

Response to A. Ridgwell comments:

We thank A. Ridgwell for his useful comments.

Comment 1: *"What was the mean carbonate saturation of the water in the two treatments? (Or did I derp completely on reading your paper and miss it?) Actually, it would be help-ful/interesting to know all of the carbonate chemistry parameters."*

Response 1: The aragonite saturation state was ~3.5 in the ambient treatment and ~ 1.6 in the high pCO₂ treatment. As suggested a table (Table 1) showing the carbonate chemistry is now included in the revised manuscript.

Comment 2: *"Also – do you have any sense for whether the ambient carbonate chemistry of the corals/algae 'downstream' in the flume, was more buffered compared to individuals growing upstream? Or is the flow sufficiently fast that the chemistry is effectively uniform along the length of the flume?"*

Response 2: We did not measure any significant change (~ > 0.01 pH unit) in pH between upstream and downstream. It was likely due to the relatively fast flow and the limited length of the working section (5 m).

Response to A. Tribollet comments:

Comment 1: "Dear Steve and co-authors, Your article is very interesting and I hope that it will be published soon in *Biogeosciences*. I have a few questions and comments though. I/ M&M and Discussion: Could you please precise if rubble and dead reef/corals were present in flumes in addition to sediments and live coral colonies? Rubble and dead reef areas are indeed an important component in reefs (see comment below)."

Response 1: Dear Aline, thank you for your useful comments. Rubble was present in the flumes and represented ~ 5% of the cover. This information is now included in the revised manuscript, lines 87-89 "In addition to corals, 6% of the surface was covered by crustose coralline algae (66% *Porolithon onkodes* and 33% *Lithophyllum flavescens*), and 5% by rubble (dead coral skeletons)."

Comment 2: "Conclusion: Bioerosion process does not result solely from the mechanical activity of organisms. It does include biogenic dissolution by microborers (e.g. review by 2008) and sponges for instance (Zundelevich et al. 2007; Wisshak et al. 2012, 2013). Please be more precise in the discussion when reporting "dissolution vs bioerosion processes". What do you mean by "dissolution"? When measuring net dissolution rates at the scale of a reef community, chemical dissolution (derived from the bacterial activity and chemical conditions) and biogenic dissolution are quantified simultaneously. Note that biogenic dissolution by microborers concerns all carbonate substrates including sediments, shells, live and dead corals, live and dead CCA, etc... This process cannot be ignored and is especially efficient in hard reef substrates (compare to sand). Similarly the whole process of bioerosion (i.e. mechanical abrasion by grazers, dissolution and abrasion by worms, bivalves, sponges and dissolution by microborers) is more intense in hard dead substrates than in live substrates and sand. The main agents of biogenic dissolution in reefs are microborers and OA should increase rates of biogenic dissolution by 50% or more by 2100 (Tribollet et al. 2009). Recent studies confirmed this positive effect and others showed a similar effects on boring sponges (Wisshak et al. 2012, 2013). Thus, part of the process of CaCO₃ dissolution is missing if no rubble/dead reef pieces were added in flumes. I suggest to precise in the discussion that (a) part of the process of [biogenic] dissolution was overlooked as dead reef/rubble were not studied (if considered, they would amplify net dissolution rates measured during the day and especially at night) and (b) the ratio between living coral cover, sand AND dead reef areas will influence greatly the carbonate budget under OA conditions.

Hoping that these comments will help. Best, Aline"

Response 2: Thanks Aline for this useful comment. The discussion has been reformulated to take into account this comment, lines 272-290 "During a mesocosm experiment, Dove et al. (2013) also demonstrated that a pH of 7.7 caused a change in sediment granularity to favor small-grained (i.e., ≤ 1 mm) sediments as a result of dissolution or increased bioerosion of larger grains. In this case, bioerosion was more likely than dissolution, as dissolution would favor a loss of the smallest grains as a result of their higher surface area to volume ratio. Size-frequency distribution of sediment grain was not different between treatments at the end of our incubations and therefore is unlikely to have affected

the treatment effects we detected. Sensitivity of coral reef communities to dissolution has been shown previously for communities constructed in mesocosms in Hawaii, where dissolution ($-3.6 \text{ mmol CaCO}_3 \text{ m}^{-2} \text{ h}^{-1}$) was detected at night under conditions of double ambient $p\text{CO}_2$ (Andersson et al., 2009). In this case, dissolution was attributed to the thin layer of sediment that accumulated at the bottom of the mesocosms (Andersson et al., 2009).

