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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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The manuscript is a synthesis of planktonic foraminiferal abundance data from vertically resolved plankton net hauls taken in the eastern North Atlantic during twelve oceanographic campaigns between 1995 and 2012. This is a very valuable study and perfectly suited for Biogeosciences. The data are very basic and exactly the kind of information urgently needed for improved paleoceanographic and -climatic interpretation. Although I do appreciate the amount of work that went into this synthesis, I think that more can and should be done with the data. The authors were more focused on statistical analyses of absolute numbers but could have done more with the data itself, e.g. size fraction analyses (I hope there will be a follow up). A few issues, however,

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should be addressed in this manuscript.

General comments

1) As the authors note themselves: ". . .. .The main uncertainty derives from the identification of living cells by the presence of cytoplasm. This causes a bias towards greater ALD, because dead cells with cytoplasm sinking down the water column still appear as living and their occurrence will shift ALD to greater depth." In my view, this is the major problem of the data and therefore for the definition of Average "Living" Depth (ALD).

I suggest that the authors add the dead ind./m3 to the stations figures in the supplementary material.

Planktonic Foraminifera produce hundreds of thousands of gametes. Only a small fraction will form zygotes. Of that fraction, only a small fraction will grow into juveniles, etc. In other words mortality is huge, especially in the smaller size classes. As gametogenesis also terminates the life of the parent, "mortality" consists of two groups. Those that die young are usually thin shelled, small and contain cytoplasm while mature specimens that reproduce, leave behind thick walled and empty shells (and are part of the larger size fractions).

The thin shelled juveniles and small adults that die are initially still filled with cytoplasm and will be more or less neutrally buoyant, i.e. they will stay much longer in the water column than those specimens that have released gametes and whose shells are empty and quickly settle to the ocean floor. Hence, the real living population is outnumbered by a "standing stock" of small dead ones.

Apparently, the authors missed the paper by Bijma and Hemleben (1994) who studied the population dynamics of G. sacculifer (now T. sacculifer). One of the things they did was to separate G. sacculifer into two size fractions ("mature" vs. "immature") in order to differentiate between the "productive zone" (related to the ALD) and the flux zone

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separated by the reproduction depth.

As a lot of information is available for G. sacculifer, I would strongly encourage the authors to add a paragraph to the present manuscript where they go over the G. sacculifer data again and size separate at 366μm (real size) which, for G. sacculifer, is equivalent to a sieve size 250μm (Bijma and Hemleben, 1994). Subsequently, they could also use "residuals" which would take care of the fact that numerically the smaller fractions outnumber the larger ones.

2) It is not clear if the authors counted only adult O. universa (spherical stage) or also the pre-adult spiral stages. If they counted both, I suggest that the authors use the relative frequency (or residual) of the spherical chambers versus time and depth to indicate reproduction depth and timing.

3) The authors discuss G. siphonifera. Is this type I or II senso Huber et al. (1997) and Bijma et al. (1998)? Please clarify.

4) The authors should closely read the manuscript again. There are a few typos and mistakes. I'm just listing a few examples:

P1; Line 30-31: ". . . . . .populations of Trilobatus sacculifer appears to descend in the water column towards the new moon." Should be appear not appears

P18; Line 15: ". . ...the number of observations form summer to fall is low. . ...". Form should be from.

P22; Line 3: ". . .calcification depth in some of the species also highlight to need to better understand the. . .." Should be "also highlight the need to"

specific comments

P1; Line 30-31: ". . . . . .populations of Trilobatus sacculifer appears to descend in the water column towards the new moon." It is interesting that the authors mention new moon. Bijma and Hemleben (1994) found that the highest frequency of sac-like cham-

ber formation coincided with new moon but that reproduction took place around full moon. This maybe a point to discuss. At the end, T. sacculifer is the only species that has only one genotype and reproduces at full moon (Bijma and Hemleben, 1994).

