

Interactive comment on “Vertical distribution of planktonic foraminifera in the Subtropical South Atlantic: depth hierarchy of controlling factors” by Douglas Lessa et al.

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We appreciated the comments of Prof. Dr. Ralf Schiebel (Referee 2 – R2), which will make important improvements to the manuscript, especially with literature suggestions. Based on his comments, we have rechecked our environmental parameters obtained by CTD and we decided to change pH and dissolved oxygen data from CTD for total alkalinity from GLODAP and dissolved oxygen from World Ocean Atlas (WOA) 2013. We observed that total alkalinity and WOA dissolved oxygen was in part similar to variation to pH and dissolved oxygen from CTD, respectively, along the M124 transect. Thus, we believe that our discussion and conclusions will not suffer major changes.

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Regarding the taxonomy concerns, our specimens exhibited in the plates were checked by all our co-author team and everyone agreed with the current taxonomy. We have carefully analyzed and responded each comment. Referee 2's comments are stated as "R2C" and our responses are stated as "R".

R2C: Long passages of the text read nicely narrative, but not scientific (e.g., the second paragraph of section 2. Material and Methods). For example, mention that a device needs to be switched on is trivial and may want to be skipped in a scientific paper. The narrative writing may result from the fact that, for example, the first two paragraphs of the chapter 2. Material and Methods read very much like the Meteor M124 cruise report (Karstensen et al. 2016). Considering this, Lessa and coauthors may want to be careful to avoid unintended plagiarism. R: We will work on the second paragraphs of Material and Methods rephrasing parts that can be narrative or similar to M124 cruise report. R2C: In general, the paper needs some reorganizing. The Results and Discussion chapters include information from the other chapters. For example: Page 5, lines 18-27: In this section, methodology, results, and discussion are mixed, and should be disentangled. R: The highlighted text interval will have some changes in order to disentangle sections. R2C: Page 5, last paragraph: results and interpretation are mixed up: please disentangle. R: This paragraph will be rephrased for better clarification. R2C: Page 7, in lines 34-38, and lines 48-49, the "vertical variation of the community" is discussed in the Results chapter. R: This part will be moved and incorporated to the penultimate paragraph of the section 5.3. R2C: Page 8, lines 14-15, results on ontogenetic effects are presented in the Discussion chapter. R: This part will be rewritten in order to remove the "result" apparent reading. R2C: Page 10, lines 5-6, is Methods, not Discussion; here the results should be discussed R: We will rephrase this text segment so that the discussion aspect is maintained. R2C: I don't understand page 10, lines 18-19; please rephrase. R: This segment will be rephrased for better clarification. R2C: From page 10, line 19, the discussion reads rambling and not to the point. Lines 24-26: The observation of Fehrenbacher et al has been on *N. dutertrei*, not *Neogloboquadrina* in general; please be specific. R: "*Neogloboquadrina*"

at line 25 will be replaced by *N. dutertrei*. R2C: Line 27, degraded organic matter; ref. to Schiebel and Hemleben (2017). R: The citation will be inserted at line 27 R2C: Lines 32-34: syntax?! R: This part will be rephrased for better clarification. R2C: Line 38: please change “hidden in” to “from”. R: The requested change will be done R2C: Lines 39-41: any proof? Please refer to data or figures or literature references! R: We will insert references to figures 2 and 7, and we will change “. . . along the transect . . .” for “. . . observed between eastern and western thermocline faunas . . .” at line 40 for better clarification.. R2C: From Figure 2, I have the impression that some of the environmental data are wrong. This might result from the fact that “raw data” are presented instead of “final data”, i.e. calibrated data. In a publication (not preliminary report), calibrated values should be presented, which have undergone quality control. In particular, the high pH values (near 8.8) are possibly not realistic in open marine waters, and the data should not be used. I would guess that the pH probe was broken or not correctly calibrated. In addition, DO values are very high, and may be revisited / calibrated. Having said that, I would suggest to revisit all data to guarantee correct values. R: We thank to Referee for spotting this issue with pH and dissolved oxygen from CTD. Considering this, we rechecked CTD data and we observed that there was a problem with pH and dissolved oxygen data. The others parameters are ok and available on Pangaea (<https://doi.pangaea.de/10.1594/PANGAEA.895426>). Based on this, we decided to use total alkalinity data from GLODAP and March’s dissolved oxygen data from World Ocean Atlas (WOA) 2013. We compared the variation between our probe and GLODAP/WOA databank and we observed that variation was in part similar along the transect. Because of this, we believe that our discussion and conclusions will not have major changes.