*In addition to chemical dissolution occurring in the communities constructed in the present study, we cannot exclude the possibility that at least some of the apparent community dissolution was caused by enhanced bioerosion, which for example previously has been shown to occur when blocks of *Porites lobata* are incubated under $750 \mu\text{atm } p\text{CO}_2$ for 3-month (Tribollet et al. 2009). In future work it will be important to census the fragments of coral and rock to quantify the presence of bioeroders and their relative contribution to dissolution under ambient and OA conditions."*

As indicated previously, rubble was actually present in the flumes.

Response to Prof Eyre comments:

Comment 1: *"I read this manuscript with great interest. It was nice to see an even treatment of the sediments along with the coral community. I have a few comments I hope you find useful. If you have dissolved oxygen data it would be good to know if benthic productivity and respiration changed in the high pCO₂ treatment. This would give some insight into the processes driving sediment dissolution. Was it due to a change in the sediment p/r (metabolic dissolution) or was it due to the reduced saturation state in the overlying water (environmental dissolution)."*

Response 1: O₂ changes were measured during the incubations but we did not detect a significant change of pCO₂ on respiration. This information is now included in the manuscript lines 265-268 *"Increased dissolution of sediments at high pCO₂ likely was caused by the reduction of the seawater saturation state in the flumes, as we did not detect any difference in respiration and photosynthesis under elevated pCO₂ (results not shown) that could also affect sediment dissolution (Andersson and Gledhill, 2013)."*

Comment 2: *Was there any surface structure in the sediments in the flume (e.g. ripples) that would drive advective flow?*

Response 2: We did not try to recreate surface structures on the sediment but let surface structure of the sediment to naturally regulate itself under the simulated flume water flow and placement of corals.

Comment 3: *Can you provide more details on the sediments such as grain size, porosity, permeability, organic matter content, carbonate mineralogy etc.*

Response 3: Data on sediment grain size are now included in the manuscript, lines xx in the Materials and Methods lines 155-160 *"Sediment grain size of each flume was analyzed in triplicate using sediment sieves. Three vertical cross sections of sand (~ 600g) were collected from each flume sediment chamber and dried at 60 °C to remove moisture. Sand then was sieved through five separate sediment sieves (149 μm, 420 μm, 840 μm, 3360 μm) yielding six size class fractions for each flume (n = 3)."*, and in the Results lines 196-199: *"Sediment grain sizes in the flumes were similar between flumes and fractionated (by weight) to 5.3 ± 0.5% < 149 μm grain size, 56.5 ± 1.4% > 149 μm and < 420 μm, 25.9 ± 0.4 % > 420 μm < 840 μm, 10.1 ± 0.5% > 840 μm and < 3360 μm, and 2.2 ± 0.9% > 3360 μm."* Unfortunately we did not measure any other sand parameter.

Response to S. Dupont comments

sam.dupont@marecol.gu.se Received and published: 21 August 2014

Comment 1: *"First, I need to highlight the fact that I am not a coral biologist and have no practical experience with the used methods. As a consequence, my report focusses on general experimental procedures.*

The most interesting part of the manuscript is the use of artificial "controlled" communities to evaluate the impact of ocean acidification on net calcification. This is an under-used methodological approach and a nice alternative to natural analogs and/or in situ mesocosms with the potential to better understand the mechanisms behind the response at the community level.

From my limited expertise, I do not see any major problem with the manuscript and only have a few comments/questions:"

Response 1: We thank Sam Dupont for his useful comments and we are glad that someone outside of the coral scientific community finds our approach interesting.

