***Reply to Editor's comments from L. de Nooijer on*"Size-dependent response of foraminiferal calcification to seawater carbonate chemistry" *by* Henehan, Evans et al.**

Comment: *"page 2, lines 24-27: I don't understand what 'conditions during life' means here. Don't these conditions include inorganic carbon chemistry at the sea surface (lines 24-25)? Or do you mean that 'conditions during life' within a foraminifer's lifetime are assumed to remain constant?"*

Response: What we mean here is that using SNW metrics as solely a dissolution index must assume relatively little influence of surface water conditions on size-normalised shell weight during the foraminifera's lifetime, such that the post-mortem signal reflects only dissolution. Conversely, using SNW as a proxy for surface water conditions requires post-depositional alteration to be minimal. We have rewritten this passage in case our writing was not clear.

Comment: *"page 3, line 11: 'Be' should be 'Bé '. Please also check other references to Bé in the rest of the manuscript."*

Response: Thank you for spotting this oversight. This has been corrected.

Comment: *"page 3, first paragraph: perhaps it is useful to include somewhere in the introduction the notion that SNW is often thought of/ assumed to reflect the 'average thickness of the chamber walls'. I would say that this may indeed be largely true, but in-/ decreased porosity (Bé et al., 1976. Progress in Micropaleontology 1-9) and differences in densities (i.e. shell walls may not be completely 'filled' with $CaCO_3$) may also determine the SNW. "*

Response: We agree and have added a section to the introduction stating this. However, preliminary results from ongoing work in our lab investigating the controls on porosity, and how it changes during ontogeny (by co-author Janet Burke) suggest that if anything changing porosity during ontogeny should work in opposition to the observed increase in CI with size. Therefore we do not dwell on this too much here.

Comment: *"page 3, line30: shouldn't this be 'μ mol m^{-2} s^{-1}'?"*

Response: Thank you for spotting this mistake. This has been corrected.

Comment: *"page 3, line 32: 'petri' should be capitalized."*

Response: Done

Comment: *"page 4, line 17: include 'mol/mol' after '6.25'."*

Response: Done

Comment: *"page 5, line 10: the 'area' reflects the size of the newly formed chambers. It should be noted here that the CI is therefore a species-specific measure (as is done in the appendix). When comparing CIs between species, morphology of the chambers should be taken into account (particularly when comparing spherical to more flattened chambers)."*

Response: Thank you for the suggestion. We have added a note to this effect at the end of this section.

Comment: *"page 5, line 24: Reiss (1957. Contribution from the Cushman Foundation for Foraminiferal Research 8, 127-145) is probably the first to have described the added layer of calcite on top of previously formed chambers. Moreover, this only applies to rotallid foraminifera (line 22), rather than foraminifera in general."*

Response: Thank you for highlighting Reiss's papers on this topic- we have cited him here. We have also clarified here that we are talking about planktonic (and so by implication rotalliid) foraminifera.

Comment: *"page 5, line 25: 'thickness' applies to the chamber wall, not to the chamber as such."*
Response: We have rephrased this sentence.

Comment: *"page 5, lines 27-28: does the thickening of the chamber walls in this case result in the formation of a crust?"*
Response: We distinguish 'crusting' from 'gametogenic' calcite, as others have (e.g. Hamilton et al., 2008, Marine Micropalaeontology). Hence we mention thickening during ontogeny in the following sentence rather than here. But we now use the word 'crust' to make this clearer.

Comment: *"page 7, line 7: 'more times' should be 'with one more layer' for this particular example."*
Response: Changed.

Comment: *"page 8, lines 32-33: sounds like a contradictory to me."*
Response: A 'not' was missing from the sentence here. Thank you!

Comment: *"page 8, line 34: effect of temperature on CI?"*
Response: Changed.

Comment: *"page 9, line 6: this is an important point, that may be stressed more in the discussion. A recently proposed model for (rotalid) foraminiferal calcification suggests that foraminifera are able to convert any DIC into [CO_3^{2-}] at the site of calcification (Toyofuku et al., 2017. Nature Communications 8: 14145). This would hint to a primary control of [DIC] rather than ambient [CO_3^{2-}], saturation state or seawater pH."*
Response: We now note the paper by Toyofuku et al. in the discussion (see below). However, our data do not show a significant relationship between residual CI and DIC (R^2 = -0.07, p = 0.553). We have noted this briefly here.

