

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

**Miguel Mallo et al.**

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We appreciate the overall positive referee remarks and acknowledge the detailed and constructive comments that greatly helped to clarify a number of points and to improve the manuscript.

Below are our detailed responses to the referee's comments, including expected modifications of the manuscript:

## **Major Comments**

REFeree#1, COMMENT: Unclear methodology for SNW: what diameter was measured? Neither *ruber* or *bulloides* approximates a sphere, so this is important to men-

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tion.

REPLY: In order to avoid any misunderstanding on the terminology “Size-Normalized Weight” (SNW), we agree to change SNW to “Density Area” (DA) in the revised manuscript. The latter denomination is less confusing and in agreement with previous work (Marshall et al., 2013). To obtain the foraminiferal DA, the ‘diameter’ measured was the longest straight line possible for each specimen. All unbroken individuals were analyzed for their maximum diameter and weight. DA determined from maximum diameter is assumed a solid measure to produce statistically significant data (Marshall et al., 2013; Marshall et al., 2015). Broken individuals were not analyzed for their SNW. To avoid confusion and potential misunderstanding, we will change in the manuscript the word “diameter” to the more precise terminology: “long axis”.

REFeree#1, COMMENT: It is unclear to me if the same specimens were used to measure diameter + area and weight, please explain. And I also understand – but I am not sure - that at each station only specimens were measured within a certain 50 m size range, is that right? If so, could that not bias the trends because samples weren’t selected randomly and trends in shell weight may be affected by trends in shell size?

REPLY: The original plankton net used for the sampling had a mesh size of 150  $\mu\text{m}$ , and the foraminifera  $\leq 150 \mu\text{m}$ , including the ones with tests partially broken were discarded. All the weighed individuals were measured (area and long axis). The individuals for which the long axis and area were measured but were not able to be weighed were extrapolated from their diameter size (Blue stars in Fig. S4 of the original manuscript).

We will update the ‘Material and Methods’ section to clarify the methodology. The two paragraphs between lines 139-155 will be replaced by a more in-depth explanation of the methodology used. One part of that replacement clarifies that question:

“For the DA study, we selected 3 main species: *G. ruber*, *G. bulloides* and *O. universa*. All the specimens of these 3 species were photographed with a Canon EOS 650 D

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camera device attached to a Leica Z16 AP0 microscope to measure their long axis and silhouette area using the software ImageJ (Schneider et al., 2012). For each station and each of the 3 selected species, the individuals were weighed together by triplicate with a Mettler Toledo XS3DU microbalance ( $\pm 1 \mu\text{g}$  of nominal precision) within  $50 \mu\text{m}$  size fraction increments ( $150\text{--}200 \mu\text{m}$ ,  $200\text{--}250 \mu\text{m}$ , etc.). Cytoplasm-filled or empty dry-weighed foraminifera tests were weighed together since dry cytoplasm has no statistically significant effect on the weight of tests  $>150 \mu\text{m}$  (Schiebel et al., 2007). Specimens containing notable organic matter attached to the test were discarded. The maximum number of individuals weighed together was 5; in some stations individuals were measured individually as no more specimens were available. In all the cases the mean weight per specimen of the three weighings was applied. The silhouette area obtained was then used to obtain the DA measurements (as is also done in Marshall et al., 2013; Marshall et al. 2015)."

REFeree#1, COMMENT: Analysis of species assemblages: I have a major problem with the fact that to analyse the species distribution the authors use relative abundances. This does not make sense if one looks at individual species (closed sum effect: the % of species A will because % B changes; see for instance station 10 and 13 where the absolute abundance of *G. ruber* ss is similar, yet the relative very different) and if one wants to investigate species assemblage variability other techniques (PCA, cluster analysis) that look at the entire assemblage are more appropriate. I don't see what bias large variability in absolute abundance could cause (L222), why would this be bias? The authors should decide what they want to do: investigate the assemblage and use a different technique or investigate individual species abundance and use absolute abundances. The discussion and conclusions will then need to be rewritten.

REPLY: Relative abundances are grouped to see which species dominate in each geographic region of the Mediterranean. There exists high variability in the sample size along the stations; we consider relative abundance a valuable data source to under-

stand better the ecology and distribution of the different species. Also our relative abundance groupings were estimated to allow the comparison with previous studies in the Mediterranean using relative abundances in a sub-basin/regional location level of comparison (Cifelli, 1974; Thunell, 1978; Pujol & Grazzini, 1998 (in text, not in figures)). Absolute abundance data is also provided and used in the results and discussion sections.

