

Interactive comment on “Calcification of the cold-water coral *Lophelia pertusa* under ambient and reduced pH” by C. Maier et al.

Anonymous Referee #2

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The title of this work leads me to believe that we may be seeing an incredibly important addition to our knowledge of the biology of *Lophelia pertusa*. Unfortunately, a poorly written manuscript, several glaring errors and misinterpretations of other's work and flawed methodology make this manuscript a poor read. I acknowledge that the authors first language is not English, but even so, with the admittance that this manuscript went through a round of reviews for another journal in the acknowledgements, I would have expected a much more polished product.

The authors themselves acknowledge several limitations in their methodology. I think that these could have been easily resolved with better planning. As it stands, I think the experiment / analysis is unfinished. This should not be published. The research is promising, and future work with more experiments that have

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had their methodological problems resolved would yield valuable results.

To conclude, I think the authors are attempting to present preliminary work that is incomplete. In a field as important as anthropogenic impacts, there is no way that this manuscript can be acceptable. It presents original science that is promising, but the severe problems with methodology and how this work is presented is both misleading and potentially damaging for research on the impacts of ocean acidification on the cold-water coral *L. pertusa*.

Introduction

Overall, this is very poorly written with no real arguments well formed, nor, well answered. Nothing leads from one to the other and it is difficult to read. I go further below:

1. The distribution of cold-water corals is controlled by temperature.

Ah! Recent work shows that temperature and salinity are important together (Dullo et al., 2008; Davies et al., 2008). This is far too simplistic and is completely misreading for a non-expert reader. Either leave this kind of statement out or enter into the major environmental drivers of this species better.

2. They are mostly distributed below the photic zone at depths between 30 and over 1000m

This reads poorly. Are they found between 30 and 1000m, or at over 1000m depth? This fact has been well covered since we first discovered *L. pertusa*. Please ensure that this information is presented correctly.

3. The impact of ocean acidification on cold-water corals has not been investigated yet even though they seem to be much more vulnerable.

Wait! In your abstract, you make an unqualified statement that they will be among the first marine ecosystems to suffer from ocean acidification8230; This kind of supposition only misleads readers. We need to be very careful about the statements we make

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regarding the impacts of anthropogenic change on organisms. As research can often be misused to the detriment of the environment. Please be clearer with your language.

4. Their physiological requirements restrict the distribution of cold-water corals to high latitudes or to deeper depths, which exhibit lower saturation state of calcium carbonate (Guinotte et al., 2006).

What are their physiological requirements? Nothing is mentioned of their requirements with respects to dissolved oxygen or food supply. How can this kind of statement be made without proper qualification?

5. It is anticipated that more than 70 of the cold-water coral bioherms will be exposed to waters undersaturated with respect to aragonite by the end of the century.

It would be very appropriate to cite the work of John Guinotte here.

Methods

A major concern I have is the lack of reporting of the amount of boxcores that were used to collect samples. Pseudo-replication and non-independence of colonial organisms collected in a single box-core is a major issue for the analysis of data collected in reef forming organisms. This affects all of the analysis performed in this manuscript.

1. Corals were sampled using a box core sampler (50 cm in diameter), which sealed itself off after closing on the bottom. This allows collecting the benthos substrate together with overlying ambient seawater.

I am not totally convinced that there was no exchange of the overlying water as the boxcore is brought back to the surface. Was any measurements made to prove this statement?

2. climate-controlled containers

Is this an aquarium? What was the volume of the container?

3. they were kept at 78211;9

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Salinity and temperature were those of ambient seawater at sampling depths (and incubations).

If you read the work of Dodds et al. (2007), you begin to see that the respiration of *L. pertusa* is strongly linked to temperature. What was the exact temperature when the samples were labelled and their calcification rate measured? This is not listed in the methods, rather a broad 7-9°C is listed. We know that the respiration rate of *L. pertusa* is strongly linked to temp, just look at the figure 2 in Dodds et al., (2007), the rate varies hugely between 6.5 and 9 °C! We need more information to be certain about your measurements regarding calcification rate.

