Ocean Sci. Discuss., 12, C63–C69, 2015
www.ocean-sci-discuss.net/12/C63/2015/

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

Anonymous Referee #3

Received and published: 27 March 2015

Review of "Constraining parameters in state-of-the-art marine pelagic ecosystem models – is it actually feasible with typical observations of standing stocks?" by Loptien and Dietze.

In this manuscript the authors apply a parameter optimization method to an N-P-Z-D model, using both twin experiments and assimilating actual data, in order to investigate why certain parameters are better able to be estimated than others, and how noise in the assimilated data impacts parameter recoveries and model-data misfits. I feel this paper should be published in Ocean Science after the following comments are

addressed.

General comments:

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.) The authors also state that their work is novel because they use red noise. Perhaps that deserves to be mentioned here? 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…"

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)

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.

Specific comments:

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

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

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

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.

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

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

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

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

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

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.

Page 235, line 6: delete “-going”

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.

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.

Page 242, line12: Shouldn't this be mu_new?

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?

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%.”

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

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

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

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.

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

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

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

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”.

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.

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.

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.

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.

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”.

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.

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?

Table and Figure comments:

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?

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.

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

Figure 5: See comment on Fig. 3 above.

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.

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?

Figure 8: The caption refers to a brown line, but the line looks orange and is very

difficult to distinguish from the red line.

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