Comment 2: *- Can you provide some information on the natural variability experienced by these communities in the field? That would help putting the tested scenarios into context.*

Response 2: Back reef communities in Moorea are exposed to relatively constant pH with diel variations of ~ 0.1 unit. This information is provided in the manuscript lines 122-124" *To match the natural diel variation in pH in the back reef of Moorea (Hofmann et al., 2011; Comeau et al., 2014a) pH was maintained 0.1 unit lower at night (from 18:00 to 6:00) than during the day."*

Comment 3: *- Please provide a Table with the seawater chemistry over the course of the experiment. Also include statistics to demonstrate that you have significant differences between treatments. Provide graphs if necessary to illustrate the variability.*

Response 3: As suggested a table (Table 1) presenting the carbonate chemistry is now included in the manuscript. Statistics on the pCO₂ treatments are now provided lines 170-176" *Mean pCO₂ in the four flumes during the 8-week incubation was $456 \pm 21 \mu\text{atm}$ and $451 \pm 21 \mu\text{atm}$ in the ambient treatments, and $1329 \pm 28 \mu\text{atm}$ and $1306 \pm 41 \mu\text{atm}$ in the high pCO₂ treatments (\pm SE, $n = 42$). pCO₂ differed between treatments (repeated measure ANOVA, $F_{1,232} = 734.38$, $p < 0.001$), but there was no difference within treatments ($F_{2,232} = 0.16$, $p = 0.852$). Communities were maintained in conditions within the flumes that were super-saturated with respect to aragonite, as $\Omega_{\text{arag}} \sim 3.5$ under ambient conditions, and ~ 1.6 in the high pCO₂ treatment. "* However, we do not believe that a graph representing the carbonate chemistry will be insightful since variability of pCO₂ was very limited (se $\sim 30 \mu\text{atm}$, Table 1).

Comment 4: *Full statistics should be better presented in the results (provide model, F and p values for tested parameters (pCO₂ and replicate).*

Response 4: As suggested more details on the statistics are now presented.

Comment 5: - *At the end of the Results, it is claimed that coralline algae experienced high mortality. Information on "health" of all tested species should be provided and presented at the beginning of the result section.*

Response 5: Thank you for these recommendations, a "health" section is now included in the Results, lines 177-182 " *No Pocillopora spp. and Montipora spp. colonies died during the 8-week treatments, but 10% of the Porites pooled across flumes died by the end of the experiment, regardless of treatment, because of an outbreak of corallivorous nudibranchs feeding on this taxon (Phestilla spp.). Coralline algae (~ 70%) died at the end of the incubation, which was likely due to sediment abrasion. No difference in mortality or bleaching was observed between treatments for corals and calcified algae.*"

Response to Andreas Andersson Comments:

General comments: *"Comeau and colleagues have carried out a very nice experiment showing how net CaCO₃ sediment dissolution in coral reef communities accelerates under a potential ocean acidification scenario. The effect of OA on CaCO₃ dissolution has been largely ignored with a few exceptions; thus, this is a timely and important study. The study and the manuscript are nicely done and deserve publication in my opinion, but the manuscript needs significant clarification and expansion in several areas that I point out in the detailed comments below. The authors have also overlooked a few papers that have done similar work or discuss the issue of CaCO₃ dissolution and ocean acidification on coral reefs (e.g., Andersson et al., 2009; Andersson and Gledhill, 2013; especially the first one is relevant in the present context). With the risk of being self-promoting, I strongly believe the results and discussions of these previous papers will be beneficial to the authors. I hope the authors will find the following comments useful in order to improve the current version of the manuscript."*

Response to General Comment: We thank Andreas Andersson for his very useful comments. As suggested the reference Andersson et al. 2009 and Andersson and Gledhill, 2013 are now included in the introduction. Andersson et al. 2009 is also discussed lines 252-261 *"Rates of calcification in the present study under ambient conditions also are similar to the 7.9 gCaCO₃ m⁻² d⁻¹ reported by Andersson et al. (2009) for a reef community from Kaneohe Bay (Hawaii) that was assembled and incubated in mesocosms. However, while community calcification was still positive under high pCO₂ in the present study, Andersson et al. (2009) measured negative calcification (i.e., net dissolution) in their coral reef communities incubated at a pCO₂ twice that of current ambient values. The differences between the present study and that of Andersson et al. (2009) may be due to methodological effects. Andersson et al. (2009) manipulated pH through acid additions (we used CO₂ bubbling), and also used a different assemblage of species and sediments in dissimilar proportions compared to the present study."*

Detailed comments:

Comment 1: *"P12324, Line 15: I suggest change sentence to ". . .switch to net dissolution as pCO₂ increases and CaCO₃ saturation state decreases. . ." as dissolution is controlled by the seawater saturation state with respect to carbonate minerals and not the pCO₂."*

Response 1: Done.

