

Interactive comment on “Calcification response to climate change in the Pliocene?” by C. V. Davis et al.

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Dr Bijma’s comments primarily focus on the justifications for the time period and study site chosen and point out some shortcomings in the discussion of bottom water and pore water saturation state at the site. We agree that these are important points and have strengthened our introduction by including a clearer outline of the rationale behind the study. Specific points are addressed below.

“The first question that came to my mind was why the authors didn’t choose a period with a higher $p\text{CO}_2$, such as, e.g. expected for the end of this century at a business as usual scenario? Although the authors mention some good arguments “continental placement and vegetation similar to modern” and going from the “end of the Pliocene Warm Period into the subsequent interval of global cooling”, the rationale didn’t be-

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come completely clear to me. Of course, our world is getting warmer but we can look at the data backwards. However, if you look at fig. 4. I do not see a long term cooling trend. Maybe they can state why they didn't go for e.g. the PETM. Also, by choosing a period of assumed atmospheric pCO₂ levels as today, the study would benefit from some comparison with today's assemblages or at least with a more thorough discussion of the experiments by Langer et al (several papers) or experimental data on weight changes as a function of carbonate chemistry. For instance, see fig. 8 in Bijma et al. (1999). Reassessing foraminiferal stable isotope geochemistry: Impact of the oceanic carbonate system (experimental results). Use of Proxies in Paleoceanography: Examples from the South Atlantic. G. Fischer and G. Wefer. Berlin, Heidelberg, Springer-Verlag: 489-512.). Interestingly, G. bulloides did not show a trend (not plotted, I'm afraid, sorry)."

The Pliocene was chosen as the best existing analogue to modern ocean conditions as similarities in continent placement, ocean circulation and flora and fauna would remove some potential variables from our interpretations and offer a more straightforward comparison to modern oceans. We include a small figure panel with figure 1 illustrating the long term record of global temperature to highlight the cooling present during our study interval. By contrast, the PETM would present ecosystems and oceanography entirely different from modern, posing additional challenges to interpretation. Importantly, we would not have been able to use extant species and would not have been able to link changes in weight with carbonate chemistry. Moreover, no published CO₂ record exists for the PETM. In the revised manuscript, we emphasize the importance of the Pliocene as a period of study. We also include a comparison with modern culture experiments gauging weight change as a function of carbonate chemistry.

"Secondly, what was the rationale for choosing the core sites? Just the difference in temperature or were there additional reasons? The manuscript would benefit if the authors indicate how these two different sites are going to help them to disentangle the different factors that control the 'calcification parameters'"

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Our rationale for site choice was based on finding sites with very markedly different temperature and latitude, but also nutrient level, and hydrologic influences through the Pliocene. First and foremost, the sites are chosen under the assumption that, as in the modern ocean, $p\text{CO}_2$ in the surface ocean would be in equilibrium with atmospheric $p\text{CO}_2$ (at least at the temporal resolution available for these samples). Thus, changes in $p\text{CO}_2$ should occur simultaneously at both sites, while we would have no such expectation for temperature, productivity or other more localized variables. Secondly, we wanted to assess species with different biogeographic adaptation and went for a tropical and a temperate setting. Thirdly, we selected sites with different oceanographic settings: Site 607 is influenced by both the subtropical gyre and North Atlantic Current frontal system, resulting in large environmental changes while 999 sits in the more confined Caribbean basin with more stable temperatures. We have included all of the above points in our rationale for site choice in a revised manuscript.

“It is important to say that we do NOT know pH nor the saturation for the period of investigation, but that we infer lower pH based on modelled higher atmospheric $p\text{CO}_2$.”

We agree this is a crucial distinction and now make the use of this assumption explicit. However, our inferences about the carbon system are based on more than modelled atmospheric $p\text{CO}_2$ – for example, the boron isotope record of Site 999 does allow a reconstruction of pH at the site. That said, we are now more explicit throughout in reference to what we can reconstruct directly from proxy data versus what parameters can only be inferred.

“Furthermore it is important to note that the most important difference between all paleo OA scenarios and the situation today is that the rate of change today is unprecedented and that pH and the saturation state of the ocean with respect to calcium carbonate are declining in concert. pH and saturation state were probably decoupled during the Pliocene and the INHG when glacial-interglacial (G-IG) changes of ca. 40 ppm in atmospheric $p\text{CO}_2$ occurred, meaning that pH could be lower but that the saturation state remains constant.”

We have included a discussion of in the revised manuscript

on p 684 line 2-4 "Changes in the relative contribution of foraminifers to coccolithophores to sediment production were calculated by dividing foraminiferal MAR by carbonate MAR" But what about pelagic bivalves, ostracods, pteropods, CaCO₃ debris? Please comment if this can be neglected or not. In my view, this might lead to a bias especially at site 999 with 40 60 fragmentation. Hence, the foram contribution may be underestimated and this would not be the result of in vivo and surface water conditions, but controlled by bottom water saturation state post mortem

We have assessed the entire coarse fraction and there are no other organisms regularly present in the assemblages. While there will be juvenile foraminifers and debris in the fine fraction, we would have been able to see significant changes of these when analyzing our coccolithophore size records. We certainly agree that preservation/bottom water saturation state may be influential on our results and the coccolith-foraminiferal ratio is influenced by both flux and preservation, the latter reducing the foraminiferal fraction as liths are more resilient to dissolution. As we do not have a strong coupling of preservation and the foraminiferal/coccolithophore ratio, this factor is likely minor. We include this in the revised manuscript.

