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Climate of the Past Discussions

Interactive comment on "Revisiting carbonate chemistry controls on planktic foraminifera Mg/Ca: implications for sea surface temperature and hydrology shifts over the Paleocene–Eocene Thermal Maximum and Eocene–Oligocene Transition" *by* D. Evans et al.

Anonymous Referee #2

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This paper assesses the impact of changing the seawater carbonate system (pH) on paleotemperatures estimated using Mg/Ca ratios in planktonic foraminifera. It reports new results of culture experiments with G. ruber, a species that is widely relied on for paleotemperatures reconstructions. This is an excellent paper that will undoubtedly improve the accuracy of Mg/Ca-based temperatures using planktonic species, as well as estimates of d18Ow and changes in the hydrologic cycle, especially for geologic periods such as the PETM which are characterized by low pH relative to modern. It

should be published with minor revisions.

Specific comments:

I think it would be very valuable to apply the Mg/Ca-pH corrections to Mg/Ca-T estimates in Holocene records from parts of the ocean with different mixed layer pH values – perhaps the eastern and western Pacific, using TR163-19 and ODP806b (data from Lea & Spero, 2000). There are fewer unknowns – the down-core data were generated on G. ruber, and we know a lot about the habitat depth, symbionts, etc of this species. It would be easier to assess how well the different forms of the calibration relationship actually work, or if indeed it makes a difference.

Since there is an offset between Mg/Ca ratios generated from laser vs solution methods in fossil samples, should the Mg/Ca-pH relationship for G. ruber (obtained via laser) be applied to fossil (or other) samples as is, or with a correction? Has there been a similar comparison between laser and solution results for cultured, sediment trap, or plankton tow samples? Since the cleaning methods are different (for fossil vs fresh forams) I doubt that the offset (10%) is the same.

Table 1: The pH values reported in Table 1 for G. bulloides and O. universa are the same as reported in the original Lea et al. 1999 and Russell et al. 2004 papers – so they are on the NBS scale, not the total scale as stated in the column header. These original values also appear to have been used in the figures, and I assume in the regresions. The corresponding total pH values would be $\sim$0.12-0.14 units lower. Although the regression slopes for these species won't be affected, the regressions for combined species may be. Also – the Kisakurek 2009 data for G. ruber should be included in the table – they're included in Figure 1 and I assume they're included in the regressions.

p. 3146, line 4: The Kisakurek and Russell Mg/Ca-pH relationships for planktonic forams show a dependence for pH lower than ambient (8.1), so I suggest replacing "significantly different" with "significantly lower"

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p. 3154, lines 9-10: What is the justification for excluding the data point at pH 7.6?

p. 3156, lines 1-6: According to De Nooijer, the volume of seawater contained in vacuoles is insufficient to deliver the Ca required for calcification so this would require internal Ca pools. These pools were not observed in experiments by Nehrke. The fact that the role of vacuoles in calcification is still under debate should be mentioned here.

p. 3156-3157: According to your model, I can see that pH is important – but [CO3] may still be important because of its effect on calcification rate. The relative rates of calcification versus Mg pumping (to remove excess Mg) could play an important role.

Figure 3: What do the error bars in part b represent, ie standard deviation or standard error (1 or 2σ); based on actual analyses or bootstrapped? (also - this caption is missing a noun and has a typo).

Interactive comment on Clim. Past Discuss., 11, 3143, 2015.

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