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Interactive comment on "Seawater pH reconstruction using boron isotopes in multiple planktonic foraminifera species with different depth habitats and their potential to constrain pH and pCO_2 gradients" *by* Maxence Guillermic et al.

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We wish to thank this reviewer for his thorough review of our manuscript and his helpful comments. We believe that we addressed all of the major comments as indicated in the discussion below and the updated manuscript (see supplement).

RC2: Depth habitats of the foraminifers are used to link the habitat to oceanographic conditions at the core locations. The supplement has a fairly thorough explanation of how the various depths were determined (using d18Oc, MgCa-derived T, and published

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depth habitats; detailed in Table 6), but I couldn't determine which depth the authors chose to use for each species (or at least is wasn't consistently clear in the text) without seeking the information in Table 7. In the intro of the MS, the authors point the reader to table 3 for the depth habitats but table 3 doesn't include this information. Also, I don't understand why sometimes the authors used published references for depth habitats and in other instances is d18O or Mg/Ca derived temperatures. For example: For core FC-01a, the Sime reference is cited for the depths used for G. ruber, T. sacc, O. universa, but oxygen isotopes are used for P. obliquiloculata and Mg/Ca derived Ts are used for tumida and menardii. Clarification here is needed.

Response: A table was given in a previous version of this manuscript, but reviewers thought that was redundant with the table in the supplement (it was table 3), I have added the selected CDs in Table S7. You are right, it has been difficult to carefully select the CDs, I have tried to do it rigorously but sometimes it still remains arbitrary. To explain: I derived both calcification depth (CD) using δ18O (CD1) and Mg/Ca (CD2). Because each of methods have their limits (species-specific calibrations for most of it, and analytical uncertainties for δ18O) I compared the calcification depths to previously published in literature (CD3). I can't say what method is the best, but in my case I would tend toward Mg/Ca because we have species-specific calibrations for most of the species and the measurements were done on the same sample analyzed for δ11B. For δ18O I had to pick again. To select the CD, the first thing I did was to look at the water column structure at each site in order to have an idea of what depth I should expect the species to be depending on their habitats (mixed-dweller, within thermocline, below thermocline). Then I compared CD1, CD2 and CD3. If the CDs were in line with the water structure and two CDs were similar I chose that one. If CD1 and CD2 were different I chose CD3. I chose CD3 when possible because even if I have confidence in my data, there are papers that have more robust CD reconstructions (main goal of their papers). Now, for G. tumida and G. menardii, there were not a lot of data in the literature to compare with and there is no species-specific δ18O calibrations but there are if using Mg/Ca. When I compared with the few data in the literature, my CD2 were

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in line than CD3 that is why I stick to CD2 for both species. For P. obliquiloculata, it was the same not a lot of data in the literature (only Sime et al.,), so we use Mg/Ca data (CD2), not δ18O even if they are similar except (FCO2a).

RC2: I don't understand why O. universa is considered a deeper dweller in this MS. There are some major assumptions made about why the d11B of this species falls below the 1:1 line – this is also discussed in Henehan et al., 2013. Why O. universa d11B is more like the deeper dweller non-spinose forams is indeed puzzling (esp. since it has same symbionts as G. ruber and T. sacculifer), but I don't think it can be attributed to a deep depth, especially given the size fraction used (>500). The larger size fraction of the samples used would suggest that these are living at a shallower depth (See Spero and Parker, 2003) and likely in the mixed layer. The correct depth habitat will impact the calibration.

Response: Yes, it will change quite a lot the position of the data, especially for site FC0-1 that probably would be an outlier, but the calibration won't change significantly when compiling all data (see Figure below). Only based on the CD calculations, O. universa is found in average $\sim$60-70m which is in line with other depth habitats reconstructions, the fact that δ11B is lowered than ambient pH can be explained by a depth habitat >50m. The data converge towards this low δ11B due to a decrease in light insolation at deeper depth. Its high symbiont density and photosynthetic activity might make it, as T. sacculifer, more sensitive to water depth changes as well, or we could assume a change in symbiont assemblage to ones that are more similar to what are found in deeper dweller species, changing host/symbiont interactions, photosynthesis and related microenvironment pH.