We will change our Pearson test analysis for a PCA. For the analysis we compare the PCA factors with absolute abundance and DA, which will be treated in the results and discussion section, leaving the species assemblage only for comparison with previous literature. A new methodology chapter is included:

### “3.3. Statistical methods

We performed a principal component analysis (PCA; Varimax rotation) using SPSS Statistic 23 software. The PCA was performed on the environmental parameters: temperature, salinity, oxygen, fluorescence, NO<sub>3</sub>, PO<sub>4</sub>, pH, pCO<sub>2</sub>, and [CO<sub>3</sub>-2], of every station. Two components, which together explain 77 % of the total variance, were obtained (REV Fig. 7), the first one (Factor 1) reflects the west-east Mediterranean gradient of temperature and salinity in opposition with the quantity of nutrients available. Factor 2 reflects the gradient in seawater carbonate chemistry. Then, absolute abundance for the main species and all the foraminifera overall plus the DA of the 3 selected species were plotted against the two factors (REV Fig. 7).”

REFeree#1, COMMENT: SNW regressions: first of all, the rationale behind the regressions isn't clear to me. Why investigate the relation between area and diameter?

REPLY: The relation between area and long axis in the three selected main species allows detection of any anomaly or changes in their growth pattern. We will add the following text in the paragraph of lines 228-236 to clarify Fig. S2 of the original manuscript:

“...The high two-dimensional (silhouette) area-to-long axis correlation is best fitted by

a power regression (Fig. S2). The same growth pattern can be seen in *G. ruber* s.s., *G. bulloides*, and *O. universa* with that correlation, represented graphically in the shape of a power function (Fig. S2). They grow slightly faster when they are smaller (steepest in the lower left part of the regression line) and slightly slower when they are bigger (less steep in the upper right part of the regression line; Fig. S2). Comparing the average values from different locations sampled within the Mediterranean. . .”

REFeree#1, COMMENT: Why is diameter of interest if one normalises to area in SNW?

REPLY: It is important to know both the foraminiferal size and the area in order to detect changes in their ratio and in their calcification pattern. See the previous answer. Also, the data on the long axis-weight makes possible the comparison with previous studies (see Bijma et al., 2002; Lombard et al., 2010; de Moel et al., 2009; Aldridge et al., 2012; Schiebel et al., 2007).

REFeree#1, COMMENT: It is entirely unclear to me what we learn from this and hence how to these analyses can be used to exclude *O. universa* from further analysis. This really needs more explanation.

REPLY: High variability in long axis-area and long axis-weight correlation was detected for *O. universa*; this variability was also present within stations. Making a SNW study of *O. universa* leads us to no trend (REV Fig. 1). No specific cause for variable density is recognizable, as we have no clear DA differences between the different geographic locations.

REFeree#1, COMMENT: Moreover, in this respect it may also be better to use the term area density (e.g. Marshall et al., 2013) rather than SNW to distinguish from sieve-based size measurements.

REPLY: We totally agree. We change all our “Size-Normalized Weight” terminology to Density Area (DA) in the revised manuscript, based on Marshall et al. (2013), and

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Marshall et al. (2015). It will also be stated in the methodology section.

REFeree#1, COMMENT: Then on to the actual regressions. What is the rationale/bio-physical reason that area and diameter should be linearly related in log-log space? First of all, this power regression implies that neither area nor diameter can actually be zero, which cannot be correct, the regression line should go through the origin. Secondly, why wouldn't a simple power regression model suffice (I'd expect the equation for *O. universa* to be close to  $\pi \cdot r^2$ ). Something similar holds for the regression of area and weight and area and diameter (again why diameter?). Why, in the case for area, a different model for each species, are there any reasons for these differences and for the choice of any model in particular? The problem with the present equations is immediately clear when looking at Fig S4c: the predicted weight of shells with an area of  $10^{-5} \text{ m}^2$  is 0 g, which is physically impossible. Since weight is linearly related to volume (through density) wouldn't one expect  $y$  to be related to  $x^3$ ?

REPLY: Area and long axis are linearly related in log-log space as is the best fit found, and the shape of the regression highly coincides with the natural shape results ( $r^2 = 0.975, 0.962$  and  $0.921$  for *O. universa*, *G. ruber* s.s. and *G. bulloides* respectively). Size and mass of foraminifera relationship does not start at the origin. The proloculus of planktic foraminifera measures between 15-30  $\mu\text{m}$  in average, and has a certain calcite mass, which has so far not been determined (see Hemleben et al., 1989). We will use the power fit in the three species treated in Fig. S4 of the original manuscript for consistency reasons.