Comment 2: *"P12325, line 7-8: See also papers by Andersson et al., 2009 and Jokiel et al. 2008."*

Response 2: As suggested these papers are now included in the manuscript.

Comment 3: *"P12325, line 20-22: Yes, dissolution is taking place at present conditions on coral reefs but mainly in sediment pore-waters or in microenvironments, where the seawater carbon chemistry is different compared to the overlying water column (see review by Andersson and Gledhill, 2013). Your interpretation of the Andersson et al.*

(2007) is somewhat incorrect. Yes, these observations reported net dissolution at present time but under elevated surface seawater carbon chemistry conditions, i.e. not conditions that are typically observed on coral reefs surface seawater today, but rather similar to conditions observed in sediment pore waters."

Response 3: This section has been reformulated, lines 52-58 " *In addition to corals and macroalgae, it is important to incorporate sediments in OA experiments, as this component of reef ecosystems may be sensitive to decreasing pH (Cyronak et al. 2013a, b; Andersson et al., 2009). Dissolution occurs on coral reefs in sediment pore-waters, or in particular microenvironments where $p\text{CO}_2$ is elevated due to biological activity (Andersson and Gledhill, 2013). Observations in Bermuda have shown that the dissolution of Mg-calcite sediments occurs in a location with seawater $p\text{CO}_2$ naturally elevated to values expected by the end of the century (Andersson et al., 2007).* "

Comment 4: "P12325, line 23-24: *Several additional studies have proposed a threshold of when different benthic communities or coral reefs in general will switch to net dissolution (e.g., Yates and Halley, 2006; Andersson et al., 2005; 2007; 2009; Silverman et al., 2009; as well as others). The exact threshold obviously differs and depends on a number of factors discussed in Andersson et al. 2009 (see section 4.3).*"

Response 4: As suggested these references are now discussed lines 61-64" *Increasing $p\text{CO}_2$ likely will lead to increasing dissolution and decreased precipitation of calcium carbonate, resulting in coral reef community calcification changing from net precipitation to net dissolution (Yates and Halley, 2006; Silvermann et al., 2009; Andersson et al., 2009).* "

Comment 5: "P12326, line 1-2: *what are these $p\text{CO}_2$ values? Mean $p\text{CO}_2$? It would be useful to show seawater saturation state with respect to aragonite and also the variability, e.g., +/- STD or SE.*"

Response 5: A table (Table 1) showing the carbonate chemistry is now included in the manuscript. Statistic regarding the difference in $p\text{CO}_2$ are also provided lines 170-176 " *Mean $p\text{CO}_2$ in the four flumes during the 8-week incubation was $456 \pm 21 \mu\text{atm}$ and $451 \pm 21 \mu\text{atm}$ in the ambient treatments, and $1329 \pm 28 \mu\text{atm}$ and $1306 \pm 41 \mu\text{atm}$ in the high $p\text{CO}_2$ treatments (\pm SE, $n = 42$). $p\text{CO}_2$ differed between treatments (repeated measure ANOVA, $F_{1,232} = 734.38$, $p < 0.001$), but there was no difference within treatments ($F_{2,232} = 0.16$, $p = 0.852$). Communities were maintained in conditions within the flumes that were super-saturated with respect to aragonite, as $\Omega_{\text{arag}} \sim 3.5$ under ambient conditions, and ~ 1.6 in the high $p\text{CO}_2$ treatment.* "

Comment 6: "P12326, line 23-25: *What type of stratification are you referring to? Sediment stratification? Chemical gradients? Did the sample collection destroy the existing stratification and was this actual reestablished after 4 days. Please clarify.*"

