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Biogeosciences 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

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We thank anonymous Referee #1 for detailed and useful comments. Many of the referee's suggestions will be considered. Below, we provide answers to all questions. With our responses we hope to sort out all ambiguities. Suggestions are made with respect to rewriting some paragraphs and restructuring the manuscript, e.g. by moving parts of the model description to the Appendix. Most of the proposed changes are currently applied to a revised manuscript version, mainly to ascertain feasibility.

General comments by anonymous Referee #1:

Comment 1: 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.
Author's response: We understand the referee's concern and we think that the model description can be shortened. However, details of our model approach need to be accessible. We prefer to leave the general description of the processes resolved by the model in the main body of the text. The detailed dynamical model equations (mathematical description of all source minus sink terms) can be moved to the Appendix with little restructuring. This way we are able to shorten text in the subsection "Modelling approach" considerably. For example, the entire paragraph on optimal resource allocation (e.g. between light harvesting complex and sites of nutrient acquisition) may then appear in the Appendix.

Comment 2: 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.
Author's response: We realised that it would not be critical to condense and remove content (text and figures) from the results section. As proposed by Referee #2, we will merge Figs. (3) and (4). We admit that an explicit presentation of simulation results of transparent exopolymer particles (TEP) is not really relevant for our study and Fig. (6) can be removed. Furthermore, the entire discussion on possible differences between specific rate constants of carbon and nitrogen exudation is not needed to interpret the major outcome of the study. In fact, we learned that the consideration of a parameter CN_{fact} that describes such possible difference is not fully consistent with the imposed assumptions of optimal resource allocation in our model. The value of CN_{fact} has to be equal to one, as assumed in our model approach in the first place. Thus, a parameter like CN_{fact} is negligible in our case and it will be removed from our model and from this study.

Figure (9) (ensemble of medium calcification solutions) does not provide much additional information, given that Figs. (8) and (10) are already shown. We decided to remove Fig. (9), which was also recommended by Referee #2. Figure (15) was introduced to clarify that model results of the medium calcification solutions are slightly biased, with a build-up of coccolithophore biomass that is too fast. This finding can be briefly mentioned in the text and need not be further documented with an additional figure. We therefore decided to omit Fig. (15) as well.

The removal of Figs. (6, 9, 15), the combination of Figs. (3 and 4), makes the study more concise.

Comment 3: 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.
Author's response: We will consider this aspect and will revise the text. Briefly, our results not only support the findings of Eggers et al. (2014), they provide additional insight to the problem of resolving a CO_2 response in the presence of variability in measurements. With our analysis we make inference about the linkage between phytoplankton growth dynamics, calcification, variability in observations and uncertainties in model results. We determine the conditional probability of how well the experimental data can be represented by our dynamical model approach. The analysis of an ensemble of statistically equivalent model solutions (according to maximum likelihood estimates) differs from a statistical treatment and analysis of the experimental data, e.g. as described in Eggers et al. (2014). Our study includes a mechanistic description of algal growth, thereby resolving nonlinearities with respect to carbon and nitrogen acquisition and chlorophyll *a* synthesis. Based on our data assimilation approach we obtain maximum likelihood estimates of important model parameters. These estimates

determine model solutions of carbon and nitrogen flux for all nine mesocosms. The model resolves temporal variations of specific variables that are not resolved by the available measurements (e.g. diurnal variations).

One added message is that our mass flux estimates are shown to differ more between the different calcification solutions than between the different CO_2 treatments. This situation exemplifies that simulation results (e.g. future model projections) may involve uncertainties in flux estimates that are larger than the CO_2 effect introduced to the model (e.g. by following Findlay et al., 2011). Another added message is that initial conditions may not be independently estimated from estimates of phytoplankton growth parameters, like α_{phy} and α_{cocco}. This is particularly relevant for model assessment and model analyses of mesocosm experiments.

Some other message is that the original design of the experiment was meaningful, in particular with respect to the initial filling of the mesocosms. The retrospective separation the CO_2 response signal from the system's variability was only possible because mesocosms with similar initial conditions were subject to different CO_2 concentrations. Such separation would be more difficult in retrospective if mesocosms with similar initial conditions would have been (by chance) exposed to similar CO_2 levels.

To facilitate data analysis of a mesocosm experiment it is meaningful to 1) analyse details of initial conditions (e.g. f_{cocco} and f_{zoo}) prior to perturbation (assuring that similar replicate mesocosms do not get exposed to identical CO_2 levels), 2) perform side experiments that can reveal the photosynthetic efficiency during the exponential growth phase.

