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Biogeosciences Discussions

Interactive comment on "Isotope data improve the predictive capabilities of a marine biogeochemical model" *by* T. Van Engeland et al.

Anonymous Referee #2

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Van Engeland and coauthors describe the procedure that was used to calibrate their NPZD model to fit the data from the EPOCA mesocosm experiments in Svalbard. They describe in a careful and complete manner which parameters can be constrained with which level of uncertainty, estimated with up-to-date methods.

I have a split opinion on this manuscript. On the one hand, I think it is highly desirable that models are not only carefully calibrated but that this calibration is also well documented and available for the community. I acknowledge that this is a lot of work and that the analysis was conducted deliberately. On the other hand, much to my regret, it's not clear to me, what the scientific significance is, in terms of what do we learn from that exercise? It seems to be basically the preparation needed for the de Kluijver et al. manuscript (Biogeosciences Discuss., 9, 8571-8610, 2012), but is that enough for

a full independent journal paper?

General comments:

The relation to the de Kluijver manuscript should be made clear and I wonder why T. van Engeland is not a co-author on the de Kluijver manuscript. I have the impression that he calibrated and prepared the model, by that he contributed significantly to the de Kluijver study and in my view should be a co-author.

The authors claim that it is new that isotope data improve the predictive capabilities of a marine biogeochemical model. It does not seem new or surprising to me that more data better constrain model parameters - given the number of equations doesn't change and the data are independent. The latter point might be of interest here, do the authors want to point out that (changes in) isotope data are independent of (changes in) biomass and concentration data? A thorough discussion and assessment of the independence of isotope data might strengthen the manuscript.

There is a disconnect between title, introduction, results and conclusions. The title highlights the importance of isotope data for model calibration, but does it necessarily have to be isotope data or could it just be any additional data? In the introduction, examples are given where use of isotope data lead to certain conclusions and raises the expectation that some new conclusion would be the outcome of this study. Such a conclusion is missing though, or rather it is given, but it is the conclusion of de Kluijver et al, that there is a CO2-effect on carbon export and grazing. The results part focuses completely on the calibration and parameter estimation, which is very interesting, but disconnected to the title, introduction and conclusion. In the conclusions, the first paragraph ("models help to identify uncertainties in experiments and sampling design") is not connected to the rest of the manuscript. Are there any suggestions the authors have to improve mesocosm experiments? The second paragraph is interesting, yet comes somewhat surprising, more evidence and discussion on the information delivered by isotopes would be needed beforehand. The main conclusion is the citation of de Kluijver, which again raises the question why the two manuscripts are not published as one. The third paragraph is indeed the only part of the conclusions based on the work presented in the results and should be more developed. The fourth and last paragraph is a summary of what the Monte Carlo technique does and is not a conclusion, rather a description of the method.

In summary, I think that the model calibration is well conducted and essential for the further use of the model in de Kluijver et al. However, I miss a message that the authors want to convey. I see different possibilities for the restructuring, it could either be a model skill assessment, but that would need more model description and comparison with other models. Or, if on the usefulness of isotope data, more evidence and discussion on that is needed. In the current version the manuscript seems to be rather a (much needed) supplement to de Kluijver et al. and a restructuring, I regret, would be more than a major revision.

Specific comments / questions:

(1) Page 9456-9457: Examples why isotopes improve model → why is this study still needed? What is new here?

(2) p. 9458, line 10-13. The authors use only the ambient CO2, no nutrient addition ("control state“). I would be interesting to see whether similar parameters would be estimated for higher CO2 etc.

(3) 2.2 Model description: Is this the first time the model is described? What is its history? What is it based on? Give references to put he model assumptions into perspective with other models or observations. A sketch of the model state variables and fluxes would help much to illustrate the model.

