

Interactive comment on “Coral reef carbonate budgets and ecological drivers in the naturally high temperature and high total alkalinity environment of the Red Sea” by Anna Roik et al.

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Author response “Coral reef carbonate budgets and ecological drivers in the naturally high temperature and high total alkalinity environment of the Red Sea”

We thank the editor and reviewers for their overall positive evaluation. We appreciate their detailed and constructive feedback and their recognition of the relevance of the presented data. We are happy to consider their recommendations in a revised manuscript.

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Overall, the reviewers suggest a similar set of improvements of the current manuscript. We propose to address these as outlined below:

*pH monitoring

-> Reviewers #1 and #3 point out that the approach for pH monitoring does not comply with best practices for carbonate chemistry analysis of seawater, which requires the use of Tris buffers (Total scale measurements). In our study, NBS scale standard buffers were used. In addition, CTD sensors, as employed in our study, are not recommended for precise seawater pH monitoring.

=> We agree with the reviewers that the collected pH data and carbonate chemistry calculations based on these are suboptimal. In a revised version, we suggest to address this by removing these data from the manuscript.

*meta-analysis

-> Our meta-analysis (distance based linear models) shows that total alkalinity is the best explanatory variable of the reef growth parameters (Gnet and Gbudget). Reviewers #1 and #3 have pointed out that the analysis may be correct, but biologically not necessarily relevant, as the changes in total alkalinity were minor and in this order of magnitude are unlikely to result in significant effects on reef calcification processes.

=> We can relate to the reviewers' reasoning. The "precise" results obtained from linear models may not be reasonably justified from a biological point of view. In a revised version, we suggest to address this by removing the results of the distance based linear models. We propose to keep the correlation analysis, as these statistics offer an insightful basic overview on the numerical relationships in our dataset. Overall, we will tone down the conclusions drawn from the results of our meta-analysis.

*sampling design and statistical approach

-> All reviewers have requested a glossary of variables included in the census-based approach. -> Reviewers #1 and #2 ask for a comprehensive scheme of the study

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design for Gnet (net accretion/ erosion measured by limestone block assay) and Gbudget (census-based reef carbonate budgets) to make the statistical procedures more comprehensive. -> Reviewer #2 asks to remove Gbudget data for the lagoonal site (a midshore backreef) from the manuscript, as it is the only data point from a backreef, thus not acceptable to be analysed at the same factor level as the exposed reefs. -> Reviewer #2 suggests a statistical approach considering repeated measurements for the Gnet data calculations.

=> We thank the reviewers for the remarks. In a revised version, we will provide a glossary and a scheme of study design to clarify the data acquisition for the two reef growth parameters assessed. Indeed, we agree that this additional information will help to clarify our results, data collection, and design of experiments. We will further revise parts of the methods section to better detail sampling procedures and design, as well as statistical testing. => We will remove Gbudget data from the lagoonal site from the manuscript as suggested. This will increase statistical power and provide clearer cross-shelf patterns of reef growth for exposed reefs. => We think the suggestion of using a repeated-measures statistical approach for Gnet data analysis is not necessarily adequate for our dataset. We assume that this suggestion is based on a misunderstanding of our description of sampling method and design. We are thankful that the reviewer brought this up. In fact, each limestone block in the assay was deployed and measured only once, which does not call for a repeated-measures analysis. To improve clarity, we suggest to provide a more comprehensive description of the sampling scheme for Gnet to justify the statistical tests that were chosen.

*bioeroding taxa

-> Reviewer #2 requests more information on the bioeroding taxa monitored in this study

=> We agree that providing detailed composition of micro-bioeroder communities (epi- and endolithic organisms) would be beneficial. Unfortunately, we did not investigate

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this aspect, as the focus of the study was to measure the mass loss/accretion of the limestone blocks. We will highlight central aspects of the limestone block assay in the revised version of the manuscript. In addition, we will include further details on bio-eroder taxa surveyed in the census-based approach (fish, sea urchins) as requested.

*historical comparison

-> Reviewer #2 recommends to tone down on the historical comparisons

=> We will shorten and tone down our discussion on historical comparison and reef growth trajectories. In addition, we will emphasize the need of comparative data for a better understanding of future projections.

*summary

Overall, we will shorten the revised manuscript and will change the focus on reef growth data of exposed reef sites along a cross-shelf gradient rather than on the physico-chemical environment. Figures and tables will be adjusted accordingly and we will consider the inclusion of the additional references suggested by the reviewers.

We would like to thank the reviewers again for their time and their offer to re-evaluate the revised manuscript. We are looking forward to compiling a revised version, once encouraged by the editor to re-submit our work.

On behalf of all authors, Christian R Voolstra

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