"With regard to the size normalized weight (SNW) proxy, the authors are referred to Bijma at al., 2002. Impact of the ocean carbonate chemistry on living foraminiferal shell weight: Comment on "Carbonate ion concentration in glacial-age deep waters of the Caribbean Sea" by W. S. Broecker and E. Clark. G3, v.3, nr. 11. Although, an important part is dependent on the in vivo carbonate chemistry, is a large share(the most?) controlled by bottom water saturation values (post mortum). This was, in fact, the reason why SNW was originally introduced by Pat Lohmann, to reconstruct bottom water [CO₃]-."

We undertook counts of fragment to whole foraminifers in an attempt to control for changes in bottom water saturation state. We do not discount that this likely has an

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effect on the measured shell weights but would argue that a) at Site 607, given that there is no indication of changes in the degree of dissolution through our study interval, changes in sample weight relative to other samples in this core are likely not impacted by differential dissolution and b) the preservation changes at Site 999 may have an effect on the relative record but if so it would be by making weights apparently lighter after 2.8 Ma, not heavier as would be expected if foraminiferal weight were following pCO₂. In the revised manuscript, we make this argument and a discussion of the potential influence of post depositional saturation state is now more explicit.

“on the bottom of p. 6844 line 27: “Unexpectedly, the deep dweller G. puncticulata shows significantly higher weight variability than the surface dweller”. Adress this observation which may have to do with structural test features. Some species may be more resistant to dissolution than others before they completely break down and disappear from the archive (but contribute to the CaCO₃ MAR). Pat Lohmann showed at a conference that on a depth transect at Sierra rise different species dissolve and disintegrate at different depths. Although, all are made up of calcium carbonate, G. ruber disappears first, G. sacculifer is still present in deeper cores and P. obliquiloculata is the last to disappear from the record. He argued that structural differences are responsible and that every species has its own “lysocline depth”. I’m not sure if it ever was published but this can be investigated.”

All the published work on foraminiferal dissolution indeed follows the sequences from G. ruber, via G. sacculifer to G. inflata and G. truncatulinoides. Therefore, dissolution should affect our surface dweller records much more than the G. puncticulata record (assuming that it behaves similar to the two latter species). This species is much heavier as it has a thicker wall than G. bulloides or G. ruber but it cannot explain the larger weight variability as this should be the other way around. Also as G. bulloides has species specific higher Mg values, and G. ruber ecologically determined values, these should react much more strongly to any dissolution than G. puncticulata

“p. 6845; line 19: I would spell INHG out and write Intensification of Northern Hemi-

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sphere Glaciation.”

Done.

“P. 6847; line 4-6.:”we would have expected smaller foraminiferal test weight changes in the Pliocene (half the magnitude observed here), if this was the main or even sole driver.”: Here you are assuming that SNW is only driven by surface water carbonate chemistry when the forams were alive. this is not the case. Address pore water saturation state as a driver of SNW.“

We now include a short discussion of alternative drivers of foraminiferal shell weight, including (as addressed above) bottom water saturation state as well as pore water saturation state. As we argue above, fragmentation counts at both sites cast doubt on a controlling influence of post mortem saturation state of relative weight changes.

“Line 7-9: “As for the carbonate MAR data, the contrasting records at these two sites suggest that regional drivers are responsible for the changes in test weight and not changes in pCO2.” Then the authors go into explaining the different temperature regimes: : ∴. Discuss pore water [CO32-] and saturation state”

We have amended this statement in the revised manuscript and include a discussion of possible pore water saturation state with that of bottom water saturation state.

“For the discussion on the coccolithophorids (later on in the msc) the authors use the argument of different genotypes to explain the observed size trends. Why not for the forams? There are many papers out, documenting that all planktonic foraminifera have multiple genotypes (with the notable exception of G. sacculifer, which seems to be genetically identical, no matter where in the world or when it is collected).”

We agree that this could be a possibility for foraminifera as well but chose to mention coccolithophorids and not foraminifera in this context as previous work suggests size trends in relation to changes in the carbon system in coccoliths may be vary based on genotype. To our knowledge, no one has yet examined the effects of genotypic

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variability on foraminiferal weight response to the carbon system, nor can we speak to that based on this study.

“On p. 6848; line 5-9, the authors say: “The different size trends seen at the two sites represent different proportions of species/genotypes/ecophenotypes.. their relationship to environmental change is uncertain.” Then the authors continue:” It is unlikely that these trends reñĀect dissolution because fragmentation is relatively stable across the record at Site 607 and improves at Site 999 while large liths disappear which are less prone to dissolution.” But maybe this disappearance of the larger liths is not related to bottom water saturation state but due to the ‘overall trend towards smaller liths through the Pliocene into the Quaternary’”

Thank you for calling attention to this confusing phrasing. We agree that this is the likeliest possibility and meant to suggest exactly that, not the opposite!

“In the last line of the conclusions, the authors say: “that rates of change rather than absolute pCO₂ values might drive a calcification response in these groups.” This is a very important conclusion but this was not mentioned or developed at all in the main text (discussion). This should be done.”

We now include this in our discussion in the revised manuscript along with a more fleshed out discussion of the carbon system.

We would like to thank Dr Bijma for his insightful comments. We have made the revisions outlined above in a revised manuscript as well as scale up figures 2 and 3 as suggested. In addition, we now include a more in depth consideration of the carbon system and post mortem saturation states in our discussion. Taking into account all of these comments will greatly improve our manuscript.

Interactive comment on Biogeosciences Discuss., 10, 6839, 2013.

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