R2C: The use of the term “permanent thermocline” (e.g., page 4, line 36), given in the manuscript, is wrong. Actually, multiple seasonal thermoclines are observed (Figure S1), out of which even the deepest seasonal thermoclines are not the permanent thermocline. In some profiles, even deeper seasonal thermoclines can be seen (e.g., Profile 370 near 200 m). The permanent thermocline is much deeper at possibly all of the

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case shown in Figure S1. Unfortunately, there is not much literature available on this topic for the very stations discussed here (for a start, Chiessi 2008, and Gordon 1981 may be consulted). R: Based on Gordon (1981), our 700 m profile reached near to the bottom of the permanent thermocline. Thus, we will keep the layer below the seasonal thermocline as permanent thermocline. However, Referee 1 stated issues with thermocline entries (seasonal or permanent), so we will check all thermocline entries and we will specify them as “seasonal” or “permanent” thermocline. We will also highlight that the “thermocline group” refers to “communities below the seasonal thermocline” for better clarification. R2C: Classification: Given the rough surface texture, closed umbilicus, and shape and number of chambers in the final whorl (6) of the specimens depicted in Plate 3 images 1-3, this is possibly *T. humilis*, and certainly not *N. dutertrei*. R: The specimens displayed in plates were carefully checked by our co-author team and everyone agree that this specimen is *N. dutertrei*. The shape of tests 1 to 3 on plate 3 is quite normal for small *N. dutertrei* specimen. There are no spines, the surface is ridge-shaped and *N. dutertrei* can have six or more chambers at the last whorl, and a small “tooth” is seen inside the penultimate chamber’s aperture. We did not understand why this specimen cannot be classified as *N. dutertrei*. R2C: I do also have a different idea about *T. iota*, shown in Plate 5, images 9-10 R: Specimens 9 and 10 from the plate were also carefully checked by our co-authors team. The specimen 9 had all morphologic requisites to be classified as *T. iota* with four chambers at the last whorl, microperforate surface, and plenty of spiked pustules in the umbilical side. We are confident about the quality of our taxonomy.. R2C: and the rather unusual distribution pattern of *T. iota* (page 8, lines 47-49, “...the shallow habitat of *T. iota* is at odds with its concentration maximum around 300 m in the NE Atlantic reported by Rebotim et al. (2017). Clearly, the ecology of this species requires further investigation”) may result from misidentification. R: The ecology of *T. iota* is little known, so it is expected that the depth habitat can vary depending to oceanic realm and the season. For example, *G. crassaformis*, was observed with very shallow distribution in the Eastern Azores current (Rebotim et al, 2017) whereas it distributed below 100 m in other parts

