

Interactive
Comment

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

Anonymous Referee #2

Received and published: 9 May 2013

Davis et al. aim to study the effect of high $p\text{CO}_2$ and decreasing $p\text{CO}_2$ on the calcification of planktic foraminifers and reticulofenestrid coccolithophores from 3.3–2.6 Ma, using two sediment cores from the central North Atlantic and the Caribbean. Assuming a simple prediction of reduced calcification under elevated $p\text{CO}_2$, one might expect thinner foraminifer shells and smaller coccoliths during periods of high $p\text{CO}_2$, followed by enhanced calcification after 2.7 Ma, when $p\text{CO}_2$ dropped to Pleistocene levels. The data collected in this study do not follow this simple prediction and the authors make a genuine effort to explain the uncertainties of their approach, including evolution/extinction, postdepositional dissolution at the seafloor and temperature changes in the surface ocean.

This study is valuable but it clearly demonstrates the difficulty of using CaCO_3 shell

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



weight evidence to constrain calcification responses to varying pCO₂, and I would recommend a major reorganization of this manuscript. As it stands, the initial goal of this study was to improve predictions for the effect of ocean acidification on marine calcification. Given the described observations, this hypothesis can neither be supported nor rejected, however, the wording used in this manuscript is much too vague about the outcome of the study and leaves ample room for misinterpretations. In the light of controversial societal and political views of this problem, I would recommend to revise this manuscript to reflect the difficulties of the selected approaches, and to provide guidelines for how the design of this kind of study could be improved to allow for conclusive results in future studies. It should also be discussed why Moy et al. (2009) and Barker and Elderfield (2002) observed a much clearer picture of foraminifer calcification in the more recent past. In the following, I will summarize specific problems of the current study design.

1. The authors aim to track the effect of pCO₂ on the original calcification, however, using seafloor sediments always adds the bias of post-depositional dissolution, which will modify the original calcification signal. The sediment cores selected for this study are located at ~3,400m and 2,800m water depth, which may introduce some significant dissolution bias in the deeper North Atlantic core. For instance, Barker et al. (2004) studied North Atlantic core BOFS 5K, which is located at 3,500 m water depth and glacial planktic foraminifer shells were found to be ~30% lighter compared to a nearby core site at only 1,100 m water depth. Furthermore, as described in this manuscript, Haug et al. (2001) observed significant dissolution changes at site 999 around 3 Ma, i.e. in the center of the interval studied here. Although these effects are discussed in this manuscript, selecting cores that are affected by dissolution are far from ideal for this kind of study. In addition, the proxies description has several shortcomings: 2. CaCO₃ mass accumulation rates are introduced as an indicator for surface water carbonate production. However, as dissolution removes CaCO₃ from the sediment, MARs are biased by this process. Furthermore, the manuscript claims that bulk MAR minus foraminiferal MAR equals coccolithophore MAR, however, in partially dissolved sedi-

ments the fraction $<63 \mu\text{m}$ also bears many foraminifer fragments, so the described approach is rather poorly constrained. It would have been better to separate coccolithophores in the $<20 \mu\text{m}$ fraction and call everything larger than that the foraminifer fraction. 3. Similar to MAR, foraminiferal test weight is also a function of calcification at the surface and dissolution at the seafloor. This is discussed somewhat later but the caveat should be introduced with the proxy. Bijma et al. (2002) would be the appropriate study to cite here. Also, it is not clear if “size normalized according to the protocol of Barker (2002)” includes removal of the clay fraction? This should be specified here, as clay removal is an important step towards reliable shell weight data. 4. The discussion discredits much of the coccolith size data because of extinction, migration and size shifts within the population. Similar to the species-specific analysis of foraminiferal shell weights, it certainly would have been better to study lith sizes of a single species. There is also no indication of the size fraction from which coccoliths were analyzed. Was this done on the entire $<63 \mu\text{m}$ fraction? Given that potential dissolution of coccoliths is also discussed, the authors should consider Chiu & Broecker (2008), who found that coccoliths are much more resistant to dissolution than planktic foraminifers. Furthermore, on page 6848, line 7 it is said that lith size shifts between genotypes/ecotypes are subtle. – Can this be supported by a citation or data evidence?

In general, it would be useful to calculate the change in carbonate saturation for the different sites and time intervals. Because pCO_2 and carbonate ion can decouple over long time scales (e.g., Hönisch et al. 2012) and because it is not clear for how long elevated pCO_2 persisted before 2.7 Ma, high pCO_2 alone does not necessarily suggest low carbonate saturation, as expected for the next century or so. In that regard study of the Pliocene is not a good analog for calcifier responses to high pCO_2 .

The study should also be more careful about the assumptions made for Pliocene carbonate chemistry. For instance, the Takahashi et al. (2002) study is outdated, Takahashi et al. (2009) show a much more detailed picture of surface ocean pCO_2 , with negative pCO_2 fluxes near site 607

and a small CO₂ source near site 999. The effect of this difference on surface ocean saturation can be taken from Takahashi's NSF report at http://www.ldeo.columbia.edu/res/pi/CO2/carbondioxide/text/OceanPH_Rpt_Assembled_010813.pdf

This report clearly shows that modern oversaturation near site 607 is greater than near site 999. Furthermore, the suggestion that the Pliocene glacial/interglacial pCO₂ amplitude was only ~40 ppm is difficult to verify, with Badger et al. 2013 being in press and not available online. It is also not clear what the significance of the alkenone temperatures in Figure 4 is for this manuscript. The sensitivity of the UK37 proxy diminishes at about 27°C, and the temperature amplitude recorded by alkenones is consequently much smaller compared to foraminiferal Mg/Ca at site 999 and the absolute values are significantly higher compared to Mg/Ca (Fig. 4). This difference is not discussed in the manuscript but judging from the absolute values, it seems likely that alkenones do not record mean annual conditions but are somewhat biased towards summer conditions. It should be discussed whether the pCO₂ amplitude is reliable under such conditions? Again, some quantification of carbonate saturation would allow a much more detailed prediction of calcification changes in foraminifers and coccoliths. Some surface ocean pH estimates from boron isotopes are available for such an approach (e.g., Seki et al. 2010, Bartoli et al., 2011), and reasonable assumptions could be made for DIC and alkalinity variations.

Some specific suggestions: Page 6841: Bijma et al. (2002) and Bijma et al. (1998) should be cited (lines 1, 20, 24) instead of Spero et al. (1997). The earlier study focuses on stable isotopes rather than calcification changes in response to carbonate ion concentrations.

Discussion, first paragraph: The “efficiency of the biological pump” is often misrepresented in the literature. It relates to the relative consumption of upwelled nutrients and subsequent removal of organic matter to the sediment, but not necessarily to absolutely high export production. For instance, the soft tissue pump is 100% effective where all upwelled nutrients are consumed but if the total nutrient supply is low (e.g.,

BGD

10, C1750–C1754, 2013

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



in the subtropical gyres), the absolute export of organic matter will be rather low. What the authors mean to say here is that CaCO_3 ballast increases export of organic matter, which is correct. However, they should also distinguish between foraminifers and coccolithophores, as foraminifers release their cytoplasm in the form of gametes near the sea surface and foraminifer shells are mostly empty as they sink to the seafloor. In that regard foraminifers contribute to the CaCO_3 pump but not to the biological pump.

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

BGD

10, C1750–C1754, 2013

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C1754

