Biogeosciences Discuss., 7, C732–C735, 2010
www.biogeosciences-discuss.net/7/C732/2010/

Biogeosciences
Discussions

Interactive comment on "Heavy metal uptake in foraminiferal calcite: results of multi-element culture experiments" *by* D. Munsel et al.

Anonymous Referee #2

Received and published: 26 April 2010

Review of manuscript by Munsel and co-authors: Âń Heavy metal uptake in foraminiferal calcite: results of multi-element culture experiments Âż

This manuscript presents data on heavy metal uptake in foraminiferal calcite in culture experiments using multi-metal cocktails with various concentrations of Ni, Cu and Mn. The study is interesting because 1) it presents interesting new data for the uptake of Ni and Cu, and especially, 2) because it compares trace metal measurements by LA-ICPMS and μ-synchrotron XRF, with surprisingly similar results, which is very reassuring, and suggests that results of both methods are fully comparable.

Unfortunately, the manuscript suffers from weak experimental design, and the very limited amount of (partially contradictory) data. The authors defend the choice of multi-metal cocktails by indicating that these are closer to the natural situation. However,

such a strategy seems premature in a situation where no coefficients are known for Ni and Mn from mono-metal experiments. Due to the strongly deviating values for all metals at the highest metal concentrations, the authors decide not to use these values for the calculation of their partition coefficients of Ni, but they do include them for Cu, although the trends are very similar. This discutable choice has a heavy impact on the calculated PC's, and is neither explained nor defended. In order to explain to surprisingly low foraminiferal metal concentrations at higher concentrations, the authors evoke toxic effects and/or chemical competition/exclusion effects. Unfortunately, the arguments raised on favour of these mechanisms are not very convincing. Of course, in the case of mono-metal experiments, the second possibility could have been excluded. It appears that the experimental approach has been too ambitious in view of the present rather limited state of knowledge.

Further comments and questions:

The paragraph on the use of metal contents in foram tests to better constrain environmental pollution (p. 956, l. 5-13) is an interesting side-line, somewhat marginal in this paper.

P. 960, l. 8. How did the authors determine that all foraminifera were alive at the end of the experiments ?

P. 960, l. 15-16. The authors say they will not consider Mn concentrations later in the paper. This is not true, since they discuss them extensively later on.

The description (paragraph 3.4) and discussion (paragraph 4.1) of the partition coefficients are extremely fast, and the finally proposed values have been chosen rather arbitrarily. I have several minor comments and questions here: - l. 7: calculated PC's for Cu range between 0.08 (not 0.1) and 0.25 - l. 8: how was the "very uncertain estimate" of at least 2.4 for Mn determined ? - l. 12-15: a systematical decline of Ni and Cu at the highest concentrations: rather surprisingly, the results of the highest Cu concentrations are no longer considered (but this is never said), whereas the highest

Ni concentrations are still used to define the PC's - l. 20: 0,1 should be 0,08 - l. 21: "DCu was found to be 0.18". How was this calculated ? Apparently, it is the average of 0.12 and 0.25, being the PC's of the medians of the 1) 5x and 20x and 2) of the 10x concentration !? How was the error envelope calculated ? - l. 22: the average value for LA-ICPMS (0.12) is obtained in exactly the same way. - l. 22: the final average DCu of 0.17 is based on a simple average of the LA-ICPMS and XRF. Are the authors sure it is a good idea to calculate the average value of two different measuring methods, and present it (in the abstract) as THE partition coefficient for Cu. This is all not very convincing for me. - L. 24: the DCu of 0.17 is lower than the value found by De Nooijer et al. However, the lower value is mainly obtained by the fact that they take into consideration the values obtained for the highest metal concentrations, which they do not do for Ni! Another question: what range of experimental metal concentrations was used by De Nooijer et al.? - P. 962, l. 3-7: Here, the values obtained at 20x are not considered. Again, the choice of the finally proposed value looks very arbitrarily.

Chapter 4.2.

p. 962, l. 22-27: because all forams survived (how did the authors test this?), under all conditions new chambers were formed, and no malformed chambers were observed, the authors conclude that culture conditions were "reasonably good". This statement contradicts the strong suggestion of environmental stress at higher metal concentrations in chapter 4.3.

Chapter 4.3.

The case made for toxic levels of metal concentrations in the 20x experiment is not very convincing, but it is hard to see how the authors could have done better. They should indicate (p; 963, line 27) above what exact concentration Ni inhibits calcite precipitation in the study of Bachmeier et al.

Along the same lines, it would be useful to know what maximum Cu concentrations were used by De Nooijer et al. and Le Cadre and Debenay (p. 964, l.4).

The antagonism between Mn and Cu (p. 964, line 21 and further) is not very convincing either. The authors suggest that this antagonism is partly reponsible for Cu and Mn concentrations, "at least in the 5- and 10-fold" concentrations. I fail to see why. In the 10x concentration both Mn and the other two metals increase. Conversely, the contrasting results for the 20x (increase in Mn, decrease in the other two) could indeed be indicative of such an antagonism. The whole paragraph is somewhat confusing.

The last paragraph of section 4.3. is very speculative.

Interactive comment on Biogeosciences Discuss., 7, 953, 2010.