REFeree#1, COMMENT: Moreover, and this applies to both the analysis of the species abundance and the SNW, looking at Fig.1 it appears that with the exception of fluorescence, all parameters are strongly correlated; so how do the authors determine which of these parameters is really important for the prediction of the species assemblage or SNW? The actual correlations between water column characteristics and foraminifera abundance or SNW are not shown, yet this is the most important of the study. This should be amended and the predictive power of the proposed models

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should be shown.

REPLY: We agree that proper statistical analysis should be conducted on our data set. This is why in the revised version we will include a principal component analysis (PCA) performed on the environmental parameters. Note that new environmental parameters will be added: the nutrients (NO<sub>3</sub> and PO<sub>4</sub>), the oxygen concentrations and the pCO<sub>2</sub>. The results of the PCA show that 2 factors explain about 77% of the total variance in the environmental parameters. The 1st factor exhibited positive loadings on the nutrients and the fluorescence and negative loadings on temperature and salinity (and to a lesser degree on carbonate ion concentrations). This factor explains 56.99% of the total variance and represents the strong west-east gradient characterizing the Mediterranean Sea as the water becomes warmer, saltier and more oligotrophic eastwards. The second factor explains about 20.02% of the total variance and is characterized by positive loadings on pH and oxygen concentrations (and to a lesser degree on carbonate ion concentrations) and a negative loading on the pCO<sub>2</sub>. It is interpreted as the variations of the carbonate system properties in the Mediterranean Sea with more acidic conditions in the western basin compared to the eastern basin. The sample scores on the 2 first factors with overlay of absolute abundances of foraminifera species (*G. ruber* (white), *G. bulloides*, *G. inflata*, *O. universa* and *T. sacculifer* (without sac)) and density area (*G. ruber* (white), *G. bulloides* and *O. universa*) are presented and discussed in the revised manuscript.

REFeree#1, COMMENT: Influence of wall thickness on shell weight: the authors mention this briefly in the discussion about *O. universa*. I'm surprised that the study by Marshall et al (2015) on exactly the same topic is not mentioned.

REPLY: We appreciate that reference. We modified text from the manuscript adding Marshall et al., 2015 reference: From line 452: "...Whereas the various types of *O. universa* differ in the size of pores (de Vargas et al., 1999; Morard et al., 2009; Marshall et al., 2015), their pore-size is also affected by environmental conditions including water temperature (e.g., Bé et al., 1973). Likewise, thickness of the test wall has been

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described to vary between types (de Vargas et al., 1999; Morard et al., 2009; Marshall et al., 2015), and is as well affected by...” From line 468: “... (Lombard et al., 2010), may be caused either by differences in genotypes or environmental conditions, or both. Thinner walls overall in our specimens with respect to the mentioned studies might be a cause (Marshall et al., 2015). In our samples from the Mediterranean...” And a new paragraph addition after line 460 comparing our area-weight results with the ones in Marshall et al. (2015): “The *O. universa* weight-area results of our study are compared with Marshall et al. (2015) from Cariaco Basin sediment trap specimens, which differentiated *O. universa* in Type I (Mthick) and Type III (Mthin), suggesting thinner test walls in the latter. In the area range of  $3 \cdot 10^5 - 4 \cdot 10^5 \mu\text{m}^2$ , our weight results coincide with the expected Mediterranean Type III variety (Fig. S4c; Marshall et al., 2015), but at  $2 \cdot 10^5 - 2.5 \cdot 10^5$  we see a mix of both types until at  $1.5 \cdot 10^5$  type I coincide more with our results (Fig. S4c; Marshall et al., 2015). We suggest that different sub-types of the Mediterranean *O. universa* variety coexist in the Mediterranean with differences in the wall thickness.”

REFeree#1, COMMENT: The conclusion that SNW and therefore calcite formation is not limited by carbonate chemistry in my opinion not supported by the data. Both pH and [CO<sub>3</sub><sup>2-</sup>] are high in the Med, so how can one exclude the possibility that both parameters limit calcification? If anything, but the authors need to firmly demonstrate this, it may be that at high pH and [CO<sub>3</sub><sup>2-</sup>] other parameters may be more important. But see also the comment above about the fact that in seawater everything appears correlated with everything making it very difficult to isolate the influence of a single parameter. Fig. 6 is not very revealing in this respect: it shows a spatial trend (based on unclear grouping of the data) that may or may not be statistically significant and that may or may not be related to carbonate chemistry or food availability. The authors have a unique dataset including ancillary data and could do better to explain the variability in SNW.

