

Interactive comment on “Factors controlling the depth habitat of planktonic foraminifera in the subtropical eastern North Atlantic” by Andreia Rebotim et al.

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Review of Rebotim et al. submitted manuscript to Biogeosciences entitled “Factors controlling the depth habitat of planktonic foraminifera in the subtropical eastern North Atlantic”.

General Comments:

This paper addresses a range of influential factors driving the abundances and distributions of a key organism group in an important, impactful, and influential region of the global ocean. The work compiles a new set of depth-stratified plankton tow samples from the water column, and explicitly tests how planktonic foraminiferal abun-

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dances vary as a function of key environmental variables affecting hydrography. Work of this kind is still very rare in the fields of biogeosciences, marine sciences, and paleoceanography, and as such I find it very welcome as a potentially key contribution to these and other fields. Furthermore, the nature of the effort seems ideally suited to a journal like Biogeosciences, in that it touches on a widely used group of zooplankton that form shells to become part of the fossil record for paleoceanographic study, but unfortunately with still too little known about their modern habitats, surface ocean ecology, complex, varied, and multiple influential factors affecting their distributions, etc. While the manuscript could have gone in a number of potential further directions, such as detailed implications on geochemistry of the foraminifera (e.g. more on $\delta^{18}\text{O}$ and other stable isotopic systems, trace element composition such as Mg/Ca, etc.), I actually appreciate how it more fundamentally adhered to a more simple property like average living depth (ALD), as even this alone can be suitably perplexing (as the authors ultimately demonstrate). There is also plenty of room for spinoff to other studies from the same sample set, hopefully involving the geochemistry and what I would refer to as “paleoceanographic implications”.

The authors ultimately differentiate 17 main species with ALD above and below 100m on average, as well as if or how other hydrographic features could be influential for predictive purposes, such as mixed layer depth, temperature, chlorophyll a concentration, etc. They also explored seasonal and lunar cycles. They explained the majority of variance in most species according to these issues, and how covariance of properties according to surface ocean dynamics could make influences become multiple rather than singular. The authors do a very commendable effort with this paper, and ultimately provide a nice, new contribution suitable for the journal after moderate revision in my opinion.

Specific Comments:

While ALD is certainly a sensible parameter to focus on, especially as this would then have a number of important knock-on effects and consequences, such as acquired

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geochemical proxy signatures that are ultimately recorded in the underlying sedimentary record, it is not the only thing. Overall, I would like to see the authors downplay ALD somewhat at the expense of overall surface ocean marine ecology. For example, life cycle characteristics of individual foraminifera species might play a significant role in explaining ALD, but instead ALD seems to be sort of prematurely accepted as the end-all parameter of importance and influence. I am not saying it is not important, as it clearly is. What I am saying is that I would like to see deeper evidence-based consideration and speculation on what actually drives ALD more fundamentally.

Technical Corrections (separated by page, as line numbers are renewed with each page):

Page 1

Line 15, insert “recorded” before “proxy signals”.

Line 16, I suggest “habitat and life cycle characteristics of individual species” instead, to emphasize further the ecological influences on ALD (specific comment above).

Line 28, should read “in at least one case”.

Introduction: Here is where more background can be inserted on the life cycle characteristics and how ecology can play a role. We published a nice paper, in my clearly biased point of view (!) on how planktonic foraminiferal life stage and life cycle differences can ultimately drive water column vs. core-top Mg/Ca variability for example. See Martinez-Boti et al., 2011. This paper affords some nice text on these issues.

Page 2

Line 24, spacing for “mostly by”.

Around Line 34, I think there should be an additional sentence of caution however, reminding the reader that *H. digitata* is only one species, and that many species will likely differ this way.

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Page 3

Line 3, delete the word “tidal”.

Line 15, I suggest quotes around snapshot as “snapshot”.

Page 4

Line 8, I think some clarification needs to be added about the “little or no vertical resolution” (suggested changed word ordering as well) comment. Is this really true? If so, why?

Page 5

Line 9, suggested wording “distribution of planktonic organisms including foraminifera”.

Line 16, I wonder if the McGregor et al, 2007 paper should also be cited here.

Line 34, change “cool stored” to “refrigerated” (presumably?).

