

# ***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.***

## **Anonymous Referee #2**

Received and published: 14 April 2018

Review for Roik et al. “Coral reef carbonate budgets...”

### **OVERALL COMMENTS:**

This is a mostly well-written MS, and the language and style is above average, the figures and tables are of high quality. The study concerns a highly relevant topic from an interesting marine area that may be less well studied in this context than other coral reefs (Berumen et al. 2013; Schonberg et al. 2017). The MS is based on a large work effort and produced a large amount of valuable data. I compliment the authors for tackling such a timely and complex task.

However, I think the study has a number of shortcomings that need to be addressed

before the MS can be published. In my opinion the data analyses were not correctly performed and will need to be redone. This will likely lead to the need to re-write a few parts of the MS. The Methods section may require some more detail for clarity, and maybe the terminology could be simplified or streamlined to make it easier for the reader. In particular the assessment of the bioerosion needs to be clearer. I have further listed references that may be useful in the context. These cannot all be included and are subject to the choice of the authors. The title seems to be a bit misleading.

I recommend publication after re-analysing the data and MS revision. It may thus be necessary to re-review the MS.

#### DETAILED COMMENTS:

**DATA ANALYSIS:** In my opinion the data evaluation is faulty, and the statistical models and means were not built respecting the existing data hierarchy. This matters even more as the sample size of 4 replicate blocks and 6 transects per subsite is very small for data that can be expected to be highly patchy and variable. To my understanding, there are two independent "between" factors, "distance from shore" (3 levels) and "time of exposure" (3 levels). Then there is the within factor "reef area" or "hydrodynamic exposure level" (backreef, forereef, lagoon or exposed, sheltered). The latter "within" levels are not independent within a given reef and would need to be nested in "distance from shore". Ignoring this data hierarchy resulted in pseudoreplication and a higher test power than actually justified (S12). This is the case for the 6 and 12 mo analyses, and the 30 mo analysis seems to include only 4 subsites, all at the same data level (near-fore, mid-fore, mid-lagoon and off-fore). I assume that the means are therefore also not correctly calculated (not stepwise). Sadly, this does not only apply to the block data. All figures and tables will need to be restructured accordingly, means will have to be calculated stepwise, following the same data hierarchy as the statistical models. It would be good to have some sort of schematic figure that visualises the data design and hierarchy. Maybe "lagoon" could be left out of the 30 mo analysis to make things clearer and possibly more powerful to pick up effects/trends? I think it

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would be acceptable to define “lagoon” and “backreef” as “sheltered” and give them the same data status, but it is not acceptable to include near-fore, mid-fore and off-fore at the same level as mid-lagoon. Unfortunately the situation will cause significantly more work effort and potentially the need for rewriting parts of the MS.

THE CHOICE OF THE BIOERODER TAXA AND THE ASSESSMENT IS NOT ENTIRELY CLEAR. The bioeroders were assessed in two groups – dominant epilithic bioeroders via biomass estimates, and endoliths in a block assay. It is not quite clear whether the counted scarids only represented “excavators”, i.e. fishes that bite and break calcium carbonate or whether the “others” contained fishes that mainly eat fleshy algae and thus dilute the overall value? Please clarify in the Methods. The blocks were weighed but not otherwise assessed? 30 months exposure to settlement is not that long, and unless you consistently found larger borers in the blocks you probably have to assume that you are still capturing earlier successional communities. Can provide more details in the results what borers were present in the 6, 12 and 30 mo blocks?

THE BIOERODER DATA MAY PERHAPS NOT BE REPRESENTATIVE, AND THERE MAY BE A RISK OF COMPARING APPLES WITH PEARS. E.g. William Kiene has assessed temporal successions of coral reef biota in a comparatively pristine environment settling onto experimental blocks. According to his data, the present 30 mo blocks may still be reflecting a developing phase of that settlement. The block assay was not controlled by implementing non-erodible blocks so that the net value could be corrected with data that matched the experimental situation. Calcification and epilithic bioerosion was assessed *in situ* and thus in all likelihood concerned a mature community, the blocks assessed borers and may not be as representative. In any case, it would be good if the authors could provide more detail on how the blocks were deployed (fixed to the reef and allowing lateral invasion and grazer access, or on a rack only allowing larval settlement and probably excluding urchins) and what were their observations after retrieval. This would enable a better understanding what the data represent. The comparison of the present data with past data from the Gulf of Aqaba

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may also be a case of apples and pears (see below). Overall the interpretation needs to be cautious. It is a pity that dominant borer distributions were not assessed in situ, which is a significant data lack for the Red Sea and could have been done while assessing the benthos. The epilithic bioeroders have always received more interest. In any case, this needs to be reflected in the wording, you did not assess bioeroders on the reef, but only the dominant epilithic bioeroder-grazers (fish, urchins).