We will revise parts of the discussion and conclusion section in order to straighten the above points.

Comment 4: 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.
Author's response: A discussion on possible collinearities of α_{cocco} with some other fixed parameter is not helpful here. However, the referee has raised some important point that needs attention and should be better described in the text. The estimates of α_{cocco} are negatively correlated with the estimates of f_{cocco} and with α_{phy} (see Table 4). With respect to the initial abundance of coccolithophores we may add few sentences for clarification. We suggest to depict relevant combinations of both parameters α_{cocco} and f_{cocco} to explain the major differences between the low and high calcification solution. The revised text (highlighted in red) would then read as follows:

"Major differences between the LC and HC solutions can thus be attributed to higher α_{cocco} values (median α_{cocco} = 1.7 mol C (g Chl *a*)$^{-1}$ m^2 W^{-1} d^{-1}) in HC posterior distribution compared to LC (median α_{cocco} = 1.4 mol C (g Chl *a*)$^{-1}$ m^2 W^{-1} d^{-1}). However, the estimates of α_{cocco} are negatively correlated with the estimates of f_{cocco} (Table 4) and we may therefore look on the combination of the two parameters. To do so we compare two extreme solutions, selected from the ensemble solutions of LC and HC respectively. One extreme solution yields the lowest calcification among all HC solutions, based on the parameter combination (α_{cocco} = 1.84 mol C (g Chl *a*)$^{-1}$ m^2 W^{-1} d^{-1} , f_{cocco} = 0.34). The other selected solution represents the highest calcification of all LC solutions, which corresponds with (α_{cocco} = 1.59 mol C (g Chl *a*)$^{-1}$ m^2 W^{-1} d^{-1} , f_{cocco} = 0.35). Thus, it is mainly the photosyn-

thetic efficiency α_{cocco} to which the model solution is highly sensitive to. Hence, a difference of $\approx$ 0.3 mol C (g Chl *a*)$^{-1}$ m^2 W^{-1} d^{-1} can effectively determine the differences in our simulations with respect to rates of carbon fixation and calcification. "

Comment 5: 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.
Author's response: A full description of the parameter selection process would go beyond the scope of our study presented here. Furthermore, there is no purely objective reasoning, but the decision is based on results from preparatory analyses. Although we are obliged to reduce content of the method section, we think it is worthwhile adding few sentences in this respect:

"The decision on which parameters should become subject to optimisation is based on a series of preceding parameter optimisations and subsequent sensitivity analyses. A major objective is to reduce the number of parameters for optimisation to a meaningful minimum. This facilitates the identification of those parameter values that are of primary concern. Since we address differences in initial conditions in our study, we consider four parameters that determine these differences and they need to become subject to optimisation. The additionally selected three growth parameters are amongst those to which the model solution is most sensitive. The model solutions are also highly sensitive to variations of the maximum potential nitrogen uptake rate (V_0^m). This parameter is excluded from optimisation, because it is not possible to obtain estimates of (V_0^m) that are independent of estimates of the photosynthetic efficiency. Therefore, a value is assigned to $^m_0$ that is typical and was used for simulations of other experiments (e.g. Pahlow et al., 2013), ensuring credible estimates of those parameters that are optimised in our study. The mesocosm experiment covers only a short post-bloom period and we found other parameters, like maximum grazing

rates and the aggregation parameters, to be weakly constrained by the available data. Their consideration for optimisation would impede the identification of the other more important parameters. Values assigned to those parameters that are excluded from optimisation are adapted from other studies (e.g. Pahlow et al., 2013; Schartau et al., 2007). "

Comment 6: 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.
Author's response: We will introduce an extra paragraph to the conclusion section, further emphasising the implications of our results (see above response to Comment 3). We will also include some suggestions for setup of future mesosocosm studies on ocean acidification. The additional paragraph (shown in red) in the end of conclusion section reads:

" Overall, the results of our data-model synthesis show that the initial relative abundance of coccolithophores and the prevailing physiological acclimation states drive the bloom development and determine the amount of calcification in the mesocosms. Small variations of these two initial factors between the mesocosms can generate differences in calcification that are larger than the change in calcification induced by OA. In spite of this difficulty, a CO_2 response signal may still be identifiable, as long as mesocosms that reveal strongest similarities (with respect to initial composition of plankton and their physiological state) are not used as replicates for similar CO_2 conditions. Instead, mesocosms with similar initial conditions should be exposed to different levels of OA. Such favourable starting conditions were met in the mesocosm experiment described in Engel et al. (2005) and Delille et al. (2005). Thus, the decision upon which mesocosms to choose as replicates for same levels of perturbation should be made after an assessment of similarities of the initial conditions between mesocoms. "

Specific comments by anonymous Referee # 1:

Specific comment 1: 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.
Author's response: We will consider this suggestion.

