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Comment on bg-2021-64

Anonymous Referee #2

Referee comment on "Quantifying functional consequences of habitat degradation on a Caribbean coral reef" by Alice E. Webb et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-64-RC2, 2021

Review of 'Functional consequences of Caribbean coral reef habitat degradation', by Webb et al., submitted to Biogeosciences 2021.

Webb et al., measured the community metabolism of small areas of a degraded Caribbean coral reef through in-situ incubations of benthic communities. Five incubation tents were deployed over coral, algae, and sand dominated benthos, representative of different states of coral reef degradation. Biogeochemical parameters were measured over 4-hour incubations at night and day. An inverse modelling approach was applied to the collected data. The key results were interpreted in the context of ecological function. Calcification and productivity were low and night-time respiration outweighs daytime productivity. The manuscript presents a unique and interesting approach to quantifying differences in biogeochemical processes on degraded coral reefs, however, there are some limitations to the study which should be addressed, and the inferences/conclusions that the authors make may need to be re-framed accordingly. The experimental design had low replication, and the measurements were made at one single location over just a few days / nights. The tents were leaking during the incubations, which would also have impacted the measurements. The logistics of such in-situ incubations are very challenging, and it was a good idea deploy the tents in duplicates / triplicates, but there is some variability within substrate replicates (in terms of composition and biogeochemical activity) to suggest that they could be evaluated individually. I think that the authors could provide some more information about the inverse modelling approach they use, and the advantages of using such an approach.

Specific points to highlight:

Introduction: Overall, the introduction is nicely written. An explanation and/or justification of the inverse modelling approach could be described either at the end of the introduction or within the methods.

L38: 'similarity' does not describe species homogenisation well. Maybe rephrase.

L39: 'This is worrisome...'; change this to something less emotive. E.g., This threatens ….

L55 'compromised' should be compromise; change 'was' to 'were'.

L59 ‘mirroring’ might not be the correct word, as the decrease in corals is the opposite of increases in the other functional groups.

Methods: The benthic incubation tent has a great design; there are some really strong features, such as the long sampling tube which samples from the middle of the incubated area rather than a typical sampling port. The photo in figure 1 shows that a metal chain was used as a weight to hold the chamber in place, which should be mentioned in the text, and perhaps some discussion of this as a source of the reported leakage.

There are some details in this section which could be clarified to assist the reader in fully understanding how the study was carried out and to improve replicability of the experiment. In particular, the sampling regime should be further detailed: at what time of day were the tents deployed? Over how many days? Were the days and nights within the same 24-hours?

Section 2. 3: How were the compositions measured? Were they 2D only, and could a 3D area be approximated from the data you collected? Normalising the rates to substrate-specific surface area and volume might give a more accurate representation of the processes being quantified.

In section 2.6 it was not immediately clear if the testing of the chamber was conducted at the same time as the incubations and, if so, for how many of them? How was the impact of high salinity controlled for? A rapid change in salinity might affect coral photosynthesis for example.

Results: In terms of structure, the order of findings could be adapted so that the key finding is first or have a summary which outlines the most important results before going through each finding in detail.

Verification of the chamber method could be its own sub-section. The rate of water exchange is high and this should be addressed in more detail. If salinity returns to the ambient level after 1-2hrs, this indicates that changes to other measured parameters are only reliable within the same time frame (i.e., the water within the tent is renewing over the course of the incubation).

It would be interesting to see TA: DIC plots and relationships between photosynthesis and calcification. If PAR data is available, you might also plot the rates against light as this could explain some of the variability. For example, if the weather conditions changed and light was reduced during some of the incubations this might explain the non-linear change in DO seen in some of the individual plots in figure S1.

For the modelling approach presented in Fig 5 and Fig S1, the models were fit to each incubation rather than each substrate type (as they were for Fig.6). Was there a reason for this? Would it be more appropriate to describe the incubated substrates as individual substrate types rather than grouping them into categories (as per Table S1)?

Table S1 presents useful information about the species composition of each chamber and could be used to describe reef patch as a distinct substrate type. This could be used to align individual compositions with variations in rates as the different species compositions might explain some of the variation between replicates.

Figure S1: The data presented in these plots should be included in the manuscript. You might consider combining the plots showing the distinct replicates / substrate types with different colours or symbols. The data could be converted to rates before plotting unless there was a reason not to do this (which might also be good to explain). Figure S1 demonstrates that the DO slopes are variable for corals, with irregularities in oxygen

evolution. If PAR data is available, the DO could be plotted with PAR to identify if there were changes in light to cause this (or were they all deployed on the same day?).

Figure 4: were the tents deployed under the same conditions, or are these from distinct days?

Figure 5: Plots should be combined so that all data can be presented. Visual notes: The parameter boxes should be tables to make it easier to read. All the axis tick labels should be horizonal.

Figure 6: for each of the bars sample size should be displayed. Since n is low (either 2 or 3), the results could be displayed differently to show each data point, e.g., jitter plot, or scatterplot by assigning a numeric value to each category (e.g., coral dominated would be 5, sand would be 0). Also it looks like there would be some differences between night respiration for example, however 'no significant differences were found' in L243. Could this be due to the way the stats were run rather than a true representation?

Discussion: The discussion is interesting and very well-written; however, it will likely need revisions according to the suggested edits in the results section. It would be good to include some discussion of relationships between the measured parameters (i.e., TA:DIC). Additionally, more in-depth discussion could be included to address the limitations of the study: (1) low replication and modelling with so few data points, and (2) the leakage of the chambers. The interpretation of findings should account for these uncertainties.

In the conclusions, information is presented about the local distribution of some coral species. This information should be described elsewhere and be incorporated into the discussion at an earlier point.

L218**:** if salinity returns to normal after 1-2 hours, this would indicate that the first hour or two of dDO or dTA is also lost?

L220: 'relatively good fit', this should be detailed further. The authors refer to Figure S1 as evidence of this, however, the model fit cannot be evaluated from figure S1 only.

L21: potential reasons for 'irregular oxygen evolution during the daytime'?

L254: 'no significant gain in primary habitat' is confusing because the measurements were over a matter of hours not months, so we would not expect any change to the habitat through accretion.

L275: 'an' = and