

Interactive comment on “Latitudinal trends in stable isotope signatures and carbon concentrating mechanisms of northeast Atlantic rhodoliths” by Laurie C. Hofmann and Svenja Heesch

Anonymous Referee #1

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The study by Hofmann and Heesch examines the $\delta^{13}\text{C}$ and related parameters of rhodolith species across a geographical gradient, with $\delta^{13}\text{C}$ being indicative of the proportion of HCO_3^- being directly taken up. I think this study is of good quality (I think) and useful, and has some really interesting findings that would be of use to phycologists. Below I detail some comments that could help the authors.

General comments I am somewhat confused with respects to the use of $\delta^{13}\text{C}$ total versus inorganic. In the discussion, the authors refer to past studies that utilise inorganic $\delta^{13}\text{C}$ values. Can the authors confirm they did not bleach these samples?

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If so, I do not consider that they can compare the $\delta^{13}\text{C}$ total with inorganic values in past studies. In my specific comments I detail why. pH drift experiments and light experiments: There are no details regarding the light experiment (which first pops up in the discussion). This needs to be fully described and re-reviewed. Also, what was the water motion levels in the pH drift experiments? Did the authors measure pH over time to determine the ideal time to stop the drift experiments? The total pH drift does not seem high compared to past studies.

Specific comments Page 1, Line 17: I would be hesitant to claim that rhodoliths are obtaining all of their DIC from CO_2 if their $\delta^{13}\text{C}$ value is ~ -25 . I would expect this to be lower (~ -29) – though I admit there is not much solid evidence that demonstrates whether the -29 “cut-off” is indeed due solely to CO_2 uptake or CO_2 -facilitated uptake via exCA. Also, this is in complete contrast to the conclusions and discussion statements later on. Page 2, Line 5: My take is that $\delta^{13}\text{C}$ is an indicator of whether or direct HCO_3^- is being used. Similar $\delta^{13}\text{C}$ values could be obtained if the individual was relying solely on CO_2 that was converted from exCA externally. The point here being that some in the community refer to exCA as a type of CCM. So this is more of a phrasing issue here. Page 2, line 26: Why was the total $\delta^{13}\text{C}$ measured, and not the inorganic compared to organic? I.e. bleaching the sample of organics before analysis. I think this would have yielded more interesting results, as the total value could be influenced by both the organic content and by changes in the inorganic $\delta^{13}\text{C}$ and organic values. Page 3, lines 8 onwards: The type of pH buffers, instruments used to measure pH, TA and salinity are all needed here. Figure 4: Pretty interesting that Lc has such high organic $\delta^{13}\text{C}$ values. I have not seen that too often in CCA. Technical comment: could the key contain the species names rather than short-hands please. Figure 6: A map could be good as another panel here to compare the $\delta^{13}\text{C}$ versus the latitude of the sites. Figure 7: That is interesting that here depth is inversely related to $\delta^{13}\text{C}$. So deeper specimens have less negative values. This is largely the opposite of what has been found with other species of macroalgae. I guess this is because other factors co-vary with depth that have stronger impacts here. I.e. organic carbon. Is

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it possible that these specimens could be using DIC that has been modified by the calcification process, and because there is so little organics, perhaps they maintain a larger proportion of their DIC budget from DIC that was once carbonate that has been re-dissolved? Just a thought. Page 5, Line 37: I would suggest some clarity needs to be invoked around the term “use” here. It is likely that HCO_3^- is taken up directly by the organism. However, what is converted at the site of calcification in calcium carbonate might not be HCO_3^- at the nano-scale. In my opinion, HCO_3^- uptake takes place for the purposes of utilising as the DIC in calcification, but this is different to what might actually be precipitated. Again, this is a minor point, but it think the authors need to be clear about what is being said. Line 30, page 6: How did the authors determine what was new growth versus old here in the light experiments? Did they stain the organisms? Then how did they process the samples: One month could be too short to see adequate growth to get enough new inorganic material for d^{13}C . I am not sure physiologically whether the organism would completely turn over the old organic tissue that was present at the start of the experiment too (as the authors allude to). Line 34, Page 6: I do not see the link physiologically with DOC and d^{13}C . The authors need to explain this a little. A few other references that could be useful are Lee and Carpenter 2001 Chemical Geology, Cornwall et al 2013 Proc Roy Soc B, and Cornwall et al. 2014 Plos ONE, which all examine either d^{13}C of organic or inorganic coralline algae (or both in the case of Cornwall et al 2013).

Technical comments Figure 5: Could the units of DIC be changed to $\mu\text{mol per kg}$ rather than moles. Figure 7: Panel labelling is needed here. Some references in the text are missing from the bibliography. E.g. Page 6, line 27: Cornwall et al. 2015.

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