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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 #3 for the detailed review. All of his comments/suggestions make sense to us. In the following we explain more specifically how we will implement them into the revised version of the manuscript.

General comments:

- R3: I feel the authors may want to rethink their title. Although their analyses have some implications for "state-of-the-art" models, their analyses are actually done with a

simplistic zero-dimensional model with only four state variables. Secondly, the question they pose in their title is not fully addressed in the abstract, which only talks about the MM parameters. Is it feasible to constrain the other parameters? Abstract: The first paragraph of the abstract contains mostly background and can be shortened. I suggest emphasizing the results more in the abstract, and the background material less. Why are the MM parameters difficult to recover? (Prior papers have commented on the difficulty of recovering MM parameters, but one of the novel aspects of this work is that the authors diagnose why this is occurring.)

-A: Thank you for the good suggestions. We will shorten the general aspects and list more of the results in the abstract of the revised manuscript.

- R3: The authors also state that their work is novel because they use red noise. Perhaps that deserves to be mentioned here?

-A: Agreed. We will highlight the importance of the noise structure in the abstract of the revised manuscript.

- R3: Finally, the last sentence says that using more data can degrade fit in some cases. But in other cases it can improve fit, right? It might be less misleading to state: "that more observational data does not always improve the ability :::"

-A: Agreed. We will change that sentence according to the reviewers suggestion.

- R3: In multiple places in the manuscript the authors comment on issues regarding the assimilation of standing stocks versus rates. For example on page 246: "These additional (compensatory) dependencies are the consequence of constraining parameters with standing stocks, as is common practice. Standing stocks alone do not contain information on residence times of the "base currency" nitrogen in the prognostic components". This is actually a hypothesis that is untested and should not be stated as a fact. Personally, I don't believe this is the case, since many other studies have assimilated rate data (most commonly primary production and sediment export) and they

still find such interdependencies. The authors should point out which previous studies have assimilated such rate data (e.g. Friedrichs et al., 2007, Ward et al., 2010, among others)

-A: Changes in the standing stock of a prognostic variable result from changes in the difference between in- and outgoing fluxes. Thus, when changing both (in- and outgoing fluxes) by the same amount, the difference (and with it the standing stock) is not influenced at all. Because biogeochemical models generally contain loops, the cycling speed of nutrients through the system can be faster or slower without impacting the standing stocks. Our statement is thus a direct implication from the model structure and is reflected in the correlations between the estimated parameters in repeated optimizations. We also give an example in Fig.7. As it seems that we did not explain this point clear enough in our manuscript, we will extent the respective explanation in the revised manuscript.

- R3: Finally, the discussion would benefit from added discussion on whether the results found here for this zero-dimensional NPZD model would likely also hold for more realistic models that include depth dependence and/or a more complex food web.

-A: Agreed. By making the model more complex the described problems do not disappear (as long as the described formulations are included), to the contrary, additional problems are prone to emerge. We will add a respective discussion to the revised manuscript.

Specific comments:

- R3: Page 229, line 22: several should be “many” or “multiple”

-A: Agreed.

- R3: Page 229, line 26: “standing stocks” should be “standing stocks and rate data” since many of these studies assimilate primary production, sediment flux, etc :::

-A: Agreed.

- R3: Page 230, line 1: Throughout the paper, “Yongjin and Friedrichs, 2014” should be “Xiao and Friedrichs, 2014”

-A: Agreed. We apologize.

- R3: Page 230, line 3: Unsuccessful is not an appropriate word here, since the studies themselves were successful. Much can be learned from studies in which parameters cannot be constrained, and thus these studies are still “successful”. The word “unsuccessful” should be deleted. It should also be made clearer that in some cases there may be an underdetermination problem, but in other cases the model might be wrong, i.e. inconsistent with the observations. (For example in Friedrichs (2002) assimilation is used to assess the conditions under which a set of observations is consistent with a model structure and when it is not.) Currently the text seems to imply that there are two competing hypotheses: Matear vs. Fasham, however, it is likely that they are both correct.

-A: Agreed. We will be more careful in using the word “unsuccessful” in the revised manuscript.

- R3: Page 230, line 13: “parameter allocation” should be “parameter selection”

-A: Agreed.

- R3: Page 234, line 6: The text after the semi-colon is not a complete sentence. (Perhaps “because” should be deleted?)

-A: Agreed.

- R3: Page 234, line 10: “compose” should be “include”

-A: Agreed.

- R3: Page 234, line 16: Delete “as regards the order of magnitude”

-A: Agreed.

- R3: Page 234, line 17: representative “of” not “for”

-A: Agreed.

- R3: Page 234, line 27: The phrase “among the models” is confusing since only one model, albeit with two different sets of forcing, is being analyzed.

-A: Agreed. It should be “model simulations” not “models”.

- R3: Page 235, line 6: delete “-going”

-A: Agreed.

- R3: Page 235, line 17: presumably this is the case for SENSI as well? Page 237, line 9: presumably Z and D data are available too? Or not? It would be helpful to have this discussed here.

-A: We did various experiments, which are explained in detail in subsection “Parameter retrieval experiments”. Z and D are partly available. We will add a clarifying sentence.

- R3: Page 238, line 21: Delete the last sentence in this paragraph. This may have been true back in the 1990s, but this is no longer true. This is a fine (albeit simplistic) assumption for this paper, but it shouldn’t be justified by saying that this is what is typically done.

-A: OK.

- R3: Page 242, line12: Shouldn’t this be mu_new?

-A; It holds for both mu_new and mu.

- R3: Page 242, line 13: In table 1 this parameter is called max grazing rate but here it’s called the assimilation efficiency. In fact, isn’t this a half-saturation coefficient for grazing, not a max grazing rate or an assimilation efficiency?