RC2: Samples were cleaned using the full cleaning method including the reductive step (cite Boyle and Kiegwin 1985/1986 as well, since they developed the method). Why was the reductive step included here? Yu et al., 2007 suggests the reductive step isn't detrimental to B/Ca ratios, but the effect of this cleaning step on for d11B analysis is unknown and according to Rae et al., 2018, the reductive step is typically not used

during sample cleaning. There are documented dissolution effects that the authors discuss in the supplement (preferential dissolution of ontogenetic calcite occurs relative to the light d11B of gam calcite) and if cleaning preferentially removes ontogenetic calcite, then the primary d11B signal has been altered by the reductive cleaning. Additionally, the reductive cleaning step IS detrimental to other elements, like Mg/Ca ratios (decreases ratios by up to 15%), which were used to estimate depth habitat for some of the species investigated here. Including the reductive step should be justified with an explanation of how this additional cleaning step could have affected results.

Response: Misra et al., (2014b) tested the different cleaning on the B/Ca and δ11B and observed no effect. However, in nodules (Mn-Fe) the concentration of B can be up to 120 ppm (Axelsson et al., 2002), Fe-Mn oxide and hydroxides can then result in non-negligible content of Mg and B contamination. We utilized the reductive step because some of the sites where not previously studied, overall the sites did not present high Mn, except for E035 which was removed due to that contamination.

Also, results of Mg/Ca would lead to a deeper CDs which is not the case when comparing with CD1 and CD3. Axelsson, M. D., Rodushkin, I., Ingri, J. and Öhlander, B.: Multielemental analysis of Mn–Fe nodules by ICP-MS: optimisation of analytical method, Analyst, 127, 76–82, 2002. Misra, S., Owen, R., Kerr, J., Greaves, M. and Elderfield, H.: Determination of δ11B by HR-ICP-MS from mass limited samples: Application to natural carbonates and water samples, Geochim. Cosmochim. Acta, 140, 531–552, 2014b.

RC2: Li/Ca ratios are included in table 2, but never discussed?? Are they relevant for this MS? If not, perhaps remove?

Response: Li/Ca are not discussed in this paper, however there is a growing interest for Li in foraminifera, we decided to publish those results in order to contribute to the existing data.

RC2: Table 3: In the text on line 329 it is stated that chemo stratigraphic data is used

to constrain depths, but this information is not summarized in the table, probably just a typo?

Response: It was a typo, changed. The CD chosen are in Table S7.

RC2: Other suggestions: Line 56-57: Not sure I agree with the statement in the abstract that the other species follow O. universa because of light limitation by symbiont bearing foraminifera. All of the deep dwellers have symbionts, all live in the photic zone.

Response: I have tried to improve the discussion, focusing on the symbionts/photosynthesis, because the story is of course more complex. Yes, all of the species have algal symbionts but they can differ. From what I see in the literature is that T. sacculifer, G. ruber, O. universa have mostly dinoflagellates symbionts (can have chrysophyte as well) where G. tumida, G. menardii, P. obliquiloculata and N. dutertrei will have chrysophyte algal symbionts. The photosynthesis is dependent of the nature of the host/symbionts interactions, symbionts type (pigment associated for light absorption efficiency), symbionts density. The recent study from Tagaki et al., (2019) is really helpful as he constrained the photosynthesis activity, light absorption efficiency and the symbiont density of those species.

