

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 constructive referee remarks and acknowledge the detailed 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.

1 General comments

REFeree #3, COMMENT: 1. While reading the manuscript I noticed that the writing style needs some attention. The manuscript is understandable, but there are plenty of orthographical and grammatical errors or weird phrasing throughout. Those should

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be dealt with (and I noted some suggestions in the detailed comments), to make the manuscript more accessible for the reader.

REPLY: Writing style and grammatical errors are now improved. We appreciated your suggestions.

REFeree #3, COMMENT: 2. The manuscript is partly missing important information, diverts from the topic, or promises undelivered results. Some examples: Parts of the manuscript, especially the section 'Oceanographic Setting', are lacking citations of information sources. Information on data sources and methods are largely missing. The temperature and salinity might come from the mentioned CTD casts, but the carbonate saturation values most certainly not: Have they been calculated on the basis of water samples (on board or in the lab) or calculated on the basis of database oceanographic data? Which of the several existing methods to calculate size-normalized shell weight has been used? Which software has been used for statistical analyses? All this information belongs in the far too short Material and Methods section!

REPLY: The Oceanographic Setting was written in a way that the references cited at the end of the paragraph were the ones used to reconstruct the paragraph. Now, in the revised manuscript, we change the way of referencing and we apply the references needed after each statement.

A new, more complete, methodology was written, explaining the data sources, the software analysis citation, and the methodology for the SNW. We decided to change "Size-Normalized Weight" to "Density Area" (A) in the revised manuscript. The latter denomination is less confusing and in agreement with previous work (Marshall et al., 2013). Here we present the fragments of the Methodology section that cover that information:

"...The sampling device was equipped with a flow-meter to have data of the volume filtered in each tow. From the upper 200 m of the conductivity-temperature-depth (CTD) stations, located near the sampling sites, was obtained water column data of temper-

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ature, salinity, oxygen, fluorescence (for the complete dataset see Ziveri and Grelaud, 2015). Seawater carbonate data (Total alkalinity (AT), and dissolved inorganic carbon (DIC)) was retrieved from Goyet et al. (2015), which was used to calculate pH, pCO₂, and [CO₃-2] using the software CO2Sys (Lewis and Wallace, 1998) with the equilibrium constants of Mehrbach (1973) refitted by Dickson and Millero (1987). The Italian National Institute of Oceanography and Experimental Geophysics obtained [PO₄] and [NO₃] onboard, filtering in glass fiber filters (Whatman GF/F; 0.7 μ m) the water samples, which were kept at -20°C. After in the laboratory, samples were analyzed with a Bran+Luebbe3 AutoAnalyzer, as did Grasshoff et al. (1999). Surface chlorophyll a concentration was obtained from MODIS Aqua L2 satellite (NASA Goddard Space Flight Center: <http://oceandata.sci.gsfc.nasa.gov/>). "For the density area (A) 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 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 (± 1 μ g of nominal precision) within 50 μ m size fraction increments (150-200 μ m, 200-250 μ 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 μ 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 weightings was applied. The silhouette area obtained was then used to obtain the A measurements (as is also done in Marshall et al., 2013; Marshall et al. 2015)." On the revised manuscript we will include a principal component analysis (PCA; Varimax rotation) using SPSS Statistic 23 software.

REFeree #3, COMMENT: (continuation) The reason for several analyses (e.g. the correlation between shell size and shell weight) is not properly explained, thus leaving

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the reader guessing why the authors deem this necessary. A comparison of assemblage data with earlier studies to study long-term trends is promised but never really delivered (not on a reasonable analytical level at least). REPLY: The relation between area and long axis in the three selected main species did not allow detection of any anomaly or changes in their growth pattern. The data on the long axis-weight make 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), also for the area-weight analysis (compared with Marshall et al. (2015) on the revised manuscript). Especially in the latter, we obtain useful information (in our case, specially for *G. ruber* (white) and *G. bulloides*) of their calcification intensity in different locations of the Mediterranean.

Our study does make detailed comparisons against prior studies (Thunell, 1978; Cifelli, 1974; Pujol and Vergnaud-Grazzini, 1998, in the revised manuscript we will include: Rigual-Hernández et al., 2012; Bárcena et al., 2004; Hernández-Almeida et al., 2011; de Castro Coppa et al., 1980). Water column plankton tow data from the Mediterranean is extremely limited, and consequently we are forced to make our detailed assemblage comparisons against sediment trap and surface sediments studies. Therefore we do as sensibly as we can, given the very real limits of existing data.

REFeree #3, COMMENT: 3. The existing images are OK, for the most part (labels might be a bit small in several of them). However, several key findings of the study are not presented in any suitable graphical manner, instead referring to figures which cannot present these data in a suitable way. Most notably amongst these, while there are several claims made about the influence of environmental factors on abundance and SNW of the species, not a single such relationship is graphically shown in a cross plot.

REPLY: Labels of the figures that need it will be increased in size on the revised manuscript. Our Figures 3 and 4 were modified for the revised manuscript (see REV Fig. 3 and REV Fig. 4). We agree that proper statistical analysis should be conducted

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on our data set. This is why in the revised version we will include a principal component analysis performed on the environmental parameters. Such analysis will include a graphical representation in which the absolute abundance and density area values are overlain (REV Fig. 7).

REFeree #3, COMMENT: 4. The manuscript uses several wrong species names and species concepts. The most prominent one is the unfortunate use of the terms *Globigerinoides ruber sensu stricto* and *Globigerinoides ruber sensu lato*, which are pooled, together with *Globigerinoides ruber* (pink), within the same species. This is blatantly wrong. Aurahs et al. (2011) has established that *Globigerinoides ruber* (pink), *Globigerinoides ruber* (white) (your *sensu stricto*), and *Globigerinoides elongatus* (your *sensu lato*) are distinctly different species, both biologically and in terms of morphology; and has therefore rehailed their Linnean taxonomy. Could we please all agree that 5 years after this publication we could at last all start to call them by their proper names and abandon this unfortunate *sensu stricto/sensu lato* distinction. It would be one thing if it would only be about names (I would still request to use up-to-date terminology, but it would be a minor mistake). Rather, *G. elongatus* is not even the adelphotaxon to *G. ruber* (white), but is more closely related to *Globigerinoides conglobatus*. Pooling them together under the same species name thus produces a polyphylum. If you want to pool them for some purposes (which can make sense) you can call them '*G. ruber/G. elongatus* plexus', or something along those lines. Second, the species *Globigerinella siphonifera* is reported from the samples. However, it is not clear whether this means that only *G. siphonifera* is present, or whether this is a collective term for the entire *Globigerinella siphonifera/Globigerinella calida/Globigerinella radians* plexus (Weiner et al., 2015), within which species have not been separated by the authors. Third, but less serious because this really is only a naming issue, the former *Globigerinoides sacculifer* should be referred to as *Trilobatus sacculifer* meanwhile (Spezzaferri et al., 2015). Furthermore, in that species your '*quadrocameratus*' morphotype is correctly referred to as '*quadrilobatus*' morphotype to my knowledge.

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REPLY: We changed the names in the revised manuscript in agreement with Spezzaferri et al. (2015) and Aurahs et al. (2011) as follows: *Globigerinoides ruber sensu stricto* changed to *Globigerinoides ruber* (white) *Globigerinoides ruber sensu lato* changed to *Globigerinoides elongatus* *Globigerinoides sacculifer* type changed to *Trilobatus sacculifer* (with sac) *Globigerinoides sacculifer trilobus* type changed to *Trilobatus sacculifer* (without sac) *Globigerinoides sacculifer quadrocameratus* type changed to *Globigerinoides quadrilobatus* *Globigerinella siphonifera* changed to *Globigerinella siphonifera/ G. calida/ G. radians* plexus

REFeree #3, COMMENT: 5. The most important issue is with the statistical analytical approach. According to lines 146–147 you are using a Pearson product moment correlation to test the relative abundances and shell calcification intensities of several species against environmental parameters. This is horribly wrong on a multitude of levels, as I will summarise hereafter. For further details you may have a look at Dytham (2011), Legendre and Legendre (2012), Faraway (2006), and McDonald (2009).

lâŒŒYou assume a causal relationship between environmental factors and SNW/species abundance. Correlation analyses are not appropriate here, regression analyses with SNW/abundance as the dependent variable against the independent environmental factors must be used. Occasionally this makes only a cosmetic difference (i.e. type I linear regression vs. Pearson product moment correlation), but even then it is of methodological and implicational importance (compare Legendre and Legendre, 2012, box 10.1). In this case, however, it is even more important because of the points below.

lâŒŒType I regression (as well as its correlation equivalent for that matter) is only applicable under certain circumstances, one of which is that x-values are measured without errors (McDonald, 2009; Dytham, 2011; Legendre and Legendre, 2012). It is therefore nearly only usable for laboratory experiments. As long as you are testing for the influence of parameters that you actually measured on board (temperature, salinity, pH), you might think this still works with a lot of good will, but I would argue that even

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then you have an error on those values, because you only have a snapshot image, and not a mean (let alone constant) value covering the entire life-time of your specimens. Further, I assume (you never state that) that at least part of the data you needed to calculate the carbonate system comes from averaged database data anyway!? And at least then, and in my opinion under all circumstances, you have to use robust type II or type III regression methods.

