

Interactive comment on “Response of benthic foraminifera to ocean acidification in their natural sediment environment: a long-term culturing experiment” by K. Haynert et al.

Anonymous Referee #1

Received and published: 8 July 2013

Dear Editor,

The manuscript by K Haynert and co-authors that you asked me to review describes an interesting and original experiment that bridges the gap between a number of published OA laboratory studies and field studies on naturally acidified environments. The experiment is clearly described and the manuscript is generally well-written. However, a number of flaws in the experimental design and interpretation of the manuscript make publication of this manuscript only possible after a set of major corrections. Below, I have listed my main concerns followed by a number of specific comments.

Major comments

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The salinity at which the culturing experiment was conducted, was ~ 16 . This makes it difficult to see how the results of this paper are applicable to foraminifera in general, since most live at higher salinities where saturation states are higher and undersaturation is less of a problem. In fact, Haynert et al., have conducted an experiment at which the effect of both low salinity and elevated $p\text{CO}_2$ is investigated. This should be stressed in the title, abstract and discussion. The resulting low saturation states furthermore make it very difficult to compare their results with previously published results and the authors should refrain from that (see Discussion). As often with OA studies, no condition was included with a lower than current-day atmospheric $p\text{CO}_2$ (i.e. < 380 ppm). This would have allowed testing whether any observed trends also hold for low $p\text{CO}_2$ conditions and whether the observed changes are not simply the result of an adaptation to natural conditions (i.e. whether decreased chamber addition rates would also have occurred at low $p\text{CO}_2$'s). This is also a shortcoming in the experimental design of the authors and the resulting potential bias should be mentioned in the discussion.

Abundances of living foraminifera are highly variable. Even right after homogenization, population densities are different between culture vessels. The problem with this variability is that it makes it difficult to interpret (small) increases or decreases in density, since they may be the result of a 'natural' variability. In my opinion, this prevents interpretation of decreases in population density (Figure 3) as reproduction events. Densities at $907 \mu\text{atm}$, for example, are all within the variability observed at $t=0$, so that occurrence of reproduction is not justified by this figure. Additional 'proof' comes from the next figure (4) by occasional increase in small-sized specimens. However, this is a relative increase, and may well be caused by decreased abundances of large-sized specimens. The periods with no claimed 'reproduction events' may well have seen reproduction, albeit at a much lower rate. If the authors could convincingly show that there has been reproduction at certain months, it should be expressed as 'increased reproduction rates' to avoid the suggestion that at other months there was 0 reproduction.

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The background variability in densities is in my opinion also clear from the occasional decrease in densities of dead specimens, even at relatively low pCO₂'s, where undersaturation cannot account for the decrease in densities. Since it is to be expected that abundances of dead specimens only increase, the decrease in densities is likely 'caused' by variability in densities that was present when the experiment started.

The EDS maps are not very informative. The obtained Mg-concentrations and distributions are at best qualitative, and any explanation based on Mg/Ca of these foraminifera may better be taken from the literature. Surprisingly, these authors do not refer to known Mg/Ca for Ammonia's (e.g. Toyofuku et al., 2011; Marine Micropaleontology, Diz et al., 2012; Biogeosciences) and only rely on their qualitative maps. These results and the associated discussion should be removed altogether from the manuscript.

The discussion lacks focus and does not take full advantage of the carefully recorded and provided parameters. Instead, it contains numerous sections (see below) that should be omitted since they are irrelevant here, or represent overinterpretation of the data. The discussion should therefore be restructured altogether.

Minor comments

Affiliations:

Is there a difference between 1 and 4?

Abstract:

Line 2: ', However,' should be ', however,'

Introduction:

Page 9525, line 8: What does 'negatively affected' mean?

Page 9525, line 10-11: What does 'indistinct sensitivity' mean? Does it mean that growth rates or chamber addition rates were not impacted by elevated pCO₂?

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Page 9525, line 13: I guess 'dry weights' mean weights of the tests. Please change, since 'dry weights' often refer to organic matter from which the water has been extracted.

Page 9525, line 21-22: What exactly was not affected? Species composition? Standing stocks?

Page 9526, line 9: 'population' should be 'community' or 'populations and community'.

Methods:

Page 9526, line 16: 'is' should be 'consist of'

Page 9526, line 22: What does 'graduated' mean here?

Page 9527, line 3: Remove 'room temperature'

Page 9527, line 7: Replace 'sucked off' with 'removed'

Page 9527, line 7-12: It is not clear to me, whether the culture vessels were filled with homogenized sediment from one sampled core (and therefore likely introducing differences between culture vessels since the cores may have different foraminiferal community compositions/ population densities) or that material from different cores were combined before homogenization.

Page 9527, line 10: Was the sediment not sieved over a 1 mm screen to remove macrofauna?

Page 9529, lines 13-14: 'in dependence of' should be 'independent of'

Page 9529, line 1-page 9530, line 16: Please include the frequency during the 6 months incubation at which samples for DIC, TA, etc were taken. Also for the pore waters.

Page 9529, lines 26-27: Was there any sign of anoxia? Particularly with the decaying macrofauna, I would expect so. This would require sampling the pore waters also under

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anoxia in order to accurately determine phosphate concentrations. Or was PO₄²⁻ only measured on the overlying water?

Page 9530, line 18: Were the contents of complete vessels processed in this way?

Page 9530, line 28: Staining with rose (no capital 'r') Bengal does not allow accurate quantification of living vs dead stocks. In these samples, I think staining with rB is likely to approximate the portion of truly living foraminifera. However, I would like to see that the authors at least mention the possibility that they have overestimated population densities (e.g. Bernhard et al., 2006; Paleoceanography).