Specific comment 2: 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?)
Author's response: Various aspects of "variability" are explicitly addressed in the manuscript: Sect. (3.2) Data-model comparison, Sect. (4.1) Uncertainty ranges in parameter estimates and variability in model solutions, Sect. (4.3) Disentangling CO_2 effect from the observed variability in PIC.

Ocean acidification (OA) is a wide and general term. In the sentence we refer specifically to a mesocosm CO_2 perturbation experiment. We do not find it appropriate tto change the term "CO_2 perturbation" to OA.

Specific comment 3: A table with the list of observations would be helpful and how it relates to the state variables.
Author's response: A list of observations and how it relates to the state variables is depicted on page 13, see Eq. (25). Abbreviations and units are described in the corresponding text. Note that Sect. (2.3.1) will be restructured, see responses to

specific comments 11 and 15.

Specific comment 4: 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.
Author's response: Yes, we agree. This is redundant information and we will therefore delete the sentence.

Specific comment 5: Page 12, line 9: "We assume a higher C :N ratio (=2*6.625) only for initial detritus". Please add a justification.
Author's response: Since the mesocosms were filled with post-bloom, nutrient depleted water masses, we assume that all dead particulate organic matter has a C:N ratio that is rather typical for such post-bloom conditions.

Specific comment 6: What are the "three distinct patterns in calcification"? I would not use attributable but observed. What do you mean by "no such clear pattern".
Author's response: For clarification we will include a new figure (uploaded as supplement material of our response). The left panel in the figure shows three distinct calcification patterns, reflected in total alkalinity (TA) data. Those mesocosms that exhibit high TA values (a reduced drawdown during the bloom and post-bloom period) feature rates of low calcification (LC, in blue color). Mesocosms with low TA values (a strong reduction of TA) reveal rates of high calcification (HC, marked red). Rates of medium calcification (MC) are assigned to the remaining mesocosms (with intermediate TA values, marked black). The right panel shows the respective different CO_2 treatments in the same colors as for LC, MC, and HC. The figure shows that each calcification case (LC, MC, and HC) includes mesocosm of all three CO_2 treatments.

Specific comment 7: Page 12, What do you mean by "adapting the same nomenclature", do you mean the same definition of partitioning of mescosms among different TA levels? It would be helpful to have some information on the general principles according to which this classification has been done.
Author's response: By "adapting the same nomenclature" we simply mean the names given to respective mesocosms are the identical with the names in Engel et al., 2005 and Delille et al., 2005. For example, mesocosm one is referred as M1, mesocosm two as M2, so on and so forth.

Specific comment 8: Page 12, 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).
Author's response: Statistical significance of the differences in TA changes was not tested in Delille et al. (2005). Both studies (Delille et al., 2005, and Engel et al., 2005) rather focused on statistical significance between the CO_2 treatments. The mesocosms were pooled according to the CO_2 levels, thereby including all variability in calcification.

If the differences in TA between the LC, MC, and HC pooled mesocosms were insignificant we would see this in our maximum likelihood estimates of the model parameters as well. This is because we consider mean data, standard errors, and the correlations between the different observational types (of the pooled mesocosms) in the cost function.

During the post-bloom period, the mesocosms pooled in HC reveal TA changes that are consistently higher than in the LC mesocosms. In fact, these differences

become well reflected in our parameter estimates. Thus, our optimised ensemble model solutions are providing the statistical evidence that HC and LC are significantly different.

With respect to the mesocosms assigned to the MC (medium calcification) case we see in our parameter estimates and ensemble model solutions that they are rather close to conditions also met by the HC mesocosms. In this case the differences in parameter estimates (between MC and HC) are small, although we find significantly different estimates for α_{cocco} and for f_{zoo} between MC and HC (see Figs. 3 and 4). Thus, we may have one or two out of the three MC mesocosms that might have been better assigned to the HC case. However, this is reflected in our DA results and we are primary concerned with the upper and lower extremes in calcification, as resolved by the six mesocosms in the LC and HC cases.