IIIâ€”You cannot simply test the same dependent variable against several independent parameters in different tests. The simple reason is that each of those test has its own type I error chance, and those are summing up until (after a sufficient number of tests) you are guaranteed to get at least one type I error in your analyses (compare Dytham, 2011; Legendre and Legendre, 2012). It is imperative that under such conditions at the very least all multiple tests (i.e. all tests for the influence of individual environmental factors on SNW or abundance per species) are corrected for this problem. Either using a correction for the family-wise error rate (e.g. Bonferroni correction), or a correction for the false discovery rate (e.g. Benjamini and Hochberg, 1995).

IVâ€”Making several such analyses and correcting them per species is still not the ideal solution, mainly because (as usual in marine environments) all independent variables show a large degree of multicollinearity (just have a look at your own Fig. 1). This means that such simple parameter-wise tests may detect an influence of several parameters, but only because they are highly correlated, and it is unclear which factor influences the dependent variable the most (or at all, for that matter). For the case of SNW in particular it might be much better to use an approach that can test for all independent variables at once, while reducing the influence of the multicollinearity between different environmental factors (Dormann et al., 2013). Such methods could for instance be generalized linear models (GLM) or generalized additive models (GAM), both of which have the added benefit over multiple linear regression that they are invariant to the order in which independent variables are added to the model (compare Faraway, 2006). For relative abundances you face the additional problem, that y-values

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are not independent of each other within a sample (e.g. if *G. ruber* already represents 50% of the assemblage, then *G. bulloides* cannot be more abundant than 50% anymore in that same sample). While there are ways around this (most notably, using absolute abundances with an appropriate link function in a GLM, or applying any of the methods described in van den Boogart and Tolosana-Delgado (2013)) you may also prefer to analyse the assemblage data using suitable ordination techniques (compare for instance Hammer and Harper, 2006). This would have the added benefit that such ordination techniques can also be adapted to properly compare your assemblage with that of earlier studies, in this way delivering on a promise made in the introduction and never fulfilled in the manuscript.

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 performed on the environmental parameters. Note that new environmental parameters will be added: the nutrients (NO₃ and PO₄), the oxygen concentrations and the pCO₂. 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 become warmer, saltier and more oligotrophic eastward. 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 pCO₂. 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.

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2 Detailed comments

COMMENT: Line 33, 'calcareous zooplankton': I would be very careful talking about zooplankton here. While it is true that all planktonic Foraminifera can live heterotrophic, many are also able to harbour photosymbionts. REPLY: We decided to change it to "calcareous plankton" to avoid possible confusion.

COMMENT: Line 35, 'Hembelen et al., 1989': Should be 'Hemleben et al., 1989'. REPLY: Changed in the revised manuscript.

COMMENT: Line 36, 'due to': Should be 'and show'. REPLY: Changed in the revised manuscript.

COMMENT: Lines 36–37, 'The species are adapted [...] spines and test shape.': They are certainly adapted to different environments, because naturally there cannot be any two species which occupy exactly the same niche, but implying such a trivial form of adaptation is far too oversimplified. _ Line 37, 'test shape': Should be 'shape, which are partly related to those adaptations'. REPLY: Changed as it follows in the revised manuscript. "The species are adapted to different environments and show differences in wall structure, pores, spines and test shape, which are partly related to those adaptations."

COMMENT: Lines 37–39, 'The distribution of foraminifera [...] which shifts during ontogeny': A citation for this statement is needed. REPLY: We added the following references for that statement in the revised manuscript : Schiebel and Hemleben (2005); Hemleben et al. (1989).

COMMENT: Lines 42–45, 'Ecological tolerance limits [...] departure from optimum conditions (Arnold and Parker, 1999)': Which is basically true for every organism, so what is the point here? Plus, this is hardly the best citation for this statement. What about Bé (1977) for example? REPLY: We consider that sentence can help some readers to understand better the article, despite others not having any new information

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from reading it. In that sentence we include the citations that prove the cause of having more or less abundance of foraminifera in a location. We include Bé (1977) in the citations. Also, that sentence provides the information that presently these boundaries are not completely defined, and work for it is still needed.

COMMENT: Lines 48–50, 'The first modern study of planktic foraminifera [...] expedition of 1947–48.': Was this study published? Cite a source. REPLY: We added the following reference in the revised manuscript: Petterson (1953).

COMMENT: Line 54, 'at 250 m depth': Should be 'of the upper 250m water column'. REPLY: Changed in the revised manuscript.

QUESTION: Line 57, 'that': Should be 'that the'. REPLY: Changed in the revised manuscript.

COMMENT: Lines 57–61, 'Thunell (1978) studied samples [...] inside the Mediterranean.': Break up this sentence. REPLY: We change it as it follows in the revised manuscript: "Thunell (1978) studied samples from the upper 2 cm of cores covering the Mediterranean, concluding that the distribution of planktic foraminifera is closely linked with the distribution of the different surface water masses. There are specific temperature and salinity ranges for each water mass, as Bé and Tolderlund (1971) stated for the Atlantic, and a partial isolation effect in the different basins and sub-basins inside the Mediterranean. Those phenomena result in different species assemblages in each region."

COMMENT: Line 65, 'wide': Should be 'large'. REPLY: Changed in the revised manuscript.

COMMENT: Lines 65–66, 'They concluded [...] variable foraminifera assemblages.': This is not entirely correct. Pujol and Vergnaud Grazzini (1995) only state that the observed assemblage patterns 'cannot be entirely explained by the general temperature and salinity differences among the different Mediterranean Basins' and are also

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strongly correlated to more regional hydrogeographic patterns. REPLY: It is true that in the sentence of the Abstract of Pujol and Vergnaud-Grazzini (1995) that they do not discard the temperature and salinity to explain their results, but also they state that the hydrogeographic patterns that regulate the nutrient dynamics have stronger weight on them. In the conclusion section they state it more clearly than in the abstract. From Pujol and Vergnaud-Grazzini (1995): “Although the distribution patterns of many species display strong differences between the two sampling periods, there is no direct correlation with sea surface temperature or salinity gradient changes. In fact, the rather large west to east gradients in temperature and salinity are not reflected in the relative or absolute abundances of the different species. The strong seasonal and regional variability of other hydrochemical parameters such as nutrients and of physical structures such as eddies or fronts may explain part of the observed differences in the distribution patterns.”

As in our sentence, we are not discarding the possibility of a temperature-salinity effect on them, despite these two parameters alone not varying enough to justify the extremely variable foraminifera assemblages, we think that there is no need to modify it.

COMMENT: Lines 70–72, ‘The calcification of foraminifera [...] (Schiebel and Hemleben, 2005).’: Those are neither the only factors influencing shell calcification intensity in planktonic Foraminifera, nor are all of the stated relationships universally true. Compare Marshall et al. (2013, tab. 1) and Weinkauf et al. (2016, tab. 7) for a summary of this matter. REPLY: We appreciate your references here. We propose the next modification: “The calcification of foraminifera is affected by the chemical state of their surrounding waters. Theoretically their shell mass is positively related to temperature, pH, [Ca²⁺], and alkalinity from its ambient water and negatively related with [CO₂] (Schiebel and Hemleben, 2005). In the different practical studies with water column plankton their shell mass was tested as positively related with [CO₂] (Aldridge et al., 2012; Beer et al., 2010a; Marshall et al., 2013; Moy et al., 2009) but also negatively

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(Beer et al. 2010a). Also, other studies relate positively foraminifera shell mass with temperature (Mohan et al. 2015; Aldridge et al., 2012; Marshall et al., 2013).”

COMMENT: Lines 70–77: I think the cited literature for calcification studies is by far not exhaustive. What about Broecker and Clark (2001b), Barker and Elderfield (2002), de Villiers (2004), Manno et al. (2012), and Marshall et al. (2013), to name but a few. REPLY: We focus on living plankton from tows and how the environmental parameters affect their calcification. The literature cited, despite not being exhaustive, represents the living foraminifera calcification studies. We propose to include the following sentence at line 75: “For further studies relating foraminiferal calcification influenced by environmental parameters see Weinkauf et al. (2016); Table 7. Since the industrial era. . .”

COMMENT: Line 76, ‘building’: Should be ‘formation’. REPLY: Changed in the revised manuscript.

