Interactive comment on "Oxygen isotope composition of final chamber of planktic foraminifera provides evidence for vertical migration and depth integrated growth" *by* Hilde Pracht et al.

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Response to Reviewer 1. Jody Wycech

We thank the reviewer for their feedback and interesting comments, and apologize for not being able to have an online discussion regarding their points. Outlined below is our responses to their questions:

I recommend the authors also cite Kozdon et al. (2009). Kozdon et al. (2009) used SIMS to measure the d18O values of the ontogenetic calcite and reproductive crust of

N. pachyderma to reconstruct the species' water depth migration.

We shall include the paper at an appropriate spot.

Page 5 - Lines 20-22: How many G. ruber and N dutertrei were analyzed or were they just not weighed and measured for size? Perhaps breaking the description of T. Sacculifer analyses into a subsection within 2.1 will help (or changing the title of this section to be specific to T. sacculifer).

We shall add in an additional section as follows: "2.X Specific methodology for Question 3 To determine whether species of planktonic foraminifera from the same geographic location share the same or similar single specimen d18O(shell) variability specimens of G ruber (n = X) and N. dutertrei (n = X) were picked from the same interval. These shells underwent the same methodology outlined in section 2.1 for photographing, weighing and isotope analysis."

What were the additional steps used for T. sacculifers prior to stable isotope analysis? If the additional steps involve the heated block, the authors should also describe how the d18O analyses were performed for the G. ruber and N. dutertrei shells.

No all isotope values are based upon a heated block, the additional steps performed on T. sacculifer are outlined in sections 2.2 and 2.3. We shall add in the following text in line 22 (Pg. 5) so that it reads: "underwent additional steps, outlined in section 2.2 (dissection of chambers) and section 2.3 (size fraction), prior to stable isotope analysis."

Page 6 - Lines 20-21: What is the vital effect d18O correction? I think readers will be able to better interpret Fig. 6b with clarification. Is the correction necessary if you use the d18O-temperature calibration of Mulitza et al. (2003) instead of Kim and O'Neil (1997) (see comment regarding Page 6 – Line 30)?

We strictly use the equation by Kim and O'Neil as the definition for the d18Oeq, similar to previous studies by our and many other groups. The use of one equation to define (inorganic) equilibrium is preferred in order to avoid different equations for different

species or even for different shell sizes. Plankton pump and multinet collected specimens from the surface mixed layer were used to establish the value for the vital effect. For both species G. ruber and G. trilobus we used a d18Ove correction of -0.48 per mille. No vital effect correction was used for the non spinose species Neogloboquadrina dutertrei. These data are from Peeters et al., 2004 (Nature) and from Peeters et al., 2000 Ph.D. thesis). The error on the estimation of the vital effect is 0.15 per mille (1 s.d.).

Here we correct the measured values for vital effects of the species and perform statistical tests on both the corrected and uncorrected values. We will add in the following text: "In order to account for similar absolute measured values between species which are not produced by concurrent depth or seasonal preferences between species but instead by species-specific disequilibria from values obtained from ambient seawater equilibrium (so called 'vital effects') a correction was applied. The d18O shells were corrected by -0.48 per mil for G. ruber and G. sacculifer. "

Page 6 - Line 30: Why use Kim and O'Neil (1997) instead of a foram-based calibration such as Mulitza et al. (2003)? I suggest adding a sentence or two for explanation

See above: we want to avoid different equations and define strictly use K&O with an extra term for the vital effect. As such the temperature equations for different species may be offset from one another but all equations are parallel having a similar slope at a given temperature. We agree with the reviewer that a foraminiferal based calibration may be more suited in some instances, however these calibrations focus solely upon temperature as a driving factor for d18O (apart from the culture based ones). Carbonate ion is known to influence the d18O, and a number of these calibrations do not take this into account. Kim and O'Neil (1997) is used because it represents unmodified equilibrium, and whilst it also doesn't take into account the CIE it is a useful base to build upon as it is not subjective. However, it is easy to vary the input using a number of published calibrations to test the sensitivity of these results.

