

## ***Interactive comment on “Low planktic foraminiferal diversity and abundance observed in a 2013 West-East Mediterranean Sea transect” by Miguel Mallo et al.***

**Anonymous Referee #3**

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Dear Biogeosciences Editorial Board

I hereby you receive my report on the MS " Low planktic foraminiferal diversity and abundance observed in a 2013 West-East Mediterranean Sea transect" by Mallo et al.

The authors provided new information on planktonic foraminiferal abundance from the upper part of the water column (200 m) in the Mediterranean Sea during May (spring) 2013 collected with BONGO nets (mesh size 150 micron and 40 cm of diameter). The authors documented a strong difference between western and eastern Mediterranean basins, and between different Mediterranean sub-basins, in terms of abundance and diversity in planktonic foraminiferal assemblage. They document 10 species and they proposed a study on the size-normalised weight (SNW) of two species (Globigerinoides

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ruber s.s. and Globigerina bulloides) and their relation with change with food availability.

The manuscript is properly constructed and it is evident that the data support the interpretation proposed in the manuscript.

I think that the authors need to stress some issues: i) the statistical analysis (in my opinion the Principal Component Analysis is the appropriate approach) carried out of the planktonic foraminiferal data [maybe including data of other authors (ie., Pujol & Vergraud-Grazzini 1995; De Castro Coppa et al 1980) to produce a complete framework of the Mediterranean]; ii) the correlation with sediment trap data (Barcena et al. 2004, Alboran Sea; Rigual-Hernández et al 2012, Gulf of Lion); iii) the comparison with data from Gulf of Naples (De Castro Coppa et al 1980), iv) the Oceanographic setting chapter (in my opinion some planktonic foraminiferal difference between different Mediterranean sub-basins could be linked to different oceanographic settings) also adding more references; v) detailed comparison between data related to the spring season (this work) with past spring seasons documented by planktonic foraminifera in the Mediterranean (living and sediment traps data); vi) the authors need to improve the figures and maybe add new ones; vii) it could be interesting to propose contouring map of the planktonic foraminiferal species viii) add a small chapter (maybe in the material and methods) concerning the criteria used to classify the planktonic foraminifera ix) I would like to suggest to add in the title of the manuscript the word SPRING.

I think that it is very important to publish these data, because of the interpretation of marine fossil archives of the Mediterranean are basically based on data (interpretation) provided by Hemleben et al., (1989) and by Pujol & Vergraud-Grazzini (1995), and it results important to improve the information on living planktonic foraminifera to better reconstruct the past climate oscillation recorded in the fossil archives. Anyway, in my opinion, the present version of the manuscript needs still important modifications concerning the presentation of data (including comparison with literature data) and discussion.

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Minor comments: Line 34: Hemleben et al. 1989 Line 36-38: please add Reference Line 47-49: please add the write reference for the Swedish Deep-Sea expedition 1947-1948 Line 61: the reference is Pujol & Vergraud-Grazzini (1995). Please modify in the entire manuscript Line 79: please modify the reference in De Castro Coppa et al., (1980) Line 87: it is necessary to compare the acquire data also with sediment trap data of Barcena et al. (2004) from Alboran Sea and of Rigual-Hernandez et al. (2012) from Gulf of Lion Line 98: SNW; please modified in Size-Normalized Weight (SNW) Line: please add Fig. 3 in the text Line 170: *Globigerinoides ruber sensu strictu* (ss) is correctly referable to *G. ruber* white variety. Please change the name in the manuscript. Anyway, I think that the authors due to the target of the manuscript have to add a small chapter where they report exactly the criteria followed to discriminate the different planktonic foraminiferal species as well as the species included in other. Line 175-176: the data clearly document higher percentages of individuals >500 micron between Sicily channel and Ionian Sea. It is important to be more precise about the geographic position of these abundances because of changes in abundance and size could be associated to change in oceanographic setting between the different parts of the Mediterranean. Line 180-181: the authors report that the *G. ruber* s.s abundance is low in the southern Mediterranean (station 16-18, 15 and 9). These data are strongly in contrast with the quantitative distribution of Thunell (1978) that reports for this area values >60%. Conversely in the Tyrrhenian Sea Thunell (1978) documents a decrease in abundance values of *G. ruber* respect to the Ionian Sea. This contradiction need to take in account in the discussion if you want to consider, for general comparison, the data proposed by Thunell (1978). Line 182: Fig. 3 is not necessary. Should maintain only Fig. 4 Line 186: see comment proposed in Line 182 Line 190: see comment proposed in Line 182 Line 191: if the authors want to use the classification proposed by Spezzaferri et al (2015), *Globigerinoides sacculifer* should be *Trilobatus sacculifer*. Once more, it is important to have short chapter concerning the criteria adopted for classification. Line 194: fraction are  $\geq 350$  micron, please add Fig. 4 at the end of the sentence. Line 195: but is usually less abundant, please add

