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Biogeosciences Discussions

Interactive comment on "Mineralogical response of the Mediterranean crustose coralline alga *Lithophyllum cabiochae* to near-future ocean acidification and warming" *by* Merinda C. Nash et al.

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Dear Drs Nash, Martin and Gatusso,

I have carefully read the two versions of your manuscript, the comments provided by the three reviewers and your response to them. The reviewers raised valuable comments that helped improving your manuscript. However, there are some issues raised by the reviewers that still need to be accounted for (in more detail) before potential acceptance in Biogeosciences. I am looking forward to see a final version of your manuscript!

Lennart de Nooijer

Major issues

1. Related to the first concern of Reviewer #2, using XRD may not be a very widely accepted, standard method to accurately determine %MgCO3 in carbonates. The method followed here is based on the one outlined in a previous article by the first author (Nash et al., 2013, J Sed Res 83: 1084-1098). Going through this paper, it doesn't appear as if the obtained %MgCO3 from the CCA samples were (directly) compared to those measured by e.g. solution-ICP-MS. There is also very little information on the used reference material ('a dolomite reference standard', page 1088 of Nash et al., 2013) and the justification for a one-point calibration strategy. Isn't it necessary to correct for e.g. drift during the different runs?

I think the authors need to provide more detail on the analytical strategy followed here. Lines 127 and 128 mention the reproducibility (+/- 0.11 mol%), but not on which material this was based on. Dolomite reference material? If so, what is the mol% Mg of this material and is it similar to that of the CCA's? It also remains unclear how the fluorite was used as an internal standard. Was the F distributed homogenously throughout the powder? How much material was analysed per run, etc. Please provide such basic analytical details to enable a detailed comparison of these settings and the obtained data with those of future studies.

2. Reviewer #2 also mentioned the potential offset in ambient conditions and those in the direct surroundings of the CCA due to e.g. photosynthesis and respiration. Although the added paragraph does refer to (recent) studies on the potential physiological controls that determine the uptake of Mg, there is no mention of the potential mismatch between ambient and micro-environmental conditions. Please add this to the added paragraph. 3. The experiment ran for 1 year, in which the T (elevated or not) varied with that of the nearby Mediterranean (Martin and Gatusso, 2009; Martin et al., 2013). The analysis of the %MgCO3 is assumed to represent the average temperature and carbonate chemistry, but this assumes that the CCA's growth was linear. If, and this is not immediately clear from this or the previous papers, this is not the case, but e.g.

growth was confined to certain months, the inferred impact of T and carbonate chemistry on Mg-incorporation (and dissolution) may be misleading. Please elaborate on this or add cautionary notes both in the methods and in the discussion on this matter.

In addition, there are some less important matters that may help you to improve the next version of your manuscript.

Minor issues

1. Line 28: please exchange 'organisms' and 'the' (like in line 47).

2. Line 29: please remove the comma.

3. Line 32 and throughout the rest of the manuscript: often the word 'mineralogical' is used, whereas only the Mg-content of the calcite is analysed and discussed. The latter suggests reporting of e.g. the mineral phase and such, which may better be avoided.

4. Line 35: insert 'their' after 'buffer'.

5. Line 36: insert 'thereby' before 'enabling'.

6. Line 48: replace 'are formed' by 'consist'.

7. Line 50: the 'calcite' here probably refers to low-Mg calcite (<8 mol%).

8. Lines 59-60: '...which is .... into the skeleton' is already in the first half of this sentence.

9. Line 62: better to use 'Crustose Coralline Algae (CCA)' at the beginning of this paragraph and only use 'CCA' in the rest of the introduction.

10. Line 64: 'differing dissolution conditions' may read better as 'varying saturation states' or something similar.

11. Lines 65-66: '...higher phases of Mg-calcite...' reads better as '...calcite with higher Mg-contents...'

12. Line 67: I don't see how this is a feedback mechanism, please clarify.

13. Line 76-77: I am not a native speaker, but I think that '...12-months exposure...' would read better as '...a 12-month exposure...' or '...12 months of exposure...'.

14. Line 102: consider adding 'in winter' and 'in summer' after '9:15' and '15:9', respectively.

15. Line 125: I guess the Mg content is calculated by also taking into account peak asymmetry (Nash et al., 2013).

16. Line 129: Should be: 'The effects of pCO2 and temperature on mol% MgCO3 were assessed...'

17. Line 135: please add: 'of the CCA's' after 'Mg content'.

18. Line 148: '...on the combination of material from 5 thalli...' reads better as '...on material from 5 thalli combined...'

19. Line 160: spell out 'vs'.

20. Line 173: could the authors include error margins for the value of 0.33 mol increase/ degree?

21. Line 179: informal phrasing: please use something like: 'The Mg content of the pink surficial crust increased with temperature similarly compared to that of the ...' By the way, if there is no statistical testing possible, how do the authors know that Mg increases with temperature for this part of the CCA?

22. Lines 191-192: the temperature range tested here is rather limited (3 âĄřC), so that I suggest not to present the reported increase in Mg as a 'robust' temperature response, at least not in general.

23. Lines 199-200: please include Willams et al. (2014, Geochim, Cosmochim Ac 139: 190-204) as a reference for algae with a considerably more sensitive Mg/Ca increase

per degree temperature degree.

24. Lines 224-226: please combine these two sentences.

25. Lines 227-232: this is unclear to me. If the % MgCO3 in the 700T treatment is not significantly different than that of the other treatments, it simply isn't. Comparing with the dissolution chips doesn't change this. Please rephrase.

26. Lines 233-241: please refer to the papers of Ries (2006, Geochim Cosmochim Ac 70: 891-900), Gamboa et al. (2010, J Geophys Res 115) and Krayesky-Self et al. (2016, J Phyc 52: 161-173) as examples of heterogeneity in the Mg content in CCA's.

27. Line 250: could the 'dissolution' also be caused by (micro-)organisms that actively dissolved part of the chips? It looks as if the authors presume in the discussion that any dissolution is a-biological. I wonder if there would be any non-biological dissolution at the saturation states/ MgCO3 contents studied here. Perhaps good to state explicitly what may (or not may) have caused the dissolution somewhere.

28. Line 278: 'less' probably means 'less sensitive'.

29. Line 293: it is obvious anyway that skeletal mineralogy is under biological control, please be more specific.

30. LIne 296: this is relatively well known for many other marine calcifyers (see e.g. Ries et al., 2011, Geochim Cosmochim Ac 75: 4053-4064 for an overview), which may be good to refer to here.

31. Lines 317-321: these sentences don't add much information to the discussion. Consider deleting them.

32. Reference to Hofmann et al. should come after that of Henrich et al.

33. Supplementary information: the tables are not in the same format. Please change.

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