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

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A. Hannides (Referee #2)

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General Comments:

The manuscript describes a study that falls into a now well-established tradition of permeable sediment experimental studies at Heron Reef. I think that it complements

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previous findings very well, by extending our fundamental knowledge of how these sands work and are expected to respond in view of future change. Surprisingly, despite their preponderance, sands do not receive more attention and remain understudied. In view of the above, I find this manuscript worthy of publication in this journal. The study is justified by a substantial review, the experiments are well designed and sufficient to support the scope of the study and to reach the stated conclusions. However, there are some aspects of the manuscript that need improvement before it is published.

Response to General Comments: We thank A. Hannides for a thorough review of the manuscript and insightful comments. We have done our best to address each individual comment and feel the manuscript benefits greatly from these edits. Please note, the referenced line numbers for each comment response refer to a new, revised version of this manuscript and may differ from the older version.

Specific Comments

Comment 1: One important correction to be made involves the recipes for organic matter addition, described in section 2.5, “Organic matter manipulations.” The math doesn’t add up. On p. 7, line 178, the phytodetritus concentrate is characterized by concentrations of 8.5 umol C/L and 0.9 umol N/L, and we are then told that when 10 mL are added to _4 L and diluted, the concentrations of C and N almost triple! Could the mentioned units be actually mmol instead of umol? On p. 8, line 191, we are told that 94 mL of mucus were added to corresponding treatments. At concentrations of 12.1 mmol C/L and 0.8 mmol N/L (line 194), dilution by 4 L of overlying water would yield 280 umol C/L or roughly 10 times higher than those in Table 1. Please re-examine these recipes and correct accordingly.

Response 1: We thank the reviewer for their detailed analysis of the organic matter manipulations and the expected concentrations. We apologize for the confusion, but the listed concentrations for phytodetritus (8.5 umol C/L and 0.9 umol N/L) indicate the final concentrations in 1 L of seawater if 1 ml of the PD concentrate is added (the

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volume filtered). So when 10 ml (10/1 = 10x) are added to 4L of seawater (10/4 = 2.5x) this is why it seems the value triples. Likewise, the coral mucus concentrations (12.1 umol C/L and 0.8 umol N/L) refer to final concentrations in 1 L of seawater if 12 ml of the CM concentrate is added (the volume filtered). So when 94 mL (94/12 = 7.8x) are added to 4L of seawater ((7.8/4 = 1.95x umol/L) the value is almost doubled. This information has now been added to section 2.5 to clarify. (Lines 184, 200)

Comment 2: Another important aspect of the study that needs improvement is the description of statistical analyses to test the proposed hypotheses and their outcomes. Currently, statistical aspects of the study are spread far and wide in the text, tables and figures, and are occasionally redundant. Below are some suggestions for improvement. A statement like the following is repeated in the legend of several tables and figures: "Values correspond to the mean ± SE. Control (C) (n = 9) and temperature (T) (n = 7) treatments were pooled together from all four incubations. Organic matter (OM) (phytodetritus (PD) and coral mucus (CM)) and combination treatments (T + PD, T + CM) are pooled together from the two incubations for that specific OM treatment (n = 4)." Mention this pooling strategy once in Methods, and that should be sufficient. This should unclutter a lot of the legends. If you so wish, include values of n in the treatment column of Tables 1 and 3 in parentheses.

Response 2: We understand the thorough explanation of values, pooling practices, and sample size can seem redundant. The pooling strategy has now been limited to the methods with the included assumption that values, where pooled together, were not significantly different between incubations. Figure and table legends have been reduced in statistical text to be less redundant.

Comment 3: The abstract states that "The combined effect of warming and OM enhanced R and GPP, but the net effect on GPP/R and Gnet was not significantly different from control incubations." A simple and important statement like this cannot be verified easily. Sure, the bar charts showing means and standard errors can be visually inspected and the statement (kind of) verified, but the statistical proof is buried in

the text. One way to resolve this is to use symbols on bar charts (Figures 4, 5 and 6) to indicate statistically insignificantly different treatments, i.e., same symbol indicates indistinguishable values.

Response 3: A similar comment was posted by the other reviewer, so we agree an amendment is necessary. To meet both requests and keep the figures uncluttered, a * has been added above mean diel Gnet and GPP/R values that significantly differed from the control.

Comment 4: The Results section is festooned with statistic and probability values in parentheses. Consider displaying all results of your ANOVA tests in a table to precede or follow Table 3 and focus your Results section on highlighting the main outcomes. In my opinion, the readability problem in this section is exacerbated by a tendency to repeat values for T, GPP, R, GPP/R etc. already shown in Table 3 and the figures. There's no need to repeat these values; just refer to Table 3 and the relevant figure.

Response 4: We agree that the results section's readability could be improved by removing redundant information. However, if these requests are met (including Comment 10), and all results and statistics are moved to a table, we feel that it becomes the case the manuscript contains too much data in table form and too little in text form. To strike a balance where both the text and the tables provide non-redundant information, we have removed mention of the actual values of each metabolic rate in the text and left this to be consulted in the tables. In turn, the statistical trends and probabilities have remained in the result text so the reader can understand if the trend was an increase or decrease and if this trend was significant. (Lines 276-313)

Comment 5: A final comment on the statistics front concerns the use of a Model I regression "to fit a linear relationship for the purpose of predicting inorganic metabolism (Gnet) from organic metabolism (NPP, GPP, R, GPP/R)." Since the latter are not true independent variables, a Model II regression may be the appropriate approach towards this goal.

