Author Response:

We thank Dr. Silverman for posting a very helpful review of the paper. In the revised paper we have addressed all of the comments brought forward by Dr. Silverman and this has improved the paper.

Our responses are interspersed with the comments by Dr. Silverman (in black), and we have used indented blue Arial font for ease of review.

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.

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I strongly support publication of this paper after revision.

This paper is among the few papers to show the dependence of coral calcification on the aragonite degree of saturation from in situ measurements. More importantly the authors have demonstrated convincingly that the threshold for decalcification due to ocean acidification speculated on in previous reports has already been crossed in the Bermuda reef. It is expected that the time period during an annual cycle in which this reef decalcifies will lengthen significantly within the next ten years. Thus, the process of decalcification will be gradual and not abrupt at least in subtropical reefs assuming the seasonal cycle of pCO_2 superimposed on the anthropogenic increasing trend in the water flushing them remains consistent, i.e. similar amplitude. This point may be interesting to address in another study. As a result the atmospheric pCO_2 threshold for decalcification is set even lower than proposed in earlier studies at least for subtropical reefs. While I do support the publication of this paper in this journal I have a number of comments that I think will help improve the manuscript as detailed below.

Specific comments:

1) The title of the manuscript is to long and doesn't make sense, especially the first part, i.e. it isn't clear what interacts with what. Perhaps it should be "The response of coral calcification to ocean acidification: . . .", or "Redefining the decalcification threshold of coral growth in response to Ocean acidification: . . .".

> This is a good point and we have simplified the title of the paper to "In situ responses of coral calcification to ocean acidification"

2) There are a number of references appearing in the text, which don't appear in the list of references. I have gone over just the first 28 lines of the introduction (p. 7629) and found a number of missing refs: Buddemeier 1996, Buddemeier and Smith 1999, Wilkinson 1998 and 1999. It is not clear which Edmunds is referred to in 2007 (there are 2 in the list). I didn't bother going over the rest of the refs, there are probably some that are missing further on and some that weren't referred to in the text but appear in the list.

> We have cross-checked the references in the revised paper to make sure that references appearing in the text appear in the citation list and vice versa.

3) Table 1 – You could include the data from Silverman et al. (Biogeochemistry (2007) 84:67–82) for both diurnal and seasonal ranges in this table as well as other studies, such as Gattusso et al. (1996) with a little extra work.

> This is a useful addition to the Table. In the revised Table, we have added the data from the Silverman et al 2007 and Gattuso et al., 1996 papers, in particular.

4) p. 7629 l. 5: it is not clear what reference refers to what effect, partly because of the missing refs in the bibliography. What ref refers to the deleterious effect that sea level rise has already had on modern coral reefs?

The sentence on the effects on coral reefs is modified in the text. Since the impact of sea level rise occurs on "geological" timescales rather than societally relevant timescale, we have removed this from reference from the text.

5) p. 7629 l. 4: is sedimentation increasing due to global change? Should this be increased sedimentation rates rather than increasing?

As above, the sentence on the effects on coral reefs is modified in the text. Since the impact of sedimentation is speculative, we have removed this from reference from the text.

6) p. 7629 l. 14: why is partial of CO_2 referred to in the plural?

This was a typo that is corrected in the revised paper.

7) p. 7629 l. 13-14 – "For example, over the last few decades, dissolved inorganic carbon (DIC) and partial pressures of CO_2 (pCO_2) have increased while pH has de- creased". The way it is written one could understand that ocean pH decreases independently of atmospheric CO_2 increase, which is of course wrong.

We have modified this sentence in the revised paper. The sentence is split and the observed rates of pH change from various ocean time-series are included.

8) p. 7629 l. 22 – ". . .will increase as W values. . ." should be Ω.

Yes, this was a typo that is corrected in the revised paper. It should read Ω of course.

9) p. 7629 l. 21-24 – "In addition, it is also likely that the dissolution of carbonate sediments and structures will increase as W values decline in the future (Wollast et al., 1980; Andersson et al., 2003; Morse et al., 2006; Yates and Halley, 2006; Andersson et al., 2006, 2007, 2009)". To the best of my knowledge there are no observations that support this statement. Yes, observations have shown dissolution at high pCO_2 but none have shown any coherent dependence of $CaCO_3$ dissolution on pCO_2 (except for the modeling papers).

Yes, we agree with this comment and have modified accordingly in the revised paper. Model studies and theoretical studies of course predict this effect but observational/experimental data is lacking. We are hopeful that observational/experimental studies will be forthcoming as one aspect of future ocean acidification research.

