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Biogeosciences Discussions

Interactive comment on "Impact of carbonate saturation on large Caribbean benthic foraminifera assemblages" *by* Ana Martinez et al.

Ana Martinez et al.

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Dear Inge,

Please find our response to your very helpful comments which improved the manuscript considerbly. Each response in listed directly following the comments made

Referee #1 General Comments:

Comment 1: The manuscript 'Impact of carbonate saturation on large Caribbean benthic foraminifera assemblages' by Martinez and co-authors aims to show the response of the benthic foraminiferal community to by using natural low pH low calcite saturation submarine springs. They show calcareous and agglutinating foraminiferal abundancies decrease, but the calcareous non-symbiont bearing species seem to be impacted the

most. The manuscript is reasonably well written, and the results are in line with some other similar studies, but I have some concerns about the methodology that could impact the observed trends. Especially lumping porcelaneous and hyaline species together and only using the larger fraction (>250 um) might bias some of the results. The discussion could use some restructuring and extra depth, by for instance analyzing trends porcelaneous and hyaline species, adding size normalized weights of Discorbis, exploring the effect of salinity and different symbiont species.

Reply: We thanks Dr. van Dijk for recognizing the importance of the study and we appreciate the suggestions to include a more detailed analysis of the foraminifera data. We have included new analyses and interpretation of abundance of porcelaneous and hyaline foraminifera and of symbiotic diatom-bearing and chlorophyte-bearing foraminifera in the new version of the manuscript. We have also included an explanation of why the >250 ïA■m size fraction was used in the analysis and a deeper discussion of the effects of salinity on foraminifera. Although we do not report size normalized weights we did make an effort to select individuals that were similar in size as much as possible and re re-weighted many samples to see if this changes the results which it did not. Regardless we acknowledge this shortcoming of not normalizing the weights. The updated discussion was structured in paragraphs to facilitate readability.

Major comments:

Comment 2. Page 3 line 14: What kind of substrate was present and was there a difference in substrate near the vents and at the control site? Reply: The substrate is coarse sand at all locations, and control and ojo sites were only a few meters apart. We have included this information on the methods section. Did you include plants (some benthic species prefer to live on plant debris)?

Reply: We did not sample plants specifically and any epiphytes that are included were in the upper sediment. Because there are no grass beds right at the springs, we set the control sites to be as similar as possible to the ojo sites avoiding grass beds as this

made sense for more direct comparison.

Did you apply rose bengal staining to only analyze living specimens?

Reply: We used Rose Bengal but pretty much everything got stained to some degree and it was hard to distinguish dead from live using this stain. Rose Bengal can stain proteins of dead specimens that are not fully decomposed, or proteins of bacteria inside or on the tests, producing false positives that overestimate abundance of foraminifera (Bernhard et al., 2006; Paleoceanography, vol. 21, pa4210, doi:10.1029/2006PA001290, 2006). In addition, it is hard to distinguish the stained specimens in some species with opaque tests such as Archaias angulatus (Wantland, 1967). We also tried CellTracker Green but, in this case, only very few forams got stained so that was not useful for the statistical analysis.

Comment 3. Page 3, line 15: Why did you choose 250 um? Normally 125-150 um is used (Schonfield et al., 2012: Marine Micropaleontology, 94–95), since you might miss the trends in the smaller community now. The trends you observed might be true for larger specimens, but perhaps the smaller specimens tell a different story. . .

Reply: We focus on the large size fraction and clearly note this in the title because this size fraction constituted the majority of foraminifera in the samples. Indeed, many foraminifera typically found in tropical lagoons attain large sizes and have mortality rates of above 95% of juveniles until they reach a diameter of 0.5 mm (Why are larger forams large? Hallock, 1985, Paleobiology) which may explain the low abundance of the smaller sized forams in our samples. We now described in the methods section that analyses of the <250 um fraction we found only 9-27 specimens per gram sediment while in the >250 um fraction around 300-500 specimens were found. The fraction of >250 represents the adult individuals more prone to be preserved in the sediment (Martin, 1986). We have included an explanation of the size fraction selection in the methods section.

