehensive explanation in the revised version of the manuscript.

- Reviewer #2: On page 234 (lines 9-10) the authors speculate that although their model set-up is not directly comparable to 3D biogeochemical ocean models, their conclusions are so "generic" that they "probably apply even more so" to the realistic

models. This is rather unsatisfactory. The results presented in section 3.1 do most likely not apply to realistic models, and the discussion in the following sections does not provide insights that are new or directly applicable to more realistic models.

- A: We apologize for being imprecise: in line 9-10, page 234 we refer to the results of the sections 3.2 and 3.3. Of course, when repeating experiment EASY (section 3.1) in a coupled ocean-biogeochemical 3D-setup, the retrieval of the genuine truth parameters might not be possible. The major reason is that (depending on resolution) the ocean component will add a certain level of noise (c.f. Dietze et al. 2014, chapter 3.8). We will add a clarifying sentence in the revised version of the manuscript.

- Reviewer #2: The discussion in section 3.2 is very naïve. The issue has been discussed at length and in much more depth already in the parameter optimization literature. The same comment holds for section 3.3. That NPZD model parameters are dependent is well known.

- A: We agree that the argumentation in 3.2 is apparently very simple. But, we think " ... It is hard to overrate the implications " (pg. 242 ln. 1) because our results go beyond the finding that NPZD model parameters are dependent and retrieval is difficult: previous studies based on twin experiments were successful in retrieving the original parameter values (which are associated to the what we dubbed genuine truth) when adding (generally white) noise to the genuine truth (even though there are parameter dependencies). What we report here is that if the observations are contaminated by a more realistic noise (rather red than white) spectrum then this does no longer hold as even very moderate noise levels do prevent the retrieval of the original parameter values. Further, our results show that this is relevant and of concern as, typically, the retrieved parameters imprint a spurious sensitivity into the system. This sensitivity towards changing forcing/boundary conditions is significantly different to the system governed by the original parameters (that are associated to the genuine truth).

- Reviewer #2: Page 239 (lines 5 - 7) "In order to test whether the optimization algo-

rithm got trapped in a local minimum, each experiment comprises an ensemble of five identical parameter optimizations." It doesn't make much sense to repeat the identical optimization 5 times. Instead the initial parameter set should be perturbed in the replicate optimizations. A few lines later it becomes clear that the authors actually did this. Page 241 "If it would, the noise added to our synthetic "observations" would induce a cost of 0.086 mmol N mˆ3" It's not clear at all where this number is coming from.

- A: We apologize that we did not mention the varying initial conditions at the first place. We will add this information in the revised version of the manuscript. Note, however, that this does not make much of a difference as the simulated annealing algorithm contains a stochastic component (c.f. line 13, page 241 and line 5, page 255).

Regarding the second issue on page 241: The cost is obtained by considering the difference between the genuine truth without noise and the genuine truth including noise. We will update the explanation in the revised manuscript as we realize that there is a need for clarification.

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