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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 #1

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General comments

This manuscript reports on the mesozooplankton community development in CO₂ enriched mesocosms in the Arctic Kongsfjord over a six-week period. The focus was on changes in abundances and taxonomic/species composition. Mesozooplankton in the water column and in the sediment traps of the mesocosms were studied. The main finding of this work is a negative CO₂ effect on the development of cirripedia larvae and on the occurrence of bivalve larvae. As a whole, the zooplankton community composition was not affected by elevated pCO₂ concentrations.

This is a well-structured and clearly written manuscript that is easy to follow for the

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reader. The experimental approach allows for the investigation of ocean acidification effects on whole planktonic communities on a large (ecological) scale, therefore this is a unique dataset. However, the authors' main conclusion "no significant change in community composition" is not justified by the way the data were analyzed and therefore, the manuscript should not be published without serious consideration of the comments below.

Specific comments

– L245/246: My major criticism is on the MDS ordination and conclusions drawn from it. MDS ordinations do not give a significant result, i.e. no significance value. They simply map (multivariate) data in an n-dimensional space by distances based on a similarity (or dissimilarity) matrix among the samples. That means an MDS plot helps to see whether samples are similar and how close they are to each other. But it is NOT a statistical test! What complicates the matter here is the repeated measures design with no replication. That means, in the analysis the factor "time" needs to be eliminated to be able to judge whether or not there is a CO₂ effect. If not, the time effect may mask a possible CO₂ effect. A simple MDS ordination technique cannot do this. Therefore, the authors have no justified reason for their conclusion "no significant change in community composition" any better than a subjective impression of their data. In fact, Fig. 5 nicely shows the separation of the samples by time, in that, I agree with the authors. But, as just pointed out, it is not the factor time that is of interest here (it is well known, that time has an effect on the plankton succession). The question is, whether or not aside from a time effect there is also a CO₂ effect? It needs more elaborate statistical techniques to determine whether or not there is a CO₂ effect. Mixed effects modeling would be an appropriate tool maybe using species richness or Shannon Wiener index as a measure.

Also, it needs to be better specified which taxa/species were included to calculate the similarity matrix. I.e. how were the cirripedia entered, as "cirripedia" or as "cirripedia nauplii and cypris"? It is also not clear, whether copepods were entered as "copepods"

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or separated by “species and even stages”? The authors mention that they have staged copepods, but the stages counts do not show up anywhere in the ms. Included or not, this will change the similarity matrix calculated and thus the outcome of the analysis. This needs to be stated clearly in the ms in order to make the reader able to assess the data/results and the conclusions drawn.

– L171–175: Sample processing needs to be clarified: What was the maximum split factor applied? Of the samples that were split, were only the very abundant taxa/species counted in the subsamples or were the abundances of the whole sample calculated from subsample counts? Usually, only the abundant taxa are counted from subsamples and the less abundant taxa/species are counted from the whole sample or the larger aliquots. What was the minimum number of individuals counted in each subsample? Please, clarify how samples were counted to make the abundance calculations reproducible.

– L96–98: Last sentence in the introduction is results, delete from the introduction

– L449–501: Is it possible that the author could not notice increased mortality of bivalve larvae because their shells dissolved already in the water column and the soft tissue was already degraded (or unidentifiable) before reaching the sediment trap?

– L525–529: The larger mesozooplankton that was not effectively collected by the 55 μm net, was it found in larger numbers in the sediment trap? If not, there was probably not too much larger plankton in the mesocosms.

Technical corrections

– L111: Off-Shore: The “S” should be underlined, too, I guess?

– L123: Delete the dot after ...2012b)

– L166: t-11 or t-1?

– L295 + L302 + L344: Fig 2? Should be Fig. 3.

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- L312 + L348: Fig. 3 must be Fig. 4
- L370: . . . up to several days . . .
- L393: grazing rates of *Calanus* spp. and cirripedia nauplii decreased with increasing or decreasing pCO₂?
- L457: Kongsfjorden lacks the “s”
- Fig. 2+6 (Figure caption): Please make the reader aware that the scales are different in the different graphs.
- Axis labels of figures: Please use consistent labels, some start with capital letters some don't.
- Fig. 2 lacks the x-axis label.
- Fig. 7: shows the ratio nauplii : cypris of only 8 mesocosms, where is number 9?

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