

Interactive comment on “Relative roles of endolithic algae and carbonate chemistry variability in the skeletal dissolution of crustose coralline algae” by C. Reyes-Nivia et al.

Anonymous Referee #2

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Reyes-Nivia et al. Review

Morse, Andersson, and Mackenzie (2006) and Andersson, Mackenzie, and Bates (2008) opined that crustose coral algae (CCA) and other high-Mg calcite skeletal organisms could be the “canary in the coal mine” of ocean acidification (OA), and that these organisms and their Mg-calcite mineral phases may act as the first responders to OA and temperature increase (see review in Mackenzie and Andersson, 2013). Recognizing this, Reyes-Nivia and colleagues devised an excellent set of experiments using the CCA *Porolithon onkodes* and exposing the skeletons of this CCA in flow-through seawater chambers to four CO₂ and T treatments (pre-industrial, present-day, and two SRES scenarios for the future). Their basic conclusion from the experimental work

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is that increasing ocean acidity and temperature “may significantly disturb the stability of reef frameworks cemented by CCA” and that “surficial substrates harboring photosynthetic microborers will be less impacted than those with algal endoliths.” Their experiments certainly justify the conclusions, and I applaud the authors on their rigorous treatment of their experimental data using appropriate statistical techniques. I have read the interactive comments of Dr. Max Wisshak and the anonymous reviewer and agree with them. Reyes-Nivia et al. could improve their manuscript by taking these comments into consideration. In addition, the first paragraph on p. 7 requires some attention. First a minor comment: mol% MgCO₃ is repeated in the first sentence on the page. Second and more important and substantive comment: the authors base some of their conclusions on the solubility curve chosen for the biogenic Mg-calcite partial solid solution. They state that estimations of the saturation state of the experimental seawater chambers “are based on the stoichiometric solubility products using the biogenic “minimally prepared” solubility curve of Plummer and Mackenzie (1974)” for the Mg-calcite phases as a function of MgCO₃. Despite the reference to Diaz-Pulido et al. (2012), I think in this paper, the authors need to justify using this curve by a fuller explanation of the problems involved with the solubilities of the Mg-calcites by considering the works of Mackenzie et al. (1983); Bischoff et al. (1987 and references therein); Morse et al. (2006); Andersson et al. (2008); and see summary in Mackenzie and Andersson (2013). Furthermore, although I know it can be done, it will not be clear to the average reader how the authors have taken the experimental data of Plummer and Mackenzie (1974) reported as ion activity products (IAP) Mg-calcite and recalculated them into stoichiometric solubility products. In addition, since the authors are interested in the rates of dissolution of the CCAs as a function of seawater acidity and T, it should be pointed out that the rates do not simply depend on solubility of the carbonate phase but also on factors like skeletal microarchitecture (Walter and Morse 1985).

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