

Interactive comment on “Effect of ocean acidification on the benthic foraminifera *Ammonia* sp. is caused by a decrease in carbonate ion concentration” by N. Keul et al.

Anonymous Referee #1

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GENERAL COMMENTS

The manuscript by Keul and others presents an interesting approach to OA research on an important group of calcifying organisms. The authors need to be reminded, however, that results on one species (an undescribed / unnamed one at that!) can not be extended to the entire group, as they currently assert in the abstract and elsewhere. The title does not reflect the content of the manuscript, which includes a lengthy review of earlier publications. If, as stated on page 1158 lines 7-9, “A thorough comparison of these studies is beyond the scope of this paper and should be addressed in a proper review paper” is true, then what is the point of reviewing all prior publications

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on foraminifera and OA, and including a multi-page table? Personally, I think a review paper and a new scientific contribution should be separate.

The statement on pg 1154 (line 8-10) that pCO₂ in the two manipulation methods were “slightly different” is a major understatement. In reality, Table 1 shows the lowest paired treatments were 61 and 217 uatm, which differ by more than a factor of three. That exceeds a “slight” difference. Plus, the 61 value differs from nominal values by a very large amount. The next higher values (479 and 396) both differed from the nominal target by quite a bit also.

Page 1158 states that “In order to address the effect OA might have on foraminifera it is crucial to develop a process understanding of the observed responses.” This statement is awkward and requires clarification. Further, it is unclear why it is supposedly imperative to distinguish between the two processes. The authors are urged to better justify the study. Just because the two parameters can be varied independently (more or less), why do it?

It is quite surprising that the authors conclude (page 1163 line 20-21) that an increase in SNW and growth rate was caused by increasing CO₂- especially given the paragraph labeled 3.2, where the authors state that “In general, we observed a high variability in final shell length and weight among specimens. Consequently linear regression of growth characteristic (final length and weight, and factors such as SNW and growth rates derived thereof) versus carbonate chemistry parameters yields low R² values in general.” These are contradictory statements that require clarification.

SPECIFIC COMMENTS

When relevant, the carbonate saturation state type should be noted. For example page 1149 lines 1-3 could be more specific (aragonite vs. calcite saturation states).

Lines 24-26 page 1149 must specify that the observations are for one species only and may not apply to other foraminiferal species.

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Given cytoplasm of other foraminiferal species are also orange colored, how do the authors know all their specimens were this one particular species of Ammonia?

What do the authors intend by reporting length for a species that is round? Do the authors intend diameter? Were tests weighed individually (line 21 page 1152)?

The rationale for “The factor 100 was added to enhance readability of SNW and growth rates” is unclear (line 18 page 1153).

It is not clear where the data are for section 3.2. Is the data discussed in section 3.2.2 shown in Figure 3? Most readers would expect that discussion about Figure 2 would appear in the text before discussion on Figure 3. This is not the case (pages 1155-1156).

The discussion on page 1157 is a peculiar mix of data and literature review.

Where is the data showing that Ca²⁺ is constant in all treatments (page 1161 line 5)?

Figure 1 is misleading because only nominal pCO₂ is listed, not the actual values, which vary wildly from the nominal values (Table 1).

Regarding Figure 3, what happens if growth rate was assessed without dividing the populations into size classes? Is it valid to break this dataset into size classes? The divisions are completely arbitrary.

TECHNICAL CORRECTIONS

Line 2 page 1148 is poor grammar (and makes little sense). It would be better to write “. . .by the oceans; such uptake causes surface ocean pH. . .”

Line 6 page 1148 should read “compiled a state-of-the-art review of OA effects. . .”

Line 9 page 1148 should read “process-based” (same for line 14 same page) and “Additionally, the benthic. . .”

Line 11 of page 1148 should read “carbonate ions were varied. . .” (not “where”).

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The sentence spanning lines 12-13 on page 1148 is awkward nonsense (“the parameter of the parameter”).

The sentence spanning lines 15-17 on page 1148 must be divided into at least two sentences, to read “We argue that [CO₃²⁻] is the parameter affecting foraminiferal size-normalized test (shell) weights (SNW) and growth rates. Based on the presented data, we can confirm the strong potential of Ammonia sp. SNW as a. . .” You can NOT assert that this Ammonia species will respond like all other foraminifera!

Line 20 page 1148: remove “the” to read “. . .start of industrialization. . .”

Line 25 page 1148L “. . .by the end of this century. This decline is. . .”

Line 1 page 1149 should read “saturation state, biogenic. . .”

Line 2 page 1149 should read “and foraminifera is expected. . .”

Line 3 page 1149 should read “generally expected that coral. . .”

Line 17 page 1149 should read “system that causes the effects.”

Line 19 page 1150 notes 8 treatments, yet only 2 are listed in the following bulleted list. Clarify.

Line 21 page 1150: please define “TA”.

What is a “pH-stable-manipulation”? First, grammatically, it should be “pH-stable manipulation”. Second, that phrase is an oxymoron (stable and manipulation).

The term “setup” is colloquial and should be avoided (appears at least twice on pg 1151).

Dunaliella does not exist. Perhaps the authors intend Dunaliella.

Line 3 page 1152 should read “Dickson’s CRMS”

Line 4 page 1152 should read “water, Marine Physical Laboratory. . .”

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Line 13 page 1152 needs an author name (“(1987)”).

Line 15 page 1152: spell out concentrated.

Line 14 page 1153 should refer to “wall thickness” not chamber thickness.

When referring to the statistical tests employed, it is “post-hoc tests” not “post hoc tests”.

What is meant by “means exactly differed” (line 26 page 1153)?

Is “quasi-constant” a word (page 1154 line 16)?

Page 1157 line 1 should read “have been documented that range from. . .”

Species specific effects should read “species-specific effects” (multiple places).

Define “normal” on page 1158 line 2.

Page 1158 line 27: please explain to which “the latter” refers.

Page 1159 line 8 should read “. . .of growth rates is . . . lower than. . .” (omit “with”).

Species names need to be italicized in the references.

Table 2 should be chronological and updated to include recent publications.

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