

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

Anonymous Referee #2

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General Comments

Culturing of foraminifera is becoming a popular approach to in some earth and biological science disciplines. This is one of the first culture studies to directly address heavy metal pollution on benthic foraminiferal shell chemistry. The authors should be applauded that they seek another avenue besides foraminiferal shell deformation as a proxy of pollution. Culturing may indeed be, as the authors state, the best means to unravel the contribution of separate variables affecting biogeochemical proxies, but such definitive statements are best avoided in the scientific literature. Perhaps it is better to consider culturing as “one of the best” means to tease apart variables affecting proxy records. It is also commendable that the authors considered bioavailability of copper in their interpretations.

Specific Comments

There are questions regarding the particular culture system design and the approaches used in this particular culture study. For example, how might the chemistry of a small recirculating reservoir be affected over a two month experimental duration? Compare, for example, the 2-L volume used here to the large reservoir of >1000 L used by Hintz et al. (2004 *Limnology and Oceanography Methods*, 2006 a, b *Geochimica et Cosmochimica Acta*). Were the authors confident that oxygen remained sufficient, that waste products remained low, and that no other confounding chemical changes occurred during their two-month-long experiments? Is a flow rate of 9 ml/hour sufficient to maintain aerated conditions? Was any substrate (“sediment”) provided during the experiment? How might this lack of material affect benthic foraminiferal behavior, and thus, potentially, calcification? Why were experiments run during calcein incubation (vs labelling the foraminiferal calcite prior to culturing)? Was it not possible to run replicate experiments for either species? It would have been valuable to show that the same distribution coefficient was obtained during replicated experimental runs. Can differences in laser strength (between the two species) impart any artifacts to the ICP-MS results?

How can the data (shown in Figures 4 and 5) give a partition coefficient of 0.25 +/- 0.15 when there are such large error bars on the few data points shown in the figures? How was this value determined? Perhaps some elaboration for the causes of the data variability would also help the reader.

It seems odd that there is some discussion on hydrothermal vents, especially given that the two species analyzed do not occur near vents and there is no further mention of vents except in the Introduction.

Technical Corrections

As is often the case these days, there seems to be a woeful lack of literature citations for many statements (eg, line 6, page 973; ecotoxicology citations in section 10.5). It is

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also strange that the Hintz et al. culturing papers are not included in the discussion of distribution coefficients determined for foraminifera (page 972, lines 13-27).

Be consistent (sea water vs seawater, MiliQ vs MilliQ). Parts of section 3 repeat Section 2.1. How can there be a section 2.1 when there is no section 2.2?

Is it appropriate to include so many significant digits in Table 1? Is it necessary to include a photograph of a tissue culture plate (Figure 1, A, B)?

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