Comment: *"page 9, lines 25-26. This is not true. There are a number of papers providing an explanation for the contrasting responses of marine calcifiers to ocean acidification by Justin Ries. Foremost, these include Ries et al., 2009 (Geology 37: 1131-1134) and Ries, 2011 (GCA 75: 4053-4064). Please include this in the discussion."*
Response: We now note these papers.

Comment: *"page 10, lines 17-32. I am not sure that I follow this completely. The distinction between smaller specimens that do not have an internal pool and larger foraminifera that require storage of DIC prior to chamber formation does make little sense to me. The references for small and larger specimens (Nehrke et al., 2013 versus Erez/ Ter Kuile) coincides with large inter-species differences (in morphology as well as carbonate chemistry). Without implying to impose my own favoured model for calcification (see e.g. Toyofuku et al., 2017), the basic mechanism in which protons are exchanged for calcium during chamber formation is suggested for (benthic) species across a large range of sizes (Glas et al., 2012. Journal of Experimental Marine Biology and Ecology 424-425: 53-58). It may be, however speculative, that the steepness of the resulting external pH gradient (Glas et al., 2012; Toyofuku et al., 2017) determines the uptake rate of DIC. This steepness may well be a function of size and hence relates CI to volume/surface ratios. Depending on the authors' preference, I suggest to re-word this paragraph*

somewhat. Without making a distinction between 'pools' versus 'no-pools', the mechanism leading to the uptake of DIC from the surrounding seawater likely depends on the amount of ions necessary for the production of one new chamber (which is a function of the specimen's volume) and the surface area across which these ions have to be transported. These two units change with foraminiferal life-time and may hence result in a more or less cost-effective uptake of DIC (and Ca^{2+})."

Response: Thank you for your suggestion. We agree that the model proposed by Toyofuku et al. [2017] may also be consistent with our findings, and have extended this paragraph to explain that this is the case. However, because internal carbon pools are unambiguously a feature of some foraminifera [e.g. ter Kuile et al. 1989], we think it may be informative to retain the existing discussion, although we also clearly state that the two models are not mutually exclusive.

Comment: *"page 11, line 10: it may be informative to include Flako-Zaritsky et al. (2011. Marine Micropaleontology 80: 74-88), who show that a number of benthic species are capable of producing their calcite despite very low pH and consequently, undersaturated conditions."*

Response: This reference and an accompanying explanatory sentence have been added.

Comment: *"page 13, line 11: there is a comma too much."*

Response: This sentence has been split and reworded for clarity.

Comment: *"figure 1: include panels (d) and (g) to the description for panel (a). Similar for (e)/(h) and (f)/(i)"*

Response: In order to avoid referencing panels in an inconsistent order, we now briefly describe panels d-i at the end of the caption.

Comment: *"figure 3: what happened to the uncertainties in the seawater Mg/Ca (panel a)?"*

Response: For Mg/Ca_{sw}, analytical uncertainty was $\pm 3\%$ [see Evans et al., 2015a] and as such is smaller than the symbol size. We have added a note explaining this in the figure caption.

Comment: *"figure 5: related to re-phrasing of the third paragraph of section 4.1, the authors may have to change the text within this figure."*

Response: Text has been changed as requested to reflect the above suggestion.

Comment: *"table 3: CIs should have a unit ($\mu g/mm^2$)."*

Response: These have been added.

Comment: *"page 30, line 6: 'our' "*

Response: Replaced with 'the calcification'.

Comment: *"Figure A2: I don't understand how in model 2, b can vary by 10%, but never exceeds beyond ~ 0.1075 or drops below ~ 0.0975 instead of 0.110 and 0.900."*

Response: Many thanks for noticing this error. The figure has been updated accordingly. Note that whilst the wrong version of this panel was included, there was no mistake in the underlying data analysis and as such it does not alter our conclusions in any way.