Response 6: Collection likely disturbed the natural chemical gradient existing within the sediment. Since we did not have additional boxes we could not check if gradients were

fully re-established after 4-d. This section has been modified lines 93-98 " *Sediments were collected from the lagoon on the north shore, ~ 200 m from the reef crest, at 2-m depth using 24 custom made boxes (0.4 × 0.3 × 0.3 m). Sediment boxes were inserted into the sediment and left in situ for 4 d to allow chemical stratification in the sediment to re-establish before transferring the boxes to the flumes. It was not possible to subsample these boxes to quantify the stratification of the sediment, and therefore we assume that 4 d was adequate for stratification to be re-established.*"

Comment 7: "P12327, line 14-16: *This sentence needs clarification. I assume you refer to sea- water pCO₂ and that the pessimistic scenario refers to surface seawater pCO₂ in the tropical(?) open ocean. However, coral reef seawater pCO₂ is currently sig- nificantly different than open ocean pCO₂ and will likely be different by the end of the century as well. See for example continuous seawater pCO₂ measurements at <http://www.pmel.noaa.gov/co2/story/Coral+Reef+Moorings> which shows that many reef experience seawater pCO₂ significantly higher than the atmosphere at present time. Thus, I think you need to clarify that the scenario you are referring to refers to the open ocean and that coral reefs can experience a radically different seawater pCO₂.*"

Response 7: Interestingly pCO₂ levels in the back reef of Moorea are similar to open ocean, however we do agree that it is not the case with most coral reefs. This sentence has been modified lines 116-120 " *As the pCO₂ level in the back reef of Moorea is close to open-ocean and current atmospheric values (e.g., Comeau et al. 2014a), pCO₂ levels for the incubations were chosen to match ambient pCO₂ (~ 400 μatm) and the pCO₂ expected in the atmosphere by the end of the present century under a pessimistic scenario (Representative Concentration Pathway 8.5, ~1300 μatm, Moss et al., 2010).*"

Comment 8: "P12327, line 14-19: *How did you maintain constant pH with your pH stat? What acids and bases were you using? What was the variability around the desired pH level?*"

Response 8: The pH-stat was controlling the bubbling of pure CO₂ or CO₂-free air. This information is now included, lines 120-124 " *pCO₂ in the flumes was controlled using a pH-stat (Aquacontroller, Neptune systems, USA) that actuated the bubbling of either pure CO₂ or CO₂-free air into the seawater. To match the natural diel variation in pH in the back reef of Moorea (Hofmann et al., 2011; Comeau et al., 2014a) pH was maintained 0.1 unit lower at night (from 18:00 to 6:00) than during the day.*"

Variability around the desired pH is now provided in Table 1.

Comment 9: "P12327, line 17-19: *Do you have data supporting that the diel variation is 0.1 pH units in the back reef of Moorea? From my own observations in other coral reef environments I have seen that the diel amplitude can be quite variable from day to day, but also vary greatly across space. Please clarify.*"

Response 9: While daily and seasonal variations are occurring, data acquired so far tend to support an average diel variation of 0.1 unit in the back reef of Moorea. Two references supporting the 0.1 pH unit diel variations in the back reef of Moorea are now included

lines 122-124 "*To match the natural diel variation in pH in the back reef of Moorea (Hofmann et al., 2011; Comeau et al., 2014a) pH was maintained 0.1 unit lower at night (from 18:00 to 6:00) than during the day.*"

Comment 10: "*P12327, line 20-27: What were the accuracy and precision of your measurements and how did you verify it?*"

Response 10: More details are now provided lines 125-134 "*pH was measured daily using a portable pH meter (Orion 3-stars, Thermo-Scientific, USA) fitted with a DG 115-SC pH probe (Mettler Toledo, Switzerland) calibrated every other day with Tris/HCl buffers (Dickson et al., 2007). pH also was measured spectrophotometrically using m-cresol dye (Dickson et al., 2007) at regular intervals. pH measured spectrophotometrically or using a pH electrode provided similar results with means differing < 0.01 pH unit. Measurement of total alkalinity (A_T) was made using open-cell potentiometric titrations (Dickson et al., 2007) using 50-mL samples of seawater collected every 2-3 d. Titrations of certified reference materials provided by A. G. Dickson (batch 122) yielded A_T values within $3.5 \mu\text{mol kg}^{-1}$ of the nominal value ($SE = 3.1 \mu\text{mol kg}^{-1}$; $n = 14$). "*