10) p. 7629 l. 25-28 – "Experimental studies have shown that the ability and the rate at which coral reefs calcify decrease as a result of ocean acidification, decreasing seawater [CO_3^{2-}] and Ω (e.g. Gattuso et al., 1998, 1999; Marubini and Atkinson, 1999; Marubini and Thake, 1999; Langdon et al., 2000; Langdon, 2001; Langdon and Atkinson, 2005)". The ref to Gattuso et al., 1998 is inaccurate because in this study artificial seawater was used and the Ω_{arag} was manipulated through changes in concentrations of Ca^{2+} and not CO_3^{2-}. Additionally, Ω in the manuscript should be Ω_{arag}, i.e. the saturation state of the aragonite mineral which is precipitated by corals.

Yes, we agree with both points and have modified accordingly in the revised paper. Throughout the revised paper, we have changed Ω to $\Omega_{aragonite}$.

11) p. 7630 l. 1 – "Observations from coral colonies and coral reef community meso- cosms exposed and equilibrated with high levels of atmospheric CO_2 (500–700 ppm) and lowered [CO_3^{2-}.] concentration (with lower values of Ω with respect to aragonite) have generally shown reduction in the rates of coral calcification". Should probably be mesocosms exposed to seawater equilibrated with high . . . You should also decide what units to use for partial pressure of pCO_2 (ppm or) and use them throughout the entire manuscript.

We have to disagree with the reviewer on this point. Atmospheric pCO_2 has units of ppm (derived from the mole fraction typically measured for dry air) while seawater pCO_2 has units of µatm (partial pressure derived from [CO_2] and Henry's Law… in a wet solution of course). In the revised paper, we add a note that the two parameters have different units, but the differentiation in units has to been consistent.

12) p. 7630 l. 20-22 - Field studies have subsequently indicated that rates of calcification are 3–5 times greater in the light than in the dark (Gattuso et al., 1999; Schneider and Erez, 2006), . . .". The Schneider and Erez study is not a field study.

Yes, we agree with this comment and have modified accordingly in the revised paper.

13) p. 7630 l. 22 – p. 7631 l. 6 – this whole section describes briefly the physiological process of bio- mineralization in corals based on the current literature which has no bearing whatsoever on the

methodology, results, relations and conclusions which are presented in the following. Therefore I think that this section should be withdrawn entirely from the manuscript.

Yes, we agree with this comment. These sentences interrupt the flow of the paper, have only an indirect bearing on the methods, results and discussion in the paper, and have been deleted in the revised paper.

14) p. 7631 l. 14 – 17 – "With ocean acidification, it has been proposed that the combination of reduced rates of calcification and increased rates of $CaCO_3$ dissolution could result in coral reefs transitioning from net accumulation to a net loss in $CaCO_3$ material. . .". Again the increase in dissolution rates statement is unfounded see item 9 above.

Yes, as in point 9, we agree with this comment and have modified accordingly in the revised paper.

15) p. 7632 l. 1 – 3 – "As stated earlier, there is very limited field data on the relationships between calcification and carbonate chemistry (with the exception of Silverman et al., 2007), particularly over seasonal to annual timescales". You should include Ohde and van Woesik in the first ref and with respect to seasonal to annual timescales you should cite Silverman et al. (2007).

Again we agree with this comment. We have modified the sentence with this in mind to clarify the timescales of the studies mentioned in the paper.

16) p. 7632 l. 12 – you wrote contain and should be constrain.

This was a typo that is corrected in the revised paper.

17) p. 7632 l. 19 – you wrote "threshoulds" should be thresholds.

This was a typo that is corrected in the revised paper.

18) p. 7633 – p. 7634 – section 2.2 is extraneous and tedious, just state what apparent thermodynamic equilibrium constants were used in your calculations. Also, no need to mention what equations were used to resolve the carbonate system, it should be obvious. If you like you can refer the uninitiated reader to Zeebe and Gladrow (2001) for a complete description of the carbonate system equations.

We agree with the point that referencing to Zeebe and Gladrow (2001) and earlier DOE 1994 reference would be useful. Although it is 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.

19) p. 7635 l. 13 – you wrote "thermister" should be thermistor.

This was a typo that is corrected in the revised paper.

20) p. 7635 – it is unclear from this section or Fig 1 where CARIOCA was deployed (on the reef flat, in the lagoon). It would be best to indicate this important information on the Site map (Fig 1).

We have modified the figure to show the deployment of the CARIOCA.

21) p. 7636 l. 21 – 22 – it is unclear from the text and the following discussion of the results what the normalization of DIC and A_T was used for if at all in the analysis and interpretation of the results of this study.

