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Biogeosciences
Discussions

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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I read this recent work by Guillermic et al. with great interest. The authors present useful new data from core-top foraminifera, which expands the array of core-top MC-ICPMS data we have in the community. The data are logically presented, and the manuscript is well written for the most part (although there are a fair few typos throughout, which I will leave to the reviewers and copyeditors). These data add more evidence that no modern species of planktic foraminifera measured to date consistently records the d11B of borate faithfully (with implications for palaeo-work), and confirms the pat-

tern of increasing d11B in increasingly shallow-dwelling symbiont-bearing foraminifera, and lighter d11B in deeper-dwelling species.

While in my view this will be worthy of publication, I would like to take this opportunity while the manuscript is still in open discussion to make some suggestions to improve the manuscript.

1) The authors on several occasions highlight the difference between their G. ruber data and the slope obtained by our culture experiments (Lines 385-387; 422-425; lines 525-526; 537-538; 563). In lines 422-425 the picture as presented is particularly confusing, since the authors first suggest "there is a difference in calibrations", then say "this is particularly notable for G. ruber", but then say "the sensitivity of the species analysed are not statistically different". In truth, only the final sentence is true (with the exception of the clause "and are close to unity". A slope of 1.12 ± 1.67 is within uncertainty of our culture slope (0.6), and could technically allow a slope as low as -0.55: i.e. there is no significant difference between the slope they suggest and the slope that we observe in culture.

Framing this as a difference seems even more odd given the authors do not draw any distinction between their T. sacculifer slope and that of previous calibrations, because their bounds of uncertainty do not allow it- so it seems logically inconsistent to draw distinctions for ruber where the statistical difference is equally unfounded.

I would suggest the authors go through the manuscript and revise their phrasing to reflect this lack of statistically significant difference. Including "the sensitivity of d11B to pH is not statistically different from unity for G. ruber" as a main conclusion in line 563, for example, implies this study is in disagreement with cultures (which it isn't), and that we should consider a slope of 1 to be potentially suitable for this species. In reality, were we to calculate pCO2 with the slope and intercept that the authors suggest (m=1.12, c=-1.23), the fit of the downcore record of G. ruber from Chalk et al. 2017 with ice-core pCO2 would be considerably worse (see attached Fig. 1). The magnitude of pCO2

change between glacials and interglacials (i.e. the parameter that is driven by the slope of the calibration) is underestimated, with pCO2 too high in glacials by ∼50 ppm. The improved fit of the down-core record with pCO2 from ice-cores when our ruber calibration is used, however (see Chalk et al. 2017), offers support for the shallower-than-unity slope we observed in this species. Incidentally, also, an R-squared of 0.98 for the ruber core-top data presented in this study seems anomalously high relative to the scatter/uncertainty bounds in the dataset- can the authors be clear how this R-squared is computed? Is it the average R-squared of Monte Carlo regressions plotted through datapoints randomly subsampled from within the x- and y-uncertainties? Or is this simply a least-squares linear regression through the central tendencies of the datapoints? The former might be more representative, but as long as the authors are clear about what they are describing that is the main thing.

2) The authors report our generic culture intercept for ruber in their Table 3 (9.52), but erroneously list the size fraction as ∼250 μm. I would like to point them to Fig. 6 of this 2013 paper, where we give the average size fraction of our cultures to be ∼380 μm. A suggested size fraction correction on the intercept is given, such that for 300-355μm it would be 8.87. On this same note, the authors also combine a wide size fraction (250-400μm) for G. ruber, which given size-related offsets from the culture calibration (as shown in Fig. 6 of Henehan et al. 2013) has the potential over such a large range to skew the data, due to size-related changes in d11B. Can the authors give some estimate as to the distribution of test sizes within their broad sample range, so as to make them more easily comparable to published data?

3) The authors screened for clay contamination using Ti/Ca ratios, as Al/Ca values were difficult to measure with their introduction system. However, they do not provide these data. Clay may carry isotopically-light sorbed d11B with it, and introduce bias towards lighter values. To allow maximum confidence in the data, and see which datapoints if any might have some influence of clay, can the authors please provide the Ti/Ca ratios in Table 2?

4) In Section 4.2.3 (note the paper skips 4.2.4 and goes straight on to 4.2.5?), the authors pool 'deeper-dwelling' foraminiferal species together, but this seems a bit unfounded since these foraminifera don't even have the same symbiont types (crysophyte vs. dinoflagellate), and have quite different ecologies. I'm not convinced there's enough of an a priori reason to even do this in the first place. However, I see that the authors do already concede this may be unfounded.

5) In section 3.9, the authors make no mention of how they calculated pK*B for each foraminifera. I take it they did indeed account for changes in pK*B with temperature, salinity and pressure? It may sound blindingly obvious, but I'm constantly amazed at how many people make this error.. On a similar theme Fig S3 the pH lines are no doubt helpful, but I'm not sure how the authors managed to calculate them, given the pK*B is different for each foram. Is this calculated using the mean pK*B of each of the forams plotted? This figure makes me worry that the authors just chose a single value of pK*B for all forams in all calculations of the paper, which would be wrong.

6) I think the decision to group 'shallow-dwelling' foraminifera (note it is not clearly defined what species this includes in any caption) in Fig. S4 (and in the text where this is referenced) is I think unfounded. It produces a correlation between B/Ca and Borate/DIC, sure, but it's entirely driven by the interspecies difference between ruber and sacculifer, and we know from Kat Allen's work for example that these species have fundamentally different B/Ca-Borate/DIC relationships.. they shouldn't be lumped together in one group. As it is, it makes this look like a carbonate system relationship when on an intra-species level there is no significant correlation with the carbonate system (as we also observed elsewhere).

I hope these comments prove useful and help with the development of the manuscript.

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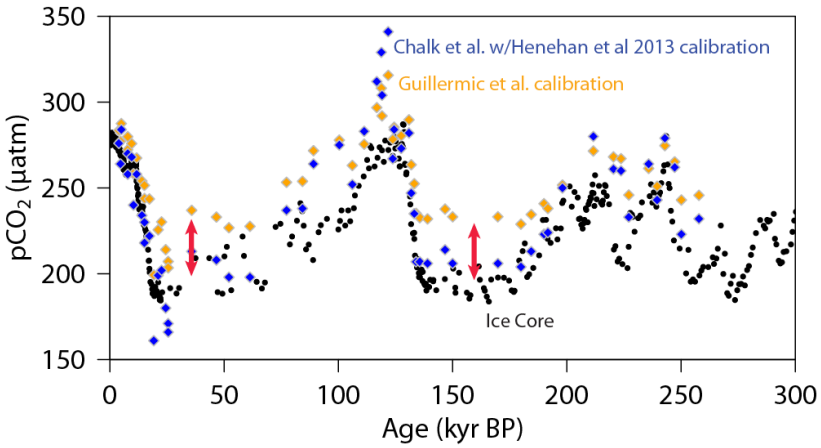
350
300
250
200
150
pCO2 (µatm)
0
50
100
150
200
250
300
Age (kyr BP)
Chalk et al. w/Henehan et al 2013 calibration
Guillermic et al. calibration
Ice Core

Fig. 1.