REPLY: We will clarify this point raised by the reviewer. In fact the overall con-



clusion of the paper is not that seawater carbonate chemistry cannot be a key driver for foraminifera calcification. The results of this study are related to the modern Mediterranean conditions where pH and [CO<sub>3</sub><sup>2-</sup>] are relatively high, well above the carbonate saturation, compared to the critical values tested in ocean acidification experiments and other oceanographic settings. The pH in the upper 200 meters is ranging from 8.047 (St.1) to 8.126 (St.20) and the [CO<sub>3</sub><sup>2-</sup>] 178.88  $\mu\text{mol Kg}^{-1}$  (St.1) to 243.560  $\mu\text{mol Kg}^{-1}$  (St.11). The Mediterranean Sea is an oligotrophic to ultra-oligotrophic environment having a strong physical and biogeochemical gradient from the Atlantic to the Eastern Mediterranean (Fig. 1 of the original manuscript; Fig. 2 of the original manuscript; MEDAR: [http://modb.oce.ulg.ac.be/backup/medar/medar\\_med\\_phph\\_spring.html](http://modb.oce.ulg.ac.be/backup/medar/medar_med_phph_spring.html); Touratier et al., 2012: [http://images.slideplayer.com/31/9579232/slides/slide\\_2.jpg](http://images.slideplayer.com/31/9579232/slides/slide_2.jpg)). A main point of the paper is to show that since the seawater carbonate saturation at the studied sites is negligible compared to other oceanic regions, the effect of parameters other than carbonate saturation could be detected as observed in other studies (e.g. Weinkauf et al., 2016). We conclude that planktic foraminifera calcification in the modern Mediterranean Sea is likely more affected by factors other than carbonate saturation. In oligotrophic regions, food availability can be critical for the fitness and growth conditions since there is the hypothesis that food availability can free more energy for calcification (Beer et al., 2010; de Villiers et al., 2004; Horigome et al., 2012).

*G. ruber* (white) is dominant in the eastern basin, whereas *G. bulloides* show its dominance in the western basin, accentuating more the differences in food availability for both species. Our conclusions also might work in similar highly oligotrophic areas. Our conclusions do not exclude that in a future with the ongoing accelerating emission of anthropogenic carbon and its uptake by the Mediterranean sea surface, carbonate chemistry will have a major effect on the SNW of planktic foraminifera, even if this is of relatively low influence today.

Figure 6 (of the original manuscript) grouping was set by location proximity in which

foraminiferal assemblages were similar, also, the grouping was done in order to achieve a minimum number of foraminifera (in Fig. 6 (of the original manuscript):  $\geq 8$  tests). We also notice that each grouping also has similar water mass characteristics. It is important to note that we work with small quantities of individuals (9 groupings of 13 in Fig. 6 (of the original manuscript) does not exceed 20 individuals) that come from a single collection in May. Further conclusions could be taken from further data availability (e.g. at different seasons).

REFeree#1, COMMENT: Lunar and seasonal abundance variations, diel migration: the possible effect of a lunar reproductive cycle on the abundance should be mentioned in the introduction and discussed (Bijma and Hemleben, 1994; Jonkers et al., 2015). And even though the authors discuss the effect of seasonality in the discussion, it would be good to mention it also in the introduction. There at least two long-term sediment trap studies from the Western Med that could be used to place these new observations in perspective (Bárcena et al., 2004; Rigual-Hernández et al., 2012). Moreover, the authors also allude to diel migration, yet don't really make anything out of this (I wonder if it's possible with nets that integrate over the upper 200 m).

REPLY: We are aware that lunar cycle can influence the distribution of foraminifera. However, in our study the lunar day influence on the total absolute abundances (REV Fig. 2) was negligible. In fact no significant correlation was detected with our results and we decided that for this study this topic was not presented in the introduction.

The abundance distribution affected by seasonal variations will be mentioned in the introduction in the following way (ending of L33-45 paragraph):

“The abundance distribution of foraminifera is also affected by a predictable and distinct seasonal cycle for each species driven by the food source content in the watermass (Hemleben, 1989; Bé and Tolderlund, 1971; for Mediterranean examples see e.g.: Pujol and Vergraud-Grazzini, 1995; de Castro Coppa et al., 1980; Bárcena et al., 2004; Rigual-Hernández et al., 2012).”