Response 5: We thank the reviewer for this notification and understand where a Model II regression would be useful. Upon conducting this analysis, many of the results do not necessarily provide additional explanatory value beyond the already presented significant and non-significant ANOVA results. For this reason, this portion of the manuscript has been removed, as we do not feel it provides additional valuable information to the reader or interpretive advantages not provided by already listed data.

Comment 6: The excellent overview of past experiments (starting on p. 4) distinguishes between "short" and "long" experiments. It would be useful if the actual time-scales are mentioned explicitly (instead of "hours to days") so that those studies and the one described in the manuscript can be placed in perspective.

Response 6: We understand the need for a more specific definition of short and long term as it relates to each study and have attempted to do so in this portion of the introduction with specific mentions of each cited study's duration of measurement. (Lines 80 – 93)

Comment 7: The "Sediment grain size: 12.1%. 2 mm . . ." statement (p. 5, lines 120-122) is awkward, not even a complete sentence. Is this information important? I think so. Please place it in a table on characteristics of the sand used, and include some basic sediment grain-size statistics (mean and median size, sorting) as well as permeability and porosity.

Response 7: We thank the reviewer for noticing this grammatical error. We agree with the need to refine this statement to a more complete sentence and have done so. This manuscript has been formatted to follow the literature from which these measurements were taken (Cyronak et al. 2013b). For a more detailed understanding of the Heron Island sediment characteristics, we direct the reader to Glud et al. (2008), and Cyronak et al. (2013a, 2013b) (Line 126). If the reviewer believes a table is absolutely necessary, one can be added with this data, but we felt it best to first point out this is cited data from previous published research.

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Comment 8: The “best of three” approach (p. 8, line 209) is too generic a term. Please define it and/or provide a reference.

Response 8: We understand where this explanation suffers from colloquialism. We have instead phrased the sentence to read “the mean of the three most similar values” and provided a reference (Dickson, 2007). (Line 214)

Comment 9: A semantic point regarding the definition of Respiration, R. I definitely understand why it is elegant to present the magnitude of R as a negative for the purposes of Figure 4. However, R values can be listed as positive values in Table 3, so that the positive GPP/R values make sense. Alternatively, modify the definition of R on p. 9, line 235, as flux across the sediment-water interface, where a negative value indicates flux into the sediment.

Response 9: We understand how this may create confusion for the reader and have added the following text to the methods on p. 9, line 235: “Both NPP and GPP are reported as positive values to represent flux of O2 from the sediment into the chamber water column whereas R is reported as a negative value to represent the flux of O2 from the chamber water column into the sediment. To calculate the ratio of GPP/R, absolute values of R were used.” (Lines 243-245)

Comment 10: Finally, please consider adding two columns in Table 3 after Gnet, to show Gnet night and day values.

Response 10: This has been added to Table 3 and removed from the text, thank you. We have also removed NPP values from the text and added these to Table 3 as a means to address the readability issue mentioned in Comment 4.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-109/bg-2017-109-AC2-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-109, 2017.

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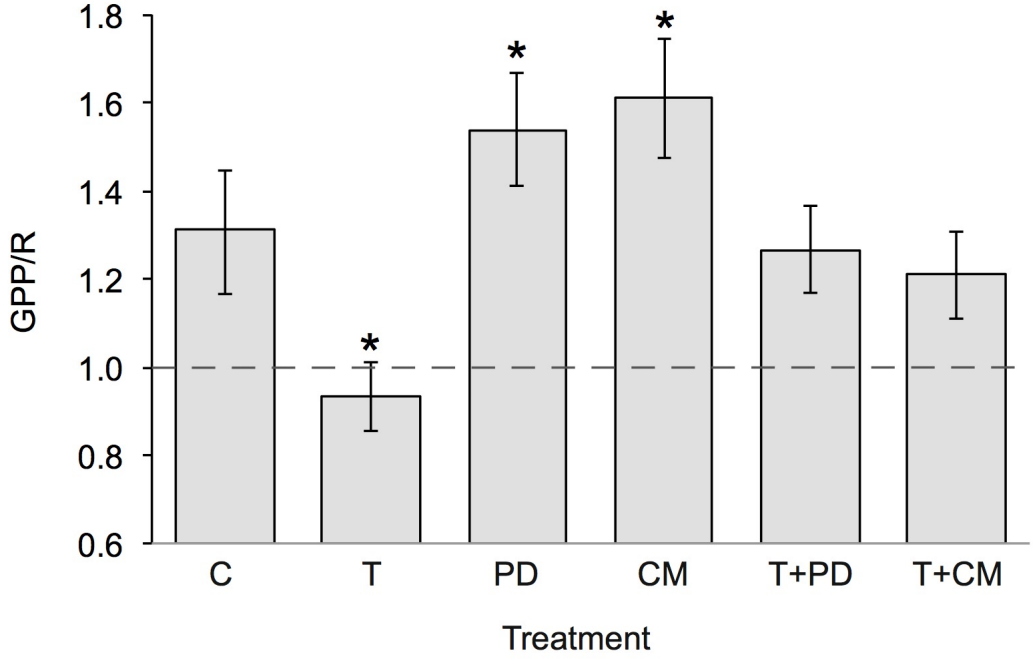