Comment 4. Page 4, line 13-14 I am not sure about 'lumping' low Mg forams together

with porcelaneous in one group, since it is known from countless studies they respond different to increased pCO2, perhaps due to e.g. solubility of high MgCO3. Did you check if both hyaline and porcelaneous species in this group show similar trends? Otherwise you might be skewing your results, especially since you see no significant change in weight of shells of Discorbis. I would also be very interested to see (relative) abundances of low (e.g. Discorbis), intermediate (Amphistegina, Astergerina) and high Mg species (Quinqueloculina, Archaias) between ojos and control. It would bring something new to the existing studies on different sites, especially since you have the opportunity to test it here on species with very contrasting Mg content.

Reply: We have included the absolute and relative abundance of porcelaneous and hyaline foraminifera as well as of low, intermediate and high magnesium foraminifera in the new version of the manuscript.

Comment 5. Discussion section: The authors do not (clearly) explain why the abundancy of agglutinating foraminifera decreases at the vents. They do not calcify or have symbionts, so the explanations given to explain the calcareous response (proton pumping and symbiont activity) do not apply. Could salinity play a role?

Reply: The absolute abundance of agglutinating foraminifera did not differ with saturation state in 3 of the 5 sampled submarine springs and we note that in the paper. The relative abundance was higher at low saturation than at high saturation at one site and did not differ in the other 4 sampled sites. We have rewritten these results in a clearer way and we have discussed why agglutinated foraminifera are not sensitive to carbonate saturation as the reviewer indicates. We also explained why we think salinity is not driving changes in abundance of any of the foraminifera at the springs. In addition, sensors deployed at the springs showed that salinity is >30psu over 90% of the time and it does not drop below 27psu at the sites we sampled. When salinity drops below 30 psu (7% of the time), the low salinity exposure lasts for very short periods of time always less than 1 hour (Crook et al., Supporting Information, PNAS July 2, 2013 110 (27) 11044-11049; https://doi.org/10.1073/pnas.1301589110). Based on literature the

majority of forams we found have very wide salinity tolerance as they are common in settings that have variable salinity such as close to shore and in lagoons.

Comment 6. Page 7 line 22-29 The authors missed a big overview study by Doo et al., 2014 (Biol. Bull. 226: 169–186.) in which they present a nice overview of response of larger benthic foraminifera to ocean acidification. I think their discussion would benefit from including these observations. For instance, to look at the different kind of symbionts (diatom, dinos) your foraminifera species have and if they follow the general trend of Doo et al., 2014. It would also be informative to add an overview of the response of benthic foraminifera (symbiont/non symbiont) in different studies, like in Keul et al., 2013 to show how your data fits laboratory and field experiments.

Reply: We have now included the absolute and relative abundance of diatom-bearing (Ampistegina and Asterigerina) and chlorophyte-bearing foraminifera (Archaias). The studies included in the review by Doo et al., 2014 did not study chlorophyte-bearing foraminifera, therefore we cannot compare our results to the trends seen in other studies that only focused on diatom and dinoflagellate bearing foraminifera. We have included discussion on the potential effects of symbionts on foraminifera calcification.

Minor comments

Comment 7. Throughout manuscript pCO2 (p in italics)

Reply: We changed p to italics throughout the text.

Comment 8. Page 2 line 9-10: Keul er al., also contains a nice overview of species-specific responses

Reply: We have added this relevant reference in the introduction.

Comment 9. Page 2 line 29: Do other chemical parameters change between ojos and control? Oxygen, sulphates?

Reply: There are some relatively small differences between ojos, for example the water

discharging at ojo Norte has lower oxygen and it is slightly more reducing than the other ojos during very low tide conditions. However, we do not have replicates of ojos that differ from each other (in fact Norte is the only that is slightly different than the other ojos) hence we cannot do a comprehensive analysis on the impact of these differences in chemistry. Specifically, we did not see any unique trends at ojo Norte hence we do not attribute this to the small difference in water chemistry. Regardless we emphasize throughout that there are advantages and disadvantages to conducting field observation with the main issue if that there are confounding variables but on the other hand the results we obtain cannot be replicated in laboratory settings and are more realistic.