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Fig. 3 at the end of the sentence. Line 196: Fig. 3 is not necessary Line 197: *Globigerinoides sacculifer* of the quadricameratus-type, should change in *Globigerinoides quadrilobatus* in the manuscript Line 201: The authors report that they grouped some stations to achieve a minimum number of planktonic foraminifera. In my opinion is not correct and I think that also the low number of planktonic foraminifera need to take in account in the interpretation. The low number is related some specific environmental setting those characterised a specific part of the Mediterranean, and you cannot lose (or overlook) this datum in this manuscript. In addition, it is not necessary to plot the % abundance of the species, because of it is not useful for comparison with data from Pujol & Vergraud-Grazzini (1995) or from De Castro Coppa et al. (1980). If you want to use the % abundance you have to covert in % also data from literature. Probably it make sense for comparison. Line 217-224: In my opinion, I consider the PCA the correct statistical approach for these data, anyway, it is important to show the complete correlation matrix where the reader can see all the obtained values for each variables. In addition, please specify the software used of statistical analysis. Line 227-235: it is very hard to follow this discussion using the diagrams proposed in Fig. S2. If you want to compare the size of planktonic foraminifera between different parts of the Mediterranean, maybe the authors can chose other graphical representation. Line 261-275: I think that a table could be useful for a visual comparison between absolute abundance in the different areas Line 276-285: I think that a graphical representation is very useful to show this comparison. In addition, the authors need to take in account also the data reported in De Castro Coppa et al (1980) from Gulf of Naples that you could tentatively correlate with the station 19 in the Tyrrhenian Sea. Line 286-288: It is not correct to group these species. They are different Line 288-292: the authors compare *G. sacculifer* morphotype *trilobus* and *quadricameratus* (please modify in *quadrilobatus*) with literature data (Cifelli 1974; Pujol & Grazzini 1995 and Thunell 1978). Please be sure that in these papers are reported these species (i.e., in Thunell 1978, *G. quadrilobatus* is not reported). In addition, the authors have to consider also De Castro Coppa (1980). Once more, a graphic representation is useful. Line 292-294: A possible reason could

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be the mesh size used in this work, even if in De Castro Coppa et al. (1980) where they used in the Gulf of Naples a mesh size of 145 micron, in May 1979, they found *N. pachyderma*, *T. quinqueloba* and *G. truncatulinoides* (no high number of individuals). However, I think that with this mesh size you lose small size planktonic foraminifera. Line 296-298: probably *Globigerinoides sacculifer* type *quadrocameratus* (*quadrilobatus*) is not reported in the previous literature because of it was included in *G. trilobus* or *G. sacculifer*. I would suggest a graphical comparison between literature data concerning *G. sacculifer* and *G. trilobus* (Cifelli 1974, Thunell 1978, De Castro Coppa et al 1980 and Pujol & Grazzini 1995) and a group *G. quadrilobatus* of your data (where you can include *sacculifer*, *sacculifer trilobus*-type and *quadrocameratus*-type). Maybe it make sense. You can try. Line 298: I think that the authors can refer to a paper spanning a more recent time interval than the Eocene. In particular, it is necessary to select a paper where *G. sacculifer* type *quadrocameratus* (*quadrilobatus*) is present. Line 302: the reference is Cossarini et al. (2015). Please modify Line 323-324: they are two different species and not varieties and they have different environmental preferences. Line 344-346: please add a reference Line 350: the authors can report as reference also Rigual-Hernandez et al (2011) where in February from sediment trap *G. ruber* pink is not present. Line 365-367: I think that is necessary to report also the data from De Castro Coppa et al. (1980) where *G. inflata* is documented in May 1979. Line 370-371: these data are opposite to data reported in Barcena et al. (2004) for sediment trap in the Alboran Sea, where in spring season *G. bulloides* is more abundant than *G. inflata*. Can the authors try to explain this discrepancy? Line 371: is van Raden et al. (2011) Line 385-386: see comment reported in Line 370-371 Line 391-392: data from sediment trap (Gulf of Lion) of Rigual-Hernandez et al (2011) report a decrease in abundance of *G. inflata* respect to *G. bulloides* during May, while in April these species strongly reduce the difference in abundance. Line 394-395: add Fig. 3 at the end of the sentence. Line 416-417: the quantitative data of *O. universa* seem to suggest a strong decrease in abundance towards eastern Mediterranean and two possible decreasing trends, one versus the Gulf of Lion and the second one from Balearic versus Alboran

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Sea. Can suggest these trends a possible explanation? Line 493: Kohler-Rink and Kuhl 2005 is missing in the references

Reference comments: Please add: Bárcena, M.A., Flores, J.A., Sierro, F.J., Pérez-Folgado, M., Fabres, J., Calafat, A., Canals, M., 2004. Planktonic response to main oceanographic changes in the Alboran Sea (Western Mediterranean) as documented in sediment traps and surface sediments. *Marine Micropaleontology* 53, 423-445.

Rigual-Hernández, A.S., Sierro, F.J., Bárcena, M.A., Flores, J.A., Heussner, S., 2012. Seasonal and interannual changes of planktic foraminiferal  $\delta^{18}O$  in the Gulf of Lions (NW Mediterranean) and their implications for paleoceanographic studies: Two 12-year sediment trap records. *Deep Sea Research Part I: Oceanographic Research Papers* 66, 26-40.

Modify Coppa et al. (1980) in De Castro Coppa et al. (1980).

Line 578-579: the reference is Bijma et al 1990. Please modify Line 615-616: this reference (Ivanov et al. 2003) is missing in the manuscript

Figure comments: Fig.1: the numbers are too small it is very hard to read. Please increase the size. If the station 8 was not sampled for planktonic foraminifera, please remove it from the Mediterranean location map. Fig. 2: In my opinion it is necessary to add close to the number of the station also the geographic location (i.e., 1-Atlantic or Gulf of Cadiz; 2 - Gibraltar; 3- Alboran Sea etc. . .). In addition, it is necessary to follow the same direction for the position of the columns (i.e., W versus E), so that for Fig. 3b the correct sequence is: 22, 20, 21, 19. The same modification you have to make for the other transect 17, 16, 16-18, 15, 14. Fig.4: see comments reported for Fig. 3

Appendix A: modify *quadrocameratus*-type in *quadrilobatus*, and *G. ruber* s.s. with *G. ruber* white or *G. ruber alba*

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