The suggested references were added and discussed in the revised version, as well as Castro Coppa et al. (1980) and Hernández-Almeida et al. (2011). For example we changed some parts of the discussion as follows: “Despite no new plankton tow study covering the Mediterranean, three regional studies based on sediment traps were realized in the Alboran Sea (Bárcena et al., 2004; Hernández-Almeida et al., 2011) and the Gulf of Lions (Rigual-Hernández et al., 2012). The one year time series of the Alboran Sea sediment traps (July 1997 – May 1998) showed big differences in the main species relative abundances and daily fluxes through the different seasons, driven by food availability (related with water mixing/stratification periods) and temperature (Bárcena et al., 2004; Hernández-Almeida et al., 2011). The 12-year sediment trap records at Gulf of Lions (October 1993 – January 2006) showed a big seasonal pattern of the species, being more than 80% of the data from winter and spring in correlation with the nutrient supply and mixed water column (Rigual-Hernández et al., 2012).” “Comparisons are made with older similar studies from Pujol and Vergraud-Grazzini (1995), Cifelli (1974), de Castro Coppa et al. (1980); Bárcena et al. (2004), Hernández-Almeida et al. (2011), Rigual-Hernández et al. (2012), and Thunell (1978).” “The presence of *G. inflata* is related with cool waters and high food availability (Pujol and Vergraud-Grazzini, 1995; Rigual-Hernández et al., 2012), following high phosphate concentrations (Ottersen, 1992).” “In winter, with cooler temperatures, the opposite process happens, and *G. inflata* becomes the dominant species in the Alboran Sea (Bárcena et al., 2004) and the southwestern basin, with high frequencies in the Strait of Sicily and just east of it.” No allusion to diel migration was stated inside the manuscript.

REFeree#1, COMMENT: Suggested trend in abundance and diversity (L28-30; 84-85; 300-305): while such a trend would be very interesting I really don't think that this is anything else than speculation. Two cruises almost 20 years apart are not enough to constrain intra and interannual variability in foram abundance and diversity, so there is simply not enough data to support this statement. There are also important sampling differences between the present study and the one by Pujol and Grazzini: 1) the maximum depth of observations (350 m vs 200 m) and the spatial distribution, which

both affect the observed abundance and diversity and a simple comparison of mean abundance or total diversity compromised. I suggest that the authors remove this speculative remark from the paper.

REPLY: The results of our study may be the first indication of a possible long term-change in diversity, which might be caused by interannual variability, and affected by climate change. Possible causes of such changes may include variability in the North Atlantic Oscillation and other regional or larger scale changes of the climate system.

We are aware of the limitations to announce that the trend in abundance and diversity is happening (few similar studies in the Mediterranean, just one with absolute abundance data, a long time span between them, sampling at different times of the year, and with different methodologies) ; we cannot prove that, we just found first insights of a possible indication. During our study the DCM is situated close to 200 m depth; between 200 and 350 m depth we do not expect to find higher numbers of foraminifera (Fig. 1c of the original manuscript), so comparison with Pujol and Vergraud-Grazzini (1995) can be made effectively.

Factors supporting that might be a future trend in reduced abundance and diversity exists too: Environmental parameters are changing in the Mediterranean (e.g. see Yáñez et al., 2010; Hassoun et al., 2015a; Hassoun et al., 2015b, Cossarini, 2015), and our absolute abundance numbers, sampled in a period of the year in which the productivity is supposed to be at its highest in the Mediterranean (e.g. see Rigual-Hernández et al., 2012; Barcena et al., 2014), are the lowest found in the literature, even lower than recent studies in other oligotrophic areas, suggesting the Mediterranean is a critical location where possible future problems of planktonic foraminifera scarcity might occur.

For the reasons above, we think our statements are appropriate. Notice in the manuscript we are not stating that a reduced trend in abundance and diversity is a fact: see wording “could be” (L 28-30), “might have” (L 84-85), and “may indicate”;

“could also imply” (L 300-305).

#### Minor/technical comments

COMMENT: Frequent use of locations that are not indicated on a map. Please make sure that each locality is indicated. REPLY: Text added in Fig. 1 legend (of the original manuscript): “Fig. 1. (a) Temperature ( $^{\circ}\text{C}$ ), (b) salinity, (c) fluorescence ( $\mu\text{g}\cdot\text{l}^{-1}$ ), (d) pH, and (e)  $[\text{CO}_3]^{2-}$  ( $\mu\text{mol}\cdot\text{kg}^{-1}$ ) values of the water column of the transect. Values follow a color scale (under every graph), also values shown in the isometric lines. X axis: water depth. Y axis: longitude (degrees). Measurement locations indicated with white dots, with the coinciding stations numbered at top. The station number and the map section correlates with the map at right of this description. For station code names see Table 1. Note reversed color scale at (d) and (e). Software used: Ocean Data View (Schlitzer, 2016)” Text added in Fig. 2 legend (of the original manuscript): Fig. 2. Sampled stations with BONGO nets (dots). The numbers in the picture represent the station codes: First leg: 1 to 13, second leg: 14 to 22. For station code names see Table 1. Colour scale at right represents the values of surface chlorophyll concentration (in  $\mu\text{g/l}$ ), retrieved from MODIS Aqua (L2), from the closest day as possible of the first leg transect.