Page 9532, line 21: From which treatment and at what time were the 100 specimens of *A. aomoriensis* taken to determine organic C content?

Results

Page 9533, line 14: Change 'achieved' into 'returned to'

Page 9533, line 15: Replace 'In dependency to...' by something like 'As a consequence of the elevated pCO₂ treatments, ...'

Page 9534, line 13: *E. exclavatum exclavatum* and *E. exclavatum clavatum* are not different species, but two subspecies/ morphotypes. Therefore, the authors have found 4 instead of 5 foraminiferal species.

Page 9535, lines 1-29: The authors repeatedly refer to reproduction events while there is no direct evidence for this. The only parameters that are measured are test diameter and abundance. Therefore, the results should be confined to reports on changes in test diameter/ abundance over time/ at certain pCO₂'s. Interpretation of these data in terms of growth cohorts and reproduction (which I doubt can be made) should be reserved for the Discussion.

Page 9536, line 4: from which condition/ month do the densities range between 24 and 61 tests/cm³?

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Page 9536, line 16: 'frequent' should be 'increased'.

Page 9536, line 21: 'dry weight' should be 'CaCO₃ weight', or combined with the measured sizes, 'size-normalized weights'.

Page 9537, lines 1-22: this whole section can be removed. I don't see the added value of dry weight vs CaCO₃ weight if the difference between the two is constant and only a few % of the total weight.

Page 9537, line 23-page 9538, line 12: the lower CaCO₃ 'production' rates may well be the result of increased dissolution at two highest pCO₂'s. This is likely because saturation states in these conditions are lower than, or close to 1. The term 'production' is therefore misleading and these observations may instead be rather trivial. Moreover, to avoid the suggestion that the 'production' rates are a direct consequence of the applied pCO₂, it may be better to refer to the treatments by their $\Delta\delta\epsilon$. This is related to one of my main comments, since the low salinity amplifies the effect on $\Delta\delta\epsilon$ caused by increased atmospheric CO₂ concentrations. Once more, the authors have investigated not just the effect of pCO₂, but the combined effect of low salinity and elevated CO₂, both reducing the saturation state with respect to calcite.

Page 9538, line 19: It is unlikely that there is only one layer of calcite (see e.g. Erez, 2003 or Sadekov et al., 2008) due to bilamellar calcification in Elphidium and Ammonia. It may be that the different layers have a similar Mg/Ca, and are therefore difficult to distinguish by EDS.

Page 9528, line 20: What is 'high Ca and low Mg calcite'? I think calcite always contains much Ca. . .

Page 9538, lines 22-23: Is the smooth surface of *E. incertum* visible from figure 7? It is true that this species has smaller pores that are commonly present at relatively low densities compared to those of *Ammonia aomoriensis*, but that is already known and much better visible on SEM pictures. Why include a scan on *E. incertum* in the first

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place? By far most of the results and discussion deals with *A. aomoriensis* anyway.

Discussion:

Page 9539, lines 1-9: The second part of the paragraph is wrong and awkwardly formulated. There is also no need for repeating the outline of this experiment. This paragraph can therefore be removed.

Page 9539, lines 12-13: Should be mentioned in the Results.

Page 9540, lines 4-6: Should have been mentioned in the Results.

Page 9542, lines 1-3: This explanation is, at best, incomplete. If dominance of *Ammonia* and *Elphidium* are both explained by availability of diatoms, what can explain their dominance in intertidal vs deeper stations?

Page 9542, lines 6-13: I don't see the added value of explaining the occurrence of a very rare species in the experimental set up. I also don't understand why it has to be brought into the experiment as propagules and not as an erratically occurring juvenile or adult.

Page 9543, lines 20-21: The authors have kept foraminifera in sediment (although not necessarily under natural conditions), but have not experimented with different types of sediment, organic matter content, etc. A general comparison to previous culturing studies (particularly those using *Ammonia*) that have not kept specimens in sediment would suffice here. The possibility that pore water chemistry changed (severely) during the experimental period should make the authors cautious in generalizing their conclusions (lines 10-12).

Page 9543, line 22-page 9544, line 5: This paragraph can be deleted since it contains very little information.

Page 9544, line 6: 'Reveals' should be 'may be explained by (a combination of)'.

Page 9545, lines 6-23: Although interesting, this information bears little relevance to

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the here measured parameters. If the organic vs calcite production would have been determined for each of the treatments, this paragraph would have been more relevant.

Page 9546, lines 19-24: Please note that these results are in line with some sediment-free experiment showing no or very little impact of pCO₂ on foraminiferal growth and size-normalized weights (e.g. Keul et al., BGD).

Page 9547, lines 8-9: Erez (2003) does not show that tests of Ammonia, nor Elphidium consists of a single layer (on contrary for Rotallids in general), characterized by a low Mg/Ca. Surprisingly, the authors do not refer to any papers that published Mg/Ca ratios of Ammonia. This genus, however, produces calcite with a low Mg/Ca (Dissard et al., 2010; Duenas-Bohorquez et al., 2011; Toyofuku et al., 2011; Diz et al., 2012). Elphidium produces calcite with a similar Mg/Ca, and the authors should avoid the suggestion that their EDS pictures (Figure 7) would provide a reliable alternative to published Mg/Ca for these genera. Solubility due to 1-2 mmol/mol higher or lower Mg/Ca is negligible, btw. Therefore, the sections on the EDS maps can be discarded from this manuscript.

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