Fv/Fm (photosyntethic activity) T. sacculifer>G. menardii > O. universa> G. ruber (white) > N. dutertrei > P. obliquiloculata σpsi (light absorption efficiency) N. dutertrei > P. obliquiloculata > G. menardii > G. ruber > T. sacculifer > O. universa Chla/biomass T. sacculifer>O.universa>G. ruber> N. dutertrei>G. menardii>P. obliquiloculata

What I assume is that T. sacculifer, O. universa and G. ruber photosynthesis are likely to be more affected by changes in insolation than other species due to their symbiont density, high photosynthetic capacity and their light absorption efficiency. Which is still in line with the argumentation we are giving. Also, the fact that the deeper dwellers have this low boron isotopic signature is likely due to a lower symbiont density, lower photosynthetic activity and a reduced insolated environment. P. obliquiloculata has the lowest density and photosynthetic activity, which would translate in a respiration driven

environment the fact that most of the species are following this trend would go in the sense of a respiration driven environment. Also the fact that O. universa is following this trend would, I think, whether be due light limitation and/or a different symbiont due to its deeper depth.

Lines 485-543.

I also changed Figure 7 to give more informations on the microenvironment pH.

I changed for “likely caused by light limitation and/or symbiont/host interactions”

RC2: Line 128: This sentence is poorly structured.

Response: Changed for: “Furthermore, δ11B differences between foraminifera species from the same pH makes the acquisition of more core-top and culture data essential for testing and applying the proxy.”

RC2: Secition 2.4: Origin of biological fractionation Paragraph beginning on line 172: very speculative and based upon benthic foraminifer experiments.

We have added a statement (line 176) to this effect, that “We acknowledge this is speculative as it is based upon benthic foraminifer experiments.”

RC2: Section 2.5: The annual vs seasonal preferences for forams is largely dependent on temperatures. For example, in some regions G. ruber and G. sacculifer can be present throughout the year if T > 25C, but will have a summer/fall preference when T drops below 15. Their choice on seasonal vs. annual presence of these species will affect the hydrographic data used and perhaps impact results.

Response: Noted.

RC2: Section 3.2: Size fractions listed in this paragraph don’t agree with size fractions in the Table.

Response: Changed for: “Around 50-100 foraminifera shells were picked from the 400-

500 μm fraction size for Globorotalia menardii and Globorotalia tumida, >500 μm for Orbulina universa, from the 250-400 μm fraction size for Trilobatus sacculifer (w/o sacc, without sacc-like final chamber), Trilobatus sacculifer (sacc, sacc-like final chamber), Globigerinoides ruber (white, sensu stricto), Neogloboquadrina dutertrei, Pulleniatina obliquiloculata. The samples picked for analyses were visually well preserved."

RC2: Section 3.8: This section could/should include some of the information in the supplement regarding the depth used to obtain hydrographic information.

Response: We acknowledge it was not clear in the manuscript. The information can be found in Table S7. Line 341: "The depth habitats utilized to derive in situ parameters are summarized in Table S7."

RC2: Line 339: Forams don't migrate in the water column (See Meiland et al., 2019), but deep dwellers may crust at depth during the END of their lifecycle, this should be clarified. This is later explained correctly (lines 452-453).

Response: Changed: line 338 "As foraminifera can migrate in the water column along their ontogeny". It is still vague but I don't know how constrained is this depth change at the end of their life cycle.

RC2: Lines 509-514: The concept of facultative symbiosis is outdated – all forams with symbionts are likely obligate and not facultative. See https://www.biogeosciences.net/16/3377/2019/. G. tumida doesn't have symbionts at all, so why does it align with the other species? Please discuss.

Response: But Tagaki et al., (2019) didn't constrain G. tumida in his study. From what I have read the family Globorotaliidae have algal chrysphyte symbionts, which should include G. tumida. If there are no symbionts or a low symbiont density/low photosynthetic activity (like P. obliquiloculata) the respiration driven microenvironment resulting from the parameters aforementioned or again light limitation would be a plausible explanation.

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I have made changes in this section in order to improve the discussion. Lines 485-543.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-266/bg-2019-266-AC3-supplement.pdf

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Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-266, 2019.

