

Interactive comment on “Potential sources of variability in ocean acidification mesocosm experiments” by Maria Moreno de Castro et al.

Anonymous Referee #1

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This is an interesting study which uses a modelling approach to (potentially) explore sources of the variability observed in mesocosm studies. Specifically, in two such studies which have investigated the impacts of ocean acidification on phytoplankton communities, and associated production of particulate organic matter (POM); specifically, particulate organic carbon (POC). I think the study is very interesting model perturbation exercise, in that it explores how specific parts of the model impact on variability in the production of POC, which could be potentially useful to the climate change and ocean acidification community. However, I have important concerns about how this is sold throughout, and believe that the inferences made about how these results may help explain variability observed in mesocosm experiments are not entirely supported by the study design used. Specifically, because of the procedures used to produce the simulations, as detailed below. I therefore find it difficult to accept this study for

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publication in its present form and would suggest that to that aim, major revisions are needed addressing the primary point raised below. I am uncomfortable with the assertion that these model simulations produced in this study can be used to define which uncertainties lead to observed variability in experimental results (L4-6, page 1). In your model, a number of mechanisms is represented, with associated parameter spaces investigated here. The optimization (as far as I understand it) of this parameter set is based on the POC observations in the experiments, and given (as far as I can gather) that POC is an emergent property of your model, your modelled POC can therefore not be used to infer causality between these mechanisms in the model and POC in the experiments. Indeed, in page 4, L29-32, it seems you optimize the parameter values by minimizing a model costs based on an emergent property (POC) (i.e. the relationship between POC_{exp} and POC_{mod}), not your state variables. Is this the case? If so, you cannot use this study to infer about what is observed in POC in the experiments, because you cannot trace how likely your parameter values used, or the simulations of your state variables are. You do not use any observational datasets to validate the state variables simulations associated with the mechanisms represented, or the parameter values chosen here (adjusted?)? Through the various optimizations of parameter values, it is therefore possible that random combinations of parameters can be achieved to produce the observed variability in POC, or close to it, but this does not mean that this is what happens in reality. I believe you address this partially at the end of the methods section (L24-27, page 5), but isn't this the rationale of your study? If so, then your study is not what is advertised in the title and abstract. What you do here is explore how a set of mechanisms represented in your model may produce a progression of POC values. It is a model perturbation exercise. But you have tuned the parameters through the simulation so that your result is close to the observations – is this true? You further tune POC_{mod} in a specific way (using an assumed relationship) because you did not include other elements of the ecosystem contributing to POC in the experiments (Appendix D, page 12). So you cannot then infer that mechanisms in your model are those affecting POC in the mesocosm experiments. This is particularly im-

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portant given that you assume that increased DIC increases primary production (L27 onwards, page 3) but this is not a ubiquitous perspective in the community (e.g. Artioli et al 2014 Biogeosciences Discussions 11: 601-612; Nagelkerken & Connel 2015 PNAS 112: 13272–13277). To do so, to investigate this causality between the mechanisms in your model and POC in the experiments, I suppose you could produce POC simulations with different parameter values without tuning them. Or you could use experimentally derived parameter values for those changing conditions and see the effect on modelled POC, in different scenarios. In these two cases you could then investigate how changes to those parameters, and the mechanisms in your model that they affect, impact on the outcome (POC). To accept this work for publication, I assume that the authors would have to explain why they disagree with my reading of their work, or they would have to requalify the assertions made throughout about the implications of their findings along these lines. Detailed comments: Text could benefit from being edited by a native English speaker, and I have not undertaken those corrections. Introduction Line 18 page 1: particular organic matter = POM; particulate organic carbon = POC. Please define all acronyms on first use, throughout. L17-20, page 2: Sections unclear. Please rephrase without jargon. L24: “alternative” is not the best word here. “Complimentary” would be more adequate. L24 page 1: sensitivity (analysis), not robustness. Method Line 18 onwards, page 3: please define all acronyms used at first mention, here and in tables. L17-18, page 4: does “adjusted” describe tuning? Are there no parameters estimated from the experiments that may be used to validate/address the set of parameter values used in the various simulations? I.e. are all parameters effectively tuned to acquire the best agreement between simulated POC and observed POC? Page 4: reference is made to the use of normal distribution to produce variation in model factors. I suppose you mean in the model parameters but not in the state variables (in page 3, L10 you say model factors are the initial parameter values and the initial conditions of the state variables). Did you constrain the normal distributions sampled in order to limit the parameter values to positive values? If so, please state this and explain how you constrained the sampled distributions used for delta phi. Per-

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haps I am missing something here? Could you explicitly provide the values you used in the definition of future, present and past CO₂ conditions? Could you provide the references you used to define your initial conditions for all your model factors (Tables I and II)? My expertise in plankton ecology is not sufficient to allow me to comment on the actual, values used as initial conditions for the model. So these should be reviewed by someone with that knowledge.

L3-5 page 5: is this a reasonable expectation, given that your parameters are not independent? E.g. aCO₂ is possibly quite tightly dependent on V*max? Please expand on why you think this is an appropriate assumption. Results L7-10 page 6: it could be argued that the purpose of conducting mesocosm experiments in real life (usually to investigate what we think are the mechanism underlying variation in some variable in real life) is to observe what mean and variability we get under pre-determined conditions. To bind the initial conditions of the experiment in order to modify the result (variability) could be perceived to be a circular argument. L14-onwards: if the parameters are tuned based on model cost calculated using POC_{exp}, how can we be sure that this matters in any way other than in the model structure used here? What you have carried out is a model perturbation experiment, with tuning of parameters. I find it difficult to determine how we can derive new knowledge about the way in which ocean acidification impacts plankton communities. Discussion L1-8: please requalify these sentences (significantly) based on main concern raised above. I found it difficult to comment on the discussion given my major concern above. I believe this is an interesting approach but that the inferences made in this section (sources of variability of POC in experimental datasets) are not supported by the study design used. Conclusion L9-11 page 9: I think that you potentially miss-sell the importance of your study. Manipulating the aspects you suggest to affect experiment results seems circular. However, you have a very interesting model with which to explore how climate change and ocean acidification impact phytoplankton dynamics, which could be very useful to the community. This is a very interesting tool for ecologists to test the influence of specific mechanisms represented in your models. Future validation of this approach through use of poten-

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tially different approaches for parameter optimization and use of observational data to evaluate model skill would be important.

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