EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-467-RC1, 2022

Comment on egusphere-2022-467

Anonymous Referee #1

Referee comment on "Production and accumulation of reef framework by calcifying corals and macroalgae on a remote Indian Ocean cay" by M. James McLaughlin et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-467-RC1, 2022

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.

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.

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.

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

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?

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?

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

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?

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?

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.

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

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.

Lines 373 - 386: Out of curiosity, to 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?

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 artificats introduced in the incubation + scaling approach?

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

Technical Comments/Questions:

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

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.

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…”

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

Line 99 - “make it a major contributor”.

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 $CaCO_3$ precipitation.

Line 277 (and elsewhere): Consider expressing O_2 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.

Lines 290 - 292: This sentence could be more concise and clear (I think). “In light incubations, O_2 productivity fluxes were postitive across all taxa.”

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