Fig. 1. Amended Figure 5

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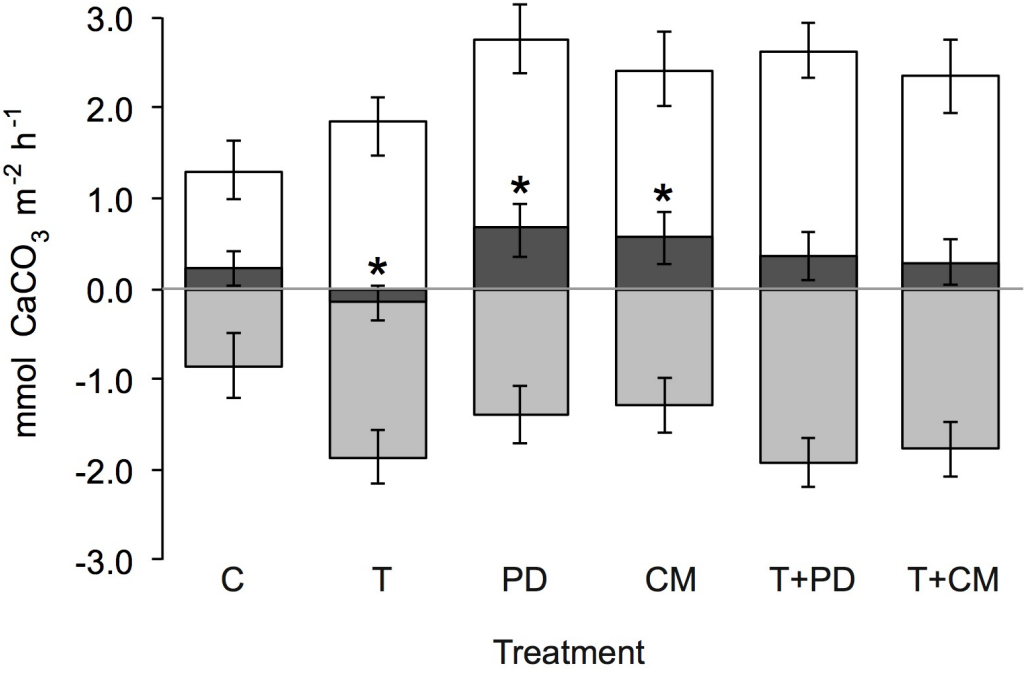

Fig. 2. Amended Figure 6

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Table 3: Calculated respiration (R: mmol O_2 m^{-2} hr^{-1}), net primary productivity (NPP: mmol O_2 m^{-2} hr^{-1}), gross primary productivity (GPP: mmol O_2 m^{-2} hr^{-1}), the ratio of GPP/R, and net calcification (G_{net}: mmol $CaCO_3$ m^{-2} hr^{-1}) in the control and treatment chambers. Values correspond to the mean ± SE.

Treatment	R (mmol O_2 m^{-2} hr^{-1})	NPP (mmol O_2 m^{-2} hr^{-1})	GPP (mmol O_2 m^{-2} hr^{-1})	GPP/R	Day G_{net} (mmol $CaCO_3$ m^{-2} hr^{-1})	Night G_{net} (mmol $CaCO_3$ m^{-2} hr^{-1})	Diel G_{net} (mmol $CaCO_3$ m^{-2} hr^{-1})
C	- 1.3 ± 0.5	1.9 ± 0.3	3.2 ± 0.4	1.31 ± 0.1	1.3 ± 0.2	- 0.9 ± 0.2	0.2 ± 0.2
T	- 3.5 ± 0.4	2.9 ± 0.4	6.4 ± 0.5	0.91 ± 0.1	1.7 ± 0.2	- 1.9 ± 0.2	- 0.2 ± 0.1
PD	- 2.6 ± 0.5	5.3 ± 0.5	7.9 ± 0.4	1.54 ± 0.1	2.8 ± 0.3	- 1.5 ± 0.2	0.6 ± 0.2
T + PD	- 3.1 ± 0.5	4.7 ± 0.5	7.8 ± 0.5	1.27 ± 0.1	2.6 ± 0.3	- 1.9 ± 0.2	0.3 ± 0.1
CM	- 2.0 ± 0.4	4.4 ± 0.4	6.4 ± 0.7	1.61 ± 0.2	2.4 ± 0.3	- 1.3 ± 0.2	0.5 ± 0.2
T + CM	- 2.9 ± 0.4	4.6 ± 0.5	7.4 ± 0.5	1.25 ± 0.1	2.3 ± 0.4	- 1.8 ± 0.3	0.2 ± 0.2

Fig. 3. Amended Table 3

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