COMMENT: Fig.1: use same x axis scale for each panel. REPLY: We agree that the 3 panels should be on the same scale. However, it appears to be barely possible as a result would be that section 2 and section 3 won't be readable (or at least the stations presented on each of these panels would be so close together so the reader can't distinguish them).

COMMENT: Fig. 3: use same y axis scale (perhaps log based?). A better representation of the data (Figs 3-6) may be to plot them on a map, or add a small map inset to the figures. REPLY: Fig. 3 of the original manuscript was modified (REV Fig. 3) and now has the same Y axis scale (see figure below). We do not consider it necessary to plot a map on Figs. 3-6, as Fig. 2 fills that purpose.

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COMMENT: Fig. 4: what is 'n' below the graphs (16-18 > 16?)? REPLY: 'n' = sample size = number of individuals. It was missing at the legend, and is now included in the revised manuscript. 16: station code: Otranto Strait. 16-18: station code: Northern Ionian Sea. See Table 1.

COMMENT: L38: perhaps replace radiation with sunlight. REPLY: Changed in the revised manuscript.

COMMENT: L39: not only depth habitat, also seasonality. REPLY: We agree. Changed in the revised manuscript: "...these factors provoke an overall water depth preference, which shifts during ontogeny, and seasonal priority for each species."

COMMENT: L50: provide a reference for the expedition. REPLY: Reference added: Pettersson (1953).

COMMENT: L52: large not high abundance variations. REPLY: Changed in the revised manuscript.

COMMENT: L57-69: From this seems that the controls on the sedimentary assemblage are different from those on the water column assemblage. The main difference of course is that fact that water column observations are mere snap shots in time, whereas the sediment integrates centuries to millennia. Could the controls really be different, could the sedimentary signal integrate enough to obscure intra- and interannual variability in food availability? I'd find it interesting if the authors could spend a bit of time on this. REPLY: See the answer to your question in L88-95.

COMMENT: L66: correlation with what? The authors appear use correlation and statistically significant quite often without referring to what was tested, how and with what confidence interval. REPLY: Correlation between foraminiferal assemblage variability, and temperature and salinity gradients regarding Pujol and Vergraud-Grazzini (1995). The sentence is one of the main conclusions from the Pujol and Vergraud-Grazzini (1995) article, not from our study.

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COMMENT: L70: Consider changing 'Its weight..' by 'Their shell mass...' to be more consistent. REPLY: Changed in the revised manuscript.

COMMENT: L73: What's the conclusion, implication of mentioning of the De Beer study? REPLY: Beer et al. (2010a) supports our results for *G. ruber* s.s. SNW being negatively correlated with [CO<sub>3</sub>-2]. Citing this article provides a point of view that shows that seawater chemistry might be independent of the shell mass of foraminifera in the Mediterranean Sea presently. We corrected the citation to Beer et al. 2010a to avoid confusion with Beer et al. 2010b (see References).

COMMENT: L78: Studies of the water column in the Mediterranean (or similar); not Mediterranean studies. REPLY: We agree: "Studies of the water column foraminifera in the Mediterranean and accurate knowledge. . ."

COMMENT: L88-95: while all of this holds true of course, the most important difference between the living (water) assemblages and the dead (sedimentary) is the time integrated in the sample (see also above) and (post)depositional changes to the assemblage. This needs to be mentioned. Living specimens are of course also advected; in fact, advection during life is probably more important than during sinking (simply because sinking takes less time). The study of Van Sebille is probably not very relevant for Mediterranean: with only six grid cells in the entire basin one can hardly expect that the circulation is realistically represented. REPLY: We are aware of the differences between sedimentary and water column samples, that paragraph is focused on proving that Thunell (1978) results are consistent for a comparison with water samples. We assume the reader knows the main differences between both sampling methods. Thunell (1978) states that its samples represent well the present foraminifera distribution, as they are from the very upper sediment (0-2 cm) and are recovered by trigger cores with little mixing. Note that the Mediterranean Sea is very CaCO<sub>3</sub> saturated, with a good preservation of the samples. We propose a slight comment on the manuscript (see below).

