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Comment

## ***Interactive comment on “Ocean acidification increases photosynthate translocation in a coral–dinoflagellates symbiosis” by P. Tremblay et al.***

### **Anonymous Referee #2**

Received and published: 8 March 2013

This paper is an intriguing and timely addition to the field of coral physiology. The paper is well written and does a good job leading the reader through relatively complex methods and results. The statistical design is appropriate for the data and ample relevant literature was used to provide this study context. My only major suggestion is to reevaluate the characterization of this study as an “ocean acidification” study both in the title, and throughout the paper. I elaborate further in comment #2 below. Despite that, I feel the work is strong and I recommend only minor changes before publication.

1. Page 5 line 7 – Please include the pH units in the methods in addition to what you have already included in Table 1. Also please include with what regularity seawater

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chemistry was sampled.

2. Page 5 line 10 - 140  $\mu\text{mol photons m}^{-2} \text{ s}^{-1}$  were used in the experimental conditions. Were any in situ light levels recorded from where the three colonies of *S. pistillata* were collected prior to the start of the experiment? If so, please include them as well.

3. Page 11 line 7 – The “important seawater acidification (pH 7.2)...” is arguably not an important seawater acidification level. This work, although novel and profound in its own right, cannot be directly compared to the vast majority of other OA studies, which commonly utilize pCO<sub>2</sub> levels of 800-1200  $\mu\text{atm}$  as their low pH/high CO<sub>2</sub> treatment. This does not mean studying the response of the coral holobiont physiology to extremely high pCO<sub>2</sub> conditions is not useful as long as it is addressed in a transparent manner, highlighting the limitations of the experimental conditions chosen. Indeed the fact that the coral host ultimately acquired the same amount of autotrophic carbon under both pH's was especially surprising given the extremely low pH treatment used. Discussing the limitations of the experimental conditions used should be added.

4. What was the overall health/condition of the coral and skeleton during this experiment? A qualitative description of general health and skeletal condition after a long-term exposure to such low pH levels would aid the reader in contextualizing your results.

5. I particularly appreciate Fig. 5. I feel it greatly adds to understanding the rather complex interactions between host and symbiont between the two treatments.

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Interactive comment on Biogeosciences Discuss., 10, 83, 2013.

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