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Biogeosciences Discussions

Interactive comment on "Mn seasonal upwellings recorded in Lake Tanganyika mussels" *by* D. Langlet et al.

S. Severmann (Referee)

silke.severmann@ucr.edu

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General Impression:

This article tests the idea that the shells of bivalves represent archives of past elemental variations in the aquatic environment that the organism grew in. In this particular example the authors have investigated the incorporation of Mn into the aragonitic shells of the bivalve Ploidon spekii in relation to the seasonal upwelling and associated increased Mn concentrations in Lake Tanganyika. The data, which includes results from a two-year monitoring program of particulate and dissolved Mn concentrations in the lake and high-resolution elemental profiling of several bivalve shells, are quite valuable, and I believe that the results from this study will enhance our understanding of the use of bivalve shells as environmental monitors. This is an interesting and innovative

study that is certainly suitable for publication in BGS. I have a number of comments and suggestions that I would ask the authors to consider before final publication of this manuscript. I noticed that many of my comments reiterate the comments already made by D.Dettman in his review.

Specific Comments:

All the samples collected and analyzed here are from the southern shores of the lake, which is most strongly affected by upwelling. The shells show Mn peaks that are appear to be consisted with increased Mn concentrations due to seasonal upwelling. However, I am missing a more thorough discussion of alternative explanations, such as vital effects. For example, how much of the variability in the shell's Mn concentration (Fig. 5), especially during the non-upwelling season, may be due to annual growth cycles of the organism? Do some of the peaks disappear if Mn concentrations are normalized to Ca, and would this improve the agreement between laser-ICP data and microdrill data? Ideally samples from the upwelling site should be compared to a background site that is not affected by upwelling and/or does not show seasonal variations in Mn concentrations.

In section 2.1 the authors note a length-increase in shell V10 of 2.9mm/8 month This appears to be inconsistent with figure 5b, which - based on the scale bar - implies a growth of ˜15mm over the 8-month period. Also, the 1mm scale bar and X-axis-length are the same for figures 5b, c and d, yet apparently the growth rates for these shells varied significantly (section 2.1). In order to put the relative sizes of the shell's Mn peaks into context it would be helpful to have more detailed information about the growth rates of the shells over the time of the experiment for each of the organism. The shells all show a similar pattern, but if they grew at different rates, wouldn't this affect the relative size of the Mn peaks in the shells? Ultimately it would be very useful if the size of the peaks in the shells could be more directly relate to the aqueous Mn concentration.

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I am not sure why the authors choose not to show the dissolved Mn concentrations for the two-year survey at the pelagic site. I think it is important to show those data to support their arguments. For example, the authors say that peaks in shell Mn coincide with increased particulate and dissolved Mn in the surface water. Further, they argue that the uptake of Mn by the shell occurs primarily through ingestion of particulate Mn and not through incorporation of dissolved Mn. Without the dissolved Mn data these claims are impossible to verify by the reader.

The authors base this assertion that Mn is incorporated primarily through ingestion of particulate Mn on results from a staining experiments, where bivalves failed to stain despite 12 hour expose to high levels of dissolved Mn. However, details of this experiment are not presented in this or any of the cited articles. Did the authors try to stain the organism using particulate Mn? Is there any alternative explanation why the staining experiment may have failed? Why would staining with dissolved Mn not work in the particular species, although it has previously been successfully demonstrated for other species of freshwater and marine bivalves?

In figure 3 and the first paragraph of the results section the authors present dissolved and particulate Mn data from an offshore station, but these data are not discussed any further in the text. Also, these two data sets are not strictly comparable: the dissolved data are from the upwelling season, whereas the particulate data are from the rainy season. I don't think much is gained from this figure unless it can be discussed in the context of seasonal variability of Mn concentrations due to upwelling. Instead ň- and as already mentioned aboveň - I would find it much more valuable if the authors would show dissolved concentrations for their pelagic station, where it can be compared directly with the particulate Mn concentrations.

I have a few questions regarding Figure 4: In the last sentence of the figure caption the authors say that upwelling and "a marked surface cooling take place during the dry season I July-November (2002) and July-October (2003)". As far as I can read from Figure 4c the cooling takes place from May-September in 2002 and from April-

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September in 2003. The most pronounced surface cooling in May 2002 and July 2003 do not align with lows in the bottom water temperature (Fig. 4b) or peaks in the particulate Mn load (Fig. 4a) - in fact, the maxima in particulate load seem to occur mostly after the upwelling event when comparing Figures 4a and 4c. Can you explain any of these discrepancies?

In the final paragraph of section 4 you speculate that - based on the bivalves Mn record - there is no record of an upwelling event during 1996. Are there any other hydrographic data that may confirm this muted upwelling during 1996? Also, it is not clear to me how years were assigned to the individual peaks. How confident are you about your assignment and what is it based on other than visually lining up the peaks? Is there an independent method to identify the annual growth bands? This goes back to my earlier comment that I would like to see better constraints on the annual growth rates. I guess this is where oxygen isotopes would come in very useful to identify variations in water temperature, as mentioned by the authors in the conclusions.

Minor comments and technical corrections:

Page 1454 line 6 and page 1455 line 10: "... were put in perspective against..." - use "...were compared to..."

Page 1454 line 25: Not sure what you mean by "pre-instrumental data".

Page 1455 line 9: replace "leaving" with "living"

Page 2455 line 25: "...combined to..." change to "...combined with..."

Throughout section 2.2 you use a number of different units for dissolved or particulare Mn concentrations (mol/L, g/L, ppm) - I suggest you stick to one unit throughout.

Page 1458 line 23: replace "superposed" with "superimposed"

Page 1461 line 17: replace "exposition" with "exposure"

Page 1461 last paragraph: Insert a reference to Figure 6 in this paragraph.

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Some of the figures are very small and hard to read, especially Figures 4 and 5.

And finally, I am a big fan of data tables, and especially in an age of electronic publishing I see no reason why not to publish the full data set for other researchers to use.

Interactive comment on Biogeosciences Discuss., 3, 1453, 2006.