Comment 11: "*P12328, line 5-8: I am a little bit confused about how you used the alkalinity anomaly technique to estimate net calcification/dissolution since your experiments were done in a pH-stat. Doesn't the pH stat compensate any addition or reduction in total alkalinity? You could certainly use this compensation to estimate net calcification/dissolution, but it is currently not clear what you did and from the text it appears that you actually used the alkalinity anomaly technique. However, if this was the case and since the incubations were closed loops, TA must have been accumulating during net dissolution potentially producing a buffer effect, and the opposite could be true for times of net calcification. At some point in the ms you need to address this potential buffering effect and the effect on the seawater carbonate chemistry during the incubation. Also, when you refreshed the water every 6 hours, was this already equilibrated to the desired pCO₂ and pH levels?*"

Response 11: We believe that this confusion comes from the fact that pH was controlled by addition of pure CO₂ and CO₂-free air, but not by addition of acid and base. Thanks to comment #8, this point is now clarified. Since we used addition of CO₂, alkalinity was only affected by calcification/dissolution.

Seawater in the flumes was refreshed at regular intervals in order to limit the magnitude of the changes in alkalinity and notably the potential buffering effect due to dissolution. Changes in alkalinity during incubations were < $50 \mu\text{mol kg}^{-1}$, which corresponded to changes in aragonite saturation state < 0.1. More details are now provided in the manuscript lines 144-147 "*To maintain A_T and nutrients close to ambient levels, water in the flumes was refreshed every 3-6 h for 30 min. Regular refreshing limited changes in alkalinity during incubations to < 50-100 $\mu\text{mol kg}^{-1}$, which corresponded to variations in aragonite saturation state (Ω) of < 0.1-0.2.*"

The seawater used during the refreshing was not already equilibrated to the desired pH, but pH-stats were efficient enough to maintain seawater pH stable in the flumes during the refreshing period.

Comment 12: *"P12328, line 14-16: What was the duration between the buoyant weight measurements of corals and coralline algae? Do you mean the entire 8 weeks incubation?"*

Response 12: Indeed buoyant weight measurements covered the entire 8-week incubation. This sentence is now clarified lines 153-155 *" For both corals and algae, buoyant weight (Davies, 1989) was recorded before and after the 8-week treatments and converted to dry weight to quantify the contribution of each functional group to the calcification budget."*

Comment 13: *"P12330, line 18-20: Please clarify what you mean by “. . .disproportionate surface area to planar area relationship in corals?"*

Response 13: This sentence has been reformulated, lines 228-234 *"The small contribution of coralline algae to the calcification budget was due to high mortality perhaps leading to potential dissolution during the last weeks of the incubation. Furthermore, while the ratio of planar area to surface area for crustose coralline algae is close to one, corals have a disproportionately large surface area to planar area ratio due to their three-dimensional structure. With such a large actual surface area, the corals made a large contribution to the calcification budget of the communities assembled in the flumes."*

Comment 14: *"P12330, line 22-25: This sentence needs clarification. What do you exactly mean by “. . .the effects of OA on coral reef communities are greater than estimates obtained by summing results obtained by incubating organisms in isolation..." Do you mean that the net community calcification decrease more than net calcification by individual organisms, which thus, is partly explained by increasing dissolution (which you demonstrate). However, this does not mean that the effect of OA on an individual coral is different than the effect on a group of the same corals, which would require a different set of experiments without the sediments."*

Response 14: We fully agree with this comment and believe that we come to the same conclusion few lines later (lines 262-265) *" The discrepancy in the evaluation of the effects of high pCO₂ at the community level (the present study) versus organismic level (previous studies) was the result of dissolution of sediments that represented up to 50% of the decrease in calcification at high pCO₂."*

Comment 15: *"P12331, line 17-19: I am aware of this statement, but must admit I am somewhat hesitant in my interpretation that this represents a dissolution signal. Increasing dissolution would favor dissolution of smaller grain sizes rather than larger grains as the surface to volume ratio increases with decreasing grain size. Also, this effect would probably only be apparent in the very small size fractions of silt and clay (see for example the work by Schmalz and Chave 1963; Neumann, 1965). However, increasing*

percentage of smaller grain sizes could certainly result from increased bioerosion, which also has been shown to increase under decreasing pH. Nonetheless, this comment is just a personal reflection of this statement, but perhaps something you want to consider."