COMMENT: Lines 82–83, ‘In addition, few size-normalized weight (SNW) studies from water column foraminifera are available in the literature.’: Then please provide such examples here in the form of citations. REPLY: We provide the following citations in the revised manuscript: Schiebel et al., 2007; Beer et al., 2010a; Aldridge et al., 2012; Marshall et al., 2013; Mohan et al., 2015; Marshall et al., 2015; Weinkauf et al., 2016).

COMMENT: Line 91, ‘more unbreakable tests’: Should be ‘tests with thicker walls’. REPLY: Changed in the revised manuscript.

COMMENT: Line 92, ‘empty tests are passive particles that ocean currents may displace.’ Which is perfectly true for living Foraminifera as well; hence they are plankton, not nekton. REPLY: We are in agreement that this characteristic is accomplished for plankton and not nekton. We considered no modifications in that sentence.

COMMENT: Lines 97–98, ‘(2) characterize, at the species level their ecology through their seasonal and geographical distribution and abundance by comparison with pre-

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vious studies;': This point is not really present in the paper, at least not above a relatively comparative level. The interpretation why abundances might be different now than they were 20 years ago, and any reliable analysis and graphical presentation that shows that in the first place, is largely missing. REPLY: The numbers of available studies generating data of this kind (water column planktonic foraminifera abundances, etc.) are extremely rare overall, and especially in the Mediterranean Sea. Therefore we are forced to compare with sediment trap and surface sediment (core-top) results from prior decades in this marginal sea (Rigual-Hernández et al., 2012; Bárcena et al., 2004; Hernández-Almeida et al., 2011; Thunell, 1978). Based on these sample format differences, time differences (e.g. late 20th century vs. early 21st century), and likely other differences as well, the basis for such comparisons is of course very far from perfect and ideal. However, given the rarity and recency of this new water column data set, we naturally use it to speculate on the comparisons and what they might reveal about changes going on the surface ocean environment in this region. This is what we can do and what anyone can do with the data in hand. To compare with comparable data from prior studies is a natural discussion aim based on it, even if the basis for the comparison is far from ideal.

COMMENT: Line 103, 'with a strong thermohaline and wind-driven circulation,': Citation needed! _ Lines 105–106, 'These basins are composed of different sub-basins due to partial isolation caused by sills that influence the water circulation, and by different water properties.': Citation needed! _ Lines 107–109, 'where the nutrient-rich Atlantic surface waters [...] (evaporation exceeding precipitation)': Citation needed! _ Lines 113–116, 'In the eastern basin, [...] and fresher toward the western basin.': Citation needed! _ Lines 117–118, 'Waters returning to the Atlantic through the Strait of Gibraltar at depth are cooler and saltier than the inbound waters, and compensate for the inflow from the Atlantic.': Citation needed! REPLY: See the answer of these questions in the major comment (2.).

COMMENT: Lines 106–107, 'World Ocean': Should be 'worlds oceans'. REPLY: We

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propose to change it to "ocean" instead of "worlds oceans". Now the sentence would be like the following: "Natural connection with the ocean is through the narrow Strait of Gibraltar,"

COMMENT: Line 111, 'until the': Should be 'and reach as far as the'. REPLY: Changed in the revised manuscript.

COMMENT: Line 135, 'at 200m depth': Should be 'from 200m depth to the surface'. REPLY: The towing is realized mainly at 200 m depth, but meanwhile it goes down and it returns up to the vessel, also the tows can catch samples. To clarify that we change "at 200m depth" for the following: "primarily 200 m depth, but also including tow time integrating the upper water column from 200m to the surface".

COMMENT: Line 141, 'counted and separated by species and size': Should be 'split into fractions by size'. _ Line 142, 'to determine the absolute and relative abundances': Should be 'and planktonic Foraminifera were counted on the species level'. Furthermore, it is not mentioned which taxonomic system is used. It is most certainly not up-to-date (compare general comments). REPLY: We do not agree here. First, the separation was made by species, then by size. If we correct like that it would seem that the process was done in the opposite way. We change, in order to clarify, that sentence to the following in the revised manuscript: "From each sampling station, the foraminifera were isolated and identified at species level. [...] For each sample, each species was counted and isolated according to 3 size fractions (150–350 μm , ≥ 350 –500 μm , and > 500 μm) to determine the absolute and relative abundances." We include, in the revised manuscript, the references used for the taxonomic nomenclature of our found species, being part of the Methodology section: "We classified the different foraminifera species with visual identification with optical microscopy with the option of picking and turning the specimens to see their different sides. We followed the morphometric guidelines and taxonomic nomenclature proposed by Aurahs et al. (2011) for *Globigerinoides ruber* (white), *Globigerinoides ruber* (pink) and *Globigerinoides elongatus*. For *Trilobatus sacculifer* (with sac) and *T. sacculifer* (without sac)

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we used Spezzaferri et al. (2015). Hemleben et al. (1989) was used as a guide to classify *Globigerinoides bulloides*, *Orbulina universa*, *Globorotalia inflata*, *Globorotalia menardii*, and *Hastigerina pelágica*. *Globigerinoides quadrilobatus* was inferred from Papp and Schmid (1985). *G. bulloides* could not be differentiated from *Globigerina falconensis* in our samples and are treated together; the *G. bulloides*/*G. falconensis* plexus is referred as *G. bulloides* in our study. *Globigerinella siphonifera*/*G. calida*/*G. radians* plexus (see Weiner et al., 2015) is treated as *G. siphonifera* in our study.”

COMMENT: Lines 144–145, ‘Individuals of the same station and species within a 50 μ m diameter size constraint were weighed with a Mettler Toledo XS3DU microbalance (\pm 1 μ g of error).’: So I assume they were weighed together (single shell measurements would require a more precise balance). But were the measurements afterwards actually corrected for mean shell size per sample (MBW approach, Barker and Elderfield (2002)), or was the simple SBW approach used (Lohmann, 1995; Broecker and Clark, 2001a). The main problem is that in the latter case, Beer et al. (2010a) has shown that the SBW method is not fully effective in eliminating the shell size effect. Additionally, results cannot be independently replicated and tested when the exact methodology is not sufficiently described. Also, ‘error’ is the wrong term in this context, and ‘nominal precision’ should be used instead. REPLY: We weighed together a maximum of 5 individuals, always within the same 50 μ m size constraint. We decided to change “Size-Normalized Weight” to “Density Area” (A) in the revised manuscript. The latter denomination is less confusing and in agreement with previous work (Marshall et al., 2013). For further details see the methodology text addition to the new manuscript, found on the major comments section, comment (2.). Changed the word “error” for “nominal precision” on the revised manuscript.

COMMENT: Lines 146–149: It is not mentioned anywhere which software was used to carry out statistical analyses. REPLY: On the revised manuscript we will include a principal component analysis (PCA; Varimax rotation) using SPSS Statistic 23 software.

COMMENT: Lines 147–149, ‘Absolute abundances [...] observed within the environ-

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mental parameters.’: This is no valid reason at all to skip this. It can be that you are not interested in this, then state why, or that you are concerned about the validity of the results, then state why. A large difference in values does not compromise such an analysis at all if the correct techniques are applied. REPLY: The relative abundances were used, as the samples have less variability and results correlate more, giving more importance to the species assemblages than the highly variable quantity of foraminifera in each station. Furthermore, now we carried a different statistical analysis (PCA) in which absolute abundances are considered.

COMMENT: Line 171, ‘*Globigerinoides ruber sensu strict (s.s.)*’: As mentioned in the general comments, this species is correctly referred to as *Globigerinoides ruber* (white). Please change in the entire manuscript. REPLY: In agreement with Auerhach et al. (2011) we change the nomenclature in the revised manuscript.

COMMENT: Line 174, ‘*Globigerinella siphonifera*’: Your species list contains only *Globigerinella siphonifera*, but neither *G. calida* nor *G. radians* (compare Weiner et al., 2015, and the general comments above). This could mean that either you checked and the other two species are not present at all, or you lumped the entire plexus into one category. Please explain what is the case here. REPLY: In agreement with Weiner et al. (2015) we change the nomenclature in the revised manuscript. *Globigerinella siphonifera* will be changed to *Globigerinella siphonifera*/*G. calida*/*G. radians* plexus. In the methodology section will be noted the use of the name *G. siphonifera* to represent the whole plexus further on the article.

COMMENT: Lines 176–178, ‘In addition, a higher percentage [...] and may not be generalized.’: Given the fact that in plankton tows you have only little control over the growth stage of your individuals, one may wonder to what degree this size trend over time may represent a reproduction event. REPLY: That is a reason why we add the last sentence of the paragraph referred in that question.

COMMENT: Line 180, ‘sample’: Should be ‘assemblage’. REPLY: Changed in the

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revised manuscript.