We used the nDIC and nTA in the model analysis later in the paper. In the revised paper, we have noted that these data are used later in the text. Both terms account for local precipitation/evaporation processes and the use of nDIC versus nTA scatter plots are also helpful as diagnostic tools for deciphering relevant processes.

22)p. 7636l. 24–p. 7637l. 8–while,themethodusedtogetatthe PAR value at the surface is quite convoluted and impressive it is unclear how you obtained atmospheric transmittance (T_r) considering the wide range that you cite (what value or values were used?). Wouldn't it be easier to get pyranometer mea- surements from the Bermuda Station, which is run by Ellsworth Dutton and can be contacted at (Ellsworth.G.Dutton@noaa.gov)? It seems from the GEWEX site (http://www.gewex.org/datasets.html) that the data is available for free.

This was a very helpful comment and this data was also incorporated in the text. We also modified the text slightly to explain how atmospheric transmittance was calculated.

23) p. 7638 l. 19 – The authors cite a report (MEP, 2006) and refer the reader to a website where he/she can download the nutrient data presumably (http://www.bios- mep.info/). Unfortunately, the following links only allow you to download an executive summary of the report, which I'm not certain are relevant to the period of measurement 2002-2003 as opposed to 2006 of the report. Under these

conditions it would be helpful to show at least a figure of the available annual cycles demonstrating the consistent cyclical nature of this parameter.

We will check the website reference and make sure that the report is available on the BIOS website. But you are right and we have included a new figure to show the annual cycles. We also use MEP data from the 2002-2003 period also.

24) p. 7638 l. 20-23 – The authors state that salinity varies annually within a range of 36 to 36.8 PSU, yet it is not clear if precipitation during summer is the major cause for reduced salinity in the lagoon (local) or is this a more general characteristic of the region.

We have modified the sentence in the revised paper. Offshore in the surrounding Sargasso Sea, mixed layer salinity typically varies between 36.2 and 36.8 with many years of observations at the BATS and Hydrostation S sites. We add references to this. Occasionally, due to the very shallow nature of the Bermuda platform, rain events can dilute salinity very slightly for a brief period depending on stratification and mixing/residence times on the reef system of course. We don't use units for salinity since salinity is dimensionless.

25) p. 7641 l. 6-8 – "If the in situ skeletal growth rates observed at Hog Reef are scaled up, we estimate that the calcification rate per unit area of the reef ranged from 1.9 to 13.1 g $CaCO_3 m^{-2} d^{-1}$, assuming a range of coral cover from 30–70

The comment ended and we're not sure what was meant here.

26) p. 7641 l. 15-18 – "The annual rate of calcification per unit area of the reef is estimated at Hog Reef to range between 0.5 and 3.5 kg $CaCO_3 m^{-2} year^{-1}$, slightly lower than the average calcification rate of 4±0.7 kg $CaCO_3 m^{-2} year^{-1}$ reported for other coral reefs (Kinsey, 1985)". Taking the average of the range and not the max rate the change in calcification relative to Kinsey's rate is 35

We agree and have modified the relevant sentences in the revised paper.

27) p. 7643 – 7645 section 4.3 – see item 13 above.

Yes, we agree with this comment. These sentences interrupt the flow of the paper, have only an indirect bearing on the methods, results and discussion in the paper, and have been deleted in the revised paper.

28) p. 7652 l. 12-14 – at least show a figure of the data from 2009.

Yes, we have added a figure that shows the 2009 data in the revised paper.

29) In the conclusions as well as throughout the discussion the authors disregard the effect of temperature on coral calcification despite the fact that coral growth has been shown to have an optimum dependence on temperature in a number of previous studies. Since the temperature range over an annual cycle is quite large at the Bermuda site the "disagreement" between the apparent coral growth independence of temperature in this study and the previously reported relation in experimental studies requires additional consideration rather than just cursory acknowledgement. Silverman et al. 2007 and 2009 consider the dependence of coral growth on temperature and Ω_{arag}, perhaps it would be useful to test the relation that they propose with your data.

In the revised paper, we have modified the text to state that the relationships between temperature and calcification could not be as clearly shown for the Bermuda reef. We agree with the reviewer that temperature is important and we have added text to balance our discussion of the results. The approach of Silverman et al., 2007 is also very useful and we have added a similar analysis of the Bermuda data to the revised paper.

30) Table 2 – what do the errors represent (min-max, STD, SE)?

In the revised paper, we have modified the Table accordingly.

31) The time scale in Fig. 2 is unclear, its hard to tell when the year begins and when it ends.

In the revised paper, we have revised the Figure accordingly to show the timescale better.