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We agree, at the horizontal scales we are working at, advection can be neglected. Moreover, live foraminifera are advected in their “own” water mass (e.g. plankton) and are indicative of their ambient seawater. Sebillé et al. (2015) reference and text will be removed from the manuscript. We provided a better reference to show that the quick vertical settling provoke minimal horizontal advection of foraminifera. Modified manuscript text (L 88-95): “The study by Thunell (1978) is based on surface sediments, which can provide information, but might be biased towards faster-sinking and more hydrodynamic tests due to shorter exposition to dissolution processes (Caromel et al., 2014; Schiebel et al., 2007), and towards tests with thicker walls that are better preserved (Thunell, 1978). The top (0-2 cm) sediment samples recovered by little disturbed and mixed trigger cores are suitable to represent modern times data according to Thunell (1978), although this sedimentary data can have a time span of some centuries and our sampling is a snap shot in time (Mortyn and Charles, 2003). In addition, empty tests are passive particles that ocean currents may displace horizontally, but that displacement is negligible due to their quick settling velocities (Caromel et al., 2014). Correlated results between plankton tows (Pujol and Vergraud-Grazzini, 1995) and surface sediments (Vergraud-Grazzini et al., 1986) at coincident places inside the Mediterranean confirm the data of Thunell (1978).” COMMENT: L112: Gulf of Lions  
REPLY: Changed in the revised manuscript.

COMMENT: L125: stratified? I’m not entirely sure, so please explain, but I thought that BONGO are not depth stratified. I understand that all the observations mentioned here are integrated over the upper 200 m of the water column. Please explain precisely what was done; I also assume that the statement that the samples were taken at 200 m depth (L135) is not correct. REPLY: We agree, the word “stratified” is eliminated from the manuscript. BONGO nets collect specimens from 200 m depth, and also the ones that are caught while the net is descending and ascending (above 200 m depth).

COMMENT: L154: what are unclassified specimens? REPLY: Unknowns, impossible to recognize at species level with the technology we have (most of them were juve-

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niles not well shaped yet). We replaced “unclassified” by “unknowns” in the revised manuscript.

COMMENT: L166: remove location 1 from the list, abundances are clearly different there. Also refer to figures in this section. REPLY: True. Station 1 average removed and figure references added.

COMMENT: L210: : : dominance ‘of a single species’: : : REPLY: Changed in the revised manuscript.

COMMENT: L233: how was significance determined? P-value? REPLY: We agree that proper statistical analysis should be conducted on our data set. This is why in the revised version we will include a principal component analysis (PCA) performed on the environmental parameters. See a more extended explanation of our PCA in the answer above in the major comments section.

COMMENT: L243; 252: how were the locations grouped? REPLY: By geographic proximity in which water mass properties were similar. See L 202 – 204.

COMMENT: L249: add SNW after G. rubber REPLY: Changed in the revised manuscript: adding DA instead of SNW.

COMMENT: L293-295: all these species mentioned here are winter species of which the flux happens in a single short pulse (Bárcena et al., 2004; Rigual-Hernández et al., 2012). Sampling at the end of spring could thus easily have missed them. REPLY: Reasons for those species missing will be incorporated in the manuscript as follows: “Some of the species not found reached high frequencies in the aforementioned studies: e.g., the winter species *Turborotalita quinqueloba*, *Neogloboquadrina pachyderma*, and *Globorotalia truncatulinoides*. The fact that these species were not sampled in the present study may be due to absence or presence at extremely low abundances of adult specimens at the sampled stations in May, as they use to have low abundances at that time according to a 12-year sediment trap record in the Gulf of Lions (Rigual-

Hernández et al., 2012). Another possibility is their presence in sizes smaller than 150  $\mu\text{m}$ , escaping from our BONGO nets mesh size, a possibility potentially supported by previous Mediterranean studies with thinner mesh sizes that found these species (see Pujol and Vergraud-Grazzini, 1998, 120  $\mu\text{m}$  mesh size; Rigual-Hernández et al., 2012, 63-150  $\mu\text{m}$  mesh size)."

COMMENT: §5.2: please separate more clearly what are new results and what is existing knowledge. REPLY: We consider that exists an appropriate separation between our study results and existing knowledge. Latter points are always indicated by their references or named inside the text; also, many times the season of the cruise is named before the reference (e.g. "The *G. ruber* results confirm the findings of the June 1969 cruise of Cifelli (1974), where. . .", "in winter", "in late summer"). For our results words like "our data set", "our study", "in May" and references to our figures, avoid confusion. To avoid any confusion, we will add on the manuscript "this study" when we discuss our results on that section on sentences that might provoke doubt to the reader: i.e.: L366-367: "Despite having similar temperature ranges as the southwestern Mediterranean, *G. inflata* is absent in the Tyrrhenian Sea and the northwestern Mediterranean in this study."