4. a triangular dredge

What were the dimensions of this dredge? How long was it dragged for?

5. Caps were placed on top of tubes, but not screwed air tight to avoid contamination by spilling of radioisotope-labelled seawater.

I don't quite understand how closing the caps would not help avoid contamination. Wouldn't it be better to close the caps tightly to avoid spillage AND maintain a closed system.

6. The incubations are nevertheless regarded as a nearly closed system since air flow and thus exchange with external air is greatly reduced by the caps.

How can a system be nearly closed? It is either closed or open.

7. For comparison with other published studies that have used different units, our units for G used in this study [% d8722;1] can easily be translated into mg CaCO₃ g8722;1 skeleton d8722;1 or mmol CaCO₃ (or Ca²⁺) mol8722;1 skeleton d8722;1 using the average initial skeletal weight given in Table 2.

This is unacceptable, please list these values in Table 2 and remove this sentence from

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the methods.

8. To characterise the seawater carbonate chemistry of study sites, data for dissolved inorganic carbon (DIC) and total alkalinity (TA) were used from sites close to sampling areas. For the Mingulay area data were derived from the GLODAP (Global Data Analysis Project) database (Key et al., 2004), cruise A24 of the World Ocean Circulation Experiment (WOCE) June, 1997 at 9.334 W; 57.75 N from 151m water depth.

This is 2 degrees west from the Mingulay site. This area has a complex topography, so there are likely to be different oceanographic conditions. Really this value should have been collected from bottle data during the cruise. To base any interpretation on this value is going to complicate matters.

The same comment goes for Skagerrak.

9. These uncertainties are likely underestimates because the TA and DIC data were, despite their proximity to the sampling site, not taken from exactly the same location and at the same time as coral samples.

Then how can you justify using them, when TA and DIC could have easily been calculated by taking a water sample from each site.

10. 8230;we used a closed-system approach with step-wise

I thought that you used a cap that was not air-tight? There is inconsistency between this sentence and your methodology onwards and what you actually state earlier in the methods.

11. Hourly coral calcification was recalculated using average G and weight of branches incubated during this study.

I assume this should read calculated?

12. while data on ammonium release by L. pertusa were derived from van Duyl et al. (2005).

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I think using the data on respiration from Dodds is acceptable for the Mingulay samples, be what of geographic variation? This needs to be explained in further detail, what temperatures did you use and what respiration values did you extract.

I think using van Duyl et al 2017's values for ammonium might be incorrect because these samples were collected at a completely different location to the samples used in this experiment, and at a different depth. This needs to be discussed in more detail.

Results

1. The DIC of seawater was 2126 and 2115 $\mu\text{mol kg}^{-1}$ for Mingulay and Skagerrak, respectively. Calculated ambient pH was, respectively 8.10 and 8.06 at Mingulay and Skagerrak (Table 1). The pCO_2 and O_a for ambient pH treatments were 352 ppm and 2.25 for Mingulay and 386 ppm and 1.89 for the Skagerrak treatments.

These are calculated from the data extracted from GLODAP bottle data for Mingulay and from the other cited source for Skagerrak? If so, then this needs to be reworded and presented more clearly. You cannot present values collected from a distant site and present it as a fact. This will mislead readers and cause confusion. If you present this, then please be clearer about where this information has come from. Otherwise, remove this from the paper.

2. The average rate of calcification was 0.067 \pm 0.019% (N=8) for Mingulay

I assume this is % d^{-1} ?

3. but calcification rates spanned 2 orders of magnitude, between 0.0027 and 0.1923% d^{-1}

Between samples or polyp rank?

4. (t-test, $t(1,22)=2.126$, $p=0.045$).

