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To Prof. Markus Kienast
Biogeosciences Editor

Ref: bg-2016-266

Low planktic foraminiferal diversity and abundance observed in a spring 2013 West-East Mediterranean Sea plankton tow transect, Miguel Mallo et al.

March 7, 2017

Dear Editor,

In the following, please find our reply to the reviewer's comments. We have addressed all of R1's comments, and below explain why we cannot sufficiently account for reproduction and stratification.

The line numbers refer to the revised manuscript attached.

Referee 1:

We have included all the reviewer 1 comments. However, we cannot do any statistical analyses on the reproduction cycle, as explained below, and we have included a sentence in the Discussion section to clarify this point. The same is true for the analysis of water column stratification.

Reproduction: Concerning the effect of reproduction and ontogeny on test calcite mass, we cannot produce a statistically significant correlation. First, synodic lunar reproduction differs between species, and we do not have a sufficiently large number of individuals per species, which would allow statistically viable numbers. Second, although reproduction occurs at a monthly rate, it can be delayed for a month, or several months. This is particularly true under environmental conditions that do not favour reproduction as, for example, scarcity of the right kind of food. Under such conditions, tests may grow larger and heavier than under optimum conditions (see Mojtahid et al. 2015). This is a problem particularly in the oligotrophic eastern Mediterranean. As much as we would like to offer an explanation that includes reproduction cycles, we cannot possibly do so in a scientifically rigorous manner. Finally, reproduction is just another measure that may or may not affect calcite mass, among other effects we do not discuss because of the inherent scarcity of planktic foraminifers in some regions of the Mediterranean Sea. This should no longer impede the publication of data on planktic foraminifers, and information on modern planktic foraminifers for the Mediterranean is crucially needed.

We made modifications in the “4.2. Area density” 1st paragraph (lines 344-346): “Similar allometric developments can be seen in *G. ruber* (white), *G. bulloides*, and *O. universa* with that correlation, graphically represented by the shape of a power function (Fig. S2). The allometric developments of species result from increasing size of tests when adding chambers during the successive ontogenetic stages from juvenile to adult: planktic foraminifera grow “faster” when they are younger and smaller (steepest in the lower left part of the regression line) and “slower” when they are older and bigger (less steep in the upper right part of the regression line; Fig. S2).”

We have added a sentence on reproduction and ontogenetic development (growth) in lines 627-630: “Unfortunately, we cannot address the effects of reproduction (e.g. Bijma et al., 1994), and ontogenetic development on the distribution patterns and test calcite mass of species, because a lack data at the species level do not allow any such statistics.”

Stratification: Temperature, which is accounted for in the PCA, is an expression of thermal stratification, admittedly not to 100 %, but good enough to produce interpretable results in our data set.

The new scatter plots are added in the supplementary material. PCA scores vs. abundance (Fig. S5), PCA scores vs. area density (Fig. S6).

We added the following sentence on the PCA results at the beginning of the discussion chapter 5.2.: “Abundance patterns of the five most frequent species in our samples possibly result from a combination of environmental conditions as, for example, food and temperature (Fig. 3).”

We agree with referee’s comment about “Changing ecology (lines 446-447)” and we modified the sentence in the revised manuscript as follows: “These changing conditions could also imply changes in environmental conditions and distribution of planktic foraminifera,…”

Referee 2:

We appreciate the effort that reviewer no. 2, Manuel Weinkauf, has put into reviewing our manuscript. We appreciate some of his technical comments (typos etc.), which we have fixed in the new version of the manuscript.

However, we are disappointed that Weinkauf tries to impose his ideas upon our work. We can of course not give a full description of the planktic foraminifers of the Mediterranean, and have just sampled populations in a sound scientific manner. We have then interpreted our data in relation to the data earlier presented in other papers, despite the differences in data type, collection procedure, sample processing, etc. (Weinkauf’s 4th paragraph).

We do not want to apply Weinkauf’s 5th paragraph. In this manuscript, we apply standard methods, including standard mathematics. The first author, Miguel Mallo, is an MSc student, who is perhaps not expected to develop new methods. Consequently, our manuscript is an honest description and basic interpretation of some new and really important data (there are not many) of upper water column planktic foraminifers from the Mediterranean. We perceived some misconceptions from Weinkauf, for example, foraminifers grow faster as juveniles than adults (e.g., Brummer 1988; please see for reviews Hemleben et al. 1989, and Schiebel and Hemleben 2017); in his comment on the ‘Lines 313-315’, Weinkauf sadly disqualifies (“This is nonsense...”). This is neither scientifically correct nor does it reflect

appropriate conduct in the reviewing process. We have nonetheless added a sentence to unequivocally clarify our point, and to avoid misinterpretation. Having said all this, we believe that the reviewer is somewhat overambitious. However, having worked in the same field for more than 30 years, it is not a pleasure to read inappropriate and incorrect statements written in your own field of expertise.

We are well-aware of the work of Andreia Rebotim referred to by Weinkauff's new review (2nd paragraph in Weinkauff's 'General comments'), as one of us (Schiebel) is co-author of the paper, which was published in late February 2017, which is from a different region (N Atlantic), and which has its own problems. In some minor points, Weinkauff might be right; in others, he is not. For example, the depth habitat varies according to hydrography, and *T. sacculifer* and *G. ruber* dwell at different depths depending on the availability of food and light (e.g., Schiebel and Hemleben, 2017, Springer, and references therein).

In the end, the manuscript is our work and our responsibility. We feel that the reviewer is beyond the limits of good scientific practice by imposing his methods to other people's work. We do feel we have addressed all the relevant points in an honest and professional manner. We are looking forward to the editor's decision regarding this manuscript.

Minor comments:

Ref.1: The minor corrections have been addressed (lines 487, L496, L506, L511, L541-542, and L571-572 in the previous manuscript; see also new manuscript with track changes).

Ref.2: The minor corrections have been addressed (L40, L41f, L50f, L105f, L202, L434, L449, L453, L487, L506 in the previous manuscript; see also new manuscript with track changes).

To avoid any confusion, we changed the sentence from L166f to "Twenty samples were collected down to 200 m water depth with BONGO nets (Table 1)."

L188: the 1/6 aliquot is possible as we used a wet sample divider.

L288: we thank the referee for noticing this error. We change this to "where *pH* is lower".

Table 2 was corrected with the variables in italics, and the area density consistently written.

Figure 5 is corrected; we meant $n=16$.

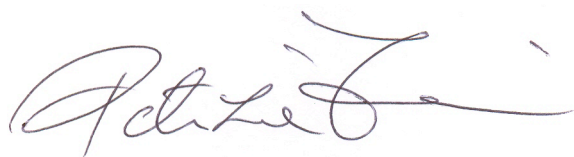
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