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Discussions

Interactive comment on "A data-model synthesis to explain variability in calcification observed during a CO_2 perturbation mesocosm experiment" *by* Shubham Krishna and Markus Schartau

Anonymous Referee #1

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The manuscript submitted by Krishna and Schartau is targeted towards the use of mathematical modelling in order to understand the variability of biogeochemical data sets collected during 9 mesocosm experiments. The approach combined data analysis, model development and data assimilation and this combination of tools is a powerful way to gain a better understanding of an interdisciplinary data set. The idea is to gain a better understanding of the factors that may explain the variability in TA dynamics (and hence calcification) between the different mesocosms of a similar treatment. This is a really important topic since the impact of OA on calcification is still under debate. The current work shows that variability in TA could be explained by variations in initial conditions and the photo-acclimation states that prevailed within the phytoplankton

community during the filling of the mesocosm. The authors are rigorous in their approach and have the willingness to describe their model application extensively. This makes that in its present form, the manuscript is not really accessible to readers that are not expert in modeling. The methodology and the analysis of model results are described in a very detailed manner (see my comments below) and this sometimes prevents capturing the forest from the trees. The results section is, in some places, a succession of facts that are not enough integrated and may loss the reader. This is sometimes difficult to understand where the authors want to go and what is the additional information brought by the analysis of a particular fact (I had often to read by several time some paragraphs in order to capture the general idea). I would encourage the authors to strengthen the main messages of their study and their biogeochemical meaning. I would like them to clarify what is the added message compared to Eggers et al., (2014) who already stressed that variations in initial plankton composition can be responsible for large differences in the responses observed. Probably the new finding is that the photosynthesis efficiency of the community is (may) also important. However, as the authors correctly pointed out parameters may be collinear and this is not sure if a variation of less than 20 % of the photosynthesis efficiency as found by the authors (page 21, line 4) is really significant and does not compensate for a change in another parameter (to which alpha is co-linearly linked) that is not included in the 7 selected for the DA experiments. Besides, I would like to see the authors explain how their results are sensitive to the choice of the 7 parameters on which they decided to spend estimation effort. These parameters are selected without any clear justification (see my points below).

At the end we are expecting that the authors conclude on how their investigations bring an information on the potential impact of OA on calcification but this is missing.

Abstract: I suggest to improve the abstract. I find that lines 1-8 would be better placed in an introduction. Some parts of the remaining of the abstract is not really accessible to a non-specialized reader in DA.

Abstract line 13: “We explore how much of the observed variability in data can be explained by variations of initial conditions and by the effect of CO2 perturbations.” I agree that this is exactly an important possible output of this type of study but unfortunately it is not enhanced enough in the manuscript. I would like to see a dedicated section/paragraph on that. (I suppose that by CO2 perturbation the authors are referring to OA?)

A table with the list of observations would be helpful and how it relates to the state variables

Page 4, line 30: ..”The first is that we distinguish between bulk phytoplankton biomass and the presence of calcifying algae, coccolithophores like E. huxleyi”. . .It is not clear why this is an additional feature compared to what is mentioned before.

Page 12: line 9 : Âń we assume a higher C :N ratio (=2*6.625) only for initial detritus Âż, please add a justification.

Page 12: What are the “three distinct patterns in calcification”? I would not use attributable but observed. What do you mean by ‘no such clear pattern”

Page 21: what do you mean by “adapting the same nomenclature” do you mean the same definition of partitioning of mescososms among different TA levels? It would be helpful to have some information on the general principles according to which this classification has been done. A significant part of the manuscript is based on the division of the mesocosm experiments in three main calcification levels and it would be appreciated that further justification is given as concern the statistical significance of the differences of the TA change between these three groups of mesocoms. (this has probably be done in other studies but some minimum justification would be appreciated).

