

Interactive
Comment

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

C. Maier et al.

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Authors general comments and answers to referees:

We would like to thank both referees for their time and interest in our manuscript. In the following we will provide some general comments at first (I) and will reply to referee 4 and 2 in the following (section II and III)

I) General comments

When evaluating this and other experimental work on cold-water corals, one has to take into consideration, that physiological experiments with cold-water corals are logistically very demanding which is why there is up to date hardly any publication using an experimental approach with cold water corals. We are aware of the short-comings of the present work, which is the control of seawater carbonate chemistry during incuba-

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tions. They were addressed, perhaps not thoroughly enough, in the manuscript. We used a very small volume for incubations (owing to the use of radioisotope tracers) and conducted experiments directly onboard the research vessel with restrictions of time, space and manpower. Furthermore, conducting radioisotope-labeling experiments onboard is not trivial which explains why no similar experiment was published until now and it is not very likely, that these kinds of experiments will be repeated in the near future. The sub-sampling of single corallites for radioisotope studies resulted in approximately 300 sub-samples that were treated and analysed during several months in the lab and resulted in additional information on the large variability of calcification rate according to polyp age. The present study addressed and provided the very first estimates of calcification rates of *L. pertusa*, calcification rates and relates them to polyp rank (as proxy for age). It also reports for the first time on the response of calcification to pH lower than ambient seawater.

Conducting short-term experiments directly onboard with freshly collected corals using ambient seawater is the closest one can get to in situ conditions. We believe that this is another strength of the present work and that it should remain a principal approach to follow in the future. Direct onboard incubations will facilitate a comparison of cold-water calcification over broad biogeographic areas which exhibit a natural variability in seawater carbonate chemistry, food supply and other parameters. The present manuscript contributes to supporting such an approach and it provides a base-line for future studies on *L. pertusa* calcification under ambient and reduced pH.

II) Comment to referee #4:

We are grateful to the constructive comments and criticism of referee #4 and will take his comments and suggestions 1) and 2) on changes in carbonate chemistry due to HCl addition as well as changes in carbonate chemistry during incubations into account when revising the manuscript. To the 3rd point he raises, the "Non-biological incorporation of ⁴⁵Calcium"; we can state with great certainty, that any non-biologically bound ⁴⁵-calcium can be neglected. We can draw this conclusion from the simple fact, that

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we sub-sampled the branches according to polyp rank. The last sample of a branch (that with the highest polyp rank) was always the sample that had the broken off area of skeleton which was partly bare of tissue. These last samples had normally neglectable low 45-calcium rates and all other polyp ranks were completely covered by tissue.

III) Comment to referee #2:

Referee #2 recommends that this work should not be published. We find this recommendation very surprising. Indeed, among his/her numerous comments made, only two major points (8./9. and 12. under methods) are valid, although they were already addressed in the manuscript. The remaining 28 points, some of them very useful, are of minor importance. We provide below a point-by-point reply to the referee's comments. It demonstrates that his/her recommendation lacks objectivity, that the strong statements used are unfounded, and that the review is borderline with respect to the ethics of peer-review. We invite the referee to reply to our points by submitting an anonymous or attributed comment before the end of the discussion period.

Introduction

Point 1 and 2: Our short introduction on the distribution of cold-water coral is not misreading. Temperature is one of the discriminating factors influencing presence or absence of cold-water corals. Stating this does not mean there are no other important factors; this will be made clear in the revised version of the MS.

3. and 4.: We do not understand the criticism as the explanation on why cold-water corals are likely more vulnerable comes in the following sentence. Of course cold-water corals are restricted in their distribution by their physiological requirements; this is common sense and should not need elaboration.

5.: This is correct. The citation will be added.

Methods

The major methodological concern of the referee is the number of box cores that were

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used and pseudo-replication due to non-independence of colonial organisms. The number of box cores used in this work will be mentioned in the revised manuscript. However, this does not provide any information on the clonal relation of the single branches used in the incubations as small branches from 1 box core are not necessarily from clones and different box cores do not guarantee that branches are from different clones. Furthermore, the branches were incubated separately and if clonal branches were present, they were distributed randomly between treatments and branches of a same clone would be in different treatments as well. Therefore, if the variability between samples would be reduced due to having clones, this would also reduce the variability between treatments.

1.: Box core sampler: The box corers used are extremely heavy gear specifically designed and used routinely for sampling benthos and overlying ambient seawater. It is proven, that they are sealing off properly unless they did not close correctly at the bottom. If there is a leakage or water exchange it is due to substrate that is caught between knife and drum (bottom) or between the lid and drum (top). Such a situation is obvious as soon as a box corer is on board and as water is already lost while hauling the box corer on board preventing the use of the sample.

2.: Climate-controlled containers: climate controlled containers are large containers used on ships which can be equipped like fully functional laboratory units or in this case a climate chamber. This is common knowledge and it is perhaps not necessary to dwell on it in the MS.

