

Interactive comment on “High tolerance of protozooplankton to ocean acidification in an Arctic coastal plankton community” by N. Aberle et al.

Anonymous Referee #1

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General

This manuscript presents results from a large coastal mesocosm experiment in Svalbard in which pCO₂ and pH were manipulated and a phytoplankton bloom triggered by addition of nutrients in order to examine the impact of ocean acidification (OA) on these planktonic communities. As part of a larger study, here the results concerning the protozooplankton are presented. As a remarkable result, the acidification did not seem to have any effect, neither on the biomass of the different groups nor on the taxonomic composition or population dynamics. This can be interpreted as a high tolerance and might be interesting for the readers of Biogeosciences. However, for a

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more comprehensive understanding of what was going on in the mesocosms, it needs some additional information and explanations. There is some confusion in the terms that are being used but not properly explained (protozooplankton vs. phytoplankton), missing statistics and some additional data that are necessary to judge the results of this study. Therefore, although the paper is well written it requires a thorough revision (outlined in more detail below).

Specific comments

Introduction

p.3, l. 5ff: do you focus in this paper only on herbivorous protists not on bacterivorous? This paragraph gives this impression but then it should explicitly be mentioned.

p.3, l.13ff: I think here also the impact of OA on calcifying planktonic protists (also coccolithophores are protists!) should be mentioned

Clarification of terms: Different and partly overlapping terms are used in this paper and the introduction should clarify their meanings: what is “protozooplankton” compared to “phytoplankton”? I think a large portion of the phytoplankton are indeed protists! And dinoflagellates can be herbivorous consumers but also primary producers. So how are the terms differentiated throughout this study? Were you differentiating according to their nutritional modes? But how were these assessed? Can be very confusing! Table A1 suggests that only ciliates and dinoflagellates were assessed as “protozooplankton”. In this case the title and the terms throughout the paper should be changed, e.g. to “herbivorous ciliates and dinoflagellates” or “microzooplankton” but not “protozooplankton” in general as this would also include heterotrophic nanoflagellates (which are not considered here) and probably also some of the phytoplankton components.

Methods

p.4, pCO₂ levels: please provide not only the average values but also a measure of

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the temporal variability of the pCO₂ concentrations and the pH in different mesocosms (e.g., in the supplement)

p.5, protozooplankton-phytoplankton enumeration: see my comment above: what is a “phytoplankton”, what a “protzooplankton” (e.g., an autotrophic dinoflagellate). Further, I do not think that the lugol-fixed samples are appropriate to distinguish auto- and heterotrophic taxa. Was this ignored?

Results

It would be nice to have not only biomasses but at some point also cell numbers of the major protist groups reported here, as only those numbers can be directly compared to data found in other systems. I suggest to extend for example Table A1 for this (or provide an additional table).

Fig. 2 is awful to read and I suggest to take out the phytoplankton lines and put them in extra plots.

Same with Fig. 3, here at least the lines should be made in colour in order to be able to differentiate the different treatments.

Statistics: In the methods it is stated that a regression analysis was used to test for OA effects on protozooplankton. I have not seen any results of those tests! Has this been forgotten entirely? I think, both the composition and biomass as well as the temporal dynamics can be tested with this experimental design.

Discussion

I think much more focus should be put on species-specific effects of OA. Even if this study could not demonstrate effects of OA on the protist communities present here, this does not mean that nowhere any effects might be expected. Try to gather all relevant data from the literature in order to discuss potential different vulnerabilities of different protist groups.

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The discussion makes it clear that other factors, mainly trophic interactions, might have a more profound shaping effect on PZP compared to pCO₂. This is an important aspect but the discussion of food quality effects (p.12) seems to me too speculative and not supported by data shown here and therefore should be omitted or strongly shortened.

Interactive comment on Biogeosciences Discuss., 9, 13031, 2012.

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