Lines 13-16 would be better placed in the analysis of the mesocosm results and not in the design of the DA experiments. This paragraph is really not clear. Reading lines

16-20 does not help me to understand how data assimilation will be used in order to investigate the variability of TA. It seems that you will group the mesocoms according to their level of variation of TA and then? Please explain the general idea already here (I agree that it is somehow clarified afterwards).

In general, the section on DA needs to be reformulated. As it is now it is excessively complicated to understand why is data assimilation exactly used and what it will bring as a new information. The authors have to make that clear and to rewrite the technical description in order to target it to the audience of Biogeoscience which is not necessarily expert in technics like DA (you may also consider to put some materials in the appendix). It will also be very helpful to see further justifications for the choice of the 7 variables/parameters that are submitted to estimation (using DA). Considering the objective of the manuscript, this is surprising that parameters linked to calcification are not selected (e.g. fPIC, fPOC). Moreover, during the modeling experiments the authors realized that other parameters are important like C:Nfact , Chla:N but they are not added to the list.

Page 12, section 2.3.1: this section needs to be rewritten this is not understandable. line 26, observational residual errors, what is the cost function, R is not defined, ..

Why pCO2 and TEPC are not used in the observation vector?

Page 14, line 1: how do you estimate the daily residual standard errors?

Page 17, line 1: please give argument why this CN factor was not submitted to calibration since it seems that it is a very critical parameter. I find critical that as shown by Figure 8-10, model performances are not optimal for certain variables like chla, PIC, POC, PON, DIC after the bloom, it means exactly when we have variability of TA. This would require further justification by the authors for considering the model for assessing the TA dynamics during that period.

Page 17, line 23: How is estimated the standard error? Please specify (R terms ?)

Page 17, line 27: “First of all, from these flux estimates we learn that the CO2 effect introduced to the model, following Findlay et al. (2011), induces deviations in C flux that are much smaller than the variational range in model results, as reflected by the respective standard errors”. This sentence is very difficult to understand. Please specify which CO2 effect you are referring? Which variation in C flux?

Page 18, line 33: “Carbon flux estimates show, carbon fixation in mesocosm with high CO2 treatment is slightly higher than in the mesocosm with low CO2 treatment”. This difference is not significant if we consider the model error.

Page 19, line 4: “Our results show, regardless of biomass, coccolithophores are always less vulnerable to grazing than bulk phytoplankton”. How does this fact result from model parameterization? The absence of data on zooplankton prevent a validation of this compartment and renders difficult the draw conclusion on the grazing.

Page 20, lines 21-24: “These considerations were disregarded when we designed this study and we originally thought of the importance of the relative mass distributions between the state variables resolved by our model, while imposing fixed initial stoichiometric ratios (C:N and Chla:N). It seems plausible to allow for some variations of the initial stoichiometric ratios as well.” Do you mean that if you had to rebuild the model experiment you would change the list of 7 parameters?

Page 21, line 16: “Model biases and compensating effects are typically seen when applying DA methods (Bertino et al., 2003; Gregg, 2008)”. This sentence is not clear. If it is necessary for the understanding of the rest of the paragraph, please clarify how DA can typically induce model bias and what are the compensating effects.

Figure 16: is the model able to differentiate the 3 groups of mesocosms (LC, MC, HC)? It seems that it overestimates calcification in the LC and underestimates it in the HC?

Minor comments

This is not clear why the salinity is decreasing during the course of the experiments. Is

it due to rainfall?

PAR is measured at the surface, which value of PAR is used to force the model

Page 5, line 21: the model maximizes

Page 5, line 27: “QN from”, remove the “from”

Page 6: an algal

Page 6, line please define theta

Page 14, Line 12: better to use starting from published typical values rather than “while considering”

Page 16, line 24: please specify what do you mean by delta C? This is not defined (C-assimilation?)

Figure 5: please add TEPC and dCCHO.

Figure 7: please correct the ordinate: “calicification:c-assimilation” and not “gross carbon fixation-respiration”.

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