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Biogeosciences
Discussions
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Interactive comment on "Fluctuations of sulfate, S-bearing amino acids and magnesium in a giant clam shell" *by* T. Yoshimura et al.

T. Yoshimura et al.

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May 25th, 2014

Dear Editor and Referees,

We enclosed a revised version of our manuscript. Please see the responses below. We express our deep appreciation to editor and referees of your time and energy to the task of improving our manuscript.

Sincerely yours,

Toshihiro Yoshimura

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P. 1614, L. 25: This sentence should probably have a reference. ⇒ We added some papers as suggested; Corrège, T.: Sea surface temperature and salinity reconstruction from coral geochemical tracers, Palaeogeogr. Palaeoclimatol. Palaeoecol., 232, 408–428, 2006; Henderson, G. M.: New oceanic proxies for paleoclimate. Earth Planet. Sci. Lett., 203, 1–13, 2002.

P. 1615, L. 14: Why is insolation singled out here? Is there preexisting evidence that insolation per se is relevant, or are the authors essentially implicating seasonality? If the latter is the case, I suggest revising this sentence. ⇒ We changed the sentence. Please see the MS.

P. 1616, L. 25: A locality map would be a valuable addition to the manuscript. ⇒ Locality map was added as Figure 1a.

P. 1617, L. 2-3: This statement should be referenced. ⇒ We added a reference; Pätzold, J., Heinrichs, J. P., Wolschendorf, K., and Wefer, G.: Correlation of stable oxygen isotope temperature record with light attenuation profiles in reef-dwelling Tridacna shells. Coral Reefs, 10, 65-69, 1991.

P. 1617, P. 5: The authors should provide more detail regarding polishing? ⇒ The cutting plane was polished using #1200, #2000 and #6000 3M Imperial polishing paper. The polished slab was cleaned ultrasonically and dried at room temperature. We added these information in Material and Methods.

P. 1619, L. 13-14: These references should be mentioned earlier (see comment above). ⇒ We added the references in Introduction.

P. 1619, L. 26-27: While I understand what the authors are inferring about growth rates,

this information is not explicitly showed in Figure 4. Perhaps this can be developed more completely. ⇒ We changed Figure 4.

P. 1620, L. 1-12: This is somewhat speculative. Can additional support (experimental or literature) be provided that would bolster this hypothesis? ⇒ A time-series measurement of calcifying fluid chemistry is the best to support the results. But, to the authors' knowledge, such data are limited. To determine the CO32− concentration of calcifying fluid, the state of aqueous carbonate system can be constrained by parameters, pH, total alkalinity and dissolved inorganic carbon. The pH proxies of extrapallial fluid such as boron isotope ratio (Heinemann et al. 2012) probably give further information to justify the ontogenetic change of calcifying fluid chemistry. The δ11B of Mytilus edulis shows no trend with seawater pH but appears to represent pH of extrapallial fluid; Heinemann, A., Fietzke, J., Melzner, F., Böhm, F., Thomsen, J., Garbe Schönberg, D., & Eisenhauer, A. (2012). Conditions of Mytilus edulis extracellular body fluids and shell composition in a pH treatment experiment: Acid base status, trace elements and δ11B. Geochemistry, Geophysics, Geosystems, 13(1). doi:10.1029/2011GC003790.

Because the data presented here are from a single specimen, it is difficult to evaluate the reproducibility of the patterns presented here (i.e., ontogenetic changes in elemental concentrations). ⇒ The synchrotron measurements provide some interesting features of element distributions and chemical speciation in bivalve shell carbonate, but unfortunately machine time of synchrotron faculty is very limited. We are going on performing measurements. We added related sentence that individual variability of shell growth partly exert systematic control on sulfate concentration in H. Hippopus.

Original comments

M. Cusack (Referee #1)

This manuscript deals with sulfate, S-amino acids and Mg in the giant clam, H. hippopus. These chemical species are considered in terms of environmental conditions and, importantly in the context of ontogeny. The strength of the manuscript is the fact

that it deals with these important chemical species in meticulous detail in the environmental and onogenetic context, providing novel insight for both. The fact that the Mg concentration correlates with sulfur amino acids, rather than sulfate, seems to contrast with some calcite biominerals where there is a correlation between Mg and sulfate and the Mg is a true lattice component. This is likely to all relate to the crystallography and the relative ease with which Mg can substitute for Ca in the calcite lattice with both Mg and sulfate being incorporated into calcite more readily at high precipitation rates. It is interesting to note the annual fluctuations in sulfate concentration and the fact that their amplitude increases with age. This contrasts with the concept of sulfate incorporation increasing with precipitation rate. The authors present interesting discussion on this phenomenon by way of the chemistry of calcifying fluids indicating that this may be a useful means of considering the influence of environmental change on calcifying fluid chemistry. The authors provide fascinating data, giving the reader a lot to think about along with an ontogenetic and environmental framework in which to consider these data.

