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Biogeosciences Discussions

Interactive comment on "The interaction of ocean acidification and carbonate chemistry on coral reef calcification: evaluating the carbonate chemistry Coral Reef Ecosystem Feedback (CREF) hypothesis on the Bermuda coral reef" *by* N. R. Bates et al.

Anonymous Referee #1

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This manuscript presents evidence from a field study that in situ coral skeletal growth averaged over a three month period is more correlated to [CO32-] and Wa (r2=0.68) than to temperature (r2=0.35) or light (r2=0.21). This is a significant finding since both light and temperature are known to exert strong control on the rate of calcification in isolation. If the manuscript stopped there it would have been great. However, Bates et al. go on to make the claim that net calcification on the Bermuda reefs will cease if the aragonite saturation state falls below 2.65. At a temperature of 20.0C, salinity of 36.6

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Interactive Discussion
Discussion Paper

and TA of 2370 this threshold will be reached when pCO2 exceeds approximately 500 uatm. This prediction is based on the intercept of the saturation state-D laby skeletal growth plot (Fig. 3b). I believe that it is important that the 95% confidence interval on this threshold be given. If the confidence interval is large the threshold may not have much useful predictive value. Also I think the authors need to clarify that there is a big difference between when conditions become unconducive for the growth of a particular coral species and when the reef framework begins to decalcify. The later reflects a host of processes that are likely not to be operative in the healthy coral colonies used in this study, i.e. processes going on in the pore waters in the dead framework of the reef. I would call what Bates et al have estimated as the threshold for zero skeletal growth of the coral D. labyrinthiformis and not the threshold for zero net calcification of the reef. The threshold for D laby seems very high to me. A look at the data for other species reveals that the threshold varies from 1.1 to 2.2 with an average of 1.6. It would be a help if the authors cited some of this relevant work. it is based on in vitro studies but still relevant. I count twelve relevant studies that are not cited. Maybe D laby has a much higher threshold that other species but it is also possible that the confidence interval on the D laby data does not allow them to say that it is significantly higher. It is conceivable that D laby growth will go to zero during the winter months in the next ten years but I don't find the data that are presented to be that compelling.

The finding that seasonal changes in CO3 and Wa are driven seasonal changes in the balance between autotrophy and heterotrophy is not surprising. Goreau and Goreau 1959 talked about photosynthesis pulling down DIC and raising CO3 and hence promoting calcification. They were talking about inside the coral but obviously the same applies outside the coral. Section 4.4 is extremely difficult to follow. They don't explain how they compute NEP. They seem to get it from the difference in pCO2 between offshore and Hog Reef but nowhere do they give how they go from that difference to a rate. The values for NEP that are given in Fig. 5a are far too high to be NEP i.e. 4-6 gC/m3/d. NEP is typically very close to zero on coral reefs and ranges from -0.6 to +0.6 g C/m2/d. I don't know why they don't use the changes in DIC and TA relative

BGD

6, C1966–C1969, 2009

to offshore waters to get their estimates of NEC and NEP. That would be the usual way of estimating coral reef metabolic rates. If they did it that way then the connection between rates of NEP and changes in the water column inventory of DIC and TA would be evident.

I do not recommend publication. This manuscript needs major revision before it can be considered again.

Below are some specific suggestions:

Section 2.2 Definition of TA not needed.

Methods A Table 3 should be added that contains all the carbonate chemistry measurements or at least the averages that correspond to the skeletal growth measurements in Table 2.

Page 7642. Line 12. Many relevant in vitro studies of coral calcification and saturation state are neglected here.

Page 7647-7648. I am unable to follow how NEP has been computed. It is usually computed from the changes in DIC and TA in the water relative to offshore values taking into account water depth and residence time. This method based on the air-sea pCO2 difference needs to be explained better and the actual equations used to obtain the NEP should be given.

Page 7651. Talks about skeletal density but the units are g/cm3/d. I think they are talking about calcification rates and the units should be g/cm2/d.

Fig. 2. Are the DIC and TA normalized to some constant salinity as suggested in the text?

Fig. 3. Skeletal growth should be plotted on the y or dependent axis and should have been regressed against the independent variable. How does the sensitivity of D laby to saturation state compare with other species of coral? This subject has been reviewed

BGD

6, C1966–C1969, 2009

recently.

Fig. 4 No reference in the text to this figure.

Table 2. Caption Skeletal growth mg CaCO3 g-2 d-1, should be g-1. It would be helpful if data on saturation state was provided in Table 2. I strongly recommend that this information be added.

Interactive comment on Biogeosciences Discuss., 6, 7627, 2009.