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Interactive comment on "Daily variation in net primary production and net calcification in coral reef communities exposed to elevated pCO_2" *by* Steeve Comeau et al.

Anonymous Referee #1

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General Comments:

Comeau et al. present results from three flume studies, a backreef from Moorea, a backreef from Oahu, and a forereef from Moorea. They present statistical models relating net production and net calcification to PAR for ambient and high CO2 conditions. They then analyze the relationship between production and calcification at ambient and high CO2 conditions. They conclude that ocean acidification conditions depress calcification without changing production.

The paper is well-written with logical sequences and transitions. I think the paper is structurally sound and well-organized. The figures and tables are clean and easy-to-read. However, I have a difficult time discerning whether the data and information

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presented in this study is new data worthy of a separate publication. As the authors acknowledge, the data for two out of the three flume studies reported in this manuscript were already discussed and presented in earlier manuscripts (Comeau et al. 2015 Biogeosciences-Moorea backreef, Comeau et al. 2016a Global Change Biology-Moorea forereef). Each of these two previously published studies present daytime, nighttime, and daily integrated calcification results for the mixed community discussed in this study, macrocalcifiers (i.e. corals + CCA), and sediment/pavement alone. Although this study presents higher resolution calcification data and new production data for the Moorea sites, plus all of the data for the Oahu site, the analysis is still limited and does little to extend beyond the two previously published studies. Furthermore, the conclusions from this paper, that ocean acidification depresses net calcification independently of production, suggests that enhanced dissolution is largely responsible for driving the decreased net calcification at high CO2 treatment levels. This conclusion can be inferred the data and figures in Comeau et al. 2015 and Comeau et al. 2016a.

Overall, I don't think there are major problems with the technical quality of the manuscript. I just don't know whether there is enough original material presented here to stand alone as a manuscript. I will leave that for the editor and co-reviewers to decide and for the authors to better justify.

Specific Comments:

Methods:

The section about flume design and setup seems very long and repetitive from previously published papers (lines 140-205). I recommend that you cite the original references as necessary and replace much of the text with a diagram showing all the important features (sites, numbers of flume replicates, flume lengths, flow speeds, CO2 treatments, pictures of community composition in the flumes, etc.). I think it will be much easier and faster for the reader to absorb the material this way.

Please include information about oxygen sensor calibration and accuracy determina-

tion (similar to what you've done for TA and pH measurements).

Please provide more details to justify your omission of air/sea gas fluxes.

Results:

Have you tested whether your hyperbolic tangent model for PAR-production and for PAR-calcification provides significantly more explanatory power than a null model? Experiments 1 and 2 for the PAR-production model (Fig. 1a and 1b) seem to fit the expected behavior of a hyperbolic tangent (or Michaelis-Menten) model, but Experiment 3 (the deep Moorea forereef) does not (Fig. 1c). Using AIC will help you identify the most parsimonious model type, but not tell you whether the model significantly increases the explanatory power of the data. In the case that any of the models for any of the CO2 treatments do not provide additional explanatory power over the null model, you are going to have to think about why not. In general, I think you should be providing the results of all model parameter fits (including mean estimates and standard errors) as a supplemental table for interested readers. Providing only your t-test results between ambient and high CO2 treatments does not provide the reader with information about the actual parameter values nor allow the reader to assess whether these models are even appropriate fits to the data in the first place (i.e. whether the model parameters are significantly different from zero).

Why did you choose an ANCOVA approach for analyzing the calcification-production relationship at ambient and high CO2 as opposed to simple linear regressions, where you could look for changes in slope and intercept in ambient vs. high CO2 conditions? I think linear regressions are more intuitive, but perhaps there is a logic behind the ANCOVA approach that just needs to be explained further? The presentation of the ANCOVA results (Section 3.2) could largely be collapsed into a table that gives slope and intercept mean estimates and standard errors for all experiments and treatments (as opposed to Table 3 which currently just lists results of tests for differences in parameter estimates between ambient and high CO2 treatments for the three experiments).

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Discussion:

I recommend that you re-structure the Discussion section to focus on your positive result (depressed net calcification at high CO2 and its possible mechanisms) first before discussing your null result (no changes in production at high CO2 or in PAR-production relationship).

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