

Interactive comment on “The short-term combined effects of temperature and organic matter enrichment on permeable coral reef carbonate sediment metabolism and dissolution” by Coulson A. Lantz et al.

Anonymous Referee #1

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This paper makes some important contributions to the topic of how permeable carbonate sediments in a coral reef setting will response to a 2.4 C warming and to organic matter enrichment. The experiment was well designed, executed and adequately replicated. They found that the sediments were undergoing net dissolution during the night time hours under control and all treatment conditions despite the fact that the overlying water in the chambers was supersaturated with respect to aragonite ($\omega_{ar} = 2.5-4.0$). This alone is noteworthy. It has been reported in field studies but it is helpful to confirm the observation under well constrained and replicated experimental conditions.

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It is also interesting that during the daylight period they observed net carbonate precipitation under control and all treatment conditions. The authors should be encouraged to comment on what they think is contributing to this carbonate production. Forams possibly?

The main findings of the study are that elevated temperature (+2.4C) caused both R and GPP to increase by significant amounts. R increased more than GPP so that the GPP/R went from 1.3 to 0.9, i.e. from net autotrophic to net heterotrophic. This is a reasonable result with many previous studies finding the dark respiration being more sensitive to temperature than photosynthesis. The Q10s for R and GPP are extremely high at 10.7 and 7.3, respectively. The authors need to discuss these results and put them in the context of the literature. Typically Q10 values are in the 2.0 to 2.5 range and this is consistent with the energy of activation for enzymatically mediated reactions which underlies the theory of why the rates are temperature dependent. Q10s are best computed on a C-specific basis, i.e. grams of C fixed or respired per gram C of organism biomass. I am not sure that a Q10 computed from R and GPP normalized to substrate area is meaningful. These high values are suggesting that something more than just a temperature effect on the energy of activation of the biological processes is at work. I think it would be better to simply report the temperature sensitivity on a mmol/m²/h per degree C basis and not suggest that that dependence might hold over a broader temperature range until there is data to support the claim. The reported effect of the temperature increase on Gnet varies between Table 3 and the text and this needs to be resolved. Table 3 says that Gnet is 0.2±0.2 mmol/m²/h under control conditions and -0.1±0.1 under the elevated temperature treatment. In the text Gnet under elevated temperature is said to be -0.2±0.1. It is hypothesized that the shift from net carbonate precipitation to net dissolution on a daily basis is caused by the shift in organic carbon metabolism from net autotrophic to net heterotrophic. This is supported by the observation that omega arag is lowest at dawn in the T treatments. The authors cite Yeakel et al in support of the connection between net heterotrophy and dissolution. It would be relevant to cite Muehllehner et al 2016 as another study

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that reported a clear relationship between reef sediment dissolution and a seasonal shift between community autotrophy and heterotrophy.

The observed responses to organic matter enrichment are among the most interesting of this study. They observed that the PD and CM enrichments resulted in increases in R and GPP, although the increase in GPP was greater than the increase in R. The effect of the organic matter enrichment also overwhelmed the effect of temperature such the GPP/R and Gnet were not significantly different from the control. The authors suggest that what happened is that first the organic matter was remineralized to its nutrient constituents. The small increase in R would be consistent with this. Then the released nutrients were immediately taken up of the autotrophs in the system resulting in the observed increase in GPP. The net autotrophy would result in a small elevation in pH which would in turn bump up saturation state and account for the shift from net dissolution to net carbonate precipitation. This scenario is reasonable to me. What is very interesting is that the system seems to be very closely poised at a tipping point. Day-night shifts in pH and temperature and organic matter augmentation are all able to shift the pore water saturation state sufficiently to shift the system between net carbonate production or dissolution.

I would encourage the authors to include a table where they compare their daily rates of carbonate production and dissolution with the rates reported in the literature for other locations.

As a small technical detail it would be nice if the authors employed the letter system to indicate in figures 4-6 which means are significantly different and which are not. The information can be obtained from the text but the figures would be more useful if the information was also supplied there.

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