COMMENT: L366: ': : : ranges as the : : : ' REPLY: Changed in the revised manuscript.

COMMENT: L367: ': : : and it was also found : : : ' REPLY: Changed in the revised manuscript.

COMMENT: L368: what characteristic of inflata shows this correlation? I don't follow this conclusion. REPLY: Abundance. Re-written sentence to avoid confusion and adapted to the new PCA statistical analysis performed in the revised manuscript.

COMMENT: L377: 'In accordance with: : : ' and ': : : Atlantic station: : : ' (there is no causal link and only one Atlantic station). REPLY: Changed in the revised manuscript.

COMMENT: L378: there isn't a station completely dominated by a single species,

please reword. REPLY: We agree. Sentence changed to: “whereas in our study it shares dominance with other species”.

COMMENT: L439: what is meant with the SNW is statistically significant? What was tested, with which confidence interval, using which test? REPLY: A proper statistical analysis should be conducted on our data set. This is why in the revised version we will include a principal component analysis (PCA) performed on the environmental parameters. See a more extended explanation of our PCA in the answer above in the major comments section.

COMMENT: L445: again, see above. In addition, there seem to be two groups in *O. universa* (Fig.S3. 4). Have the authors looked at the spatial pattern of the SNW? REPLY: See figure of *O. universa* density area by location groupings attached on the 3rd question in “Major Comments”: “SNW regressions”. Also see answer of the 5th question in “Major Comments”: “Influence of wall thickness on shell weight”, where we compare *O. universa* weight-area relation with Marshall et al. (2015) results. It really seems two different *O. universa* types, further genetic research should be useful for that species inside the Mediterranean.

COMMENT: L478: the distributions are significant? Please reword. REPLY: We agree. The final part of the sentence (“...which are statistically significant.”) is removed from the manuscript.

COMMENT: L478-484: is there an inverse relationship between *ruber* abundance (absolute) and SNW? If so, it would be good if the authors could discuss why food availability has a different effect on abundance and SNW. REPLY: Abundance of *G. ruber* is related to sunlight (as it is symbiont-bearing species) and food availability. Whereas shell mass is related to [CO<sub>3</sub>-2] (Schiebel et al., 2004, Arabian Sea; Beer et al., 2010a). Theoretically we do not expect an inverse relationship.

COMMENT: L511: reword ‘: : heavier average weight-diameter relation: : ‘. REPLY: We guess that Referee #1 meant to quit the “a” before “heavier average...” here. Done.

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Now the sentence remains like that: “Schiebel et al. (2007) found heavier average weight-long axis relation. . .”

COMMENT: L515: reproduction? Not calcification? REPLY: Changed by the word “calcification”.

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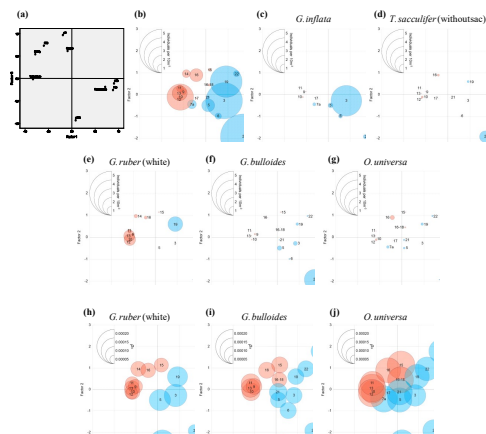
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REV Figure 7: Sample scores on the two PCA factors with (a) the loadings of the environmental parameters on each factor, (b) with overlay of the absolute abundance values (individuals · 10 m<sup>-3</sup>) of every station of all the foraminifera sample, (c) *G. inflata*, (d) *T. sacculifer* (without sac), (e) *G. ruber* (white), (f) *G. bulloides*, and (g) *O. universa*. With overlay of the  $p_A$  values ( $\mu\text{g} \cdot \mu\text{m}^{-2}$ ) of (h) *G. ruber* (white), (i) *G. bulloides*, and (j) *O. universa*. In blue color western Mediterranean stations (incl. Atlantic and Strait of Gibraltar), in red colour the eastern Mediterranean stations.

Fig. 1.

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