COMMENT: Lines 183, 187, 191, 197, '(Fig. 3; Fig 4)': The referred information is illustrated by neither of these figures, because Fig. 3 does not give shell sizes and Fig. 4 does not distinguish between species. Unless Fig. 3 would only represent the fraction >350 μ m, but then this is stated nowhere in the figure caption. REPLY: Now references to Fig. 3 and Fig. 4 are separated and located after the exact sentence each one. We also include the citation of Appendix A (where absolute abundance data for each size fraction in each species is provided) in the revised manuscript.

COMMENT: Line 192, '*Globigerinoides sacculifer*': This should be *Trilobatus sacculifer* (compare Spezzaferri et al., 2015, and general comments). REPLY: In agreement with Spezzaferri et al. (2015) we change the nomenclature in the revised manuscript.

COMMENT: Line 198, '*quadrocameratus*': Should be '*quadrilobatus*' in the entire manuscript. REPLY: Changed in the revised manuscript.

COMMENT: Line 218, 'A Pearson test': This is the wrong method for the question that should be answered (compare general comments). By the way, even if correlation per species was the correct approach, abundance data are by default not normally distributed but follow a Poisson distribution. This rules out any parametric test in the first place, and would leave Spearman rank-order correlation or Kendall rank-order correlation as the only reasonable alternative. Compare the general comments section, however, why neither of these is appropriate here. REPLY: See the answer of this question in the "major comment (5.)".

COMMENT: Lines 222–223, 'Relative abundance was selected instead of absolute abundance to avoid bias due to the big differences between stations' results in absolute abundance.': This approach, however, introduces new problems because now the abundances per station are not independent; and the given reason for this decision is invalid anyway. Compositional regression (van den Boogart and Tolosana-Delgado, 2013) or other adequate approaches would be needed. Compare general comments

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section. 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 understand 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. For the analysis we compare the PCA factors with absolute abundance and SNW, which will be treated in the results and discussion section, leaving the species assemblage only for comparison with previous literature.

COMMENT: Lines 220–222: All p-values reported here are invalid, because they have not been corrected for multiple testing on the species level. The general comments section gives more discussion about this. Additionally, why is nothing of that presented in a graphical form? REPLY: We performed a PCA analysis on the revised manuscript. See the answer at "major comment (5.)".

COMMENT: Lines 223–225, 'The remaining species [...] abundance and environmental parameters.': This is no reasonable explanation. The mere lack of the species at some stations would not rule out such an analysis, if there are still enough stations with values >0 left. REPLY: See the answer to that question in "major comments (5.)".

COMMENT: Lines 229–230, 'The high two-dimensional (silhouette) area-to-diameter correlation is best fitted by a power regression (Fig. S2)': As would be expected. But why is this important in the context of that paper? Additionally, from a purely modeling-point-of-view I might argue that the regression should be fitted so that they are forced to have a zero intercept (everything else seems wrong). REPLY: The relation between area and long axis in the three selected main species does not allow 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: "...The high two-dimensional

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(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. . .”

Size and mass of foraminifers relationship does not start at the origin (zero). The proloculus of planktic foraminifera measures between 15-30 μm in average, and has a certain calcite mass, which has so far not been determined (see Hemleben et al., 1989).

COMMENT: Lines 230–235, ‘Comparing the average values [...] northwestern Mediterranean (Fig. S2):’. If the idea is to compare shell sizes between different basins, then this is hardly the best method of presentation. A boxplot or barplot would be much more appropriate here. Further, it is stated nowhere which statistical techniques were used to test the shell size differences between basins. I assume an ANOVA followed by post-hoc tests, but this is explained nowhere. REPLY: We consider our graphical representation appropriate for the function it has. We will change the word “Comparing” to “Presenting”, to avoid confusion in the interpretation of the sentence.

COMMENT: Lines 237–239, ‘The diameter-to-weight relation [...] ($r^2 = 0.516$; Fig. S3):’. If you want to imply a dependency relationship (which can make sense, depending on your intention), then it would probably be more logically to assume that weight is dependent on size, so you should exchange the axis in your Fig. S3. Otherwise, here a correlation would be more appropriate. Furthermore, the question is again what is the sense of this analysis in the context of that paper. It should be made clear for the reader, why this analysis is performed. In agreement with that question we will exchange the axis in our figure in the revised manuscript. We find useful our “Weight vs. Long axis” study as its comparable with other studies in the literature (see Bijma et al.,

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2002; Lombard et al., 2010; de Moel et al., 2009; Aldridge et al., 2012; Schiebel et al., 2007) and make our own conclusions after its comparison (in our discussion section).

COMMENT: Lines 239–240, ‘*O. universa* was finally discarded for comparisons between SNWs at different locations due to a low area–weight correlation, while data from *G. ruber* s.s. correlate well (Fig. S4a):’. I do not really see the reason for this.

1) The weight–size relationship is not that bad (p-values are not given, interestingly).

2) I do not understand why the authors would insist in such a relationship to be a necessity for the interpretation of SNW. Sure, if there is no good relationship it would be difficult to predict shell size from shell weight or vice versa. But especially if you imply a relationship between calcification intensity and the environment you would expect to see deviations from this relationship. Otherwise, shell weight would be a function purely of shell size, and size-normalized shell weight would not have any value in environmental interpretations. Now, a lower R^2 value in *O. universa* in my opinion only means, that its shell weight is to an even lower extent controlled by shell size than it is in other species. This could mean, that *O. universa* is more susceptible to environmental protrusions in regard to its ability to control calcification, which would by some standard make it an even better proxy species. I can think of no reason why a low correlation value itself would make SNW interpretations invalid, however. REPLY: We decided not to show *O. universa* density area in Fig. 6 as no pattern was seen in its data but the data are presented in REV Fig. 1.

In the PCA realized for the revised manuscript we overlay the results of *O. universa* density area on the two factors obtained, which reflect environmental parameters of our sampled stations.

COMMENT: Lines 240–242, ‘The eastern Mediterranean [...] *G. ruber* s.s. (Fig. S4d-e):’. This is again not an appropriate way of presenting those results. Use a boxplot/barplot instead. REPLY: We consider our graphical representation appropriate for the function it has.

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COMMENT: Lines 243–244, ‘The eastern Mediterranean individuals have the lowest median SNW’: Is this just eyeballing or has it actually been tested somehow, which regions are different and which are not concerning SNW? REPLY: The median values were obtained from the density area approach; we observed that its values were lower in the eastern Mediterranean. We do not need a statistical test to know which is the smallest value. No statistical test was done regarding Fig. 6; on the other hand, statistically robust results regarding density area are presented in the revised manuscript with a PCA (see the answer to the question “major comments (5.)” for further details).

COMMENT: Line 245, ‘_g _môÃÃ2’: So from this unit I assume the authors yet used the MBW approach, instead of SBW!? It is imperative that this is made clear in the Methods section. REPLY: Yes, see answer to your comment at Lines 144–145 and the new methodology section written in the major comment (2.).

COMMENT: Lines 248–251, ‘A Pearson correlation test [...] correlation with fluorescence ($p = 0.01$)’: Apart from the fact that this technique is again inappropriate for the data (compare general comments and discussion for the abundance data) it is interesting that this important result is not graphically presented in any form. If such relations really exist, you should show them in the form of a figure. REPLY: See the answer to that question in “major comment (5.)”. A graphical representation of the PCA overlaid with the absolute abundance and density area results will be included in the revised manuscript.

COMMENT: Lines 252–253, ‘The Atlantic has [...] opposite trend as in *G. ruber* s.s.’: Again, eyeballing or tested? REPLY: The median and IQR values were obtained from the density area approach and whisker-box plot conversion. We do not need a statistical test to know which is the smallest value. No statistical test was done regarding Fig. 6; on the other hand, statistically robust results regarding density area are presented in the revised manuscript with a PCA (see the answer to the question “major comments (5.)” for further details).

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COMMENT: Lines 256–257, ‘*G. bulloides* is positively correlated with pH and [CO_2] ($p = 0.05$) in the Pearson test’: Which is again not shown in any graphical representation. REPLY: See the answer to that question in “major comment (5.)”. A graphical representation of the PCA overlaid with the absolute abundance and density area results will be included in the revised manuscript.

COMMENT: Line 280, ‘occurs in a’: Should be ‘come from’. REPLY: Changed in the revised manuscript.

COMMENT: Line 280, ‘season of the year’: Should be ‘seasons’. REPLY: Changed in the revised manuscript.

COMMENT: Line 283: Delete ‘eastern’. REPLY: Changed in the revised manuscript as follows: “...western Mediterranean abundances are higher than the eastern ones overall, due to more oligotrophic conditions and higher temperature and salinity values in the east that limit foraminiferal production during winter and late summer.”

COMMENT: Line 284: Delete ‘both’. REPLY: Changed in the revised manuscript.

