

Production and accumulation of reef framework by calcifying corals and macroalgae on a remote Indian Ocean cay.

Response to RC1: 'Comment on egosphere-2022-467', Anonymous Referee #1

General Comments:

This study assessed the metabolic and calcification rates of a variety of cultured, reef-dwelling marine calcifiers and algal taxa found in the anthropogenically-pristine Kimberley bioregion of Western Australia. The values measured were then related to the areal extent of benthic coverage across the various local reef habitats and further argued to provide a baseline for understanding shifts in metabolic and calcification rates in this region in response to environmental stressors (i.e. those that induce bleaching and mortality). While the results presented are surely significant in that they represent novel and important reef metabolic data from a unique location, more time/space could be spent discussing the methods used, assumptions made, and data generated in relation to previously published studies and long outstanding questions in the field (particularly with relation to future impacts of anthropogenic change). The authors should potentially consider reorganizing the key takeaways of the article - particularly in the Discussion section - around these topics as they are currently lacking and/or given short shrift. As it is currently presented, the Discussion section reads as a series of descriptive statements rather than a connected narrative that binds the manuscript together, interprets and provides context for the results of the study, and proposes potential mechanisms and future directions. Overall, I think spending a little more time thinking about the selected location, taxa, observed rates of metabolism/calcification, and trends in O₂, pH, and TA in relation to future projected impacts of environmental change in this region (quantitatively, if possible) is a worthwhile endeavor and will only strengthen the impact of this work.

We thank the reviewer for their insightful and considered comments and suggestions. We have made significant changes to the introduction and discussions sections to address some of the constructive criticisms provided by RC1, which we think adds considerable value and strengthens our manuscript.

Specific Comments/Questions:

Lines 52 - 61: Just to clarify, it is commonly argued that net community productivity (NCP) rates in many reef ecosystems, while certainly variable over the course of a diel cycle, tend to balance out over longer temporal scales such that nearly all of the organic carbon produced during periods of high photosynthesis is consumed on annual timescales or greater (i.e. Ware et al., 1992; Frankignoulle and Gattuso, 1993; Gattuso et al., 1999; Bates, 2002 and others). Thus, while it is true that CO₂ source/sink behavior is possible in coral reefs on short timescales, overall they are believed to be net sources because of high calcification rates.

We thank the reviewer for providing this clarification. Upon review of the citations provided we have rewritten the second paragraph to make clear any vagueness in our initial draft around rates of photosynthesis and calcification to overall source/sink of CO₂ by reefs. We have incorporated the citations of Ware et al, 1992; Gattuso et al, 1993; Gattuso et al, 1995; Smith, 1995; Frankignoulle et al, 1996; Gattuso et al, 1996b and subsequently Kayanne et al, 1995; Gattuso et al, 1996a; Gattuso et al, 1997; Gattuso et al, 1999 to this section of the introduction.

Lines 123 - 125: I'd argue it's more the magnitude and the net effect of these changes that is difficult to predict rather than conceptually reasoning through the effects themselves. We know the respective impacts of photosynthesis/respiration and calcification/dissolution on many parameters

of carbonate chemistry very well, but “hybrid” organisms that have both NCP and NCC rates (like corals and calcifying algae) make predicting the values of these rates difficult.

We thank the reviewer for highlighting this. We have sought to address ambiguity by adding “The magnitude of reef contributions to changes in water column chemistry is difficult to predict because of the net effect of local oceanographic conditions, relative abundance of the different members of the reef community and their individual metabolic rates.” And further down in the paragraph “The effect on water column chemistry by hybrid organisms like calcifying primary producers such as corals with zooxanthellae and calcifying algae becomes very challenging to measure in situ.”

Lines 131 - 134: Include citations for those that do exist and potentially some discussion on what has been learned and/or what is left to explore or challenge?

We have restructured the introduction and previous mesocosm experiments is now covered in more detail within the discussion section.

Section 2.2: What is the rationale/motivation for selecting these particular taxa for incubations - both abundance-wise and otherwise?

The taxa used for incubations were chosen based on abundance on the reef (i.e., were there enough individuals of a species to collect 6 or more replicates?) and size (< 90 mm) to fit inside our incubation cores without damaging the coral or algal tissue. This is detailed in section 2.2 Algae and coral collection.

Section 2.3: In the “real” world, PAR has a more parabolic shape with time over the course of a day than the more step-wise shifts induced by the incubation setup. It follows that photosynthesis vs irradiance is often modeled as a hyperbolic tangent function (i.e. Atkinson and Grigg, 1984; Langdon and Atkinson, 2005; Bouman et al., 2018; Bolden et al., 2019). Has any thought been given to what artifacts the simplified 4-hour approach (2 hours light - 2 hours dark) to incubations presented here may have on measured and scaled-up metabolic and calcification rates?

This is indeed a simplified approach. We have added some wording around this in the discussion.

Lines 231 - 242: Is this instrument/method calibrated using any kind of standard (such as the Dickson CRMs or another internal standard)?

The methodology from SOP3b in Dickson et al., 2007 was used to determine the alkalinity for single replicates due to sample volume constraints. The text has been changed to From SOP3b in Dickson et al. 2007, total alkalinity was determined for single replicates to the nearest...

Dickson et al. 2007 has been added to the list of references.