Page 6

Line 2, if done by Rose-Bengal staining, then this should probably be indicated.

Line 12, I suggest corroborating this with a reference citation.

Line 16, delete “existent”.

Line 28, and thereabouts. Are there reference citations to corroborate this way of treating and calculating ALD?

Line 33 and thereabouts. Are there problems potentially with the GLM? What assumptions are built into it?

Page 7

Line 19, isn't this called Julian Day? If so, it should be indicated as such.

Line 27, 34 species strikes me as a lot. Could this be actually too many for significant

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relevance to the paleoceanographic record, for all practical purposes? Perhaps the authors should comment.

Page 8

Line 17, how is this evident in Fig. 6?

Page 9

Line 3, perhaps the significance of lognormal property should be embellished upon.

Page 10

Line 7, I suggest replacing “opposing to” with “in contrast with”.

Lines 25-28, good point about the differentiated ALDs. Perhaps you should also comment on how this might argue against significant advective transport or homogenization this way (akin to the work of van Sebille et al., 2015)?

Page 12

Lines 5-6, if it is a circulation and/or advection effect then maybe it should be explicitly described as such.

Line 33, how does this work if there is less mixing and nutrient entrainment to support a much higher standing stock?

Page 13

Line 22, associated “with” the thermocline, instead of “to”.

Page 15

Line 8, insert “in the Southern Ocean” after “for this species”.

Line 27, it is still unclear to me how the symbiont-barren conclusion is arrived at. Can you elaborate further or clarify better somehow?

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Page 16

Line 2, spelling of “considered”.

Line 27, insert “relatively” before “unaffected”.

Line 31, could there be some other ecological aspect considerations beyond a thermal and/or density niche?

Page 17

The 5 grouping concept should probably be better defended against some kind of advection or homogenization idea (ala van Sebille et al., 2015).

Page 18

Line 25, “Although the data are “certainly” not conclusive, . . .”

Page 20

Line 16, perhaps this point about ecological knowledge needs is relevant to more than just this small and obscure species?

Line 33 and just beyond, here should perhaps consider further life cycle issues from the text of Martinez Boti et al. (2011) paper.

Page 21

Line 24, can more be said on cryptic species and their potential for confounding much in the way of isotopic signature understanding? Can this go beyond just *G. siphonifera*?

Page 22

Line 9, replace “taken together” with “collectively”.

Line 18, replace “variability” with “range”.

General point. Perhaps it is better to format conclusions as numerical bullet points,

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after a short introductory paragraph. This might be more systematic and concise.

Table 4 caption, “stations” should be plural in the 2nd line.

Figure 1 caption. Perhaps the last sentence of the caption can be deleted; it is not really necessary.

Figure 2 caption. Same comment as for Figure 1 caption.

Figure 4. C and D with subscripts should be explained in caption text, as abundance and depth of each interval A, B, and C.

Figure 5. Is *N. incompta* usually this shallow in its abundance peak?

Figure 6. Are ML T and Chl. values averaged over the whole ML interval? This should be clearly spelled out. Also, is it clearly explained anywhere the basis by which the 3 regions were distinguished?

Figure 9. I have the same question here about T and Chl values, as for the Figure 6 caption.

Figure 10. I think panels C and D got switched around. If the caption is correct, then these should simply be switched back. Alternatively, re-order the caption text. Also, I think the groupings and numerical scheme in C needs explanation in the caption.

Figure 11. It might be interesting as a 2nd panel to this figure to have a cross-plot of CD vs. ALD, which would presumably show considerable scatter, which would be the point I think.

References:

Martínez Botí, M.À., P.G. Mortyn, D. Schmidt, D. Vance, and D.B. Field, Mg/Ca in foraminifera from plankton tows: evaluation of proxy controls and comparison with core-tops, *Earth and Planetary Science Letters*, 307, 113-125, 2011.

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van Sebille, E. P. Scussolini, J.V. Durgadoo, F.J.C. Peeters, A. Biastoch, W.Weijer, C.Turney, C.B. Paris, and R. Zahn, Ocean currents generate large footprints in marine palaeoclimate proxies, *Nature*, 6, doi: 10.1038/ncomms7521, 2015.

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