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Comment

## ***Interactive comment on “Marine carbonate system evolution during the EPOCA Arctic pelagic ecosystem experiment in the context of simulated Arctic ocean acidification” by R. G. J. Bellerby et al.***

**Anonymous Referee #1**

Received and published: 10 December 2012

The study describes the evolution of seawater carbonate parameters as a result of net biological production and gas exchange with the atmosphere during a mesocosm study in an Arctic fjord. A model study is used to investigate whether the mesocosm response can be extrapolated to the wider Arctic under different SRES emission scenarios.

The major finding of this study is that the levels of carbonate chemistry parameters measured during the mesocosm study represent the range of projected carbonate system parameters, however because atmospheric CO<sub>2</sub> concentration was not kept at constant levels representing the SRES emissions scenarios throughout the 28 daylong

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experiment, the mesocosm results should not be extrapolated to other regions based on certain emission scenarios.

Overall, I don't think that the results of this paper are sufficient to support the interpretations and conclusions drawn by the authors. The paper is written in a confusing manner and without sufficient information to assess the conclusions that have been reached. The model study is not needed in order to reach the above stated major conclusion. Their conclusion is solely based on the fact that the evolution of atmospheric CO<sub>2</sub> in the mesocosms was not the same as projected in the SRES emissions scenarios. Overall, there is no connection between the presented data sets and the conclusions. Also, the authors attend to compare the time-series of a 28 daylong mesocosm experiment with a seasonal cycle of a model. This comparison seems to be unnecessary because it is clear that the variability of such a short experiment does not represent a seasonal cycle, unless the experimental design explicitly addresses this question.

However, I believe that the development and usage of the Arctic model is of great importance to study the regional changes and future evolution of the carbon system, as long as a thorough model evaluation has clearly outlined the shortcoming and caveats of the model.

Major concerns:

I appreciate the description of the evolution of carbon system parameters during the Svalbard mesocosm experiment. However, I would like to see a more detailed analysis of what exactly changed the pH, pCO<sub>2</sub>, TA and DIC over the course of the experiment. The authors generally state that the changes are due to gas exchange and net biological production, but I wonder whether the signal of net biological production is visible at all, given the large differences in atmospheric and oceanic pCO<sub>2</sub> in some of the mesocosms. I believe that air- sea CO<sub>2</sub> fluxes and biological production, respiration etc. were measured during the mesocosm study. However, the authors don't use this information as underlying evidence for their statement that the carbon changes are due

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to gas exchange and net biological production. In general, I was constantly missing information (i.e. how was the CO<sub>2</sub> perturbation done), feeling the need to read other papers about the mesocosm experiment in order to be able to follow what was done. In short, I don't think the paper is self-contained.

In general, since this is a model study I miss a paragraph about the overall model performance. Nothing is said about how closely the model simulates the natural variability of the system. Has the model been compared to in situ measurements? What are the caveats of the employed model and how do these caveats potentially affect the findings of the paper? On page 15549 Lines 12 – 14 the authors state that this model study highlights the necessity to operate mesocosms closely simulating natural variability” – implying that the model closely represents natural variability, without any proof of evidence that the model actually is a close representation of reality (or not).

Furthermore, wouldn't in situ measurements be more helpful and “reality-representative” to come to the conclusion that future mesocosm studies need to more closely simulate the natural variability?

Also, the authors compare the variability of the mesocosm study that lasted less than a month, to the seasonal variability of the model. Such a short mesocosm study can't be used to study the natural seasonal variability, as it is not exposed to seasonal freshwater influence, temperature etc. (unless the experimental design is drastically changed).

In general, it would help the readability of the paper if the text would more often cite the figures. For example, key figures 4 and 5 were never cited, making it very complicated to understand the text.

Minor corrections:

Page 15542 Line 5 Add “concentrations” – “to future CO<sub>2</sub> concentrations

Page 15542 Line 17 What are you referring to by regional ocean acidification? Are you referring to the mesocosms?