A: You are right in that there is some inconsistency due to some former notation. When following Oschlies and Garcon, 1999, our Hz denotes the quotient of max.grazing rate and prey-capture rate. We will clarify the notation.

- R3: Page 242, line 16: The text is confusing here. It sounds as if the authors are saying that the changes in parameter values are as low as the changes in costs, which is clearly not the case. Delete “while at the same time the corresponding costs :::.level.”and replace this with: “whereas costs decrease by only xxx%.”

A: Agreed. Thanks for the suggestion.

- R3: Page 242, line 26: “cost” should be “model-data misfit”

-A: Agreed.

- R3: Page 242, line 27: “even so” should be “even though”

-A: Agreed.

- R3: Page 242, line 28: It’s confusing to talk about models (plural) because only one model is being used in the analysis.

-A: Agreed.

- R3: Page 243, line 11: I don’t find this paragraph (and Figure 5) to be necessary to the main point of the paper and suggest removing these.

-A: We disagree. We think it is important to showcase how two massively wrongs can make one right (to cite Flynn (2005)).

- R3: Page 246, line 3: “pertubed” should be “perturbed”

-A: True.

- R3: Page 246, line 14: I think the authors mean that the results are significant at the 95%level?

-A: True.

- R3: Page 246, line 15: This sentence is unclear. Perhaps “with” should be deleted?

-A: We will reformulate this sentence.

- R3: Page 247, line 10: Please be more specific regarding the time of the information in SPARSE2. It looks like data are predominantly available from Jan-May? (Which is longer than simply “spring”.

-A: Agreed. We will add the respective information in the revised version of the manuscript.

- R3: Page 249, line 16: As above, I would argue that the studies described in this paragraph were very successful, so I would suggest deleting the word “unsuccessful”. In fact this paragraph repeats the information provided in the introduction, so I would suggest removing this paragraph.

-A: Agreed. We will remove the respective paragraph in the revised version of the manuscript.

- R3: Page 250, line 12: It would be helpful to the reader to have this paragraph in the introduction, so s/he understands what the noise is supposed to represent, before reading the results section.

-A: True.

- R3: Page 251, line 1: In fact, Xiao and Friedrichs (2014, JGR) also find these “spurious” minima associated with a cost lower than the cost associated with the genuine truth simulation, without using red noise.

-A: We will check that.

- R3: Page 251 line 4: Note that Xiao and Friedrichs (2014, Biogeosciences; www.biogeosciences.net/11/3015/2014/) found that although simple NPZD models

such as that used in this analysis were not affected by the presence of random noise in assimilated data, more complex models (e.g. 3P2Z models) were sensitive to the level of random noise added to the data prior to assimilation. It should be noted that whether or not noise in observations affects parameter estimation depends not only on the white vs. red noise issue, but also on the complexity of the model being used.

-A: True. It certainly depends on the model structure in so far as only that part of the noise is relevant for parameter estimation, which acts on scales which can be impacted by changing the model parameters. We will add this point to the revised paper.

- R3: Page 253, line 24: Why the Baltic Sea? The paper is generically relevant to all oceans, so this should be changed to something such as “the ocean”.

-A: Agreed. We will change that.

- R3: Page 253, line 24: The authors have not demonstrated that this is due to the MM formulation. (To do this the authors would at a minimum have to demonstrate that this did not occur with another formulation other than MM.) This is a “hypothesis” and should be presented as such.

-A: It seems our formulation is misleading here. We refer to the fact that the MM-parameters have a stronger impact than the parameter dependencies related to the cycling speed of nutrients (and do not talk about other formulations for light limitation). We will make this more clear in the revised manuscript.

- R3: Page 254, line 7: To demonstrate this degradation, the authors would need to demonstrate that the parameters obtained in SPARSE1 are significantly closer to the true parameters than those obtained in SPARSE2. This doesn’t necessarily appear to be the case?

-A: We refer to Section 3.3 and Figure 4 here. Thus, we compare experiment NOISE to MISSING-ZD (and not SPARSE). Our statement is a direct result of the model structure and is confirmed by Figure 4. We will clarify this in the revised version of the

manuscript.

Table and Figure comments:

- R3: Table 2: It would be helpful to explicitly list what data are available (instead of listing what data are not available.) Also which data (Z? D?) are available at BY5? Also does “daily sampling of all prognostic variables except Z and D” mean that the sampling was not daily for Z and D? Or there was no Z and D data at all? Maybe it would be helpful to include another column in which a list of which variables are assimilated (P,Z, N and/or D) are assimilated?

-A: We apologize for the confusion and will revise the table.

- R3: Figure 2: Do the red and green symbols shown here include noise in this figure? The caption implies that the noise is included in the figure, but it the symbols appear to fall exactly on the black line.

-A: There is noise (at a low level) included as indicated by the caption, but we agree that it is hard to identify – particularly for nitrate. We will change the symbol size to make the noise more visible.

- R3: Figure 3: Remove “cost” from title and add “model-data misfit” to y-axis.

-A: We will change the caption of the y-axis.

- R3: Figure 5: See comment on Fig. 3 above.

-A: We will change the caption of the y-axis.

Figure 6: What does agN represent on the y-axis? Can the shading be made darker? It doesn’t appear on my hard copy of the figure.

-A: We will change the shading.

- R3: Figure 7: In (a) it would be helpful to the reader if the absolute magnitudes of the fluxes could be shown, rather than just the % changes. Can all the fluxes be presented,

instead of just three?

-A: We will add the absolute numbers of all fluxes.

- R3: Figure 8: The caption refers to a brown line, but the line looks orange and is very difficult to distinguish from the red line.

-A: We will change the color of the orange/brown line.

Interactive comment on Ocean Sci. Discuss., 12, 227, 2015.