C3

Page 8 - Lines 7-9: I'm not sure how Figs. 5c-d show statistical significance (although the trend is quite obvious visually). Perhaps you could plot the 95% confidence interval on the robust regression (iteratively reweighted least squares) to show the slope is statistically different from zero. Alternatively, you could leave the plot as is and change the sentence to state D18O is dependent upon the d18O of the measured fragment.

We tested the significance (r being not zero) using Pearson's correlation coefficient: The critical value for the absolute value of the correlation coefficient for alpha = 0.05 and N=57 (Deg. of freedom = 55) is 0.273 for DF=50 and 0.250 for DF = 60 (our table does not give a critical value for DF=55). Since our correlations coefficients are higher than these Critical values we may conclude they are different from zero. The correlation coefficients are also significant at the alpha = 0.01 level for which the highest critical values is 0.354 (for DF = 50). The graphs indicate that there is a relationship between the d18O value of the shell minus the final chamber and the d18O difference between the final chamber and the remaining shell value. This can be interpreted that the difference between final chamber and remaining shell may be a function of the surface water temperature as the (temperature difference between final chamber and shell minus final chamber) decreases with decreasing temperature. Potentially for low SST's we may face a situation that the final chamber may even be warmer compared to the remaining shell value.

Page 9 – Lines 18-19: "above and below 100 m" is a bit vague. Note which water depths have temperatures of 24.5âŬęC and 25.5âŬęC at this site. If 25.5âŬęC measured from the <F fragment is not in the upper 0-50 m, that would suggest the fragment is also partly composed of GAM crust (see comment for Page 10 -Lines 18-21).

We chose to be a bit vague with the depths, however, we will add a section to the text considering the potential for the two proportions to have GAM addition. We will further consider that if GAM calcite precipitates upon the outside of the shell, then it could be that the proportion of GAM for F and <F may have different amounts if F has a larger surface area.

C4

Page 10 – Lines 18-21: I've used image processing of shell walls in cross-section, which suggests that GAM crust composes 32-44% of T. sacculifer shells. I've also used SIMS to analyze the δ18O of PREGAM and GAM calcites in the penultimate chamber of Holocene and Pliocene T. sacculifer shells from several Atlantic and Pacific sites, and have found that the GAM is âĹij1‰ higher in δ18O than the PREGAM. That work is still in prep, but you could cite one of the following abstracts or my dissertation (refs below). I think Lines 18-21 undervalue the issues with GAM crust so at the very least, I suggest noting that the presence of GAM crust in the <F fraction may skew your results towards higher δ18O (colder temperatures and a deeper depth habitat).

In shells of living O. universa we have noted (after cracking shells and observing under SEM) that there is a linear relationship between the width of the inner wall and the width of outer wall (as delimited by the POM) in 'surface'-'mixed layer' samples that is not seen in the settling flux/dead population. We considered whether this reflects potential gam-calcite, therefore we look forward to seeing your results published and will include a citation.

This could explain why sometimes the last chamber is warmer compared to the re-maining shell. However, if gam-calcite forms on the outer 'exposed' edges on the outer margin of the shell (and on the outer chambers) then the amount of GAM calcite would relate to the surface area exposed and how much GAM calcite may be added to the previous chambers (whole shell minus the last one) and the last chamber. Attached is a x-ray through a shell showing the complicated 'thickening'. We shall expand our discussion to incorporate this.

Page 11 – Lines 32-33: This section really highlights issues with the δ18O proxy, and suggests that the values do not reflect sea-surface conditions. I love the statistical methods used to reconstruct depth habitats, but the estimates seem too deep relative to culturing and plankton tow observations. For example, foram culturing scientists collect wild G. ruber and T. sacculifer from the upper 20 m of the water column so the probability of finding these species in the upper 50 m is not zero as your δ18O results

suggest. I recommend shifting the focus of the section away from where the forams are actually calcifying and focus instead on your δ18O results, why they deviate from field observations, and what this means for others who try to use δ 18O to infer ocean conditions.