COMMENT: Lines 285–286, ‘no significant differences are observed between samples collected during day and night’: Is this a subjective impression or was it tested statistically, because only in the latter case you should use ‘significantly’. Further, why is this not presented graphically somewhere? REPLY: We delete “significant” in the revised manuscript, as no statistically test was performed on that matter.

COMMENTS: Lines 287, ‘accounting for a single species’: Which is blatantly wrong for virtually every perceivable species concept. _ Lines 288–289, ‘*G. ruber*: sensu stricto, sensu lato (containing different cryptic species; Aurahs et al., 2009a),’: This is no up-to-date information in this regard anymore. Furthermore, the references contain only one ‘Aurahs et al. (2009)’, not an ‘a’ and ‘b’ version; please correct this. REPLY: In agreement with Aurahs et al. (2011) we delete that paragraph and we change: *Globigerinoides ruber* sensu stricto changed to *Globigerinoides ruber* (white) *Globigeri-*

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noides ruber sensu lato changed to Globigerinoides elongatus. Aurahs et al. (2009) has been removed from our references.

The deleted paragraph will be substituted by the following: “Comparing with previous studies that covered the Mediterranean, we notice that Thunell (1978) and Pujol and Vergraud-Grazzini (1995) did not find *G. menardii*, despite it being found in this study and Cifelli (1974), both in very low quantities. The lack of data from surface sediments and their tropical water preference suggest that is a new species in the Mediterranean (Cifelli, 1974), possibly caused by warmer conditions than in past times. The rest of the species found in our study are found in the past studies covering the Mediterranean Sea (Cifelli, 1974; Thunell, 1978; Pujol and Vergraud-Grazzini, 1995), but it remains in doubt if whether Pujol and Vergraud-Grazzini found *G. falconensis* and classified it as *G. bulloides*; or if Thunell (1978) found *G. elongatus* and *T. sacculifer* (without sac) and classified them as *G. ruber* and *G. sacculifer*. The former problem is also found in Pujol and Vergraud-Grazzini (1995). Also, it is not certain if Cifelli (1974) found *G. calida* and classified as *G. aequilateralis* (old equivalent of *G. siphonifera*). For the figures in Cifelli (1974) we deduce that *G. elongatus* was classified as *G. ruber* in the study. In the same way, we do not find any evidence of finding *T. sacculifer* (with sac) from the Cifelli (1974) figures, but we cannot discard the possibility of it being classified as *G. trilobus* (*T. sacculifer* without sac). Finally, we do not have the evidence if Cifelli (1974) found *G. ruber* (pink) and classified it together with the white variety into *G. ruber*.”

“To be able do a quantitative comparison of the number of species found with previous Mediterranean studies, first, we make the following simplification: *G. bulloides* and *G. falconensis* count as one species for that comparison; the same is applied for *G. siphonifera* and *G. calida*, and *G. ruber* (white) and *G. ruber* (pink). Secondly, we made the assumption that all the doubtful species found in previous studies (see two paragraphs above) were found (e.g.: we assume that Thunell (1978) found *G. elongatus* and he classified it as *G. ruber*). After applying these conditions we arrive at an “apparent number of species” able to be compared. Our apparent species becomes

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11, clearly inferior to Cifelli (1974) with 19 apparent species, and Thunell (1978) and Pujol and Vergraud-Grazzini (1995) with 17 apparent species. In station 3 of this study (Alboran Sea), we found 8 apparent species; meanwhile the number ascends to 12 in Rigual-Hernández et al. (2012) apparent species flux in the same month.”

COMMENT: Line 292, ‘with Cifelli (1974)’, ‘with Pujol and Grazzini (1995)’: ‘with’ should in both cases be ‘by’. REPLY: Changed in the revised manuscript.

COMMENT: Line 294, ‘reach’: Should be ‘reached’. REPLY: Changed in the revised manuscript.

COMMENT: Lines 294–295, ‘*Turborotalita quinqueloba*, *Neogloboquadrina pachyderma*, and *Globorotalia truncatulinoides*.’: Another problem for some species (certainly not *G. truncatulinoides*, but probably *T. quinqueloba* and potentially *N. pachyderma*) is that you used a 150 μ m mesh size. Most studies by default use 100 μ m for plankton net hauls, and part of the discrepancy you see (also in terms of general abundances) might be that you missed a lot of the small specimens. From my experience (compare Weinkauff et al. (2016) vs. Storz (2006)/Storz et al. (2009)) you can miss the majority of specimens in some species by just switching from 125 μ m to 150 μ m. In this regard, Pujol and Vergraud Grazzini (1995) used 120 μ m, potentially explaining a lot of your observed differences. REPLY: We treat that problem in the revised manuscript discussion section “5.1. Abundance and diversity patterns”. The problem in this question is addressed there 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 caused by their 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 μ m, escaping from our BONGO nets mesh size, a possibility that could be supported by previous Mediterranean studies with

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thinner mesh sizes finding that species (see Pujol and Vergraud-Grazzini, 1998, 120 μm mesh size; Rigual-Hernández et al., 2012, 63-150 μm mesh size)."

COMMENT: Lines 297–298, 'G. sacculifer type quadrocameratus was not found in previous studies': A potential problem with this statement is whether in those previous studies *T. sacculifer* has been consequently subdivided. While most studies I am aware of distinguish between the sacculifer- and trilobus-morphotypes, it is often unclear whether the quadrilobatus- (or immaturus-) morphotypes would be counted separately if discovered and truly are absent in the samples, or if they are by default pooled in with the trilobus-morphotype. REPLY: We treat that problem in the revised manuscript discussion section "5.1. Abundance and diversity patterns". The problem in this question is addressed there as follows: "G. quadrilobatus was not found in previous studies working with plankton tows in the Mediterranean, despite its abundance in sedimentary cores (i.e. Cramp et al., 1988; Rio et al., 1990); there exists the possibility to classify it as *G. sacculifer* or *G. trilobus* in previous studies as was suggested by Hemleben et al. (1989)."

COMMENT: Lines 300–302, 'The lower absolute abundance [...] recent years': Yes, it could. But again, given that Pujol and Vergraud Grazzini (1995) definitely used a finer mesh size, this could simply be the result of you missing a lot of specimens. I would therefore be very cautious with this interpretation. Berger (1969) provides equations with which observed abundances could be calibrated for different hypothetical mesh sizes, and such a correction of your data might provide a much better comparability with earlier studies. REPLY: We treat that problem in the revised manuscript discussion section "5.1. Abundance and diversity patterns". The problem in this question is addressed there as follows: "Note that our mesh size is bigger than Pujol and Vergraud-Grazzini (1995) and Rigual-Hernández et al. (2012), but is similar to Cifelli (1974): mesh size of 158 μm . The wider mesh size could be a cause of our lower numbers in absolute abundance and reduced diversity, but the bigger results in species diversity of Cifelli (1974) in June, a theoretical lower foraminiferal presence month than

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May (Rigual-Hernández et al. 2012) supports our statement."

COMMENT: Line 311, '(Fig. 4)': Again, as this figure does not distinguish between species it cannot illustrate the trends you describe here. REPLY: We add Appendix A reference here, where abundance per each size fraction is found.

COMMENT: Section '5.2. Factors controlling the abundance of the main species': The authors try to interpret each individual species' abundance in terms of seasonality and compare it with other studies. However, it is not fully clear what the purpose of this is supposed to be. Many of the described trends are not new, and while it is always good to replicate results, this should not be the main purpose of the manuscript. Rather, the comparison of abundances with studies from several years ago, and the interpretation of potential reasons for changes (as promised in the introduction) is largely missing. REPLY: We disagree with the notion that we do not deliver on the promise of detailed comparison against other studies. As described earlier, the nature of this data set is rare and we make comparisons as well as we can to other works. We also highlight that the basis for such comparisons is far from perfect given a number of factors such as sampling format, time of study (late 20th century vs. early 21st century), and more. Given this rare opportunity with our new data however, we profit from it as much as possible and exploit it as much as possible, and compare against prior works as well as we can. This naturally becomes a major discussion point of the paper.

COMMENT: Line 314, 'results': Should be 'samples'. REPLY: We propose "sample" better than "samples".

COMMENT: Line 324, 'Both varieties *G. ruber* sensu stricto (s.s.) and sensu lato (s.l.)': Those are not varieties but distinctly different species, *G. ruber* (white) and *G. elongatus* respectively. Moreover, they are not even sister-taxa, but *G. elongatus* is the adelphotaxon to *G. conglobatus*. While they have comparable environmental preferences, and might thus be pooled for such an analysis as you intend to do, they should under no circumstances be treated in a way that implies they are remotely the

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same species. REPLY: We agree (see you major comment (4.)). The discussion is focused now on *G. ruber* (white), also some information about its difference with *G. ruber* (pink) is provided. *G. elongatus* is discarded for discussion and no pool with *G. ruber* is done anymore. Paragraph of lines 324-327 is deleted.

