

Interactive comment on “Seasonal Net Ecosystem Metabolism of the Near-Shore Reef System in La Parguera, Puerto Rico” by Melissa Meléndez et al.

Anonymous Referee #2

Received and published: 18 December 2018

This paper presents estimates of NEC and NEP on a reef in Puerto-Rico based on continuous monitoring of pCO₂ and O₂, and discrete bottle sampling for TA and DIC. The authors used a large dataset and applied a simple 1-D model to estimate the metabolic rates of the reefs. The main result is that the reef is currently dissolving at a rate faster than what has been estimated before using other methods. This result is highly interesting and also shows that other methods such as the reef budget of Perry et al. should be used with caution. The methods used seem to provide reliable results even if large errors in the estimates of TA are problematic. This problem will need to be overcome, likely by increasing the frequency of sampling in further research. The paper is well written and the data presented are of interest for biologist and biogeochemist. However, I regret that the paper is that long. I do understand that some details were

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needed but I believe that a shorter version of this paper would attract a broader readership. For example the discussion is rather long (~10 pages) with some repetitions. The introduction could also be shortened by maybe not providing trivial information on carbonate chemistry.

I have listed below some specific comments:

- Introduction: The two first pages could be shorten
- L133-134: It would be good to add 1-2 sentences on the poor coral cover/health of Caribbean reefs here.
- 160-161: The link NEP NEC is not clear here, why “relative to NEP”?
- Methods: The method section is a bit confusing as some parts read more like discussion/ result material (for example L 249-253). I recommend reformatting this section to make it a bit easier to read by removing all the materials that is not methods.
- L317-319: Please provide more details on the methods used to determine TA and pH (accuracy, etc).
- L336-338: The errors with this method are very large and could potentially bias all further results. Looking at Fig S3 it looks like for a given salinity it is possible to obtain a TA range of up to 200 $\mu\text{mol kg}^{-1}$. . . It would be good to discuss this potential pitfall in the discussion.
- L480-489: again an example of a section that has nothing to do in the methods.
- L517: Did you measure any seasonal changes in phosphate and silicate?
- L599: What about the changes in coral cover between studies?
- L611: Are they any other major calcifying organisms at this site? What about CCA, Halimeda, or forams that can contribute massively to NEC?
- L661-663: What about the role of temperature. Could these results also demonstrate

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that 1) Corals calcify more slowly when temp > 27, and 2) that bacterial activity is enhanced by increasing temperature which favour the dissolution of sediment, etc. in interaction with increasing DOM. It is also interesting to see that there is maybe no relationship NEP

–nutrient, could that demonstrate that one critical nutrient is missing in the system (e.g Iron)?

-L683-684: This decoupling between omega and NEC is very interesting. The role of SST on the biological activity is probably very important here (see my previous comment).

-L712-715: This is a critical point. Is there any reason to believe that Enrique reef is a “special case” or is it likely to observe the same discrepancy on other reefs?

-L728: Where does that come from? This claim needs a reference because the link between net heterotrophy and algae dissolution is not clear.

-Section 4.5: I am not sure about the utility of this section. The manuscript is already rather long and this section reads like another story.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-408>, 2018.