Comment 10. Page 3, line 08-10: It is more common to use the K1 and K2 values from Lueker et al., 2000. I would suggest recalculating your carbonate parameters with these, since Millero (2010) are known to cause discrepancies in the results amongst programs (for details see Orr et al 2015). Please also specify in more detail what constants were used for carbonate system calculations. For example, what term was used for KHSO4? Dickson (1990) is commonly used.

Reply: Thanks for this important suggestion. We recalculated the carbonate chemistry parameters with K1 and K2 from Lueker et al. 2000 and included a more detailed description of the constants used (KHSO4 from Dickson 1990 and total boron from Uppström, 1974) in the methods section.

Comment 11. Page 3, line 14-17: How much gram of sediment was counted?

Reply: At least 1gram of sediment and on average 2 grams of sediment (per replicate) was analyzed. We have inserted this information in the methods section.

Comment 12. Page 3, line 20-23: Even though only specimens from 250-355 um were picked, the test weights have to be normalized for size to be able to compare between sites and studies.

Reply: We agree with the reviewer that this would be useful. However, we have not done this and in an attempt to resolve this issue we re-analyzed 7 representative samples of the 50 sediment samples we collected for this study (5 replicates at 5 ojos and 5 control sites) normalizing to size, and still did not find any statistically significant difference in the weight. It seemed to be a major waste of time and effort to re-analyze again all 50 samples. We report on that and acknowledge the need to do so in the manuscript.

Comment 13. Page 4, line 7: There is no seasonality in the output/flux of the vents?

Reply: Yes, there is an increase in groundwater discharge during the rainy season and during low tide. We refer to a paper that descries the variability at the site. We note however that the foraminifera in the upper sediments represent decades or longer and these organisms grew under all the different conditions at the sites.

Comment 14. Page 4, line 24-26. The abundance of agglutinating foraminifera is very low already in the control sites. Do you think the numbers are high enough to make big statements of agglutinating foraminifera being more resilient to low calcite saturation state?

Reply: It is true that the abundance of agglutinating foraminifera is very low already in the control sites and most likely the numbers are not high enough to make big statements of agglutinating foraminifera; however, our data is in agreement with other high pCO2 field studies in Papua New Guinea (Uthicke et al., 2013) and Italy (Dias, 2010). We note that they are rare but still report on the results as we think this is useful.

Comment 15. Page 5, line 31: Fig 4 not 5

Reply: Thank you for spotting this mistake, we changed it in the newest version of the manuscript.

Comment 16. Page 5, Line 17-20-21: Fig 5 not 4

Reply: We changed this mistake in the newest version of the manuscript.

Comment 17. Discussion: The discussion needs some restructuring, perhaps adding paragraphs might help?

Reply: As the reviewer suggested the discussion was divided into different sections to improve the organization of information.

Comment 18. Page 6 line 25: 3-6 units is in my opinion not a 'slight' but a big difference and should be taken into account or at least discussed

Reply: We have included further discussion on salinity effects to make it clearer in the manuscript (see reply to comment 5).

Comment 19. Page 8, line 7-10. There is also evidence from culture experiments showing very species specific response of agglutinating foraminfiera with pCO2 (e.g. van Dijk et al., 2017, JFR).

Reply: Thank you for the suggestion. This is a relevant paper that has been included in the discussion of impacts of carbonate saturation on agglutinating foraminifera.

Comment 20. Page 8, line 19-25 This is not really discussed in detail the discussion and has therefore no place in the conclusion. Could you add a paragraph on this in the discussion section.

Reply: We moved and rewrote these lines in the discussion section.

Comment 21. Table 1: check number of decimals for consistency. Why is there no error on calculated CO2sys values, you could apply a propagating error.

Reply: We updated the number of decimals for consistency. We included the std on the actual reported values that we measured in the field using these values if anyone is interested the error of the calculated values (pH and carbonate saturation) could be determined. However, we did not include this because the difference between the ojos and control are so large that this will not really change any of the conclusions and discussion.

Comment 22. Figure 3: Top three panels: Can you put the 0 on the intersection between y and x axis?

Reply: The new plots have now the same Y-axis scale and are all aligned at 0.

Is it possible to order the ojos from e.g. South to North or vice versa?

Reply: The sites are now organized from North (Norte) to South (Gorgos) in plots and tables.

Thank you Adina

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