Reviewer Comment #2

General Comments

Overall, the theory of the study is great, and inter-disciplinary work like this is great to see. However, the linking of study elements (for example, quantification of the relationship between Symbiodiniaceae densities or photosynthetic yield and NEC) could be more deeply explored. Importantly, I find the methodology lacking necessary information to determine the validity of the results and many facets of the methodology and further analyses require justification. In the supplementary material, the equations used to calculate metabolic rates are not well defined and in their current state may be incorrect. The authors should take care with the accuracy of information presented from the literature and with the appropriateness of citations to fit the manuscript's narrative. I find the results interesting, but their main point seems oversold and broadly declarative with the data that is presented.

We thank Reviewer #2 for their comments. Similar to other reviewer comments, moving methods text from the supplemental material to the main text will help address these concerns. If the reviewer is referring to the thermally-accelerated calcification hypothesis as the "main point", we would invite the editor to see our explanation provided for reviewer #1 clarifying this is not the main conclusion of the paper but 1 of 3 proposed hypothesis to stimulate thought around this subject.

Specific Comments

Line 67-69 this information is incorrect. The bleaching event year is 2016, and the 2016 survey by Pisapia et al. cited here occurred after the bleaching event occurred in 2016. The 2018 survey showed an increase in NEC compared to 2016, but was still depressed related to pre-disturbance rates (see Abstract and Table 1 in cited paper).

Thank you for catching this. This is a typo, the text should read 2016. This text is straddling two studies (Pisapia et al., 2019 and McMahon et al., 2019).

I disagree with the sentence from Line 74-76: A community transitioning to algal dominated from a coral dominated community would likely demonstrate changes in NEC and NEP, as indicated by many prior studies. The citation here (Courtney et al., 2018) is inappropriate because this paper does not indicate that NEC will become less effective with reef state transitioning. Rather, the authors state ". . . bleached coral reefs that recover quickly likely experience ephemeral reductions in reef NEC while systems shifting to alternative non coral-dominated states are likely to face lasting decreases in NEC." Reduced or low NEC, regardless of the cause or the dominant benthic class, is what is useful when investigating reef state.

The purpose of this text is to explain that if anthropogenic stressors continue to increase the ratio of algae:coral on reefs, then NEC may no longer be representing reef accretion by hermatypic coral. This idea is proposed by Courtney et al. 2018 in the discussion:

In contrast, measurements of NEC at Shiraho Reef, Japan did not change during the September 1998 bleaching event, where 51% of the total 7.1% total coral cover was bleached compared to a recovery survey conducted in September 1999 with 6.7% total coral cover and no bleaching observed (Kayanne et al. 2005). Kayanne et al. (2005) hypothesized

that calcification by living bleached corals, calcifying algae, and benthic foraminifera may have compensated for bleaching-induced losses in NEC at Shiraho Reef. Indeed, the dominant calcifiers of coral reefs include corals, red coralline algae, molluscs, green calcifying algae, and benthic foraminifera (Montaggioni and Braithwaite 2009), but their relative contributions to coral reef CaCO3 budgets and how these change under different reef states are uncertain. This raises the question and need to further quantify the relative importance of contributions by other calcifiers to coral reef NEC especially for low coral cover (< 10%) and bleached coral reefs.

Line 84-85, this statement is incorrect. The Kayanne et al. 2005 abstract states 'All the metabolic parameters, Pg, R, E and calcification (G) were reduced by half after the bleaching,', and no pre-bleaching rates were estimated.

In addition to the text above from Courtney et al., 2018 summarizing Kayanne et al., 2005 Shiraho Reef results, there is also specific text in the Kayanne paper which explains differing results between Palau and Japan. Yes, the abstract states G was reduced by half but that is for Palau. Within the actual text they found differing responses in Palau and Shiraho reef at Ishigaki Island, Japan. The following is taken specifically from Kayanne et al., 2005 section 3.1.4:

However, E was lower in September 1998 (36 mmol m_2 d_1); this was during the bleaching period, when the mean coverage of living coral was 7.1% (51% of which was bleached, so that the coverage of corals with symbiotic algae was 3.6%). After the bleaching, E increased over the course of recovery. By contrast, G (calcification) remained nearly constant during the September 1998 bleaching at Ishigaki Island.

We fully admit starting on L56 that the normal expectation is for bleaching-induced decline in NEC. We can add Kayanne's Palau results to further support this expectation. When we found no change in NEC, we performed an exhaustive literature review to find any possible examples where this was also observed. We fully admit this should not be the norm and present the information specifically in this order.