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of the tropical Atlantic (this study, Jentzen et al, 2018, Meilland et al, 2019). Thus, it is more probable that the environmental conditions of the South Atlantic Subtropical Gyre during the late summer controlled the distribution of *T. iota* instead of taxonomic misidentification. R2C: Another misunderstanding concerns the classification of adult versus pre-adult individuals (lines 31-33): "...were classified as "pre-adult" when their identification was performed at a magnification higher than 100x and surface features typically found in adults (e.g., spines, pustules, large pores) were lacking." This is not a valid method. To distinguish adult from pre-adult individuals, GAM calcification should be looked at to get an idea about the average size of adult vs. pre-adult individuals. If this is not possible, the terms small (i.e., smaller than ...) and large (i.e., larger than...) tests may be used. R: We thank to Referee 2 for his worry about adult and pre-adult terms. Based on it, we will carry out a rephrasing of this part in order to clarify the used method for adult and pre-adult specimens separation. The classification of adults and pre-adult specimens was based on morphological aspects described by Brummer et al (1986). The "pre-adult" term was used to separating specimens in juvenile and neanic stages from adults, since these two initial stages shared morphological similarities that could not be correctly separated on M124 collection. GAM calcification is a feature observed in specimens that reached the terminal adult ontogenetic stage. This feature is difficultly recognizable on light stereomicroscope and it may not correctly separate immature from mature adults. GAM calcification is also difficult to recognize in Brummer et al (1986) images of adult specimens. R2C: The use of statistics in this paper is the wrong way round, or presented in the wrong way. In general, statistics may be used to confirm and explain observations, and may not be an end in itself in paleoceanography (in mathematics, this may be the other way round). R: The statistical analyses used in our study had the purpose to give a numerical support to our observations and providing valuable information about our described faunas and their relationship with environmental processes that occur in the upper ocean. Indeed we have the purpose that our findings may also be applied on paleoceanography. Based on this, we believe that providing statistics between our faunas and environmental parameters is an effective

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tive way to characterize the relationships between the species relative abundances and environmental variables.. R2C: In general, referencing in the manuscript is selective, and much important information has not been included in the paper. This is particularly inadequate, because little has so far been known on the planktic foraminifers from the region sampled here, and the results would need to be discussed in comparison to existing studies in a similar setting, as, for example, the northern limit of the North Atlantic subtropical gyre. Referring only to Rebotim et al. (2017) is not sufficient. Most importantly, the paper of Kemle-von- Mücke and Hemleben (1999), in “South Atlantic Zooplankton” needs to be discussed. R: We thank Referee 2 for his references suggestion, which will give a high improvement to the manuscript. Indeed searching for references was a tricky part of the manuscript’s development, since there is very little information for the tropical and subtropical South Atlantic. We agree with the close-up to North Atlantic Subtropical Gyre to compare our results, which will be incorporated in the main text on Discussion section. R2C: Page 2, line 2: Temperature is possibly an indicator, not “control”; see, e.g., Jentzen et al. 2018 R: We will change the segment “. . .dominant temperature control on. . .” for “. . .high relationship of the temperature with. . .” and citing Jentzen et al (2018) R2C: Page 2, lines 8-9: please see also Schiebel et al. 2001 (among others) R: The citation will be inserted. R2C: Page 2, line 18: please see also Schiebel 2002 (among many others) R: The reference will be inserted together with Bé, 1960. R2C: Page 8, line 12: please refer to Bijma et al 1990 R: The reference will be inserted prior to figure S3 call. R2C: Page 8, line 37: “in many other studies/regions”: please be specific; which studies/regions? Refer to the papers of Bé, Bijma, Jentzen, Salmon, Schmuker, Schiebel, etc R: The sentence will be rephrased to better clarification. R2C: Page 8, line 39: why only “thermally constrained”; please discussion with reference to the existing literature (e.g., Jentzen et al. 2018, etc) R: The sentence will be rephrased to better clarification R2C: Page 9, line 6: what is meant by majority? Please be specific, and discuss the different species. R: We revised that sentence and we concluded that it is irrelevant and it will be deleted. The cited reference will be placed at the end of the previous sentence. R2C: Page 9, lines

