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Biogeosciences Discussions

Interactive comment on "Calibration of δ^{18}O of laboratory-cultured deep-sea benthic foraminiferal shells in function of temperature" *by* C. Barras et al.

LJ de Nooijer (Referee)

lnooijer@awi.de

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Dear Dr. Bijma,

The manuscript from Barras and co-workers ("Calibration of δ18O of laoratory-cultured deep-sea benthic foraminiferal shells in function of temperature") you asked me to review, is in my opinion straightforward and clearly written. I find the dataset rather small (especcially in comparison to the accompanying manuscript by Filipsson et al.) and urge the authors to expand the discussion of their paper. After consideration of the following issues, an improved version of the manuscript would likely be fit for publication in Biogeosciences.

Here and there, phrasings could be more precise: particularly in the abstract. Examples from the abstract and elsewhere include the following.

The title reads much better as something like "Calibration of δ18O of cultured benthic foraminiferal calcite as a function of temperature"

Page 336, line 2: It is not the chemical composition of the foraminifera, but that of their calcite that is used for paleoreconstructions.

Page 336, line 3: “in situ” reads better as “field”

Page 336, line 8: “in experimental conditions” should be “under experimental conditions”

Page 336, line 15: “benthic foraminifera” should be “this species”

Page 337, line 8: “all these factors are interfering” reads better as “many of these factors co-vary”

Page 337, line 14: “On the contrary” reads better as “However”

Page 339, line 14: “very clean (. . .)” reads better as “transparent with no mineral adhesives visible”

Other comments:

Best to state explicitely that Bulimina marginata has no photosynthetic symbionts.

Could the authors include (table?) the reproduction and growth rates for the different conditions?

The axes of figure 1 should be switched so that T is on the horizontal one and the δ18O’s on the vertical one (like in figure 2). I also think the figure would improve if all four panels are combined into one.

The data in figures 1 and 2 may be better plotted as averages and SD’s, with the number of measurements (n) in the caption.

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Section 3.3 relates the δ18O results from cultured Bulimina's to inorganically precipitated calcite (Kim and O'Neil, 1997). Previously inorganic-biological/δ18O-T relationships have resulted in the same conclusion: namely that the effect of temperature on fractionation of oxygen isotopes during calcification in foraminifera follows equilibrium values. Therefore, δ18O of foraminiferal calcite is believed to be a relatively good proxy for sw temperature. I don't see why a complete section should be devoted to this comparison. More interesting is the evidence for a significant, size-specific offset in the oxygen isotope fractionation for Bulimina marginata. Why would this be? And how does this influence the use of this species in paleoceanography? Therefore, I think that the porté of section 3.3 could be summarized in a few sentences and that the implications of the results explored in section 3.2 could be widened somewhat (see also suggestions below).

The first and final conclusions are essentially the same.

On the limited size of the dataset:

However well-executed the culturing experiments are, the presented dataset is on the small side. The main conclusions of Barras et al. confirm a similar T-dependency and ontogenetic offset previously found in other species (see refs in sections 3.1 and 3.2). The authors therefore should take their discussion one step further: which, in fact, should be easy with their novel way of culturing (benthic) foraminifera. The obtained calcite should provide more than enough material for other analyses (e.g. Mg/Ca and Sr/Ca from single chambers, δ11B, δ26Mg from complete specimens, morphological characteristics).

An alternative option is to link the δ18O measurements to the mode of chamber formation. Since measurements of large individuals present a mixed signal from small and large chambers, the values for large specimens are 'diluted' by relatively low δ18O values for the smallest chambers. This means that the difference in oxygen fractionation between small and large chambers is even bigger than seems from the presented

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results. Could the authors estimate these differences? What does this mean for measurements based on dissected chambers (e.g. Filipsson et al., 2010)? Could there be a relation between the mode of chamber formation and size-specific patterns?

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