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Interactive comment on "Planktonic foraminiferal spine versus shell carbonate Na incorporation in relation to salinity" *by* Eveline M. Mezger et al.

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Dear editor and Prof. dr. Ralf Schiebel,

Thank you very much for the useful comments, additions and suggestions on our manuscript. We have changed and/or answered these comments step-by-step in the text below, explaining why we agree or respectfully disagree. Our answers are directly below the individual comments.

Also on behalf of the other authors,

Respectfully yours,

Eveline Mezger

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Discussion paper

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-454, 2018.

ralf.schiebel@mpic.de Received and published: 2 January 2019 It is a pleasure to read the considerate and well-written paper of Mezger and coauthors on “Planktonic foraminiferal spine versus shell carbonate Na incorporation in relation to salinity”. In addition to the review of Takashi Toyofuku’s, I only have two more comprehensive points to be considered by the authors, as well as some small points to be taken into consideration before publication of the paper.

1) It would be interesting and important to know, which of the morphotypes of T. sacculifer, and which morpho- and genotypes of G. ruber are analyzed here and are represented by the data. Since some of the different types have different ecological demands, the new Na/Ca possibly represent very specific ecological conditions, which in turn may be reconstructed when using Na/Ca as a proxy of past conditions. The data plotted in Figure 4 may even show more structure when being plotted separately for different morphotypes? Also, “variability within one species” (line 256) may turn out to make sense for different morphotypes (also lines 258-259) ?!

Thank you for this suggestion. We are aware that morphotypes might be reflected by elemental composition of the species (e.g. for G. ruber: Steinke et al., 2005), mostly because of the different depth habitats these morphospecies live in. At line 256 we refer to the internal shell Na variability, which seems similar for all specimens observed here (namely higher Na in spines, lower in the shell). Apparently, this internal variability does not differ between morphotypes. For the shallow-collected (surface water, multinets up to ∼50 m depth), we were not able to differentiate between morphotypes, as the morphological traits for both species are mainly, and more clearly, pronounced in terminal reproductive stages. In the upper part of the water column, the G. ruber sensu stricto morphotype is known to dominate, whereas at great depths, the sensu lato is the dominant species (Wang, 200). Therefore, we believe for the shallow water-collected specimens that the impact of different morphospecies is rather limited. As we agree

that this is an important consideration for the development of a salinity proxy, we added your suggestion at line 257, also referring to Schiebel and Hemleben, 2017.

2) In section 3.2, Scanning electron microscopy, the authors may want to consider that there are two different types of spines both in G. ruber and T. sacculifer, one being round and the other being triangular (see, e.g., Schiebel and Hemleben, 2017). The very different spine widths reported in lines 205 to 208 may result from the two different types of round (thinner) and triangular (thicker) spines and may hence represent two groups of data in each of the species, and, even more importantly, Na/Ca may vary between the two types of spines.

Thank you for this useful comment. So far, based on SEM imaging and EPMA imaging/polishing of the shells, it looks like all spines end circular rather than triangular. Based on you comment, this implies that our conclusions can not necessarily be translated to all spine morphologies. We now added this suggestion to the discussion at line 316, also referring to Schiebel and Hemleben, 2017.

Minor points:

3) Line 22: better refer to carbonate, not calcite, because of other carbonate species like vaterite (Jacob et al., 2017); Thank you, we now changed the text accordingly.

4) in the entire manuscript Line 23: better use “taphonomic alteration“ than “taponomy” (also line 79)

Thank you, we now changed the text accordingly.

5) Line 56: bivalves have no spines (Zhao et al. 2017), and I cannot see any connection here

Thank you, we now changed the text accordingly and deleted the reference

6) Line 108: the “Whole shell” is called “test” in foraminifera; why do the authors avoid the term in the entire manuscript?

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Apparently, journals nowadays apparently prefer using ‘shell’, which I have also adopted for my other publications. Furthermore, the use of the word ‘test’ might be confusing, as people that are less into the material/foraminifera vocabulary might be looking for ‘shell’ rather than test.

7) Line 171: s-pecimen; Line 179: ruber italic; Lines 197-198: “in mixing signals between spine base and shell carbonate“, possibly resulting from the resolution of measurements; Line 208: “Salinity correlates negatively with spine width“, may be turned around to keep the right order; Figure 5: please indicate which data refer to entire test and which to shell (-only) measurments: One comma too many in the caption. Line 355: better: "When spines fully account for. . .” Line 367: better “up to 2-3 mm long;...” Line 374: (Figs. 5 and 8), check in the entire manuscript Line 378: . . . sacculifer); open parentheses Line 395: Na concentration?

Thank you for these comments, we now changed the text accordingly.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-454/bg-2018-454-AC2-supplement.pdf

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