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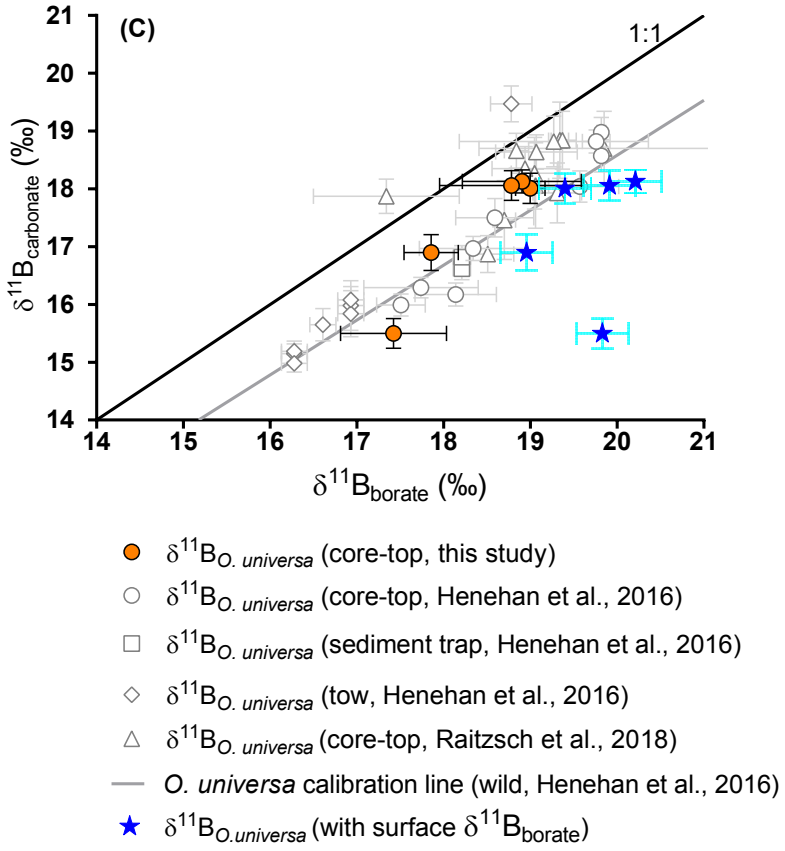

Fig. 1.

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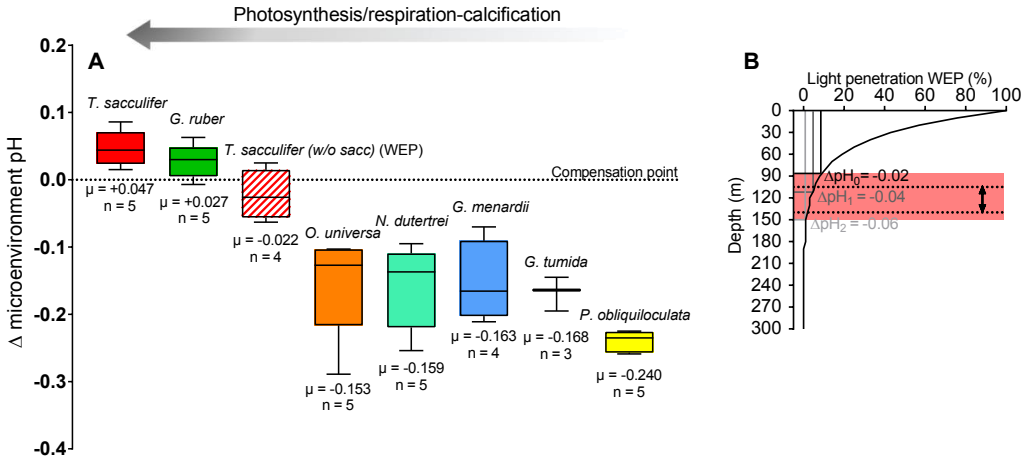

Fig. 2.

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