COMMENT: Lines 324–325, ‘share similar habitats’: Yet they have different environmental preferences, with *G. elongatus* living deeper (Steinke et al., 2005; Numberger et al., 2009) and showing different seasonality (Weinkauf et al., 2016). REPLY: We appreciate those references. That paragraph is now deleted from the article.

COMMENTS: Lines 331–332, ‘as demonstrated by positive significant correlations with temperature in the *G. ruber* s.s. variety ($p = 0.01$)’: Not that I would oppose this interpretation, but it is yet derived from inappropriate analytical techniques. _ Line 338, ‘strong positive correlation with salinity ($p = 0.01$)’: Derived from invalid methods! REPLY: See the answer to those questions in “major comment (5.)”.

COMMENT: Lines 340–341, ‘The findings of Watkins et al. (1996) are supported by the negative correlations of standing stocks’: Are they? If Watkins was right, would you not expect no correlation at all between nutrient availability and abundance of *G. ruber*? Rather, it seems that *G. ruber* is faring less well in regions with more nutrients (if this trend is supported by proper statistical analyses, this is). This means that higher nutrient availabilities are negative for the species, maybe because it loses its competitive advantage against other species, or the higher nutrient concentration reduces light levels, thus hampering the photosymbiont activity. REPLY: We rephrase the sentence according to the PCA results conducted in the revised manuscript. The sentence will change as follows: “The findings of Watkins et al. (1996) are supported by our PCA results, where higher abundances are affine with low nutrient and fluorescence concentrations, with the exception of station 19 (REV Fig. 7e).”

COMMENT: Lines 341–342, ‘*G. ruber* s.s. and fluorescence data of our study ($p = 0.05$)’: Derived from invalid methods! REPLY: See the answer to those questions in

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“major comment (5.)”.

COMMENT: Lines 352–353, ‘Hydrographic conditions and consequently food availability seem to be the factors limiting more its abundance once it has reached its habitable temperature range’: Yes, but is this not what would be expected? Liebig’s law of the minimum is equally valid for protists and animals as it is for plants. REPLY: We are announcing what features limit its abundance (food availability determined by the lowered or enhanced stratification of the water column inside its temperature range). That sentence gives information of the environmental preferences of *G. bulloides*. We consider that no modification has to be done here.

COMMENT: Line 359, ‘shows’: Should be ‘, the species shows’. REPLY: Changed in the revised manuscript.

COMMENT: Line 368, ‘positive correlation with fluorescence ($p = 0.05$)’: Derived from invalid methods! REPLY: See the answer to those questions in “major comment (5.)”.

COMMENT: Line 372, ‘Raden et al., 2012’: Should be ‘van Raden et al., 2012’. REPLY: Changed in the revised manuscript.

COMMENT: Line 377, ‘specie’: Should be ‘species’. REPLY: Changed in the revised manuscript. COMMENT: Line 383, ‘opportunistic species’: Opportunistic species are such species which can cope with highly unstable and/or unfavourable conditions better than other species can do. They thus massively dominate environments where few other species can live, resulting in very low diversities in those environments. This is often a transitional process until the environment becomes more stable/habitable, after which the opportunistic species are replaced by a more diverse community, because in developed environments they are at a competitive disadvantage to such species. I therefore do not believe that ‘opportunistic’ is the correct term to describe *G. bulloides*, which is cosmopolitan and often occurs in rather diverse assemblages. REPLY: We decide to maintain the term “opportunistic” as is used also in Rigual-Hernández et al. (2012), Schiebel and Hemleben (2005), Ottens (1992), among others.

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COMMENTS: Line 384, 'It correlates with fluorescence peaks since it feeds on phytoplankton': Probably correct interpretation, but derived from invalid methods! _ Line 408, 'Its negative correlation with temperature ($p = 0.01$)': Derived from invalid methods! REPLY: See the answer to those questions in "major comment (5.)".

COMMENT: Line 417, 'only absent from': Should be 'being absent from only'. REPLY: Changed in the revised manuscript.

COMMENT: Line 428, 'even if this is not supported by our Pearson correlation.': Which is an inappropriate method anyways! REPLY: See the answer to those questions in "major comment (5.)".

COMMENT: Lines 438–439, 'The size-normalized weight (SNW) of tests of both *G. ruber* s.s. and *G. bulloides* are statistically significant': This statement is nonsensical, a value itself cannot be significant, it can only be significant in regard to a null hypothesis. I assume you refer to the fact reported in the Results section and Suppl. Fig. 4, that size and weight are not perfectly correlated in *O. universa* (otherwise I do not even know what you want to imply). However, as already mentioned above, this is in my opinion no prerequisite for the SNW to have a meaning. This is even leaving aside, that it is never established whether this relationship is really insignificant ($p > .05$) or if the R^2 value is simply too small for the authors' taste. REPLY: We appreciate the question of Referee #2 for noticing our mistake. We change the following sentence as follows: "The size-normalized weight (SNW) of tests of both *G. ruber* (white) and *G. bulloides* follow a systematic change from the Atlantic towards the eastern Mediterranean (Fig. 6)."

COMMENT: Line 439, 'follow a systematic change from the Atlantic towards the Eastern Mediterranean': This might be, but it was never properly tested or depicted graphically. REPLY: We consider Fig. 6 a proper way to show that pattern. With our PCA graphical representation that will be included on the revised manuscript also is noticeable that trend (REV Fig. 7).

COMMENT: Section '5.3.1 Unknown control of the SNW of *O. universa*': OK, now your

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regression between shell size and shell weight makes more sense, and it would have been good to explain this in the beginning already. I do appreciate that you discuss this possibility of cryptic diversity and gametogenic calcite meddling with your data. However, what André et al (2014) detected are subtypes, they do not even rank on the species level. On that level you have also several subtypes in *G. ruber* and *G. inflata*. To be honest, it could be that the lack of strong correlation between size and weight in *O. universa* results from such an effect that the subtypes react differently. But it can still as well be, that this species simply reacts more heavily towards environmental factors concerning its calcification. I would thus not go so far as to categorically rule out that species for a calcification analysis, because you simply do not know what is the case here. It is still interesting to see SNW values for that species as well, although they might suffer from higher uncertainty. Even more so since despite a large spread, the correlation between size and weight does not seem to show bimodality (indicative for the cryptic species problem), and possibly the SNW data would not do so either. Conversely, the values seem to show a wider spread for larger shells, which can mean that gametogenic calcite is more of a problem, or simply truly that this species is more variable in calcification intensity (then presumably influenced by environmental factors). After thoroughly discussing why this might be a less reliable signal, I would therefore still want to see how SNW in *O. universa* scales with environmental factors. REPLY: See the density area of *O. universa* in Fig. 6 format (REV Fig. 1) in the answer of the question about lines 239–240. Also, *O. universa* density area results are considered in the new PCA. Here is the graphical representation of it (to see the environmental parameters loading the two factors see REV Fig. 7a).

COMMENT: Lines 441–443, 'In contrast, [...] environmental effects.': Incorrect! The strict correlation between size and weight may not exist, but this only means that especially in this species there must be other factors influencing calcification intensity. REPLY: We change the following sentence as follows: "In contrast, changes of the SNW of *O. universa* do not create any trend within locations (Figs. S2c, S3c, and S4c; Fig. 7j), and cannot be used to identify and quantify particular environmental effects."

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COMMENT: Lines 448–449, ‘Weight-area relation data do not show any statistically significant systematic distribution (Fig. S4c).’: You probably mean ‘correlation’, not ‘distribution’. REPLY: Changed in the revised manuscript.

COMMENT: Lines 453–454, ‘their pore-size is also affected by environmental conditions including water temperature (e.g., Bé et al., 1973).’: This statement is critical. Bé et al. (1973) did not know about different cryptic species. It might be that pore size is indeed influenced by environmental factors across all cryptic species, but also that cryptic species prefer different water temperatures and what Bé et al. (1973) interpreted as pore size changes within the species is simply the result of different species (with inherently different pore sizes) dominating different water masses. REPLY: It is true that we do not know if in cryptic species it will be like this without genetic studies; the citation of Bé et al. (1973) just contributes to the possibility of such a relation in cryptic species as well.

COMMENT: Line 476, ‘nutrient concentration and food availability.’: Which is basically the same thing in the context of this study, isn’t it? REPLY: We agree, “nutrient concentration” eliminated from that sentence in the revised manuscript.

COMMENT: Lines 476–478, ‘However, in contrast to *O. universa*, the SNW data of *G. ruber* and *G. bulloides* follow systematic distributions, which are statistically significant.’: It is again not clear what you mean with ‘distributions’. All data have a distribution, and values themselves cannot be significant or insignificant. I assume you refer to a significant correlation between size and weight in those species. REPLY: We agree. “distributions” removed by “correlations” in the revised manuscript.