P2; Line 17-18: I suggest to include Bijma, J. and Hemleben, C. (1994) Population dynamics of the planktic foraminifer Globigerinoides sacculifer (Brady) from the central red sea. Deep-sea research part I: oceanographic research papers 41, 485-510.

P3; Line 32-34: "This phenomenon is known from geochemical studies, indicating large shifts in calcification depth across oceanic fronts or among regions, in absolute terms or relative to other species (Chiessi et al., 2007; Farmer et al., 2007; Mulitza et al., 1997; Simstich et al., 2003)." It should be noted that these studies usually estimate calcification depth from equilibrium calcification and do not consider vital effects, gametogenetic calcification and crusting. Consequently, the real calcification depth can be offset from the equilibrium calcification depth.

P15; Line 5:". . .. . . appears deeper compared to the results by Bé and Hamlin (1967) in the same area,. . .". The authors should realise that Bé and Hamlin only used 0-10m and 0-300m vertical hauls.

P16; Line 31:". . .. . . the depth habitat of such species reflects a (passive) tracking of a prefered thermal and/or density niche.". What is meant by "(passive) tracking"? How does that work?

P17; Line 20-21:". . .. . ., we observe that most values of σALD are skewed towards the lower edge. This could be an indication that density is more important than temperature in determining the depth habitat of planktonic foraminifera." This is not clear to me. Please rephrase in the text to make it understandable.

P18; Line 22-23:" In the studied area the export flux and therefore reproduction of G. truncatulinoides and G. scitula occurs in a short period in winter and spring (Storz et al., 2009).". Together with your data, this suggests an annual cycle. A few lines further

you argue that G. truncatulinoides may have a lunar cycle. Since you only have the 5th, 10th and 24th day of the lunar cycle, I assume that the sample on 24th day of the lunar cycle is the same as the spring sample. I would use this argument and the above findings to suggest that G. truncatulinoides has no lunar cycle!

P19; Line 2-3:". . . . .with greatest ALD towards the end of the synodic cycle is consistent with its hypothesized reproductive behaviour (Erez et al., 1991)." Bijma and Hemleben, 1994 clearly demonstrate, using a much stronger data base, that the reproduction depth of T. sacculifer is between 60-80m.

P19; Line 9-10:"The relationship of its ALD to the lunar cycle is thus likely an artefact due to interdependencies among the tested variables in the available dataset." Yes, see my previous comment (P18).

P20; Line 20:" These observations are consistent with opportunistic behavior and lack of symbionts in this species,. . .". Why is this consistent with opportunistic behavior? Not clear to me.

P21; Line 16-17:" This appears puzzling and must reflect differences in the water column structure such as a thinner mixed layer depth in the Sargasso Sea." Don't forget that your ALD depth estimate is too deep as it is biased by a flux of dead specimens that you count as alive because they still contain cytoplasm.

P21; Line 24-25:". . .. cryptic species (Weiner et al., 2014), such as in G. siphonifera, which is characterised by two different genotypes that appear to be associated with different isotopic signatures (Bijma et al., 1998)." You could add that the symbiont of the, presumably, deeper living type II have higher concentrations of light harvesting pigments (same authors).

P22; Line 2-4: ". . .... , but the discrepancies between habitat and calcification depth in some of the species also highlight to need to better understand the causes and effects of secondary calcification.". There is some information available that you could add

as examples. For instance, Nürnberg et al. (1996) demonstrated that the Mg/Ca ratio of gametogenetic calcite is systematically higher than the Mg/Ca ratio of ontogenetic calcite grown at the same temperature. This translates into a 6°C warmer “geochemical temperature” for "GAM" calcite of G. sacculifer. Hamilton et al. (2008) comment that: “..... it should be noted that although the carbon and oxygen stable isotopic composition of ontogenetic and gam calcite are indistinguishable when secreted under identical conditions, that the Mg/Ca ratios are significantly different (Eggins et al., 2004; Nürnberg et al., 1996).” These examples show that the calcification depth cannot be simply calculated from the “geochemical temperature signature” but requires detailed knowledge of the primary and secondary calcification mechanisms.

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