Specific comment 9: Page 12, 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.
Author's response: It is not meaningful to move this to the results section. Since this seems to be the core of confusion we suggest introducing a figure that documents the differences between LC, MC, and HC mesocosms. We will revise the text on page 12 accordingly. The following points will be further clarified: 1) the selection of mesocosms assigned to LC, MC and HC is entirely based on the observational data, 2) we simulate all (three) mesocosms of each case LC, MC, HC, 3) in the cost function we compare the daily means of model results (of those mesocosms of the respective calcification case) with the observed means (of the same mesocosms), 4) we thus obtain parameter estimates for the cases LC, MC, and HC.

Specific comment 10: Page 12, 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).
Author's response: We think we have addressed this in our responses to the specific comments 6), 8) and 9).

Specific comment 11: 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).
Author's response: The subsection on data assimilation is an essential part of the study and it describes important aspects. We again critically reviewed the subsection and we find the description appropriate with respect to accessibility, style and content.

Equations (24, 25 and 26) provide important information, since definitions of the cost function are the major integral part have been different studies, which has consequences for parameter estimates. However, we will restructure parts of Sect. (2.3.1), see response to specific comment 15 below.

Some of the complexity of the actual optimisation procedure is reflected in the subsection "Parameter optimisation procedure". As this might only be of interest for those readers who are involved in applying similar approaches, we suggest moving this subsection (Sec. 2.3.2) to the Appendix entirely.

Specific comment 12: 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).

Author's response: This comment conforms to the general comment 5) above. You may refer to our answer above, the response provided to the respective comment.

Specific comment 13: Considering the objective of the manuscript, this is surprising that parameters linked to calcification are not selected (e.g. fPIC, fPOC).
Author's response: f_{PIC} is an auxiliary variable (see Table 1) and not a parameter. To our model we have not introduced a parameter referred to as f_{POC}.

Specific comment 14: Moreover, during the modeling experiments the authors realized that other parameters are important like CNfact , Chla:N but they are not added to the list.
Author's response: CN_{fact} was included in Table (1). Some confusion might have been caused by a small typo that we found in the text: the parameter name is CN_{fact} not C_{fact}. This parameter will be removed from the study and its discussion is not meaningful, as pointed out before in the response to general Comment 2.

θ^N (Chl*a*:N ratio) is an auxiliary variable (see Table 1). It expresses the photoacclimation state. It varies with time and we therefore do not need to estimate a constant value for it.

Specific comment 15: 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.
Author's response: We will revise parts of Sect. (2.3.1). Some restructuring should make it easier to understand. The observation vector $\vec{y}_i$ can be introduced first together with its model counterpart ($H_i(x)$. This will be followed by the equation that represents the residuals between data and model results (currently given as Eq.

25). We may then explain the derivation of the cost function $J(\Theta)$ (Eqs. 23 and 24). Thereafter, the calculation of the covariances (R) will be explained, with reference to details given in the Appendix.

Specific comment 16: Why pCO_2 and TEPC are not used in the observation vector?
Author's response: We have included dissolved inorganic carbon (DIC) in the observation vector together with total alkalinity, which accounts for similar information as pCO_2. Thus, no additional independent information would be introduced to the cost function if pCO_2 data were added.

With regard to TEP, we found few discrepancies between TEP data depicted in Joassin et al. (2011) and Engel et al. (2005), and the measurements available from PANGAEA data library. We could not resolve this problem and have therefore decided not to assimilate TEPC (Alcian blue concentrations converted to carbon units) into our model but to compare the typical concentration range only.

Specific comment 17: Page 14, line 1: how do you estimate the daily residual standard errors?
Author's response: The term "residual" will be removed, as it can be confused with the residuals between the data and the model results. For every calcification case we calculated daily standard error: standard deviation (σ_{std}) divided by the square root of the number of samples (mesocosms) available on that particular day (n).

Specific comment 18: 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.
Author's response: The model parameter CN_{fact} will be removed from the study and its discussion is not so meaningful, as described before in the response to general comment 2. Our model represents basic biogeochemical and eco-physiological processes related to plankton dynamics in a mesocosm experiment. We show in our results (Fig. 5) that the model does reproduce most of the data. Given the complex dynamics involved, the model performance is very good. Due to the simplifications introduced, any model will remain imperfect and will be limited in resolving the complexity of a plankton ecosystem. In Sect. (4.2) we have explicitly stressed systematic model deficiencies and discuss these biases.

