Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-481-RC2, 2018

Biogeosciences Discussions
Open Access
EGU

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 #3

Received and published: 28 February 2018

The study by Geerken et al. reports the influence of salinity on Na/Ca, Mg/Ca and Sr/Ca of the benthic foraminiferal species A. lessonii and A. tepida, with a focus on Na/Ca as a possible salinity proxy. Element ratios have been determined by laser ablation ICP-MS and foraminiferal specimens have been collected from cultures with different different salinities. The authors found a significant correlation between salinity and Na/Ca and additionally made elemental mappings of the different elements on cross-sections of selected specimens with electron microprobe.

First of all I have to say that I was impressed to see the amount of data and work which has been put into the analyses and culturing. More than 200 specimens have

Printer-friendly version

Discussion paper

been analysed with more than 600 measurements! I regret to say after reading the manuscript carefully that there is a substantial problem with this study and at least major revisions are required for the manuscript. The problem is already mentioned by the authors themselves in the first paragraph of the discussion: The cleaning procedures.

The authors mention differences to former calibrations of Na/Ca as salinity proxy within the same species (A. tepida). Wit et al. (2013) used 5% sodium hypochloride to bleach the forams and remove organic contaminations. Geerken et al. now report that they bleached the forams in 70% H2O2. I do not think this can be true. Most of the H2O2 solutions which are commercially available are only 30%. In this case it would be 70% of a 30% solution which would be around 21%. This is still much to high! There are a lot of studies about cleaning procedures for analyses on foraminifera and standard methods have been developed based on studies which started already in the 1980s. The standard method for organic matter removal, which is widely applied in most of the labs, is bleaching in a 1% H2O2 solution (Barker et al., 2003). Barker et al.(2003) also showed that there is a significant impact of different oxidative treatments on foraminiferal Mg/Ca, though not on Sr/Ca ratios. Why did the authors not apply these standard cleaning techniques?

The SEM pictures within appendix C already show that, especially A. tepida appears to be not very well preserved anymore after the treatment. The EMP mappings shown in figure 4 also indicate chemical alteration on the outer part of the test walls of A lessonii (see Mg/Ca mapping and SEM picture). While most of the standard cleaning procedures are used for analyses of bulk samples with ICP-MS, I am not aware if there is any study on the impact of different cleaning procedures on laser ablation analyses. Are there studies at all on different cleaning procedures regarding microanalyses? Otherwise I would strongly recommend to develop a standard treatment also for laser ablation studies. Only this would ensure comparability between different studies.

From this point of view I think the authors cannot provide a quantitative calibration for

Printer-friendly version

Discussion paper

Na/Ca ratios as a salinity proxy. They can show that there is a significant relative effect of salinity on Na/Ca and thus give further evidence for the validation of this proxy. I suggest to rewrite the manuscript bearing this in mind.

Below I listed some additional points of revision:

General:

There are a lot of repetitions in the text, especially in the discussion. The manuscript is quite long and would benefit a lot by shortening some parts of the text.

Line 46 - 50: Nürnberg et al., (1996) was on Mg/Ca and not on Sr/Ca as far as I know.

Page 71 - 74: Please rewrite. This sentence is very long and complicated, especially the clustering of adjectives like “intermediate-Mg calcite, benthic symbiont bearing, tropical foraminifer”. I am not sure if it is correct to say “intermediate-Mg calcite foraminifer”.

Line 100: This cannot be. Please double check the H2O2 concentration you used.

Line 120: Ass space after with.

Line 148: A clustering of terms like S40/27/2 within the main text is very confusing for the reader. I would suggest to reformat this part.

Line 157 to 163:

Of course the number of calcium counts would affect the element/Ca ratio if it would only vary alone. This would mean that this element is not bound in calcite since it is enriched in regions of lower calcite density. I am not sure if it is right to exclude these points from the dataset.

Line 170:

This is not an artificially grown inorganic crystal but biogenic precipitated calcite. Of course there are heterogeneities within foraminiferal test calcite.

Printer-friendly version

Discussion paper

Line 208: Delete space before Smirnov.

Line 246: I am not sure if it is correct to write “the groups produced specimens”.

Line 247:

All not al.

Line 255: Add -4 to superscript. Also I would use another symbol than *.

Line 293: It should be possible to determine the genotype and especially regarding A. tepida I would strongly suggest to do this if you culture them.

Line 320: Again, I would use another symbol than *.

Line 329: Delete strong in brackets or the brackets.

Line 334: A complete reconstruction of past seawater conditions? Probably we would need a lot of more proxies for this.

Line 337: Two sentences in a row start with however.

Line 345: Several parts in this part of the discussion are repetitions from the last paragraph of the part before.

Line 348: Also here the aggressive cleaning procedure might have had different impact on different specimens. Line 352: There hasn′t to be a linear correlation. If salinity exceeds a threshold of the "sweet spot" for a certain species this might influence the size outside this window...

Line 358: There is that new study by Fehrenbacher et al., 2017. They show that all chambers are uniformely overgrown by a thick layer of calcite even after the last chamber has been formed. If you do not find any significant trend between the last three chambers this might be worth to be discussed here.

Line 362: The precision gets better statistically, due to the higher amount of measurements but I agree if measurement time is limited and samples are abundant it would be

Printer-friendly version

Discussion paper

better to analyse more specimens rather than doing replicates on a single individual.

Line 388 and below: This part indeed is interesting but also quite speculative because nothing is known about ACC precursor absence or presence within these species as far as I know. I would tone this part a bit down making clear that the authors are hypothesizing.

Line 390: Write phase instead of phases.

Line 396: There are possibly more than these two mechanisms. Also the rest of the paragraph here is very speculative.

Line 406: Where is the primary organic sheet in figure 4? Aren't these bandings supposed to record day/night cycles during calcification (Fehrenbacher et al., 2017)

Line 409: It is not surprising that these bands are spatially correlated. Actually I think they are sitting in the same banding.

Line 410: Not Surprising?

Line 411: It should be possible to quantify this in your laser ablation profiles or don't you see these bands in there?

Line 403 and following: The discussion here is far too long and doesn't really come to a conclusive point.

Line 602: Figure 1: n=?

Line 625: Figure 4: POS is mentioned before it is defined in line 627 of the figure caption. Also it would be helpful if you mark in the picture what you think is the POS.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-481, 2017.