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Interactive comment on "Impact of salinity on element incorporation in two benthic foraminiferal species with contrasting Magnesium contents" *by* Esmee Geerken et al.

Anonymous Referee #2

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General comments

A paper by Geerken et al. reported variabilities in incorporation ratios of Na and other elements to Ca under different salinity levels, observed in various scales of geochemical anlalyses (intra-chamber, inter-chamber, inter-specimen, and inter-species levels). Overall, this paper is well-written, well-considered, and well-structured. The results would be useful for paleo-salinity reconstruction. However, I suggest that the authors consider the following criticisms and comments to improve the final version of this paper.

The main criticism of this paper is a high inter-specimen variability of incorporation

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ratios despite using asexually reproduced juveniles incubated in the same petri dish. The authors explained this was caused by differences in the efficiency of calcification processes between specimens (L424-425). If so, what caused the efficiency of calcification process of the specimens? Physiological conditions of each specimen (nutrition, health, and food availability) and the microenvironment in culturing dishes may be clues to answer this question. These have not been measured in this study, but are partly reflected in growth rates of cultured specimens. I suggest the authors compare a relationship between TE/Ca and growth rates for each specimen, not comparing by their average values. This has been briefly described in L 243-248, but data and figures are not shown.

Another criticism is a covariation between salinity and DIC. In this paper, these two parameters are positively correlated. The authors explained that this phenomenon is similar to the natural environment (L311-312), but this does not always occur in the natural environments (e.g. groundwater-seepage area). The authors could have manipulated the carbonate chemistry of culturing media to keep them constant and to make salinity the only variable. I suggest that the authors discuss the limitation of application for this proxy calibration to field specimens where salinity and DIC have not covaried/negatively covaried.

The Discussion of Sections 4.2 and 4.3 should be combined and conclusions in this paper should be based on combined results of various analytical levels of all data sets including EPMA results. For example, Mg and Na are richly concentrated in organic layers based on EPMA results (Section 4.3). I wonder if this finding might affect your conclusions described in former sections.

Specific comments

L32-33: unclear what the authors mean to say.

L35-39: need references

Section 2.1: Descriptions of two species are mixed up and make me confused. Better to separate if culturing conditions are different. For example, did two-third of all incubated specimens reproduced for both species? Were Ammonia specimens incubated at 25C in spite of their original cold habitat from the Wadden Sea? In addition, culturing protocol should be described after explaining culture media preparation (Section 2.2.1).

L95-98: As far as my understanding, you changed culture media every week, then you put algal food and kept the culture media for one week. In that case, did the water quality (salinity and nutrients) change during a week? How did you seal the petri dishes to avoid the salinity changes?

L99-100: Does organic material removal affect the element ratio because Mg and Na are richly incorporated in POS?

L109: Describe the definition of salinity and how to measure it.

Table 1: Do these values indicate the mean values? Better to show the variations prior to and after changing culture media.

L127: Is a laser spot diameter small enough to measure the single chamber for small Ammonia specimens?

Table 2: Explain Ac and Pr, and add the error. List the result of MACS-3 as well. I do not understand why accuracy is over 100%.

L147-150: List them in Table or Appendix.

L243-248: Data and results are not presented.

L246: need references

L276: How did you identify POS from the SEM/EPMA maps?

L325-328: Do you have any ideas how inorganic carbon chemistry affect growth rates

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and shell weights? I think in future you have to more carefully check the physiology of cultured foraminifers to understand the proxy calibration, as described in the L364.

L329-334: I think this paragraph is meaningless, better to delete.

L357-358: I think the latter sentence does not support the former sentence.

Conclusions: This is just the summary of results. What did you think about these results?

Technical corrections

L56 (Allen et al., 2016): not italicized

L117: delete : or (

L120: space between with and 0.14

L224: Fig. 3 should be Fig. 2 in the order of appearance.

Figure3, L612: Results of A. lessonii should be placed on the left side for consistence throughout the paper.

L613: Delete “for” after “regression”

L234: average between inter-specimens?

L235: unclear after =

L243 and 247: replace al to all?

L273: Fig. 6 must be Fig. 4.

L274: Appendix D must be Appendix C.

Table 3: What are A.l. and A.t.?

Fig. 4: should be subdivided by a, b, c, d,. . .

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L625: transact > transect, No explanation for POS

L627: POS should be indicated in the EPMA maps.

L284-286: better to write "(for Ammonia tepida, Wit et al., 2013; for cultured Globigerinoides ruber, Allen et al., 2016; for field-collected G. ruber and G. sacculifer, Mezger et al., 2016)".

L288-289: better to write "... reported previously for the planktonic G. ruber and G. sacculifer (e.g. Mezger et al., 2016; Allen et al., 2016)."

L290: not targeted > non-target

L329: Unclear expression: The absence of an (strong) impact of ...

L329: Fig. 1 is related to this sentence?

L337: comma between 14% and 19%

L347: RSD needs explanation

L375: The difference in (an absence of) a correlation between elements between the two species > The difference or an absence of a correlation among elements between the two species?

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