Please follow standard format for presenting degrees of freedom etc:

t-test, $t_{(1,22)}=$

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5. A one-way ANOVA reveals a significant effect of pH on calcification rates of *L. pertusa* collected in the Skagerrak ($F_{2,45}=7.03$, $p<0.001$). Subsequent comparison revealed that G was significantly different between ambient seawater and the treatment where pH was lowered by 0.3 units (HSD, $p<0.01$), but relative to treatment with the 0.15 pH unit reduction, there was no significant difference (HSD, $p=0.22$ and $p=0.31$ between ambient and 8722;0.15 pH and between 8722;0.15 pH and 8722;0.30 pH units, respectively).

This would be better reported in section 3.2.3.

Discussion

1. **Despite several methodological limitations due to our simple experimental approach 10 and the uncertainties with respect to the carbonate chemistry the results appear to show realistic calcification rates of *L. pertusa* with youngest polyps calcifying fastest and of the order of magnitude as the daylight calcification of slow growing tropical corals.**

This needs to be qualified by references and comparative statistics with existing literature.

2. **Due to decreases in pH as a result of coral respiration and calcification during incubations, our estimated calcification rates may well underestimate the actual calcification rates of cold-water corals under in-situ conditions.**

This shows that this research is unfinished. The respiration rate should have been recorded at the same time as calculating calcification rate.

3. **but it is not known which factors trigger budding in *L. pertusa*. It could be intrinsic (genetically controlled) as well as environmentally induced. Food and**

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nutrient availability are suggested to be important factors providing the necessary energy for calcification and reproduction (Spiro et al., 2000), but changes in abiotic environmental parameters such as temperature, current regimes and carbonate chemistry might further influence new corallite formation. Also, fragmentation and other disturbances may eventually trigger higher budding rates.

Is there any evidence from tropical calcification to support these hypotheses?

4. Growth morphologies with densely spaced new corallites and high budding rates were observed in dying *L. pertusa* colonies (Freiwald et al., 1997).

This is contradictory to what you say earlier in the discussion.

5. It is not clear what causes this difference and whether this constitutes a site-specific characteristic or if differences are due to different sampling depth, seasonality or seawater carbonate chemistry. The higher calcification rates of Mingulay corals do reflect the higher initial \dot{O}_a at this site. Also, many of the branches of *L. pertusa* sampled from Skagerrak showed encrustations or overgrowth by the sponge *Hymedesmia coriacea* (van Soest et al., 2005; Maier et al., 2007). Such sponge overgrowth can constitute an additional stress factor. Energy spent for chemical or mechanical defence would consequently be not available for calcification and might have caused reduced calcification of Skagerrak corals.

Another example of unfinished research, I don't think comparisons can be made between Mingulay and Skagerrak yet, as there are too many unknowns. The presence of samples that were overgrown with sponge is a problem as it shows an initial difference between the samples from the two sites prior to experimental work. The samples

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need to be standardised to ensure a comparison between sites. More sites could have answered this question, plus some observational work on the local environmental conditions would have been valuable.

6. 4.3 Calcification rates in response to reduced pH

This entire section is initially flawed, as by the author's admission, the samples used were overgrown with sponges and reflected a different calcification rate to samples that appeared cosmetically healthy. To test the impact of pH on less optimal samples, and to use only a few replicates is going to be problematic when it comes to forming strong conclusions.

7. It is also pivotal to carry out longer-term experiments that take into account coral adaptation and to test the effects of ocean acidification in combination with elevated temperature to better predict the fate of cold-water coral bioherms.

And these experiments should be the ones being published, not the preliminary work presented here.

Figures/Tables

1. Table 1

This data is misleading as mentioned earlier in this review.

2. Figure 2 + 3

Are generally of low quality, I don't know whether this is an issue caused by the PDF making process or the initial quality of the figure imported into the document.

Interactive comment on Biogeosciences Discuss., 6, 1875, 2009.

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