(4) Is a model with a fixed C:N ratio adequate to study fluxes in mesocosms with future CO2 levels, which have been previously used to argue for carbon overconsumption (e.g. Riebesell et al., 2007, Enhanced biological carbon consumption in a high CO2

ocean., Nature, 450, 545–548, 2007)? One would think that this misses important information and needs at least explanation if not comparison with the second model in the appendix, Recom with variable stoichiometry.

(5) Line 24: What type of measurement do you refer to when you state that the two phytoplankton groups were distinguished based on experimental observations?

(6) p. 9459, line 1: "the data showed no strong changes...“ over which time period considered and in which mesocosm (only present day CO2 or also other?)?

(7) Line 13ff: “the nature of the increase in phytoplankton isotope signature combined with the increase in phytoplankton carbon indicated that mortality had to be negligible in the first few days“ . So phytoplankton 13C and phytoplankton C increased by the same rate? Specify.

(8) Line 19: DIC is not conserved, because it is implemented as a forcing – how does it evolve over time?

(9) p. 9460: line 4-5: the preference factor is not included in the automatic calibration? How does it compare to literature data?

(10) Line 8-9 Why did you decide to assign the two phytoplankton groups the same half saturation constant? Usually it is assumed that larger cells have larger half saturation constants, e.g. Aumont, O., and L. Bopp (2006), Globalizing results from ocean in situ iron fertilization studies, Global Biogeochem. Cycles, 20, GB2017, doi:10.1029/2005GB002591.

(11) Line 18: At what depth are the sediment traps? What do you mean by "because the zooplankton actively migrated...“? How does that relate to the preceding sentence? If zooplankton actively migrates into the traps, for me that would then be C_zoo and not C_det.

(12) p. 9461, line 3-7: Regarding the supplement, there is little description of what of this EPOCA folder is used for this manuscript. A readme file would be of great help.

For example, the RECOM model, which is also in the supplement, is not used for this manuscript, is it?

(13) p. 9463, line 12-15: It is not clear to me, whether the model equations are different between your “first phase” and “second phase”. Do you have the same number of equations and parameters whether or not isotopes are included (I assume so, at least that was mentioned in the introduction as the advantage of using isotopes, but should be made clear)?

(14) Line 21-22: “a limited number”, please specify how many model runs per parameter and the ranges for the parameters including references for the “plausible range”.

(15) p. 9464, line 6-8: why are model sensitivities and multicollinearity indices not given in a table for the different parameters? All the paper is on the calibration procedure, so this is of interest here.

(16) p. 9464 line 9-24: These automatic calibration methods are probably not known to most readers of BG, so they should be explained in some more detail here.

(17) p. 9465 line 22-23: Which criteria were used to choose the 7 parameters?

(18) Line 24-25: “even this simple model became quickly overparameterized”. What criterion is that based on?

(19) p. 9466, line 5-6: “Only a relative weak correlation...” The correlation is 0.49, right? That is probably acceptable, but I would reformulate the sentence to say that this is the highest correlation between all parameter pairs considered and possibly explain why you think it’s acceptable or what the trade-off would be if you would not accept it.

(20) From your analysis, can you give recommendations, which parameters are most important to constrain better from measurements, which ones can confidentially be a priori fixed because they are relatively well known etc?

(21) Line 21-22: It would help the reader to mention again which isotopes were measured in which compartments.

(22) p. 9468, line 3-5: If both calibration methods provide good fits, then why does it matter for your purpose whether or not isotope data are included?

(23) Line 13-15: Why is d13C_zoo not used for the calibration? This is in contrast to Table 1 and p. 9461, section 2.3, which imply that all d13C and biomasses/concentration data were used for calibration.

(24) Line 22-23: What is the difference in the carbon export rate based on your analysis? How much does it “improve” by adding isotopes to the calibration data set? How big is the uncertainty, how does this compare to literature and previous studies?

(25) p. 9468, line 28: it is not clear how the sensitivities are calculated here.

Interactive comment on Biogeosciences Discuss., 9, 9453, 2012.