3.: Here our statement is also correct, corals were kept (when not used in experiments) at a temperature that ranged between 7 and 9 °C due to the temperature regulation in the climate containers (+/- 1 °C). Since corals in situ can also experience temperature changes of more than 2 °C as a consequence of short-term changes in current direction this does not cause a problem. During incubations (radioisotope labelling of 24 h duration), ambient seawater temperature was used; it was controlled using a thermostat bath which was better regulated (+/- 0.1 °C). We are well aware of Fig. 2 in

Dodds et al. (2007) as it is used as a source of data in the manuscript. Respiration approximately doubles between 6.5 and 9 °C. In our calculations we used corresponding maximum respiration rates as a conservative estimate for respiration of Skagerrak and Mingulay corals, respectively.

4.: Triangular dredge:the dimensions and time of dredging will be mentioned in the revised MS.

5. and 6.: The referee is right; these sentences will be re-phrased. What we intended to say: in calculations we assume a closed system, but we cannot completely exclude the possibility that a small amount of air exchange took place despite the fact that caps were used to avoid spilling of 45-calcium contaminated seawater. It would have been preferable to have an open system to allow for exchange of metabolic CO₂ released into seawater with air. By assuming a closed system; for calculations, we actually over-estimate the likely range of change of the carbonate chemistry induced by calcification and respiration.

7. What is unacceptable? To provide information enabling to express calcification in different ways? We believe that it is useful and should be done in every paper. Providing results in all units is unnecessary and a waste of valuable space.

8. and 9.: Here the referee's criticism is founded and this is one of the short-comings of our approach. This is openly addressed in the manuscript and we try to provide the best possible estimates with the data that are available. Clearly bottle data from the study sites would have been much better than using data observed at some distance and some time ago. If this were so easy, there would be in situ data from the vicinity of cold-water coral ecosystems available in the literature. Yet, there are none. It is actually our group that now strongly recommends sampling in situ water for DIC and TA analyses from cold-water coral vicinities to better characterize the carbonate chemistry on a broader geographical scale (Maier et al, 4th ISDSC, Wellington, NZ, 2008). In the present study, we primarily focus to provide the first estimate of calcification rates of L.

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pertusa in ambient seawater and in seawater with reduced pH.

10.: This is the same as 5. and 6. and we will carefully check the manuscript to ascertain, that this is clearly and correctly expressed.

11. Ok, will be corrected in the revised version of the manuscript.

12.: Here, the referee is also right. We used data on ammonium release by *L. pertusa* derived from our studies conducted in the Rockall Bank, a much deeper site and further away from any coastal areas. Same is true for the data on respiration from Dodds et al (2007) as they are "only" data representative for corals sampled from the Mingulay area. However, these are the only data available as yet. For the time being, we can, as the reviewer suggests, discuss this in more detail and provide further estimates using a broader range of ammonium release and how this would change our estimates with respect to the carbonate chemistry during incubations.

Results

1.: We actually think, that this is stated clearly with no intention to mislead the reader, but this will be rephrased as "were estimated to be...", otherwise, we think, it is clear that our data to estimate seawater carbonate chemistry were not measured directly, but derived from data available as close as possible to our sampling sites.

2. Yes, we change to % d-1.

3. This is what it states: "calcification rates spanned 2 orders of magnitudes, between 0.0027 and 0.1923 % d-1". Meaning: 0.0027 is the overall minimum value measured and 0.1923 is the overall maximum value measured. These are 2 extremes and illustrate the large range. To give any further information on these 2 extreme values would be misleading and statistically unsound. The statistical comparisons according to polyp rank, treatments or sites are given elsewhere in the manuscript.

4. Ok, the respective numbers will be in subscript.

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5. This ANOVA is related to bulk samples, that is why it is in section 3.2.1. and not in 3.2.3., but we agree that it is confusing. The headings will be changed to distinguish that 3.2.3. is related to pH effects according to polyp rank, while 3.2.1. related to whole branches and includes response to pH.

Discussion

1.: Four references are given to support this statement that maximum calcification of 1 %d-1 is similar to daytime calcification of some tropical coral. How shall we do comparative statistics without having access to the full data sets published on tropical corals (all papers report aggregated values as means)? We can elaborate a bit on the results provided by the literature cited, but not on a statistical basis.

Point 2., 5. and 6.: We are at loss to understand the comments "unfinished research" that the referee is using in several occasions. Reading his/her comments suggests that the minimum amount of data required to get a paper published in this issue is to:

- perform perturbation experiments that combine elevated pCO₂ and temperature, as pCO₂ by itself is found insufficient
- investigate more than two study sites, as two sites are considered insufficient
- compare only "standardized" corals if from different sites
- measure a suite of biological functions as calcification is not enough in the referee's opinion (perhaps excretion, respiration, reproduction and ingestion?)
- perform long-term experiments (only, long-term experiments will have other numerous flaws. The major problem is the keeping of corals under artificial aquarium conditions that do not mimick in situ conditions well)

So, although these points are all perfectly legitimate and desirable, one would want to be pointed to publications which actually fulfil all these requirements. Of course we are aware that there are none. To conclude, the statement "unfinished research" is unsubstantiated.

3. and 4.: These are hypotheses and there is no contradiction. One of the hypotheses,

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the role of disturbance on higher budding rates is actually supported by observations (Freiwald et al, 1997) and this was cited in the manuscript.

5. and 6.: see 2. above

7.: It should be obvious from the reply to comments 2, 5 and 6 above that we strongly disagree with this statement.

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

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