Anonymous Referee #2

General Comments This study examines S and Mg element profiles from the shells of the giant clam Hippopus hippopus, which were collected in Ishigaki-Jima, Japan. The main goal of the study was to document ontogenetic trends, with the ultimate goal of understanding elemental partitioning during biomineralization. The authors document elemental concentrations in both organic and inorganic phases from a single specimen. Several specific hypotheses regarding the mechanisms and controls on S and Mg variations are proposed. These ideas will likely serve to motivate more detailed future experiments/monitoring studies. Because the data presented here are from a single specimen, it is difficult to evaluate the reproducibility of the patterns presented here (i.e., ontogenetic changes in elemental concentrations).

Specific comments P. 1614, L. 25: This sentence should probably have a reference. P. 1615, L. 14: Why is insolation singled out here? Is there preexisting evidence that

insolation per se is relevant, or are the authors essentially implicating seasonality? If the latter is the case, I suggest revising this sentence. P. 1616, L. 25: A locality map would be a valuable addition to the manuscript. P. 1617, L. 2-3: This statement should be referenced. P. 1617, P. 5: The authors should provide more detail regarding polishing? P. 1619, L. 13-14: These references should be mentioned earlier (see comment above). P. 1619, L. 26-27: While I understand what the authors are inferring about growth rates, this information is not explicitly showed in Figure 4. Perhaps this can be developed more completely. P. 1620, L. 1-12: This is somewhat speculative. Can additional support (experimental or literature) be provided that would bolster this hypothesis?

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C1858/2014/bgd-11-C1858-2014-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 11, 1613, 2014.

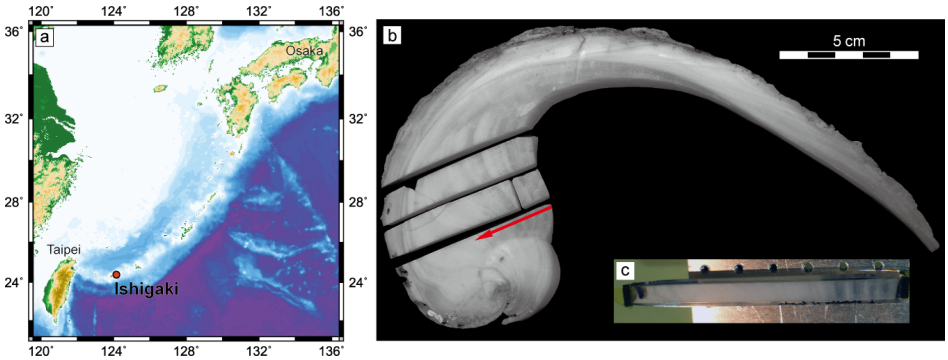

Fig. 1.

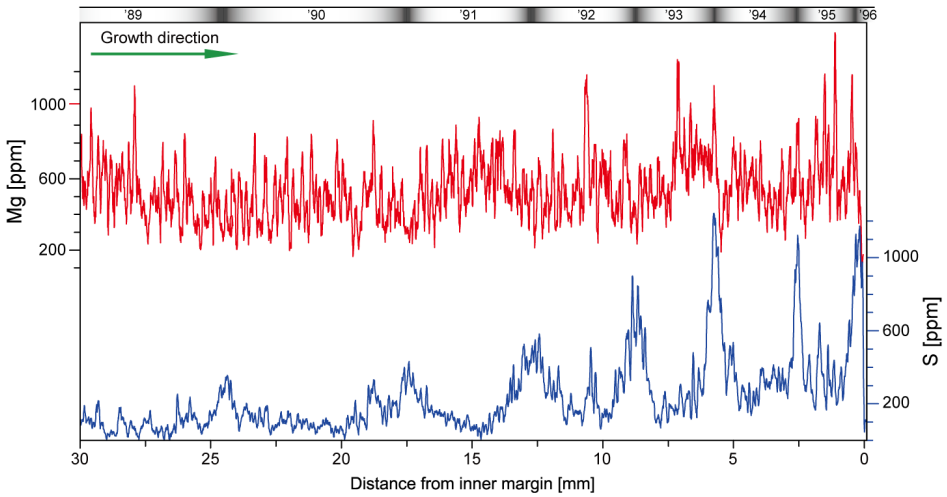
'89
'90
'91
'92
'93
'94
'95
'96
Growth direction
1000
600
200
Mg [ppm]
1000
600
200
S [ppm]
30
25
20
15
10
5
0
Distance from inner margin [mm]

Fig. 2.