

Interactive comment on “Impact of seawater $p\text{CO}_2$ changes on calcification and on mG/cA and sR/cA in benthic foraminifera calcite (*Ammonia tepida*): results from culturing experiments” by D. Dissard et al.

Anonymous Referee #3

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I recommend rejection of the current manuscript barring major revision and resubmission with the inclusion of control experiment results that the authors have in their possession.

This manuscript reports new results from culture experiments for changes in the composition of calcite and calcification of the benthic foraminifer *Ammonia Tepida*. This represents a highly relevant area of research both in terms of the impacts of ocean acidification on this and related taxa (squarely within the scope of BG) and potentially also for paleoenvironmental reconstruction.

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A major flaw with this study is the lack of any reported control experiments performed at modern and/or preindustrial $p\text{CO}_2$ levels. This is particularly crucial to the significance of any conclusions that can be drawn given the two experimental $p\text{CO}_2$ conditions that have been reported (ie $p\text{CO}_2 = 120\text{ppm}$ and $p\text{CO}_2 = 2000\text{ppm}$). These both represent ‘extreme’ seawater carbonate system conditions, (as pointed out by one reviewer already) with the experiments at the nominal $p\text{CO}_2$ of 2000ppm seawater being highly undersaturated at 10°C and only slightly oversaturated at 15°C. One might hypothesise in the lack of evidence to the contrary that these conditions are likely beyond the boundaries that this particular species might encounter in any modern environment and that this could impose a significant associated stress on the organism. Control experiments are vital to drawing any significant conclusions from these experiments, given the reported experiments provide only a comparison of opposing extreme conditions.

The lack of any reported control experiments is particularly puzzling given these same authors have conducted and reported relevant control experiments (performed at a nominal $p\text{CO}_2$ of 380 ppmv and salinities of 24 and 33) in a ‘sister’ manuscript that has been submitted recently (early 2009) to *Geochimica et Cosmochimica Acta*. These experiments appear to have been conducted at the same as those reported in the current submission, and are highly relevant to the interpretation of any results. Indeed the results reported for the $p\text{CO}_2 = 380$ ppmv experiments go counter to the several of the ‘significant’ trends that have been based on the extreme $p\text{CO}_2$ experiments reported in this study.

It is my strong view that the authors be required to incorporate results from the ‘ambient’ $p\text{CO}_2$ experiments into the current manuscript and resubmit it for review.

In undertaking this I suggest that that authors need to further explain why the measured seawater carbonate system properties return much higher than the nominal experimental $p\text{CO}_2$ values for the 120 and 380 ppm experiments. Is this a problem with achieving equilibrium in these ‘gas bubbling’ experiments or with the measurement of one or more carbonate system properties?

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The current manuscript harbours a number of additional significant shortcomings that require attention in the event of revision and resubmission. These include: The assessment of calcification changes during culture is compromised by the impossibility of weighing the amount of foram calcite prior to culture and by the addition of new calcite layers in culture over pre-existing chambers. Importantly, to what extent are new chambers identified by the presence of the fluorescent marker, instead the occurrence of pre-existing chambers with new calcite layers. How could you distinguish these two cases.

One possibility for distinguishing new versus merely thickened chambers during culture might be the laser ablation ICPMS profiles. However, based on the appearance of the ablation 'pits' in the provided SEM image I would be surprised if analyses undertaken as part of this study have the necessary depth resolution. The laser ablation 'pits' shown in the SEM are surprisingly poorly formed, with evidence of mechanical fragmentation of the shell to the extent that it is not clear what has been sampled and analysed during the ablation process. This is also important for confidently interpreting any Mg/Ca analysis given the strong concentration of Mg in any residual cytoplasm that may not have been removed from within or on the test prior to analysis.

Lesser technical and other comments The data tables are difficult to read in some instances due to the use of too many significant figures, the inconsistent use of orders of magnitude and uncertainties (Table 4 specifically), and unclear statement of uncertainties (1 or 2 stdev?) in tables and figures.

References for Bentov and Erez, and also Nehrke et al are missing. Bernhard and Benthov are misspelled.

Beyond an introductory sentence to the impacts of ocean acidification driven by rising pCO₂, the first three sentences of the abstract are generic and uninformative. I suggest their removal.

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