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Page 15543 Line 10 Please rephrase last part of the sentence, I don't understand: "One approach to understand ecological and biogeochemical responses to ocean acidification is to deliberately perturb marine ecosystems and organisms and then propagated through climate driven ocean models (. . .).

Page 14543 Line 13 Exchange "maybe" with "may" – " This form of information may then be used. . ."

Page 15543 I would appreciate more information about the sampling method of DIC and TA. Were the samples fixed? Were the samples immediately analyzed?

Page 15544 Line 23 Change ""encompassed" to "encompassing"

Page 15545 Lines 12 – 26 What are the initial and boundary conditions for Alkalinity and DIC? I would appreciate more information about the physical forcing of the model? Is it forced on a monthly or daily basis?

Page 15545 Line 24 Change "For Arctic Rivers flows, data were obtained. . ." to: "Arctic river flow data were obtained. . ."

Page 15546 Line 13 Was the water really isolated on t-5, after the CO2 characterization was performed on t-3? I don't understand.

Page 15546 – Page 15547 Please refer to your figures 4 and 5. In general, a table would be helpful to successfully follow the presentation of the results on page 15547. This table could contain initial and end values for every mesocosm and studied parameter.

Page 15547 Line 1 Here, the authors state that the addition of nutrient did not affect the TA, but on Page 15546 Line 25 claim that the changes in TA were partially due to nutrient uptake. Please explain.

Page 15547 Line 25 I would like to see some discussion about primary production and respiration rate. Since there is no horizontal transport of organic material out of

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the mesocosm I would like to know how big of a role remineralization and respiration played in the calculation of the biological net carbon production.

Page 15548 Line26 Change “the seasonal variability in the bi-weekly, mixed layer means...” to “the seasonal variability of the bi-weekly, mixed layer means. . .”

15549 Line 2 – 5 I don’t agree with the sentence: “Further, due to the unseasonal forcing through a significant nutrient addition to the mesocosms, it is difficult to determine which stage of the year the mesocosms were simulating and thus allow a complete comparison with rate of change of the spring bloom.” Why would the study allow a complete comparison with the rate of change of a spring bloom, if the seasonal stage can’t be determined? Please rephrase in order to resolve this confusion.

Page 15549 Line 5-8 Please rephrase, it is hard to follow your point in this sentence: “The comparison does, however, enable a regional scaling of the experiment to anticipated changes in the coupled Arctic system and thus can inform on the limits to representation of ocean acidification- ecosystem responses founded from the mesocosm results.”

Page 15549 Line13 Change “simulating natural variability the CO2 system” to “simulating natural variability of the CO2 system”

Page 15549 Line 15 Change “. . . , and not one simulating. . . “ to “. . . , and not one was simulating. . . ”

Page 15550 Line 7 The authors describe a great heterogeneity in the carbon parameters, which cannot be seen in figure 7. The figures and thus contour labels are too small to see these distinct features described in the text. Please enlarge or change colorbar.

Page 15550 Line 17 Please rephrase the following sentence because some words are missing “ There is a very clear demarcation where the largest changes in sea ice cover (. . . ).

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Page 15550 Line 25 I don't agree with the sentence "This study has documented the potential for significant ocean acidification perturbations in a future Arctic."

I agree that the Arctic (with its naturally low carbonate concentration) is a perfect region to study the effects of ocean acidification. But this is a rather general statement and I don't agree that this conclusion is based on the result of this study. Please clarify.

Page 15550 Line 26 Remove extra "the"

Figures Captions Fig 4 – 5 Please explain what the black line illustrates. In general, it is hard to distinguish between the black and grey lines. Please increase the font size on the figures and add the letters (a – e) that you mention in the caption to the figures.

Caption Fig. 6. Revelle factor is shown in the figure, but is neither mentioned in the caption nor in text.

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**BGD**

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