Specific comment 19: Page 12, line 27: How is estimated the standard error? Please specify (R terms?)
Author's response: How standard errors are calculated is described in the response to Comment 17, above. The correlation matrices for the exponential growth phase and post-bloom period are given in the Appendix, see Eqs. (B1 and B2).

Specific comment 20: 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?
Author's response: The regression model of Findlay et al., (2011) is implemented in our model to quantify the effect of different CO_2 perturbation on PIC formation, which we refer as CO_2 effect (as given in Eq. A21). We show in our study (Figs. 11 and 12) that simulated carbon and nitrogen mass flux estimates differ more between the mesocosms with different calcification rates than between the mesocosms with the

different CO_2 treatments.. However, we do agree with the referee that the line, "as reflected by the respective standard errors ",might be misleading. Therefore, we will remove the same line..

Specific comment 21: Page 17, 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.
We fully agree with the referee's comment. Therefore, we will remove the above quoted sentence from our manuscript.

Specific comment 22: 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.
Author's response: We agree with the referee's comment. No solid conclusions can be drawn with respect to differences in grazing. This is because we have no information about the grazing rates. The sentence expresses model behaviour. To avoid any misunderstanding we suggest rephrasing the sentence:

" According to our model solutions, the coccolithophores are always less vulnerable to grazing than the bulk phytoplankton. This model behaviour may not be representative or conclusive, because we have no information about the actual grazing rates or about grazing preferences. "

Specific comment 23: 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?
Author's response: Correct, now that we have indications of the initial acclimation state being important, we would set up initial C:N and Chl*a*:N ratios as additional parameters for optimisation. Thus, the number of parameters would increase from 7 to 9.

Specific comment 24: 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.
Author's response: The sentence should be revised:

" Model biases disclose systematic deviations of simulation results from observations, which may point towards i) erroneous model counterparts to observations (definition of $H(\vec{x})$ in Eq. 25) or ii) deficiencies in model dynamics (errors in $\vec{x}$). A noticeable bias is related to the increase in PON concentration during the late phase of exponential growth (between days 10 and 12). This offset... "

Specific comment 25: 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?
Author's response: As described in response to specific comment 8, we see differences in the maximum likelihood estimates of the model parameters for the mesocosms of LC, MC and HC. Further, we show in Figs. (8) and (10) of our

manuscript that the model successfully reproduces high and low observed PIC values. However, the referee rightly pointed out that the model overestimates calcification in LC case (especially for the mesocosm with high CO_2 treatment). This issue has been addressed in the Sect. (4.3) of our manuscript. In the HC case, the observed PIC values are well in range of high calcification model solutions (ensemble spread).

Minor comments by anonymous Referee # 1:

Minor comment 1: This is not clear why the salinity is decreasing during the course of the experiments. Is it due to rainfall?
Author's response: We have not cross-checked for why there is a gradual decrease in salinity. We did not find any explanation in the respective publications, neither in Delille et al. (2005) nor in Engel et al. (2005). A freshwater influx by rain is a possible explanation, since the decrease is a signal that is consistent among all mesocosms.

All other minor comments and corrections suggested by anonymous Referee #1 will be implemented.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-405/bg-2016-405-AC1-supplement.pdf

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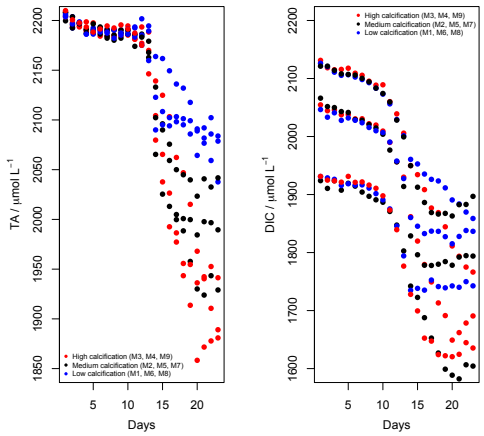

Figure 1. DIC (dissolved inorganic carbon) and TA (total alkalinity) observations. Red solid circles represent data from mesocosms (M3, M4, M9) with high calcification rates. Black solid circles depict observations of mesocosms from medium calcification case (M2, M5, M7) and blue circles show data from mesocosms (M1, M6, M8) with low calcification rates.

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Fig. 1.