Biogeosciences Discuss., 9, C221–C224, 2012
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Biogeosciences
Discussions

# *Interactive comment on* "Relationships between bottom water carbonate saturation and element/Ca ratios in coretop samples of the benthic foraminifera *Oridorsalis umbonatus*" *by* C. F. Dawber and A. Tripati

**Anonymous Referee #1**

Received and published: 5 March 2012

This manuscript discusses new trace metal data from the benthic foraminifer O. umbatonus and discusses two possible biomineralization mechanisms (the surface entrapment model and the Rayleigh fractionation model) governing trace metal incorporation into foraminiferal calcite. The results suggest both mechanisms could be responsible for some, but not all of the trace metal variability. I found the discussions thorough and the figures interesting (though not that straight forward to interpret). However, the author's point out that neither mechanism could be responsible for all four metals investigated. This left me feeling unsatisfied with their findings. Rather than focus on the



two mechanisms for all four metals, I think it would have been better to discuss each of the metals separately – e.g. establish which biomineralization model can best explain the Sr data, which can best explain the Mg data. Then synthesize these findings.

The paper also illustrates a positive, but weak, relationship between the trace metals and bottom water carbonate ion concentration (ΔCO32-). The paper only correlates the trace metal variability to bottom water carbonate ion concentration. Temperature does not vary enough for the author's to assess any possible control temperature may have on the trace metal variability of this species. The author's do not attempt to correlate their data with any other indicators of preservation (i.e. percent-calcite, shell fragmentation). The author's are quick to point out that "it is clear that other parameters must also influence some/all of the X/Ca ratios, and that the sensitivity of individual X/Ca ratios to these additional parameters may be different." Clearly, the R2 values are not very high (Figure 1), thus while ΔCO32- likely plays a role in the trace metal incorporation, 'other parameters' controlling the trace metal variability in this species ought to be investigated. For example, even though the temperature range is small, plots detailing the temperature and trace metals could be included or the dataset expanded to include a wider range of temperatures. Correlating the relationships between the trace metals and other indicators of preservation (fragmentation, shell weights, etc.) would strengthen their findings.

Major concern with this study is the use of the calibration equations established here in the other study the author's have in review (Climate of the Past). The author's state in their paper that another strategy to examine the cause of the X/Ca variability would be to compare the X/Ca data down core and compare those results with other proxy data, which they handle in a separate manuscript (in review in Climate of the Past). I disagree that this is an appropriate method for testing the validity of the X/Ca to ΔCO32- relationship. The X/Ca data from the core tops should be correlated with other information gleaned from the core top samples and other hydrographic data (e.g. temperature) in order to establish which of these elements/Ca ratios could possibly be used as proxies for carbonate ion concentration. The calibration equations established in this study should not be applied down core until the comparisons between the trace metals and other environmental parameters or indicators of preservation (in addition to $\Delta$CO32-) are established. The Author's did not convince me that $\Delta$CO32- should be used as a proxy down core yet.

In addition, the other paper the Author's have in review in Climate of the Past shows that the metals are not, in any way, correlated in deep time. It was interesting to see the metal/Ca ratios detailed in a downcore record (in the other paper in Climate of the Past), however, the results of the Climate of the Past paper illustrate that despite the efforts in establishing a mechanistic cause for the controls on the metal/Ca ratios of this species, there is much to be learned. As stated above a larger core-top calibration dataset that correlates the X/Ca ratios to other MODERN hydrographic data and other preservation proxies (fragmentation, percent calcite, etc.) would better establish which X/Ca ratios are truly correlated to carbonate ion concentration. In summary, though this paper attempts to place the focus solely on a mechanistic control on metal incorporation in O. umbatonus, too much effort is spent discussing the positive correlation between the all of the X/Ca data and $\Delta$CO32-. This positive relationship is not corroborated with their down core data presented in their CPD paper and this is a major concern that should be properly dealt with prior to publication of this (or both) manuscripts. This is the reason for the Fair rating in the scientific quality section.

Minor concerns: Figure 1: The Pacific data appears to have a different slope in figures A, B, and C. In Figure A (Li/Ca vs. $\Delta$CO32-) the slope may be steeper than the Atlantic and Indian Ocean. In Figure C, there is NO correlation between B/Ca and $\Delta$CO32- in the Pacific. Perhaps the B/Ca – $\Delta$CO32- relationship asymptotes at low carbonate ion concentrations. These relationships could be better established with a larger dataset.

Figure 2 isn't really discussed at all. Again, these cross plots illustrate the X/Ca data in the Pacific samples may have a different slope in comparison to the Atlantic and Indian Ocean samples.



The author's should include a map of the core locations.

Dawber and Tripati, 2011 is cited in the first paragraph of the introduction, but is not in the reference list. Please check this and other references for any other omissions.

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Interactive comment on Biogeosciences Discuss., 9, 1483, 2012.