Foraminiferal d18O does not reflect SST for two reasons, the first is that the vital ef-fect leads to an offset and thus we must either alter the equilibrium line of KO or the individual d18O of the foraminifera. Secondly reflects the depth habitat of foraminifera: The depth habitat of foraminifera is a continuous variable however the calcification depths ('apparent calcification depths) represent a series of discrete intervals within the total depth habitat of a single specimen. Foraminifera construct a shell in which along the whorl the chamber size is 'exponentially' increasing in size, so that the cu-mulative fraction of each chamber to the total shell increases iteratively. Therefore, whilst foraminifera can be caught (via the authors own experience and as stated by the reviewer) in tows or via divers in the upper water column the seafloor shells are them-selves skewed toward either a 'colder signal' or the signal with the greatest 'mass'. This mass balance approach can be seen, and is outlined, in Wilke et al., 2006. We com-pletely agree with the reviewer that foraminifera do not catagorically record sea-surface conditions because of this 'weighted averaging'/cumulative mass balance.

I agree with the reviewer that "the probability of finding these species in the upper 50 m is not zero as your δ18O results suggest" and will alter both the text and figure ac-cordingly, instead what these plots show is the depth of the apparent weighted average signal. One should interpret the zero probability above the interval with 'zero' probabil-ity as being part of the depth habitat, to make this more clear, we will mask the upper section to indicate that this should be seen as part of the depth habitat.

This brings us back to the reviewers comment regarding species-specific d18O equa-tions, the culture derived d18O-temperature approximations took pains to remove the field-grown portion of the shell (via dissection and subsequent pooling of culture-grown chambers), with respect to other field based methods (such as from tows or pump

samples) this mass balance might skew the results.

ref: Wilke, I., Bickert, T., and Peeters, F.J.C., 2006. The influence of seawater carbonate ion concentration [CO32−] on the stable carbon isotope composition of the planktic foraminifera species Globorotalia inflata, Marine Micropaleontology,

Page 19 (Fig. 2): I suggest adding labels in the figure to identify the s.l. and s.s. morphotypes of G. ruber (or add a note into the figure caption which numbers are s.l. and s.s.).

Will be added in a revised version of the text

Page 21 (Fig. 4B): I think it may help to color code the points based on the morphologies noted in figure 2. I like the inset images and I think you should keep them, but I was curious about the morphology of the shells with high whole shell area and high final chamber area that didn't have a corresponding inset photo (points in the upper right)

We will endeavor to color code the points based on the morphologies noted in figure 2, but this might be a subjective approach (i.e. dependent upon the interpreter), we will see if another morphological parameter (such as deviation from a circle) could be used instead. However, we shall definitely add the morphology of the upper right.

Technical Corrections Even though the mass spec produces $\delta 18O$ values to many decimal places, values beyond the tenth place are uncertain so d18O values should only be reported to the tenth decimal place (i.e., 0.1‰ precision).

Whilst, the reviewer is correct regarding isotope values there is also the problem of rounding error. We will add within the text a statement to that affect: "Isotope values are reported to 2 decimal places, however this should not be misconstrued as reflecting a degree of certainty but to prevent rounding error."

Page 1 - Lines 16-17: I suggest noting the direction of this difference, I.e. "We show that the d18O of the final chamber ($\delta 18OF$) is 0.2 ‰$\pm$0.4 ‰ (1σ) higher than the

d18O value of the test minus the final chamber ($\delta 18O<F$) of T. sacculifer"

We will modify the text accordingly

Page 1 - Line 17: Specify if sigma is standard deviation or standard error. Also, note how many shells you analyzed in the parentheses "(n=__)"

Sigma by itself is standard deviation, the symbol for standard error is sigma subscript SE or x-hat We will modify the text accordingly, by adding the n

Page 2 - Line 16: Remove the double parentheses

We will modify the text accordingly

Page 3 – Line 2: The word "do" is not necessary

We will modify the text accordingly

Page 4 - Line 10: Change "were" to "was" if only one test was performed (ANOVA with a post-hoc test)

A single test was performed, we will modify the text accordingly

Page 4 – Line 16: I stumbled over the statement "[...] our third objective Seasonality is a [...]"

We agree and have modified the text as follows:

"Having focused upon a single species for the first two objectives, our third objective focuses upon the variability of foraminifera isotope values, which are considered to represent seasonality, and whether fossil shells from different species have similar d18Oshell variability. "

Page 5 – Line 2: Condense the sentence to read "[...] same location, which would mean [...]"

We will modify the text accordingly

Page 8 - Line 16: I suggest separating these sentences to read something like "[. . .] Figure 6b). An ANOVA to test whether the species had equal means resulted in a p value of 0.0001 and led to a rejection [...]"

