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Biogeosciences Discussions

Interactive comment on "Copper incorporation in foraminiferal calcite: results from culturing experiments" *by* L. J. de Nooijer et al.

L. J. de Nooijer et al.

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We are pleased to receive the detailed comments provided by the three reviewers and would like to reply to their suggestions. Most of their comments were used to improve the text; detailed replies are listed below.

Hathorne

The reviewer's main comment concerns the incorporation of calcein and its effect on the measured TE/Ca ratios. We cannot fully eliminate the possibility that the presence of calcein influenced Cu uptake. We have however, analyzed Mg/Ca ratios in both chambers that had incorporated calcein and those that did not (i.e. older chambers of the same specimens). We did not report on those measurements, but the Mg/Ca ratio's in the 'calcein-chambers' did not deviate from the 'calcein-free chambers' (within nat-

urally occurring variance). Such measurements indicate that the presence of calcein does not significantly alter the uptake of TE's within the resolution of the experiment (as suggested by Hintz et al., 2004). However, in the revised manuscript we have stressed our (and Hathorne's) concern to the use of calcein in the way we did. Levels of incorporated calcein were so low that they did not affect measured Ca-concentrations. It is correct that the accuracy of Cu data was not tested in the Mason & Kraan (2002) reference. However, the data in this paper show that the internal standard correction, a major potential source of error that is applied uniformally to all elements including Cu, is robust with this experimental setup when using NIST glass to calibrate a calcite matrix. Furthermore spectral interferences on Cu are unlikely since both 63Cu and 65Cu give identical results. This lends support to the accuracy of our results.

Reviewer #2

We did not explicitly monitor dissolved oxygen content, although we estimated that oxygen consumption must have been limited and oxygen could dissolve from the air into the seawater vessels. It is true that A. tepida can survive for long periods in oxygen-depleted environments, although in the tidal flat we sampled, specimens are usually found on top of the sediment, associated with dense patches of diatoms. In such environments, it may well be that calcite is formed largely in well-oxygenated seawater. Even when calcification takes place in deeper, dysoxic layers, the uptake of seawater (and thus heavy metals) may likely take place in the well-oxygenated zone. This point has been combined with the reviewer's first comment and added to the discussion. The reviewer's comments on the microenvironments near the site of calcification and the kinetic effects on heavy metal partitioning are now mentioned in the discussion ('Biological control on DCu') in the sense that the pH may be very different in the cultured species. Other factors may also be responsible for the variation in the estimated partition coefficient. We have widened the statements in this paragraph to include these other factors. In the discussion ('Experimental uncertainties') we listed all other factors that may have contributed to the observed variance in DCu.

BGD
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EGU

Reviewer #3

The reviewer's comments on hydrothermal vents are covered by Hathorne's comment on this subject and so is the reviewer's concern about calcein. The first manuscript contained information about added artificial sediment (methods, first part, first and last paragraph) as well as the possible effect of the different laser strengths (discussion, first paragraph). We realize that more culturing experiments would have resulted in more data and would have increased the precision on foraminiferal DCu's, but for many, practical reasons we have not yet been able to achieve this. We did not calculate exact partition coefficients, but have indicated the range in which they lie and our mean value is likely to be close to the true value. In some cases (introduction, conclusions), we have adjusted the text to avoid the suggestion that we calculated the partition coefficient (which indeed is not possible with our dataset). In the discussion ('Experimental uncertainties') we listed the possible reasons for the variability in the data and have included the reviewer's concern on the possible limitations of our culturing set-up. Technically: only the temperatures are repeated in section 3 to list all relevant conditions to calculate Omega. In Table 1, only and always three significant numbers are given, but we chose not to use the '10-x' notation to make comparison between numbers more easy. The other technical comments are used to improve the text.

Interactive comment on Biogeosciences Discuss., 4, 961, 2007.