Response 15: Indeed results obtained by Dove et al. (2013) were likely due to bioerosion since dissolution effects would be more severe on smaller grain sizes. This sentence has been slightly modified accordingly, lines 272-279 "*During a mesocosm experiment, Dove et al. (2013) also demonstrated that a pH of 7.7 caused a change in sediment granularity to favor small-grained (i.e., ≤ 1 mm) sediments as a result of dissolution or increased bioerosion of larger grains. In this case, bioerosion was more likely than dissolution, as dissolution would favor a loss of the smallest grains as a result of their higher surface area to volume ratio. Size-frequency distribution of sediment grain was not different between treatments at the end of our incubations and therefore is unlikely to have affected the treatment effects we detected."*

Comment 16: "*P12337-P12339: The figures are nice and clear, but I would have liked to see a little bit more data and just not the mean results of seawater chemistry and net calcification results. In addition to the current figures I would have liked to see: 1. Seawater physical and chemical properties over time that were measured, i.e., temperature, salinity, pH and TA, as well as your calculated aragonite saturation state and pCO₂ (Also, in the text of the results section when you talk about seawater pCO₂, I think you should also mention what the aragonite saturation state was). 2. The results for the alkalinity anomaly incubations and how chemistry changed over these incubations. This information could be provided either as a supplementary table or a figure."*

Response 16:

1. Details on the seawater carbonate chemistry are now provided in Table 1. Additional details on the carbonate chemistry are now included in the Results, lines 170-176 "*Mean pCO₂ in the four flumes during the 8-week incubation was 456 ± 21 μ atm and 451 ± 21 μ atm in the ambient treatments, and 1329 ± 28 μ atm and 1306 ± 41 μ atm in the high pCO₂ treatments (\pm SE, n = 42). pCO₂ differed between treatments (repeated measure ANOVA, $F_{1,232} = 734.38$, $p < 0.001$), but there was no difference within treatments ($F_{2,232} = 0.16$, $p = 0.852$). Communities were maintained in conditions within the flumes that were supersaturated with respect to aragonite, as $\Omega_{arag} \sim 3.5$ under ambient conditions, and ~ 1.6 in the high pCO₂ treatment. "*

2. As mentioned in the Response #11, changes in alkalinity during incubations were < 50 μ mol kg⁻¹ in most of the measurements. Data presented in this paper are currently being deposited on the BCO-DMO database.

Comment 17: "*As a final comment, one of my students pointed out the following that you may want to consider: There is a major assumption that is not discussed in the paper. Granted, I have much to learn regarding net calcification, but I would think that calculating the calcification by organisms is not as simple as Net - Sediment = Coral + Algae. The assumption they are making with this calculation is that the rate of*

calcification for the sediment is the same regardless of the presence of organisms. Won't the presence (or lack thereof) of coral and algae affect the calcification rate of the sediment? Again, this could be an ignorant observation, but I would assume that the calcification rate of the sediment is altered by the presence of organisms. If I am correct, it obviously doesn't negate their work, but an acknowledgement of the assumption needs to be clearly stated."

Response 17: It is indeed one potential weakness of our study that will need to be addressed in future work. This assumption is now discussed in the manuscript, lines 299-310 "*As the decrease in calcification recorded in the present study for corals and coralline algae alone was within the range of previous studies, this supports our assumptions that calcification of macro-calcifiers is equal to the difference between net sediment calcification and net community calcification. This "subtraction method" for calculating the calcification rate of corals and coralline algae included in community experiments has some limitations, as it assumes that the calcification of the sediments and the macro-calcifiers are independent. Such interactions might occur, for example, if dissolution of the sediment would locally enhance total alkalinity that would, in turn, favor calcification by macro-calcifiers. Testing for such feedback mechanisms among the different compartments of the communities we built was beyond the scope of the present study, but it will be important to consider such effects in future experiments."*