COMMENT: Lines 478–480, ‘High SNW in the Atlantic [...] also noticeable in Fig. S2d-e and Fig. S4d-e).’: Those graphs are all not appropriate to show that. Rather, an actual crossplot between SNW and the individual environmental factors must be shown. Interestingly, this trend is reversed to what has been reported from the Azores Front (Weinkauff et al., 2016). REPLY: We consider our graphical representation appropriate

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for the function it has.

COMMENT: Lines 480–482, ‘At the same sites, [...] interpretation of the data (Fig. 6).’: Which could be shown effectively by calculating and presenting the coefficient of variation at those stations. Additionally, how could this trend then be interpreted? REPLY: Figure 6 is simply a box plot comparison that yields information on how population statistics differ across regions. There is no correlation among properties here and therefore no “coefficient of variation” to present on a station-specific basis, as far as we understand the reviewer comment. We currently fail to understand what calculation method the reviewer is suggesting here, and wonder also if it is beyond the scope of this study.

COMMENT: Lines 485–486, ‘The relationship between food availability and SNW in *G. bulloides* is opposite to that in *G. ruber* s.s. (Fig. 6).’: A better figure is needed to illustrate this. REPLY: We consider our graphical representation appropriate for the function it has. In the revised manuscript, we add the figure of the overlaid density area results of both species: shown in the answer to the question about line 439.

COMMENT: Lines 488–489, ‘In both species *G. ruber* s.s. and *G. bulloides* larger IQRs are found toward higher absolute SNW.’: Which is perfectly normal stochastic behaviour. This is why it is important to normalize variation for expected value by reporting the coefficient of variation instead of raw variation under such circumstances. REPLY: As also described above, in our comment to the reviewer comment about lines 480–482, we are unsure about what statistical method and / or calculation the reviewer is referring to here. Is there a distinct suggestion of some kind, with a reference? We are not sure how to calculate a “coefficient of variation” with regard to box plots and their statistics.

COMMENT: Lines 490–492, ‘An opposite trend in SNW [...] growth conditions.’: I assume this refers to Beer et al. (2010b). Please cite your sources properly. REPLY: We add the following citation to the revised manuscript: Beer et al. (2010a): Beer, J.,

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Schiebel, R., and Wilson, P. A.: Testing planktic foraminiferal shell weight as a surface water [CO₃2-] proxy using plankton net samples, *Geol. Soc. Am.*, 38, 103-106, 2010a. COMMENT: Line 494, 'Köhler-Rink and Köhl, 2005': This citation is missing in the list of references. REPLY: We agree. Done.

COMMENT: Lines 496–497, 'additional calcite layers might be added to the proximal text surface before reproduction, similar to the process described for *O. universa* (see above)': Yet to my knowledge, those two species are not known for excessive amounts of gametogenic calcite (e.g. Deuser, 1987; Hamilton et al., 2008). Also, the alternative interpretation would be that more optimal conditions trigger faster growth and earlier reproduction, resulting in a trade-off for calcification intensity of each individual chamber already during growth (i.e. before gametogenic calcite is added). Additionally, 'text' should be 'test' (Line 496) REPLY: It has not necessarily to be an excessive calcite addition, just enough to be detected. We appreciate your interpretation here. "Text" substituted by "test" in the revised manuscript.

COMMENT: Lines 505–506, 'However, the comparison might be biased by the fact that *G. ruber* s.s. and s.l. morphotypes were analyzed together in the study of de Moel et al. (2009)': It most certainly is. Compare Weinkauf et al. (2016). REPLY: Weinkauf et al. (2016) will be taken in to account for the useful density area results that it presents.

COMMENT: Lines 514–516, 'All of these [...] in an increased SNW': They also support the interpretation, that a multitude of factors influences shell calcification in planktonic Foraminifera. REPLY: We agree with that statement. In oligotrophic regions, like the Mediterranean Sea, planktic foraminifera calcification is affected by a combination of factors like carbonate saturation and food availability (Beer et al., 2010a; de Villiers et al., 2004).

COMMENT: Line 517, 'given that carbonate chemistry does not limit calcite formation in planktic foraminifera.': This is a blatant misrepresentation of basically the entirety of existing literature (compare Marshall et al. (2013, tab. 1) and Weinkauf et

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al. (2016, tab. 7)). REPLY: We will clarify this point raised by the reviewer. In fact the overall conclusion 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 [CO₃2-] 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 [CO₃2-] 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; Fig. 2; MEDAR: 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). 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., 2010a; 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 surface sea carbonate chemistry will have a major effect on the SNW of planktic foraminifera, even if this is of relatively low influence today.

COMMENT: Line 522, 'reflect high': Should be 'show large'. REPLY: Changed in the

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revised manuscript.

COMMENT: Line 526, 'ten morphospecies in total.': This is wrong since at least the individual species *G. ruber* (white), *G. ruber* (pink), and *G. elongatus* have been pooled together. Furthermore, it is unclear whether *G. calida* and *G. radians* also occur and have been pooled into *G. siphonifera*. REPLY: See the answer to the question about line 287, from the Discussion section. We will change it as well in the Conclusions section of the revised manuscript.

COMMENT: Line 548, 'These observations highlight the need for more interdisciplinary studies on the causes of changing foraminiferal assemblages and decreasing shell production': If this is supposed to hint at the promised comparison with earlier studies then I must state again that 1) since you used a larger mesh size without correcting your data for that fact you cannot compare your abundances with those of earlier studies and 2) you never presented a thorough discussion whether species compositions have been significantly changing during the last 20 years and if so, why. REPLY: See the answer to your question about lines 300–302 for point 1). See the answer to your question about lines 97–98 for point 2).

COMMENT: Lines 588–589: There is no Bijma et al., 1990a, so remove the 'b' after the year. REPLY: Changed in the revised manuscript.

COMMENT: Lines 625–626: Ivanova et al. (2003) is not cited anywhere in the manuscript. Remove from list of references. REPLY: Changed in the revised manuscript.

COMMENT: Lines 650–651: 'Grazzini' should be 'Vergnaud Grazzini'. REPLY: Changed in the revised manuscript.

COMMENT: Lines 682–683: '*Orbulina universa*' should be set in italics. REPLY: Changed in the revised manuscript.

COMMENT: Caption Fig. 1, '(a) Temperature (°C), (b) salinity, (c) fluorescence (μg _

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l^{-1}), (d) pH, and (e) $[\text{CO}_2]$ ($\mu\text{mol kg}^{-1}$): Information where these data come from are missing completely. Additionally, the software used for plotting (I assume Ocean Data View, Schlitzer (2014)) has not been cited. Especially Section 2, and to a lesser extent Section 3 involves a huge amount of interpolation due to the large spatial distance between measurement profiles. This makes the reconstructions very unreliable. REPLY: The source of our data is solved on the methodology section of the revised manuscript; see the answer to "major comment (2.)", where it is specified. Fig. 1 software source will be cited on the figure legend with Schlitzer (2016): Schlitzer, R.: Ocean Data View, <http://odv.awi.de>, 2016. We consider that the aim of Fig. 1 to show the environmental parameters of the Mediterranean Sea is suitable, even if local hydrographic features are not presented here.

COMMENT: Caption Fig. 2, 'First leg: 1 to 13, second leg: 14 to 22.': It might be nice to distinguish the cruise-tracks of the two legs by colour. REPLY: The two legs represent two different lines in Fig. 2, which is why we consider it unnecessary to distinguish by colour.

COMMENT: Caption Fig. 2, 'MODIS Aqua (L2)': What is this? This source has not been cited in any way (published article, url, ...) and was not mentioned in the Material and Methods section. REPLY: MODIS (Moderate Resolution Imaging Spectroradiometer) Aqua L2 is a NASA Satellite that views the Earth's water surface to acquire data to understand global processes and dynamics. In the new methodology we mention it; see the answer to your question in major comments (2.). The reference is: NASA Goddard Space Flight Center, Ocean Ecology Laboratory, Ocean Biology Processing Group; (2013): MODIS-Aqua L2 Data; NASA Goddard Space Flight Center, Ocean Ecology Laboratory, Ocean Biology Processing Group. <http://oceandata.sci.gsfc.nasa.gov/>. Accessed on 06/06/2013.

COMMENT: Caption Fig. 2, 'from the closest day as possible': Which means exactly what? 1 day, 10 days, 100 days,...? Also, I would have assumed the dates given in the map are the dates for which chlorophyll data have been plotted, or what else is

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displayed there? REPLY: Chlorophyll a data was measured the day that the satellite image was available, noted in the upper part of the figure. The reader can see the days that separate the towing and the chlorophyll a values looking Table 1: i.e. Stations 1, 2, 3 were sampled days 3rd to 5th (Table 1) and the satellite image and its corresponding Chlorophyll a values are from day 5th (Fig. 2). We clarify Fig. 2 legend to avoid confusion as follows: “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, specified in the upper part, of the first leg transect.”

