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Interactive comment on “Mesozooplankton community development at elevated CO₂ concentrations: results from a mesocosm experiment in an Arctic fjord” by B. Niehoff et al.

Anonymous Referee #2

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General comments:

The manuscript reports the study of the effects of ocean acidification on zooplankton community composition in a mesocosm set-up. The study is one of the first of this kind and therefore novel and interesting. The manuscript is well structured and also well written. From their results, the authors conclude that ocean acidification could have some future impact on zooplankton composition by negatively affecting particular groups such as cirripedia or bivalves. Mesocosm studies like the present often suffer from restricted sampling by the limited volume. The authors overcame this problem by relatively infrequent sampling (six times) and by averaging data over the whole sam-

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pling period for subsequent linear regression analysis. I have some problems with this method as it assumes that responses to increasing CO₂ will be linear and that zooplankton development is independent of time and not different in the various mesocosms. This is not necessarily the case, particularly in such groups which have several development stages, e.g. copepods. For instance, mortality is not independent of development rate as it is stage dependent, which has not been analyzed here. Differences in timing, caused by various environmental factors, as also discussed by the authors, can therefore influence the results of the analysis. Moreover, conclusions drawn for cirripedia and bivalves are based on selected data (either from the sediment trap or the water column) and it is not clear for me why this selection is justified. Apart from these critical issues, the methods lack some detailed information on procedures and some wording could be corrected.

Introduction

p. 11481, Line 6: Before CO₂ dissolves it is absorbed by the sea. The carbon system is described too short (only by one sentence) for a general introduction. Improving this would increase understandability of the following for a non-specialist and shorten text, e.g., line 31 when 'carbonate ion' is used and when 'decreasing pH-increasing pCO₂' can be exchanged by OA.

p. 11481, Line 17: Citations are missing for the observed changes; e.g. are there many examples for a changed stoichiometry? I thought changes occur largely when carbon availability is limited.

p. 11482, Line 5 ff: long sentence

p. 11482, Line 14: Bergen experiments: any citations?

p. 11482, Line 20: Any important results from Caretenuto?

p. 11482, Line 24: Any hypotheses? One wonders if there no strong effects, why is this studied?

Methods

p. 11483, Line 14: I guess 15 m 'depth' not 'length'.

p. 11483, Line 19: explain: t-7; what defines day 0 or 1?

p. 11484, Line 8: Just out of curiosity: how much water was added to each of the mesocosms? Did the amount of water (dilution) differ substantially between different CO₂ treatments?

p. 11484, Line 14: Please give the time span for the decline in CO₂. From which day are the final measurements?

p. 11485, Line 12: At which time of the day the samples were taken? During the day, zooplankton might have been missed due to vertical migration. Sub-sampling from the fjord would have helped to resolve the insecurity about the abundance estimates.

p. 11485, Line 13: The order of days is confusing. Why is d-2 twice mentioned? D-11 is before the filling (and if it is d11, then this is close to biweekly, not weekly).

p. 11485, Line 19: What were the criteria for splitting the samples, how many individuals were counted? What about the treatment of rare vs. abundant species regarding the splitting?

p.11486, Line 5: What is the underlying hypothesis for assuming linear responses to pH/CO₂?

p.11486, Line 16-17: Citations are missing in the references.

Results

p.11486, Line 20: While I can understand that sampling in the mesocosms was restricted by the available volume, this was not the case in the fjord. Here one would have expected that replicated sampling would have been done. Furthermore, one wonders why the last day of mesocosm run was not used to establish estimates of sampling

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variability.

p.11487, Line 7: 'The number of organisms changed with time...' contrasts with 'the total abundance changed only slightly...' on page 11486, line 22. It is doubtful to give numbers here (averaged over all mesocosms? What justifies this?), as there are no estimates of sampling variability and trends can result from compositional differences.

p.11487, Line 26: AS described in the legend to Fig 3, M7 is not visible, but is one of the 185 μatm CO₂ mesocosms.

Fig 2C: Does the copepod composition include nauplii? Otherwise it should be mentioned that this describes the copepodite composition.

Fig 1C: The zooplankton carbon and total carbon can be separated in this figure, as zooplankton were mostly swimmers (according to line 4, p 11487) while diatoms are probably 'real' export. Why were they summarized here?

p.11488, Line 5: The differences between fjord and Meso cosms are no surprise considering the mesh used to exclude large zooplankton. The manuscript lacks information on the composition of the zooplankton left out: was this Calanus or other species?

p.11489, Line 2: Please specify: what is meant by 'development was influenced by CO₂'. Do you suggest that the development rate was reduced, so that nauplii instead of cypris settled? How much is this trend driven by the two 'outliers' at day 16? If this trend is related to the CO₂, should it not be visible in the whole mesocosms, and not only in the sediment traps?

p.11489, Line 7: Throughout the text the labeling of mesocosms according to number (M1, M2...) is not very satisfying as no additional information is provided and one always has to go back to the Mat& Meth to look up which mesocosm these have been. Can these be exchanged to the CO₂ labeling (185 μatm ...). This is also consistent with the labeling in figures.

p.11489, Line 11: This is very unclear. Polychaetes apparently settled as they were

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found in the sediment traps, and were removed. What kind of larvae was then found at day 11? The large size argues against that these have been trochophora larvae. So where did these larvae come from? The mesh size of the net used should have allowed collecting trochophora larvae, but these should have been present earlier then.

p.11490, Line 15: Correlation between what?

p.11491, Line 20: Which data went into the linear regression of copepods? Stages? Why is – as in all the other cases – a linear response to CO₂ expected? Dose-effect responses often follow a sigmoid response. Furthermore, it is not clear for me how the data has been treated: was the abundance at the end used, was the data averaged over time – and if yes – is regression analysis correct statistic here? Abundance of copepods is a function of stage dependent mortality and development rate – and thus the treatments cannot necessarily be compared by averaging or using end samples when the different mesocosms develop differently over time – so time and timing cannot be ignored.

Discussion:

Apart from the discussion of the set-up (outgasing/uptake of CO₂), I miss a critical evaluation of the lacking estimate of sampling variability on potential conclusions. The sample volume of an Apstein net is small (in the case roughly 0.2 m³), but small differences occurring in the analysis are potentially up-scaled (by a factor of 5). This might be critical for estimating effects of OA on rare groups (e.g., bivalves). In addition, time periods in between single samples were long. Moreover, the first samples revealed a strong variation in the initial abundance of zooplankton. Can this influence detection of any trends? There is also a layer of 3 m depth between sediment trap and the depth of net sampling.

p.11494, Line 18: I might have missed it, but were mortality rates estimated that would allow this conclusion?

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p.11494, Line 27: When OA is delaying the development of nauplii, one should see this as well in the mesocosms which contained by far the larger pool of cirripedia nauplii. Fig 6 A,B, however, do not indicate such a delay, and one wonders if such trend could have been detected with the low sampling frequency.

p.11496, Line 25: Again, the conclusions seem to be based by using only one of the available data sets. The data from the sediment traps is ignored here, although they constituted a considerable pool for thios group. In addition, please specify what is meant by negative influence on 'development'.

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