

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Coulson A. Lantz et al.

Author comment on "Will daytime community calcification reflect reef accretion on future, degraded coral reefs?" by Coulson A. Lantz et al., Biogeosciences Discuss.,
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Reviewer Comment #1

This study presents an interesting idea that temperature-induced increases in metabolism of various non-coral calcifying organisms could offset the expected effects on coral calcification during bleaching events, and thus that ecosystem-based measurements of calcification may not fully represent disturbance to these degraded habitats. The data are interesting and are presented in this new and insightful light, but the manuscript needs clarification in many aspects, and especially further consideration of statistical power.

Methods: not enough detail is provided to understand the methods. The supplement helps, but is far too long in my view (the supplement itself is separated into the main sections normally in a paper (Methods, Results, Discussion), so it almost reads like a separate paper. Importantly, essential details to understand the study are in the supplement. For example, the description of the Eulerian approach to NEC and NEP is too brief in the main text. The reader needs to access the supplement to begin to understand what was done here. Additionally, even the supplement is lacking some details, like precisely which samples were used for upstream and downstream TA and DO, why there is a 3600/100 in the equations, how was residence time calculated (e.g., where was the current meter?), how was TA titrated, etc.

We thank Reviewer #1 for their comments. We understand the reviewer would like more of the methods in the main text and we oblige. We can provide details of the placement of the current meter and more specific information on sample collection and alkalinity analysis. The core citation for these methods (Langdon et al., 2010) details why 3600/100 is in the equations (to convert cm s⁻¹ to m h⁻¹). We will endeavour to include as much of this information as possible in the main text.

We would also clarify that thermally accelerated calcification is not the main conclusion of this paper but rather a proposed explanation, among 2 others (algal calcification, nighttime dissolution). Since the submission of this paper a publication by De Orte et al., 2021 (Unexpected role of communities colonizing dead coral substrate in the calcification of coral reefs, 2021, L&O) has provided compelling evidence for our other proposed mechanism: inorganic precipitation in dead coral substrate caused by localised algal photosynthesis. We plan to add

this citation and more back of the envelope calculations (similar to Figure 4) which mathematically explain how the domination of fleshy algae in the lagoon growing on dead coral may have masked the decline in coral calcification. Finally we will also be adding more to nighttime dissolution aspect given other recent work.

Overall, the main conclusion of this study is that 2 traditional flow metabolism approaches (Eulerian and Slack Water) did not discern differences in daytime calcification due to bleaching. We then propose 3 mechanisms in the discussion based on the available literature: thermally-accelerated calcification, algal-dead coral calcification, and increased nighttime dissolution.

Results/Discussion: given the relatively high variance and large error bars on the NEC rates, is it surprising that there were not significant differences observed before/after bleaching, especially given the low number of independent samples (days)? An assessment of statistical power would be highly useful. Of course, it shouldn't be concluded that bleaching doesn't affect NEC. Rather, this study did not reject the null hypothesis that bleaching has no effect on NEC. It's a key distinction, one that is glossed over and somewhat misinterpreted here.

Perhaps these concerns will benefit from moving more methods from the SI to the main text. There were triplicate transects for 2 different reef zones measured daily. We understand the key distinction between not rejecting the null hypothesis and will endeavour to use more statistical approaches to clarify this point. We will also employ more statistical power to show there were no significant differences (or elucidate if there are in fact are).

Throughout: more clarity needed in how quantities were calculated and exactly how each type of data was used. For example, the text section 3.2 describes satellite SST begin to accumulate heat stress in February and refers to Figure 1. But from what I understand of the caption of Figure 1, only the in situ logger data are shown in that figure.

This confusion is understood. Figure 1 displays the in-situ logger data (black line) and the accumulation of heat stress in these data (red line). The text discusses that accumulation of heat stress in the satellite data was the overall driver to begin this study. This can be clarified.

Need to describe statistical approach in main text.

Supplemental material discussion of statistical methods will be moved to the main text.

Throughout: it seems odd to say "community NEC" — usually, it's either "net community calcification" or "net ecosystem calcification"

We understand this point. The word "community" is used to help differentiate from organism-level calcification. We can change the nomenclature to net ecosystem calcification and net organism calcification.

Throughout: need to decide if there is a space between numbers and % symbols or not.

Will edit and add a space between numbers and %.

Introduction: the discussion of existing literature is good and thorough, but perhaps there should be clearer differentiation between the effects of ongoing bleaching vs bleaching-induced mortality. Currently, the text describes these similarly, but it seems likely there

would be different NEC responses to bleached (but living) corals as opposed to dead corals.

Line 88: Didn't Kayanne also observe a decline in NEC after bleaching in Palau though?

This is correct, they found differing responses at different reefs. We recognize this reviewer's work and others earlier in the introduction where we established that the expected response is a decline in NEC in response to bleaching (L54 – 65). We can add the Kayanne Palau results to this evidence as well. The purpose of the text in L88 is to highlight examples in the literature that have curiously not shown a decline in NEC.

Lines 88-89: again, bleaching vs bleaching-induced mortality seem to be conflated. Kayanne describe changes after bleaching-induced mortality, which the present text is comparing to NEC during a bleaching event (but with still-living corals). More clarity is needed about the difference between the two.

We can add into the text the words "after" instead of "during" for the bleaching event. We understand the reviewer's point of conflating NEC during bleaching vs. after bleaching, but studies are so limited of either situation that we instead use the introduction to simply introduce any and all studies with NEC and bleaching through the lens of described coral cover. This critical difference between during bleaching vs. after bleaching is discussed on L330 in the context of the results here. We will endeavour to clarify better measurements that are during or after bleaching events.

Line 134: should state how many points were used on each image, and if the points were randomized.

100 points, not randomized, its a standard grid. This will be added to text.

Line 152: "using using"

Line 232: delete "extremely"

Noted will change these typos. Thank you

Figure 4: why not just have 2 bars for each temperature?

The figure is shown as one bar to illustrate the relative change in each benthic category as temperatures increased.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2021-61/bg-2021-61-AC1-supplement.pdf>