**THE DATA NEED TO BE USED AND INTERPRETED WITH CAUTION.** The data are still valuable and good to have. However, in view of some of the above comments, and the extremely large error values, the interpretation cannot be generalised too much and needs to make allowances – in a larger dataset with smaller error the data may have shown different patterns. The temporal analysis across decades appears risky in that the present study was conducted in the central Red Sea, with a different protocol and focus, the earlier studies in the Gulf of Aqaba. I think it would be better to resist and not try to construct a long-term trend, but rather point out the lack of comparable data? Also, when data are plotted respecting the data hierarchy I can only see significant crossshelf trends for the parrots.

**SOME PARTS WERE CONFUSING.** I found it at times difficult to keep track of the data and terms. Is it correct that the bioerosion (net) value was called Gnet, net accretion/erosion values and Gnetbenthos in different parts of the MS? Gbenthos and Gnetbenthos are too similar, wouldn't it be better to clearly separate those terms? What does G stand for? Growth? Why then is E marked separately, it is negative growth? Wouldn't it be easier for the reader to skip the Gs and Es and use more intuitive words? To abbreviate in situ accretion, in situ bioeroder biomass = approximation of bioerosion, assay net accretion/bioerosion? I am not sure what you mean with "cumulative" data in the block assay over time. You did not repeatedly measure the same blocks, right? And if you added data from the second and third block set to the first that would be inappropriate, as the successional stages change over time. You need to evaluate the block sets separately and can only display a 30 mo net accretion/erosion situation from

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blocks that were exactly that long in the water. Please clarify what you did. For urchins and parrots all 6 subsites were assessed for biomass and abundances – why are only 4 subsites presented for the respective bioerosion data?

THE TITLE COULD BE TWEAKED TO BETTER REPRESENT THE CONTENTS OF THE PAPER. I feel that putting the budget first is misleading, because these data are only a small part of the paper, and the huge error value makes the overall budget estimate a highly unreliable value. Would it be OK to change the title to something more along the line of: Ecological drivers of coral reef carbonate cycling in the central Red Sea – a high temperature, high total alkalinity environment

Berumen ML, Hoey AS, Bass WH, Bouwmeester J, Catania D, Cochran JE, Khalil MT, Miyake S, Mughal MR, Spät JL, Saenz-Agudelo P. The status of coral reef ecology research in the Red Sea. *Coral Reefs*. 2013 Sep 1;32(3):737-48. Schönberg CH, Fang JK, Carballo JL. Bioeroding sponges and the future of coral reefs. In *Climate Change, Ocean Acidification and Sponges* 2017 (pp. 179-372). Springer, Cham.

TECHNICAL COMMENTS: Are included as attached.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-57/bg-2018-57-RC2-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-57>, 2018.

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comment

Roik data plotted

SS: Sea urchins

Distance from Reef area	Abundance	SE	Biomass	SE	Assumed bio SE
Nearshore	0.016	0.008	0.48	0.98	0.228
Midshore exposed	0.002	0.004	0.25	0.19	-0.024
Offshore	0.004	0.002	0.05	0.04	-0.019
Nearshore sheltered	0.005	0.003	0.98	0.81	
Midshore sheltered	0.005	0.003	1.36	1.07	-0.187
Offshore sheltered	0.007	0.006	0.15	0.11	0.193

Nearshore mean and SD	0.0095	0.006	1.21	0.318	-0.228	0.189
Midshore mean and SD	0.0035	0.002	0.785	0.785	-0.106	0.115
Offshore mean and SD	0.0055	0.002	0.10	0.071	-0.019	0.003
Total mean and SD	0.0062	0.003	0.70	0.559	-0.118	0.105

This is not quite good, it's the only reef where the sheltered data would be included in the mean; the overall mean is then affected the same way - I would restrict this to the exposed sites

Parrot fishes

Distance from Reef area	Abundance	SE	Biomass	SE	Assumed bio SE
Nearshore exposed	0.17	0.06	82.18	46.67	-1.360
Midshore exposed	0.15	0.01	50.85	5.44	-0.444
Offshore exposed	0.13	0.01	67.97	9.21	-0.727
Nearshore sheltered	0.05	0.01	19.54	5.56	
Midshore sheltered	0.08	0.01	24.69	6.04	-0.338
Offshore sheltered	0.10	0.02	36.62	8.54	0.271

0.11	0.085	50.86	44.293	-1.360	1.886
0.12	0.049	37.77	18.498	-0.391	0.075
0.12	0.021	52.30	22.168	-0.727	0.307
0.11	0.003	46.98	8.004	-0.826	0.492

This is better than for the urchins, because the values were quite similar. But still not quite good, it's the only reef where the sheltered data would be included in the mean; the overall mean is then affected the same way - I would restrict this to the exposed sites

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Fig. 1. Demonstration as figures