Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-355-RC2, 2019

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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Received and published: 13 November 2019

The paper of Lessa and coauthors on the "Vertical distribution of planktonic foraminifera in the Subtropical South Atlantic: depth hierarchy of controlling factors" examines the so far neglected ecology of planktic foraminifers of the southern Atlantic Ocean, and adds important data to the understanding of planktic foraminifers in general. Therefore, the papers merits publication. However, the manuscript needs substantial improvement of data base, syntax, organization, and chain of argumentation (incl. use of references) before publication. I would suggest rejection of the current manuscript and resubmission of an improved paper.

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.

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. Page 5, last paragraph: results and interpretation are mixed up: please disentangle. Page 7, in lines 34-38, and lines 48-49, the "vertical variation of the community“ is discussed in the Results chapter. Page 8, lines 14-15, results on ontogenetic effects are presented in the Discussion chapter. Page 10, lines 5-6, is Methods, not Discussion; here the results should be discussed I don’t understand page 10, lines 18-19; please rephrase. 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. Line 27, degraded organic matter; ref. to Schiebel and Hemleben (2017). Lines 32-34: syntax?! Line 38: please change “hidden in” to “from”. Lines 39-41: any proof? Please refer to data or figures or literature references!

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.

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 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).

Classification: Given the rough surface texture, closed umbilicius, 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. I do also have a different idea about T. iota, shown in Plate 5, images 9-10, 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.

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.

The use of statistics is 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).

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. Page 2, line 2: Temperature is possibly an indicator, not "control"; see, e.g., Jentzen et al. 2018 Page 2, lines 8-9: please see also Schiebel et al. 2001 (among others) Page 2, line 18: please see also Schiebel 2002 (among many others) Page 8, line 12: please refer to Bijma et al 1990 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 Page 8, line 39: why only "thermally constrained"; please discussion with reference to the existing literature (e.g., Jentzen et al. 2018, etc) Page 9, line 6: what is meant by majority? Please be specific, and discuss the different species. Page 9, lines 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. 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

Finally, the chapter 6. Conclusions may be rewritten follwing the changes in the manuscript.

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...". Page 2, line 39: not "cod-end" (which are soft) have been used, but “sampling cups” 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” Page 3, line 3: pH, not PH Page 3, line 9: skip “planktonic” Page 3, line 39: change “concentrations” to “standing stocks“ Page 4, line 5: change “trace” to “confirm” Page 4, lines 42-44: change “first” to “upper” Page 5, lines 18-19: unfinished relative clause: higher than what? Page 5, lines 44-45: unfinished relative clause: higher than what? Page 6, line 44: change “revealed” to “confirmed” Page 7, line 15: (refs Moery etc) is not the correct way of referencing Page 8, lines 29-30: better change “this reference” to “this depth level” Page 11, line 9: How should pH affect species distribution? Any data that may support the statement? Page 13, lines 17-18: not eds. but authors Figure 4: The upper 260 m max are displayed, not 700 m as stated in the caption. 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. 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. Plate 2: change second (12) to (13) Appendix A: The species descriptions read good in general, but some typos (e.g., page 25, line 6, change “trocospiral” to “trochospiral”; page 29, line 39, change pakerae to parkerae; etc etc) may be corrected. I have no clue what is meant by granules (in T. iota, and T. fleisheri), and pustules may be meant.

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