Section 2.6: This is an interesting approach. To clarify, there are four total incubation periods: two, 1-hour light periods and two, 1-hour dark periods. The per-hour rates of O₂ (or alkalinity) production or consumption are then multiplied by 24 to get respective light or dark “daily” rates of net productivity (and calcification). However, why is expressing light and dark incubation results in terms of *daily* fluxes valuable? Photosynthesis only occurs during sunlight hours; in your assumptions, there are only 12 hours of photosynthesis and 24 of respiration in a day. Not that these are invalid assumptions, but would it not make more sense to express the light and dark rates on hourly scales *or* do the calculation for the net flux (GPP - R) and express this as 1 daily value for each light+dark pair?

We have expressed the values as rates per day to enable comparisons with other values in the published literature. The daily values for each light-dark pair per day are also given.

Lines 280 - 284 (and Table 2): Any comment on why this may be and/or what it implies about seasonal/interannual variability of carbonate chemistry in the offshore waters that are assumed to supply the reef ecosystem?

Unfortunately, we were not successful in collecting carbonate system parameters in April of 2016 so we were unable to compare Austral autumn with Austral spring in October 2016. While we could discuss inter-annual variability, we felt that our sample size was insufficient to confidently elucidate any trends.

Section 3.2: I think this goes back to my earlier comment - I would think about expressing the respective light and dark fluxes in hourly units rather than daily. I am guessing that the calculations of net autotrophy/heterotrophy are based on light rates minus dark rates. *However* I would double-check to make sure the equations used are consistent with a 12-hour photoperiod and 24-hours of respiration. Including the equations used in the text would be a valuable addition.

The light flux (and changes in pH) reported are the changes in O₂ in the light thereby including both photosynthesis and respiration. The dark flux includes only respiration. The balance between the two is the net flux, which is obviously based on the assumption of equal periods of light and darkness. We acknowledge that the balance between photosynthesis and respiration would not be consistent across all parts of the day. This assumption has now been highlighted more clearly in the discussion.

Lines 315 - 318: How are the r² values “adjusted”?

This is a standard output from an AOV in R. The R² values are adjusted for the number of parameters in the model relative to the number of points in the design. It thereby takes into account how many independent variables were used to predict the target variable.

Lines 365 - 369: Why is the alkalinity anomaly technique prone to overestimates of calcification rates? A small clause (and citation?) for this would balance the underestimate assertion for the CaCO₃ content/growth method.

A citation to Hart & Kench, 2007 has been provided in the text to address this.

Lines 373 - 386: Out of curiosity, do measured pH and alkalinity values produce calculated DIC values (using CO₂SYS or seacarb) consistent with the trends in O₂ in terms of the magnitude of heterotrophic/autotrophic behavior across taxa?

Yes, this was pretty consistent with what we expected. Another assessor commented that the apparent dissolution of CaCO₃ in some species during night-time was surprising. We have now included some wording around this in the results section.

Lines 422 - 430: Are there any hypothesized observations/mechanisms for explaining why the calcification rates here are lower than other reported values - particularly as they relate to local open ocean chemistry variability and/or artifacts introduced in the incubation + scaling approach?

We have added commentary around the CaCO₃ saturation state for the Northern Indian Ocean and impacts on calcification in the revised discussion.

Lines 460 - 462: Why were CCA species not included in this incubation study?

CCA was not readily available in quantities to incubate on the reef flat at low tide when accessibility to collect was available. It was more prominent on the reef slope, but safety concerns prevented us from diving to collect specimens at deeper depths. Bessey et al., 2020 utilised exclosure cages on settlement tiles in the lagoon at Browse Island and no CCA was observed on those after being deployed for 6 months. Turfing algae seemed to dominate the settlement tiles.

Technical Comments/Questions:

Line 69 - No need for possessive. "Coral skeletons are..." is fine.

Text changed to "Coral skeletons are made from the mineral phase of calcium carbonate..."

Lines 84 - 85: This concluding sentence reads as a bit of a non sequitur, and this paragraph overall could use some refocusing. I would suggest taking a step back and thinking about the key points the reader is meant to take away from this section. It seems like it's about scleractinian coral contributions to the reef framework and threats to that contribution (based on lines 65-80, but lines 80 - 85 suddenly shift focus to metabolism.

We have re-written the introduction and through that process the text in this paragraph has been changed significantly or removed.

Line 87 - I think this first sentence could be stated more concisely. "Reef algae are also an important structural component of coral reef ecosystems. Their morphological diversity provides..."

Text change to "Reef algae are also an important structural component of coral reef ecosystems."

Line 95 - Sentence could be more concise. "Calcified macroalgae can also contribute significantly to the deposition of carbonates in coral reef environments."

Text changed to "Calcified macroalgae can also contribute significantly to the deposition of carbonates in coral reef environments."

Line 99 - "make it a major contributor".

Text changed to "Production rates of Halimeda make it major contributor to CaCO₃ in reefs in..."

Lines 99 - 102: Here and throughout, be careful and consistent with the use of the term "production" to refer to organic carbon/oxygen production vs CaCO₃ precipitation.

We have changed the text in the introduction so it now reads "Calcification rates of Halimeda make it a major contributor to CaCO₃ in reefs in the Caribbean..."

Line 277 (and elsewhere): Consider expressing O₂ concentration in molar units (as you do in subsequent discussions). It would be more consistent with the other measured chemical constituents and allow readers to think about potential stoichiometric relationships between variables more easily.

We have changed the units for oxygen from mg L⁻¹ to umol L⁻¹ for consistency.

Lines 290 - 292: This sentence could be more concise and clear (I think). "In light incubations, O₂ productivity fluxes were positive across all taxa."

Text changed to "In the light incubations O₂ productivity fluxes were positive for all taxa (Fig. 4, top panel)."

Lines 395 - 398: This is repeated at line 364.

The repeated line has been removed.