Biogeosciences Discussions

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Comment on bg-2021-61

Thomas DeCarlo (Referee)

Referee comment on "Will daytime community calcification reflect reef accretion on future, degraded coral reefs?" by Coulson A. Lantz et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-61-RC1, 2021

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.

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.

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.

Need to describe statistical approach in main text.

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

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

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?

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.

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

Line 152: "using using"

Line 232: delete "extremely"

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