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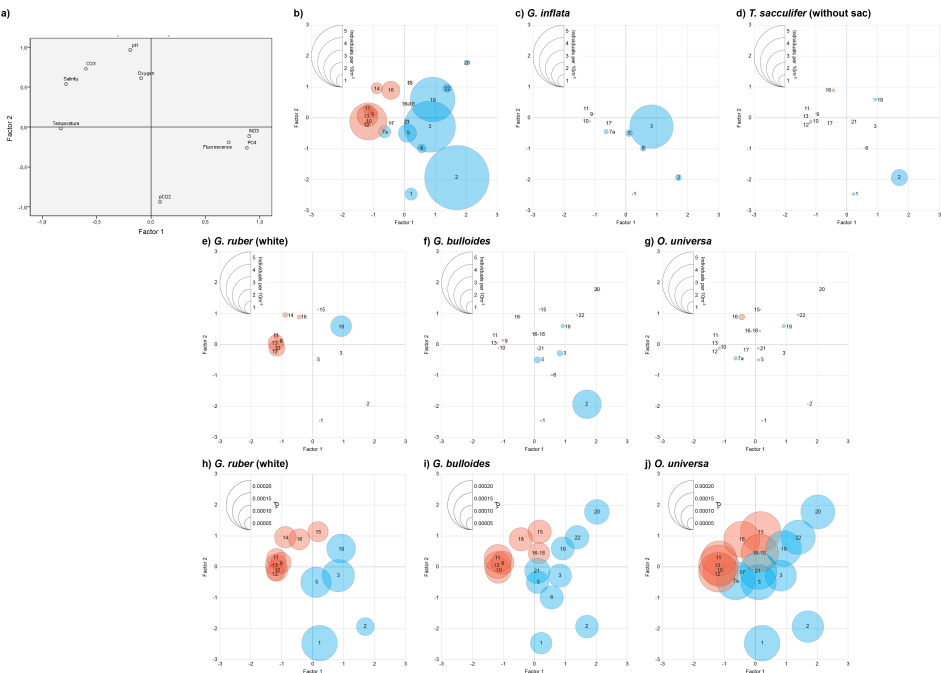
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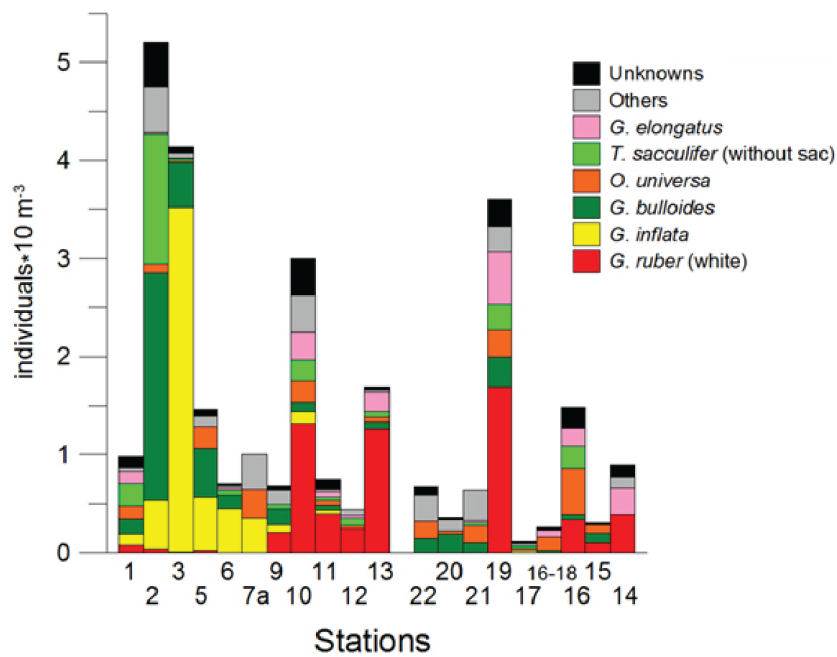
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REV Figure 7: Sample scores on the two PCA factors with (a) the loadings of the environmental parameters on each factor, (b) with overlay of the absolute abundance values (individuals $\times 10^{-3}$) of every station of all the foraminifera sample, (c) *G. inflata*, (d) *T. sacculifer* (without sac), (e) *G. ruber* (white), (f) *G. bulloides*, and (g) *O. universa*. With overlay of the pA values ($\mu\text{g}\cdot\text{m}^{-2}$) 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.

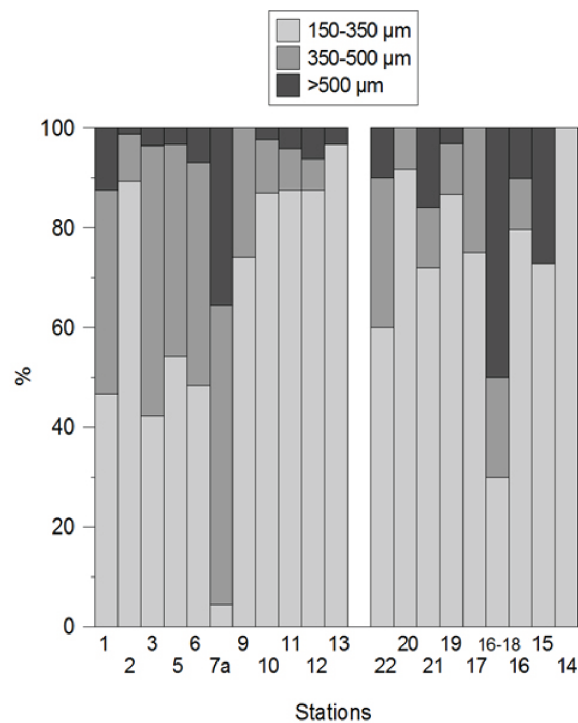
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REV Figure 3: Absolute abundance of planktic foraminifera from BONGO nets during leg 1 (stations 1 to 13) and leg 2 (stations 22 to 14). Category 'Others' is comprised by *G. siphonifera*/*G. calida*/*G. radians*, *G. quadrilobatus*, *H. pelagica*, *G. ruber* (pink), *G. menardii* and *T. sacculifer* (with sac).

Fig. 2.

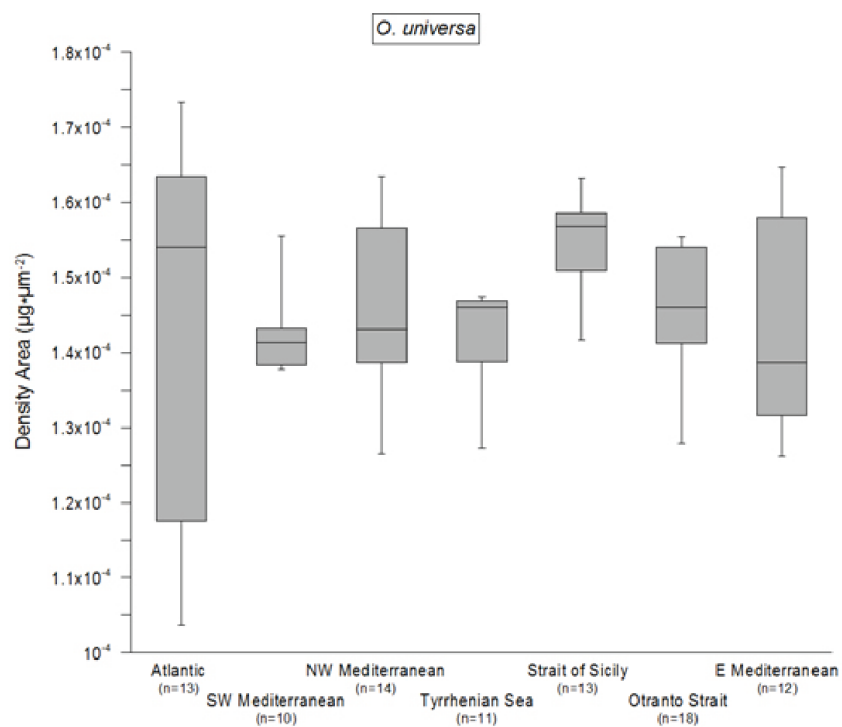
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REV Figure 4: Percentage of each planktic foraminifera size fraction in leg 1 (stations 1 to 13) and leg 2 (stations 22 to 14).

Fig. 3.

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REP Figure 1: Average density area for *O. universa* according to the different sub-regions of the Mediterranean Sea.

Fig. 4.