

Interactive comment on “Copepod feeding and reproduction in relation to phytoplankton development during the PeECE III mesocosm experiment” by Y. Carotenuto et al.

Anonymous Referee #1

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The study by Carotenuto and co-workers assesses effects of elevated CO₂ on the reproduction and feeding of the marine copepod *Calanus finmarchicus*. This is achieved by exposing females of *C. finmarchicus* to seawater sampled daily from two mesocosms initially adjusted to CO₂ concentrations of 350 (1 × CO₂) and 1050 μatm (3× CO₂). After 24 hours of incubation, the female copepods were transferred to water freshly sampled from the mesocosms. The eggs were separated from the females and were incubated for 48 hours to determine the hatching success.

In principle, two types of CO₂-induced effects may occur in herbivorous zooplankton with this experimental set-up: (1) direct effects of elevated pCO₂ levels and lowered

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seawater pH, e.g. on egg and larval development, and (2) indirect effects of e.g. altered food quality and quantity in response to differences in CO₂ enrichment. While the focus of this study clearly is on possible indirect CO₂ effects via changes in food quality, it is not entirely clear whether direct effects of pCO₂ and pH in the incubation medium can be completely excluded. Unfortunately, information about pCO₂ and pH in the medium used in incubations of both adult copepods and copepod eggs is missing in the manuscript.

Methodological comments/questions:

1. The water used in the incubation experiments of the adult copepods was sampled daily from two of the PeECE III mesocosms (M2 and M8) and should therefore initially have the pCO₂ and pH levels occurring in the mesocosm upper mixed layer at the time of sampling. It is unclear, however, to what extent the sampling and handling of this water contributed to CO₂ gas exchange. Also, how did pCO₂ and pH change during the course of the 24 hour incubation?
2. Was the same water used in female incubations also utilized in the egg hatching experiments? If so, how did pCO₂ and pH levels change during the 48 hours of incubations?
3. Copepod ingestion rates were calculated from faecal pellet production measured in female incubations. The formula used to calculate ingestion rate is not clear to me: i) the numbers given in equation (1) do not appear to match with the units for ingestion rate and faecal pellet volume; ii) shouldn't the units for faecal volume read $\mu\text{m}^3 \text{f}^{-1} \text{d}^{-1}$, i.e. without the C?
4. P. 3916, lines 16-17: Initial pCO₂ levels in the 1x and 3x CO₂ treatments were 350 and 1050 μatm , respectively.
5. P. 3919, line 17: There was no silicate added to the mesocosms.
6. P. 3925, lines 8-9: POC accumulation should not be regarded a sufficient indicator

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for effects at the phytoplankton community level.

Comments on data presentation and interpretation:

1. P. 3915, lines 12-13: I think it's not so clear what is the predominant carbon source used for photosynthesis by *Emiliana huxleyi*.
2. P. 3915, lines 24-26: This is a model study relying on multiple (and partly untested) assumptions. Hence, a predicted 14% contribution of copepod grazing to calcite dissolution should be seen as a working hypothesis rather than as evidence for copepod induced calcite dissolution.
3. P. 3920, para 1: The apparent difference in Chlorophyll a between mesocosms 2 and 8 is not representative for the respective CO₂ treatments when considering the triplicate mesocosms (see Schulz et al., this volume). The way it is stated here may give a false impression regarding possible effects of CO₂ on phytoplankton development.
4. P. 3920, lines 11-12: "... with up to 5.6 $\mu\text{g l}^{-1}$ in both mesocosms ..." Referring to diatom or prymnesiophyte Chl. a?
5. Section 3.2 Phytoplankton development and Figs. 1 and 2: Much of the information provided in this section and in the corresponding figures (based here on only 2 mesocosms) is presented in a more comprehensive way for all 9 mesocosms in the manuscripts by Schulz et al. and Paulino et al. (both in this volume). The description here should therefore focus on differences immediately relevant to the zooplankton experiments reported in this study. Also, restricting a data comparison to only two mesocosms may give a wrong impression regarding potential CO₂ treatment differences.
6. P. 3925, bottom line - p. 3926, first line: "This may be explained by a combination of food saturation and/or lower quality/deleterious food composition in the 3x CO₂ treatment." It is not clear what the statement in italics is based upon. The POM data presented in Schulz et al. (this volume) do not show significant treatment differences.
7. P. 3926, lines 8-13: Is there any evidence (in the literature or from this study)

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indicating a connection between CO₂ and any of the proposed causes for recruitment efficiency?

8. P. 3926, lines 18-19: Note that the enhanced DIC relative to nitrate drawdown in response to elevated CO₂ is not mirrored by a corresponding signal in the C:N of suspended organic matter (see Schulz et al. (this volume) for POM stoichiometry and Riebesell et al. (2007) for a mechanism possibly explaining this discrepancy).

9. P. 3927, lines 17-23: See also the following references: Urabe et al. (2003) *Global Change Biology* 9, 818; Jeyasingh (2007) *Ecology Letters* 10, 282-289; Jensen & Hensen (2007) *Oecologia* 152, 191-200.

10. P. 3927, lines 24-26: see points 6 and 8 above.

Editorial comments:

1. P. 3915, lines 3-8: The list of impacts of rising atmospheric CO₂ given in this paragraph mixes impacts which will occur with absolute certainty (e.g. altering buffer capacity and changing seawater carbonate chemistry) with others, which are more speculative (changing the strength of the biological pump).

2. P. 3922, lines 3-4: Shouldn't it read "... both..., and ..." instead of "... either ..., or ..."?

3. P. 3924, line 11: delete reference to Schulz et al. since the paper does not provide data relevant to this statement.

Concluding remarks:

The data presentation should be confined to those data immediately relevant to the interpretation of the zooplankton results. On the other hand, more information is needed about the pH and pCO₂ conditions during incubations of adult females and copepod eggs. Finally, the data interpretation should differentiate between possible direct pH and pCO₂ effects and indirect effects via changes in food quality. It should be noted

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that the proposed lower food quality at 3x CO₂ is not supported by measurements of the composition and stoichiometry of suspended particulate organic matter.

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