

Author Response:

We thank the anonymous reviewer for posting helpful comments on the paper. We feel it is important to note that there are very few in situ studies of the relationships between coral calcification and carbonate chemistry in the natural environment of which this paper is one. The revised paper compares both in vitro and in situ studies

Our response to the review is presented in indented Arial font for ease of review in the supplement.

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

Received and published: 11 September 2009

This manuscript presents evidence from a field study that in situ coral skeletal growth averaged over a three month period is more correlated to $[\text{CO}_3^{2-}]$ and Ω_a ($r^2=0.68$) than to temperature ($r^2=0.35$) or light ($r^2=0.21$). 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 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.

This is shown in the revised figure of the revised paper.

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 uncondusive 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.

We agree with the reviewer that other processes are important, and have modified the text accordingly to reflect these other factors.

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 than 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.

Several more references to relevant in vitro studies are included in the discussion of the revised paper. We feel it is important to note that there are very few in situ studies of the relationships between coral calcification and carbonate chemistry in the natural environment of which this paper is one. The in vitro studies indeed have a lower threshold, and a brief discussion of factors at play for the in situ and in vitro condition are brought into the revised paper. Amongst many factors, there are probably large taxonomic differences, acclimatization issues, energetics and feeding issues that need to be resolved, with the results that there is a spectrum of responses/thresholds in the natural environment.

The finding that seasonal changes in CO_3 and Ω_a 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 CO_3 and hence promoting calcification. They were talking about inside the coral but obviously the same applies outside the coral.

We agree with the reviewer about the Goreau and Goreau 1959 paper. It is important to show that this process occurs in the natural environment outside of the coral and that biological process and seasonal changes between net autotrophy and heterotrophy feedback to calcification.

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 $p\text{CO}_2$ between offshore and Hog Reef but nowhere do they give how they go from that difference to a rate.

We have revised the section accordingly so that the methods and discussion are easier to follow. The details of the method are clarified in this section of the revised paper, including how data were used to derive rates.

The values for NEP that are given in Fig. 5a are far too high to be NEP i.e. 4-6 $\text{gC/m}^3/\text{d}$. NEP is typically very close to zero on coral reefs and ranges from -0.6 to +0.6 $\text{g C/m}^2/\text{d}$.

In the revised paper, we show that the annual NEP is very close to zero. However, our model suggests (with caveats and uncertainties of using mass balance approaches explicitly stated) that there are larger deviations from zero over shorter seasonal timescales. Observed diurnal changes in DIC, $p\text{CO}_2$ and TA would also suggest higher daily rates of NEP on the reef system also.

I don't know why they don't use the changes in DIC and TA relative to offshore waters to get their estimates of NEC and NEP.

DIC and TA data are also used in the revised paper to derive rates of NEP. Both mass balance approaches had similar results.

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.

In the revised paper, we have directed the reader to Zeebe and Gladrow (2001) and earlier DOE 1994. Although it is perhaps repetitive for those of us who know the carbonate system, we still believe that for a broader audience that it is still useful to define explicitly what is meant by the terms DIC, TA, and Ω . Those who are familiar will probably skip this brief section.

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.

We agree with this point and have revised the Table accordingly.

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

Several more references to relevant in vitro studies are included in the discussion of the revised paper. Again, we feel it is important to note that there are very few in situ studies of the relationships between coral calcification and carbonate chemistry in the natural environment of which this paper is one.

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 $p\text{CO}_2$ difference needs to be explained better and the actual equations used to obtain the NEP should be given.

In the revised paper, we have clarified our methods for computing NEP and NEC and modified the text accordingly. Two different approaches are used with both DIC/TA and $p\text{CO}_2$ difference giving similar results. The $p\text{CO}_2$ difference method uses the difference in offshore and onshore $p\text{CO}_2$ data following mass balance concepts shown in earlier papers (e.g., Bates, 2002). We have also endeavoured to make sure that the caveats and uncertainties are clear for the reader.

Page 7651. Talks about skeletal density but the units are $\text{g/cm}^3/\text{d}$. I think they are talking about calcification rates and the units should be $\text{g/cm}^2/\text{d}$.

In the revised paper, we have slightly modified the text to explicitly describe the different rates presented.

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

[Yes. This is clarified in the revised paper.](#)

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 recently.

[This is clarified in the revised paper. The figure is revised and compared to other studies.](#)

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

[This is clarified in the revised figure caption.](#)

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.

[This is clarified in the revised Table.](#)