Methods:

Overall, I found the methods do not provide enough information or justification to assess the validity of the rates or to undertake a follow-up survey. One major concern is that the time window stated (1100-1500) is not representative of calcification over a diel cycle (which is what is typically used when discussing reef state). Different stressors can affect calcification/dissolution differently depending on the time of day. Prior studies have shown that impacted corals show a significantly higher rate of low-light dissolution than non-impacted corals, even when daytime or peak sunlight calcification is not affected.

Enough information is provided in the supplemental material to conduct follow up measurements and this can be easily rectified with more information moved to the main text.

We agree that calcification can differ depending on time of day and light. Perhaps this needs to be further clarified, but measurements on the Heron reef flat can only be conducted during a 3 hour period around low tide. Outside this period, there is too much water on the reef and mixing with the offshore water to measure adequate

changes in water chemistry. Therefore, the tides must be followed and one cannot make multiple measurements on the same day.

It is important to note that the purpose of this study was not to describe the diel calcification trends on Heron reef flat, this has already been done multiple times, most recently by Stoltenberg et al., 2020. The purpose was to compare pre-bleaching and during-bleaching NEC. Midday at full light provides the best opportunity measure peak community metabolism and ensure changes in seawater chemistry were large to reduce error. We fully admit in the discussion as a follow up more measurements need be conducted now at dawn, dusk, and night. Processing samples along with other coincidental work on the reef during the bleaching event prevented enough time in the day to do it all, especially nighttime measurements.

Why were coral fragments gathered for PAM fluorometry rather than assessment in situ? More information is needed on PAM measurements. How were those fragments chosen, how many of 'bleached' and healthy? Was there a control for PAM measurements or Symbiodiniceae densities during the bleaching? Were pre-bleaching yield or Symbiodiniceae densities measured, it does not appear so in Fig. 3.

More information can be provided on exactly how coral fragments were gathered. In short, the listed taxa were gathered across the reef during the bleaching event. We gathered live branches of all these taxa to see if they were healthy or not.

The model PAM listed is a benchtop unit, we did not have an underwater PAM, so corals could not be measured in-situ.

It is important to note that examples of a flow metabolism study setting up all of these type of control measurements before the bleaching begins is extremely rare simply because our predictive abilities are generally not dependable enough to mobilize an entire flow-metabolism team and gear. This is why most studies like McMahon et al., 2019, Courtney et al., 2018, Pisapia et al., 2019 measure either during or after.

This study did not begin with a certain expectation of bleaching, it was initially started as a study to relate community-level NEC and NEP to the census approach from benthic surveys. In the middle of this satellite data indicated accumulation of heat stress and signs of bleaching began. Realizing this opportunity, the study objectives were quickly shifted to take advantage of this event. We recognized that qualitative examples of bleaching (photo quadrats, white corals) may not be enough to prove the coral's health was compromised and decided some physiology measurements (PAM, symb. Densities) would be a great way to add strength to this statement. Perhaps some text would be beneficial to explain how this "opportunity" arose and why pre-bleaching physiology measurements are not available.

Equation 1 & 2 in the Flow Respirometry Approach: I am making assumptions here because I don't understand these equations. If u u is current speed (cm s^{-1})(not stated), then in the context of these equations, you are multiplying current speed by 3600 to transform from per second to per hour and dividing by 100 to transform from cm to m. However, if I am correct, this means that you have divided out your length component (m/m). The residence time is the term that needs to be on the denominator to provide the unit of mmol/m^2/hr. If I am incorrect here, then more definition needs to be put into your equations to show that you are

calculating the correct values. Typically, residence times are calculated separately than metabolic values using transect length, current speed, and length/time averaged depth (See Supplementary Information in DeCarlo et al., 2017, or Davis et al., 2020).

The questions used in this study, including why the equation has 3600/100, is detailed in Langdon et al., 2010. Since this seems to be a point of concern, we can add all of these details to the main text.

'Slack water' sampling is not made under the correct conditions. Slack water indicates no (or very slow) water movement, so this may be an issue of semantics and you really mean Eulerian sampling (see Silverman et al., 2014 and McMahon et al., 2019 for examples and comparisons). More information is needed here. If you are looking at changing water chemistry in the same place, you need either 1) and end member (an initial value), or 2) More specific calculation of your depth-averaged residence time over space. Did you have a current meter? If so, where was it placed? Maybe add to site map?

