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Biogeosciences Discussions

Interactive comment on "Net loss of $CaCO_3$ from coral reef communities due to human induced seawater acidification" *by* A. J. Andersson et al.

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This paper is interesting, concise, and well organized. It describes methodology used to measure net ecosystem calcification (NEC) in a continuous flow system, and how the method was used to measure the effects of ocean acidification on NEC in coral mesocosms.

Overall, I like the paper and with some minor to moderate revisions, it should be published. I have a few comments for improving the paper below. I did stumble with some clumsy wording in a few places, and I recommended changes where I could. Otherwise, the objectives, methods, results and discussion are otherwise well-organized and mostly clear. I address below (1) general comments on the manuscript; (2) specific and technical comments; and (3) for completeness, a quick checklist of the specific

evaluation criteria identified by Biogeosciences.

1. General Comments

The main conclusion of the paper is that NEC in these coral mesocosms became negative (e.g. net dissolution) at conditions simulating pCO2 levels that are double the ambient conditions (in the experiments, ambient varied diurnally between about 300-800 ppm and double ambient about 650-1600 ppm). This is a well controlled study that clearly illustrates that dissolution rate can exceed calcification rate even when the aragonite saturation state is well above 1. This value is also consistent with other studies (one of which should be mentioned that found a similar result: Langdon et al. 2000 found that in the Biosphere 2 experiments, dissolution exceeded calcification at an aragonite saturation state of 1.7; note this study was similarly rich with high-Mg calcite secretors). See also comments below related to the discussion on page 2173.

The big 'missing link' for me was a comparison of the NEC calcification rates derived in this study with calcification rates derived from other methods in the same study that are already published (e.g. Jokiel et al. 2008). If Montipora capitata and rhodoliths were quantitatively the most import calcifiers in the mesocosms, couldn't the authors compare the NEC rates from the seawater chemistry measurements with the calcification rates reported for M. capitata and rhodoliths from the buoyant-weighing technique (although I understand that buoyant weighing of rhodoliths was not routine)? Of course there is calcification by other organisms, but the residual values (difference between the two techniques) may reflect that. If there are reasons why this comparison cannot be made, they should be stated.

The authors talk about high-Mg calcite and it's variable but generally higher solubility relative to aragonite. This appears to be a major factor affecting the relative calcification versus dissolution in the mesocosms. It may therefore be a major factor determining the balance between calcification and dissolution in the real world. But this point could be much better addressed in the discussion.

2. Specific and Technical Comments

2164 line 4: suggest “serious concerns regarding its adverse effects on corals.”

lines 21-23 suggest “A decrease in CO32- concentration produces a drop in the saturation state (Ω) of carbonate minerals in seawater, which . . .”

line 15: suggest rewrite “carbonate dissolution rate has received relatively little attention compared to the effect on the ability of organisms to calcify. . .”

2165 line 17: Regarding “it is more or less certain that carbonate dissolution will increase in response to ocean acidification.” Should back up with reasoning and/or references (e.g., explain that dissolution does occur even with saturation state of water column is > 1, because of variability at smaller scales, etc.)

lines 19-25: This paragraph is one sentence and it is cumbersome. Suggest a rewrite.

2166 line 4 and line 18: is “plain tap water” the same as the seawater pumped from off the reef, or is it something else? Please clarify the two.

line 8: suggest rewrite: “The remaining three mesocosms were maintained at a chemical state (with respect to the same parameters) anticipated under a doubling of ambient seawater pCO2 conditions.”

2167 lines 15-30: The discussion of using the NBS scale. While the relative change is what your own study is addressing, a short explanation could be made here as to why the NBS scale is “not ideal,” and under what circumstances your data should NOT be used. That is, the absolute values should not be used in comparison with others' results. In order to avoid such confusion, a note should also be made to that effect in Table 1.

2168 line 5: “Most important calcifying component” – by what standard? Quantitatively? Most surface area? Highest rates per surface area? . . .

2169 The cyanobacterial mats and high ammonium story is somewhat confusing. You

state that the higher ammonium levels in the treatment tanks may be due to the fact that walls in two of the tanks had extensive cyanobacterial mats. But then you state that the mats occurred in both control and treatment tanks. I surmise that the statistical difference in ammonium concentrations in the control and treatment tanks are simply a result of the randomness of these mats being in some tanks and not in others, and that they just randomly occurred in the treatment tanks more often? Cyanobacterial mats in reef environments are capable of calcification (there is extensive literature on this; e.g. stromatolites). This may or may not be important in your study, but since they did come and go in the mesocosms, I thought it was worth mentioning here.

2170 lines 24-28: This reads as if the “marginal positive calcification” at night was an unexpected result and it is too strongly addressed here. Other studies have shown that systems can and do continue calcification at night (e.g. Gattuso et al. MEPS 1996), so this description of whether the marginal positive calcification was actually marginal dissolution does not seem like an important point.

2171 paragraph starting with line 16: This paragraph makes many different points (respiration, energy partitioning, light versus dark calcification, bioerosion, the fact that corals continued to calcify, etc) and it comes off as rambling with the overall point of the paragraph lost. This needs to be better organized. You might want to include a reference to Schneider and Erez (Limnol. Oceanogr. 2006) when talking about the light/dark differences in calcification.

lines 18-20: suggest rewrite: “During daytime, community primary production consumes CO2 at a greater rate than community respiration produces it. This raises the seawater saturation state of carbonate minerals. . .

line 30: suggest deleting “if the seawater becomes undersaturated with respect to a particular carbonate mineral phase.”

2172 line 8-9: delete “Evidence of” and delete “producing”

line 27: change “seawater” to “sea level”

2173 lines 14-21. The authors recognize that “Other investigators have attempted to define a critical threshold value with respect to either pCO2 or [CO32-] when dissolution will exceed calcification in different tropical and subtropical coral reef environments (Yates and Halley, 2006).” But he dismisses the chance to compare his results with those other values. While the authors point out that “such threshold values will vary significantly from environment to environment as a result of a myriad of factors that are difficult to account for accurately and depend on the temporal and spatial scale being considered,” I still think the reader would benefit by seeing how his results compare with the range of values reported elsewhere.

3. Biogeosciences Evaluation Criteria

Does the paper address relevant scientific questions within the scope of BG? Yes.

Does the paper present novel concepts, ideas, tools, or data? Yes.

Are substantial conclusions reached? Yes, but they could be expanded somewhat (my suggestion to quantitatively compare these alkalinity anomaly-derived calcification rates with those form the buoyant weighing data previously reported for this study).

Are the scientific methods and assumptions valid and clearly outlined? Yes.

Are the results sufficient to support the interpretations and conclusions? Yes.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, particularly when combined with previous papers describing this study.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Proper credit is mostly provided, but could be improved (some suggestions are made above).

Does the title clearly reflect the contents of the paper? The title implies that the exper-

imental results on coral communities in mesocosms can be applied to coral communities everywhere. I think the implication is fair, but strictly speaking, the title may be too broad.

Does the abstract provide a concise and complete summary? Yes.

Is the overall presentation well structured and clear? Yes.

Is the language fluent and precise? Mostly (recommendations for minor improvements are made above).

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes. But I suggest a careful check that “calcification” versus “calcification rate” are properly distinguished.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No.

Are the number and quality of references appropriate? Yes.

Is the amount and quality of supplementary material appropriate? there is no supplementary material.

Interactive comment on Biogeosciences Discuss., 6, 2163, 2009.