We will modify the text accordingly

Page 10 - Line 8: Change negative to positive (colder SSTs = more positive foram d18O values)

We will modify the text accordingly

Page 11 - Line 18: Add parentheses around the figure reference

We will modify the text accordingly

Page 10 - Line 28: Change the comma to a period

We will modify the text accordingly

Page 11 - Line 27-29: The verbiage is a bit awkward. I suggest dividing it into two sentences, I.e. "Wit et al. (2010) stated [...], which was inferred from measurements of single species (G. Ruber) at multiple core locations. Here we test [...]".

We agree, we will modify the text

Page 12 - Line 3: The verbiage is a bit awkward. I suggest, "First, we tested depth migration and found [..]". Page 12 - Line 7: Similar to line 3 I suggest, "Second, we tested covariance with size and found [..]". Page 12 – Line 9: If you use my suggestion for lines 3 and 7, this should be consistent, i.e. "Third, we tested [...]."

We will modify the text accordingly

Page 12 - Line 8-9: "found" is used twice in the sentence. I suggest deleting the second one, "[...] the three measured size classes."

We will modify the text accordingly

Page 12 – Line 10: Divide into two sentences. "[...] archives. Comparison between[...]"

We agree, we will modify the text

Page 12 – Line 21: Remove period after "BM"

We will modify the text accordingly

Page 17 – Line 31: Add superscript to 18 inδ18O

We will modify the text accordingly

Page 24 – Line 8: italicize the latin "in situ"

We will modify the text accordingly

Page 24 (Fig. 6 caption): Add in what the whiskers represent (e.g. 95% confidence interval) and if the horizontal lines within the boxes are the median or mean. The lines are typically medians, but the text compares means of the datasets so you may want to show both the mean and the median (perhaps one as a bold line and one as a dashed line?)

We will expand the caption to include a note regarding the various components, the central bar of the boxplots is the median, the top and bottom of the box the 25th and 75th percentile, the whiskers are 1.5*IQR +/- the 75th or 25th percentile. The 95 % CI on the median is not shown here, as that is a box and whisker with 'notch' plot.

Page 26, 27, 28 (Fig captions): Note what "p(δ18O)" is. I initially thought it was a p-value.

We will modify the text accordingly

Page 10 - Line 43: Use parentheses only around the year, i.e. "Berger et al. (1978b)"

Page 11 - Line 5: Use parentheses only around the year, i.e. "Brummer et al. (1987, 1986)"

Page 11 - Line 6: Use parentheses only around the year, i.e. "Peeters et al. (1999)"

Page 11 - Line 27-29: Use parentheses only around the year, i.e. "Wit et al. (2010)".
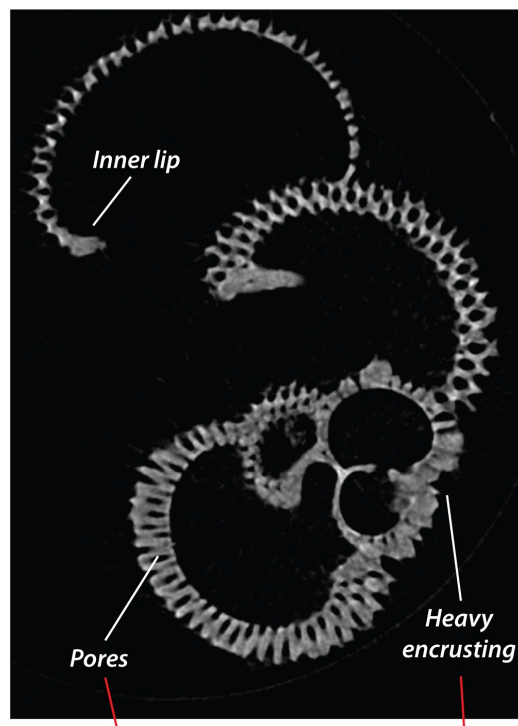
Page 14 – Line 12: Remove the extra ", "

Page 17 – Line 24: Remove the extra ", "

We will correct these Endnote 'cite as you write' mistakes and modify the text accordingly

Fig. 1. Cross section of a shell of T. sacculifer