

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.

We welcome the comments from the two reviewers who both found value in our paper. We especially welcome the generous comments of reviewer #2 who identified clearly what the message of the paper was. We have taken on board the comments from both reviewers and adapted the text accordingly where we are in agreement. We have also further developed the text as requested by the guest editor to make the message flow and the manuscript more readable.

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 CO₂ concentration was not kept at constant levels representing the SRES emissions scenarios throughout the 28 day long experiment, the mesocosm results should not be extrapolated to other regions based on certain emission scenarios.

Here we see that the reviewer has not understood the major finding of the study. We have thus made it clearer in the text what the major findings are.

We do not see where reference is made in our paper to the fact that “atmospheric CO₂ concentration was not kept at constant levels representing the SRES emissions scenarios throughout the 28 day long 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 paper has been rewritten to make the major findings clearer

The model study is not needed in order to reach the above stated major conclusion.

The model study was not done to reach the above stated major conclusion.

Their conclusion is solely based on the fact that the evolution of atmospheric CO₂ 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.

It was exactly the point to compare the seasonal cycle in the model to the changes seen during the experiment. There is broad use of mesocosm and experimental results in understanding future ecosystem responses. It is the point of this paper to compare the seasonal and regional changes around the study site to indicate to what degree the experimental results bear relevance to the real/model world

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.

We are thankful for the support of the review in our modeling approach

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, pCO₂, TA and DIC over the course of the experiment.

Here, we have made reference to other relevant results and manuscript references from 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 pCO₂ in some of the mesocosms. I believe that air- sea CO₂ 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 to gas exchange and net biological production. In general, I was constantly missing information (i.e. how was the CO₂ perturbation done), feeling the need to read other papers about the mesocosm experiment in order to be able to follow what was done.

The gas exchange is discussed in detail in Czerny et al., 2012. Both Czerny et al., 2012 and Silyakova et al., 2012 use this gas exchange to identify that there was a

considerable net biological production in all mesocosms. The description of how the perturbation was done is detailed in Riebesell et al., 2012. We have made more detailed references to the results and methods in these papers

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).

We reference submitted work that illustrates the good performance of the model against the CARINA dataset (Silyakova et al., submitted)

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

We do not understand the point of the reviewer here. How could contemporary in situ measurements help understand future seasonality and regional variability with non-linear feedbacks to ocean acidification such as production, freshening, ice cover?

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).

That was one major point of the paper to compare the ranges and responses of the mesocosm and the model results to check to see if these external influences would have a significant role in the development of future OA scenarios in the Arctic and to test the applicability of extrapolating the mesocosm results from the fjord to the wider Arctic Ocean. This has been made clearer in the manuscript

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.

We have increased references to the figures in the text.

Minor corrections:

Page 15542 Line 5 Add “concentrations” – “to future CO2 concentrations

done

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

Added regional ocean acidification “simulations”

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 (. . .).

rephrased

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

done

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

Added “No poisoning of samples was done, samples were stored in the dark and analysed within 12 hours of sampling.”

Page 15544 Line 23 Change “encompassed” to “encompassing”

done

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?

As is stated in the text, the initial and boundary conditions are from the Bergen Climate Model data that has been corrected to the CARINA climatology and the correction is propagated throughout the century using the delta change method.

The temporal frequency of the model forcing is 6 hourly – this has been put into the text

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

done

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.

t-5 (t minus 5) comes before t-3

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.

This information is supplied in Riebesell et al 2012. so we reference it here.

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.

Addition of nutrients was balanced by the addition of HCl to balance the total alkalinity perturbations as is described in the text. Subsequent nutrient uptake by phytoplankton would increase the total alkalinity.

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 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.

This is not possible using the data analysed for this paper and is not important for the message we are relaying. The information you are looking for is in Czerny et al., 2012, Engel et al., 2012 and Silyakova et al., 2012 and we now reference more clearly these papers in the text.

Page 15548 Line 26 Change “the seasonal variability in the bi-weekly, mixed layer means...” to “the seasonal variability of the bi-weekly, mixed layer means. .”

Done

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.

We have removed reference to the spring bloom and added instead “our approach does not allow a direct comparison between the timing of the model seasonal pCO₂ cycle and the drawdown in the mesocosms.”

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.”

Replaced by “However, it is possible to compare the amplitude of the seasonal cycles of the marine carbonate system in the coupled Arctic model simulations with the changes in mesocosm, and thus inform on the representativeness 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”

done

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

done

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.

The figures submitted to BG were clear but the journal reduced the figure size. We will address this with the journal directly

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 (. . .).

Added ice cover “occur”

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.

We disagree. Both the experiment and the models show that the addition of CO2 to the Arctic ocean will result in “significant ocean acidification perturbations in a future Arctic”. We see no contradiction with this statement and the findings of this study

Page 15550 Line 26 Remove extra “the”

Done

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.

The black line in Figure 4 is the background fjord variability. This has been added to the Figure legend and described in the text

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

The Revelle factor figure has been removed

Anonymous Referee #2

Review of "Marine carbonate system evolution during the EPOCA Arctic pelagic ecosystem experiment in the context of simulated Arctic ocean acidification" by Bellerby, Silyakova, Nondal, Slagstad, Czerny, de Lange, and Ludwig.

This manuscript documents the changes in the carbonate parameters throughout the Arctic Ocean CO₂ perturbation experiment. The authors thoroughly described the evolution of the carbon parameters throughout the experiment. The paper was straightforward and clearly written. I think this part of the paper only may not merit publication. However, this paper is unique in that it puts mesocosm experiments into a regional perspective (Arctic Ocean). In particular, it highlighted the caveats associated with the CO₂ perturbation experiments and also emphasized the needs of more refined experiments that better simulate the acidification conditions in the Arctic Ocean. As far as my memory concerned, the modeling analysis included in this paper is the first attempt that brings the community attention to the needs of new experimental manipulations that better represent the local and seasonal variability in ocean acidification parameters. Since the paper is clearly presented, I believe the paper can be published as it is. I have a few minor comments that the authors consider in preparing any revision.

1. In CO₂ system calculations for mesocosm and modeling works, the authors used the different carbonic acid dissociation constants. For consistency and direct comparison, I would suggest the authors use the same sets of constants.

We acknowledge that there will be small differences in carbonate calculations resulting from the use of different carbonic acid constants. Compared to the very large temporal and regional changes documented in this study the errors from using different pKs is very small and we now state this in the text – "Differences in choice of carbonic acid constants are deemed insignificant for the purposes of this study (e.g. Millero et al., 2006)

2. Although there were small alkalinity changes during the experiment (2242 à 2247 à 2242 $\mu\text{mol kg}^{-1}$), the authors should be more quantitative in explaining the changes. For example, Can the 5 $\mu\text{mol kg}^{-1}$ increase be explained by salinity increase? Another issue is the 5 $\mu\text{mol kg}^{-1}$ decrease in the later part of the experiment. The authors attributed this decrease to calibration problem. This should be better explained.

We believe we have covered this question already in the text "The increase was due to freshwater losses, following evaporation, and nutrient uptake (Silyakova et al., 2012).".