Yes, the slack-water name sounds like water isnt moving, but there are multiple studies which define that slack water simply means the water is contained in a basin and circulated within. This method has been used previously in the exact same location by Stoltenberg et al., 2020 (Late afternoon seasonal transition to dissolution in a coral reef: An early warning of a net dissolving ecosystem? GRL) and at nearby One-Tree island by Shaw et al., 2012 (Impacts of ocean acidification in naturally variable coral reef flat ecosystems, JGR). It has been traditionally employed on reef flats that are separated from the open ocean at low tide even if there is still water moving within the reef flat.

We have a site map (Fig. 1) and can move text from the supplemental material which indicates where the current meter was placed (at the end of the middle transect) for the eulerian measurements into the main text and make a note on the map in Fig. 1. The current meter was moved between the three end sample locations before the study began to ensure the current speeds were roughly the same 50m apart and this was corroborated with flourescein dye measurements across the transect area. This text is in the SI and can be moved to main text.

'Slack water' Methods state: water samples were collected from the same three locations (n = 3 day-1) two hours before 84 peak low tide and one hour following." Where were these locations and at what interval were they taken? Were they taken at the same time/place as the 'flow' samples were taken?

This is all detailed the supplemental material and we will move this text to the main document. The slack-water interval is 3 hours (two hours before, one following = 3 hour interval). The eulerian interval is as close in time as possible, as fast as we could walk 200m across the reef (upstream and downstream sample). The locations of all these sampling locations is noted in Fig. 1. This will be further clarified by which sites were specifically used for Eulerian and which for Slack-water, but in general the downstream samples for the eulerian approach is the same location of the slack-water samples (which are collected in the same location 3 hours apart).

We thought that the effort of using both the Eulerian and Slack-water approach would impress reviewers, as it is rare to compare both approaches simultaneously to provide strength to community metabolism estimates. Unfortunately, this seems to provide more

confusion, specifically with a misunderstanding of the slack-water approach. To prevent this with other readers, we will endeavour to be more explicit with this in the main text.

Figure 4: More information is needed on where the calcification rates were taken from in the literature. How were differences in calcification rates determined for different specific temperatures? Is this what's described in L285 – 288? If so, is the 1.1 degree change in temperature determined from absolute differences in temperature or for corals which bleached after 1.1 degree increase? If the bleaching temperature was 29.1, the calcification rate indicated here at 29.1 degrees should represent calcification rates under bleaching. Please specify.

We will take this as a point of emphasis to move the text into the main document so other readers do not do the same. Information on where calcification rates are taken is currently provided in the supp (S.7) and this will be moved to the main text with more detail.

Per L271-273, L306 in the main text and the description of Figure 4, the calcification rate for coral indicates the rate expected based on coral NEC at 29.1 x reduced calcification rate expected under bleaching x the amount of coral bleached on the reef.

Lines 306–308: How does this 9.8% expected decline in NEC compare with your observed results?

There was no significant changes in NEC (Table 4), so its a 9.8% larger decline than observed. We can add more text to L306 that the observed change was essentially 0%.

L 312–316: This is a good description of this result, and it is an interesting result. However, I think the wording in the abstract and conclusions are too strong with the data provided to support the argument.

We can reduce the strength of the conclusions in the abstract. Overall, the unobserved decline in NEC despite observed bleaching provided the opportunity to hypothesize 3 different drivers behind why this occured: 1) Thermally-accelerated calcification 2) Algal/Dead Coral calcification 3) Increased nighttime dissolution. On L312-316 we discuss the thermally accelerated calcification in unbleached sessile calcifiers. Since the submission of this manuscript, 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 hypothesis outlined in L343 (dead corals with algae might be calcifying) and we will be using these new data to add to this section.

Overall, perhaps it is necessary to correct the text to clarify that the thermally-accelerated calcification is not a conclusion. Its simply an idea in the discussion to stimulate thinking about what could be driving daytime NEC during a bleaching event. We provide back of the envelope calculations and Figure 4 to flesh out this idea and as noted will now add more to flesh out the idea of algal calcification.

Technical Corrections

Fig.7 is referenced a few times but no figure 7 is included, change to Fig. 4.

Noted, thank you. Fig. 7 will be changed Fig. 4

Information in the table 2 caption should be placed in the methods.

Noted, this will be moved to the methods.