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

**C. V. Davis et al.**

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We thank the anonymous reviewer for their many insightful comments. The comments primarily address the assumptions we made about the state of preservation and the Pliocene carbon system at the two sites. In line with the reviewer's suggestions we now also include a more robust discussion comparing our study with more recent foraminifer weight records in a revised manuscript. These are both important points that we seek to elaborate upon and clarify. Our responses to specific comments are as follows:

*“...using seafloor sediments at 3,400m and 2,800m water depth adds the bias of post-depositional dissolution, which will modify the original calcification signal.”*

We have addressed some of the comments in response to reviewer 1. We agree that

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the dissolution signal will always have some impact on shell weight in deeper cores such as the ones used in the analysis though we would like to emphasize that both cores are significantly above the current CCD. We attempted to constrain changes in preservation through a measure of foraminifera fragmentation, by this measure, we see no changes in preservation at Site 607, but do see a change in preservation at Site 999 around 2.8 Ma. We cannot argue that preservation has no effect on shell weight, but we argue that a) at Site 607, given there is no indication of changes in the degree of fragmentation, shell 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 the 2.8 Ma shift in preservation, not heavier as our hypothesis would have suggested. In a revised manuscript, we make all of these caveats more explicit.

*“...<63 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 m fraction and call everything larger than that the foraminifer fraction.”*

At the site the <63  $\mu$ m fraction was primarily made up of coccolithophores with very few foraminifera fragments or other debris observed. We would be happy to make explicit in our introduction to this approach that some foraminiferal fragments are inevitably included in the smaller size fraction (as they would be in a <20  $\mu$ m fraction). However given the nature of the sediment at this site, we maintain that 63  $\mu$ m stands as a reasonable size cut off by which to separate the primarily foraminiferal versus primarily coccolithophorid size fractions.

*“Discuss earlier the caveat that foraminiferal test weight is a function of calcification at the surface and dissolution at the seafloor... Bijma et al. (2002) would be the appropriate study to cite here.”*

Thank you. This is now included in the revised manuscript.

*“...size normalized according to the protocol of Barker (2002)”*

Our understanding of Barker and Elderfield's (2002) methodology is that the clay removal step took place after size-normalizing and crushing shells, and prior to Mg/Ca analysis. We report here only the results of weighing and size-normalization - not further crushing, cleaning or trace element analysis as reported in Barker Elderfield (2002).

*"what is the size fraction from which coccoliths were analyzed. Was this done on the entire <63  $\mu$  fraction?"*

Yes. We now state this in a revised manuscript.

*"consider Chiu Broecker (2008), who found that coccoliths are much more resistant to dissolution than planktic foraminifers."*

This is a good point and we now include this in our discussion of foraminiferal and coccolith composition.

*"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 modern species, differences between different morphotypes of *E. huxleyi* for example translate into small changes in size (Young, J. R. and P. Westbroek (1991). *Mar. Micropaleontol.* 18: 5). While within group variations has been documented for example for *Geophyrocapsa* (Bollmann, J. (1997). *Mar. Micropaleontol.* 29: 319) and *Calcidiscus* (Knappertsbusch, M., et al. (1997). *Mar. Micropaleontol.* 30: 293) these differences are between species and not phenotypes. Shifts in these genotypes as for example documented in sediment traps (Renaud, S., et al. (2002). *Mar. Micropaleontol.* 46(3-4): 363) for *Calcidiscus* do not lead to large changes in size within the population.

*"...calculate the change in carbonate saturation for the different sites and time intervals. Because  $p\text{CO}_2$  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  $p\text{CO}_2$*

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*persisted before 2.7 Ma, high pCO<sub>2</sub> 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<sub>2</sub>. 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<sub>2</sub>, with negative pCO<sub>2</sub> fluxes near site 607 and a small CO<sub>2</sub> 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/OceanPHRptAssembled010813.pdf>. This report clearly shows that modern oversaturation near site 607 is greater than near site 999."*

We acknowledge that this is a significant assumption, and although at present must remain so, we make an effort to support this assumption in a revised version by expanding our discussion of the Pliocene carbon system. We thank the reviewer for making us aware of Takahashi's 2013 NSF report and take into account potential differences in the rate of change in saturation state at each site in the added discussion text. However, we do not think that such issues represent a fundamental issue as a) at the temporal resolution of our samples, we can still assume equilibrium; and b) as we cannot directly compare weights between sites, but only weight trends and ranges from each site, some small difference in saturation state at the sites should not affect our results.

*"The suggestion that the Pliocene glacial/interglacial pCO<sub>2</sub> amplitude was only 40 ppm is difficult to verify with Badger et al. 2013 being in press and not available online."*

Badger et al., 2013 can be accessed online in pdf form at: [http://www.paleo.bris.ac.uk/~ggdjl/warm\\_climates.html](http://www.paleo.bris.ac.uk/~ggdjl/warm_climates.html)

*"...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 tem-*

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perature 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 pCO<sub>2</sub> amplitude is reliable under such conditions?”

The sensitivity of alkenones diminishing at higher temperatures has emerged only in sediment trap calibrations, while Badger et al., 2013 and previous core top based calibrations (e.g. Muller et al., 1998) found no such problems. They argue that discrepancies between Uk37 and Mg/Ca temperature reconstructions are more likely due to assumptions regarding Mg/Casw and that the absolute values obtained from Uk37 are actually more reliable. This is discussed in Badger et al., 2013 (link included above) and while certainly a matter of interest, not the focus of this paper.

*“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 calciññAcation changes in response to carbonate ion concentrations.”*

We thank the reviewer for noticing this. We will keep Spero et al., 1997 as an important example of early work on foraminiferal changes in response to the carbonate system but will also include Bijma et al., 2002 as another seminal study which is indeed more pertinent to our questions. The list here, however, if meant to me illustrative and not exhaustive.

*“Discussion, first paragraph: The “efficiency of the biological pump” is often misrepresented in the literature. In that regard foraminifers contribute to the CaCO<sub>3</sub> pump but not to the biological pump.”*

The reviewer raises a very good point about the nuances of discussing the biological vs CaCO<sub>3</sub> pump, and this too will be amended and clarified in a revised manuscript. We have made all of the changes outlined in the above comments in a revised manuscript

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These comments all greatly improve our final manuscript.

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

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