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41-42: “G. truncatulinoides replaces G. scitula towards. . .”; please compare to the distribution of G. truncatulinoides and G. scitula in the Azores Front Current System, which is a similar hydrological and ecological setting as studied here. R: A comparison with the Azores System will be inserted in this paragraph. R2C: Page 9, line 54: “. . .the result of seasonal superposition of. . .”; please discuss in comparison to earlier papers. You may start from Schiebel and Hemleben, 2000, and Schiebel et al. 2001 R: This part will be rewritten comparing the requested references. R2C: Finally, the chapter 6. Conclusions may be rewritten following the changes in the manuscript. R: Based on our response given about CTD quality query, our conclusions may not have major changes since the discussed processes linked to pH also have effects over the dissolved oxygen. R2C: Some details: Title: I wonder why a rather self-limiting title has been chosen for the much broader topic presented in the paper. I would suggest to skip “Vertical” and make the title “Distribution. . .”. R: The change will be implemented. R2C: Page 2, line 39: not “cod-end” (which are soft) have been used, but “sampling cups” R: The issue will be corrected at this point and elsewhere R2C: Page 2, line 51: I wonder how the nets were changed “manually” at grate depth: I guess that the right expression is “changed by remote control” R: “Manually will be changed by “offline” R2C: Page 3, line 3: pH, not PH Page 3, line 9: skip “planktonic” R: This issue was also signaled by Referee 1 and it will be corrected R2C: Page 3, line 39: change “concentrations” to “standing stocks” R: In this segment, we refer to the general concentration calculation, which includes both living and dead shells. Since “standing stock” is only referred to the living shells, we decided to maintain the word “concentration”. R2C: Page 4, line 5: change “trace” to “confirm” R: The change will be done R2C: Page 4, lines 42-44: change “first” to “upper” R: The changes will be done R2C: Page 5, lines 18-19: unfinished relative clause: higher than what? R: The sentence will be rephrased for a superlative clause R2C: Page 5, lines 44-45: unfinished relative clause: higher than what? R: The sentence will be rephrased for a superlative clause R2C: Page 6, line 44: change “revealed” to “confirmed” R: The change will be done R2C: Page 7, line 15: (refs Moery etc) is not the correct way of referencing R: This issue was also signaled

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by Referee 1 and it will be corrected R2C: Page 8, lines 29-30: better change “this reference” to “this depth level” R: The change will be done R2C: Page 11, line 9: How should pH affect species distribution? Any data that may support the statement? R: The sentence will be rephrased to better clarification R2C: Page 13, lines 17-18: not eds. but authors R: The correction will be done R2C: Figure 4: The upper 260 m max are displayed, not 700 m as stated in the caption. R: “700” will be changed by “550” in order to include the second panel of the figure 4. R2C: Figure 9: What shall “light” to “dark” mean in this context? The different parameters from Chl-a to O₂ may not be easily put into relation. R: The figure 9 will have modifications since pH and dissolved oxygen data will be changed by GLODAP and WOA 2013 March data. R2C: Plates: I congratulate the authors on the quality of the light micrographs. However, using a ring light produces light rings on the reflecting surface of chambers. The authors may want to play with more diffused light to produce even better images in the future. R: We thank Referee 2 for your comment about our light micrographs. We acquired this microscope little time prior to the manuscript production. However, many light combinations were tried, but specimens with flat and smooth tests produced reflected light on our photos. This light combination produced the smallest impacts. We keep working on light combinations in order to remove the reflectance effect over smooth tests. On the other hand, this reflected light may give to readers, the idea that these tests have a glass-like appearance on light stereomicroscope. R2C: Plate 2: change second (12) to (13) R: The correction will be carried out R2C: Appendix A: The species descriptions read good in general, but some typos (e.g., page 25, line 6, change “trocospiral” to “trochospiral”); R: The text will be checked for issues typos R2C: page 29, line 39, change pakerae to parkerae; etc etc) may be corrected. R: The correction will be carried out R2C: I have no clue what is meant by granules (in *T. iota*, and *T. fleisheri*), and pustules may be meant. R: We will change “granules” for “pustules” along the appendix.

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