

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

**Anonymous Referee #1**

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Mallo et al. present new data on planktonic foraminifera abundance and shell weight from the upper water column in the Mediterranean Sea. They observe large differences in the species composition across the basin and suggest a suite of parameters (stratification, temperature, food availability etc) that could be responsible for the observed patterns. They also investigated the size-normalised weight (SNW) of two species and suggest that food availability, rather than carbonate chemistry of the seawater, may be the dominant control on shell weight of these species.

The data appear of good quality and manuscript is well written and most of the figures are clear. It is also good to see that the data are made available. I think this paper could be a valuable addition, but the interpretation of the data is not well thought through

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and clearly needs more work/analysis. At present it is not really clear what is new about this study, or – and that wouldn't be a disadvantage – how existing concepts on foraminifera abundance and shell weight variability are confirmed. In addition, I have numerous small questions and some major doubts about the (statistical) treatment of the data and hence the interpretation.

#### Major comments

Unclear methodology for SNW: what diameter was measured? Neither *ruber* or *bulloides* approximates a sphere, so this is important to mention. 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?

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 be 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.

SNW regressions: first of all, the rationale behind the regressions isn't clear to me. Why investigate the relation between area and diameter? Why is diameter of interest if one normalises to area in SNW? 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. 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.

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  $x^3$ ?

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 should be shown.

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.

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.

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).

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

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from the paper.

Minor/technical comments

Frequent use of locations that are not indicated on a map. Please make sure that each locality is indicated.

Fig.1: use same x axis scale for each panel.

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.

Fig. 4: what is 'n' below the graphs (16-18 > 16?)?

L38: perhaps replace radiation with sunlight.

L39: not only depth habitat, also seasonality.

L50: provide a reference for the expedition.

L52: large not high abundance variations.

L57-69: From this §it 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.

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.

L70: Consider changing 'Its weight..' by 'Their shell mass...' to be more consistent.

L73: What's the conclusion, implication of mentioning of the De Beer study?

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L78: Studies of the water column in the Mediterranean (or similar); not Mediterranean studies.

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.

L112: Gulf of Lions

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.

L154: what are unclassified specimens?

L166: remove location 1 from the list, abundances are clearly different there. Also refer to figures in this section.

L210: ... dominance 'of a single species'...

L233: how was significance determined? P-value?

L243; 252: how were the locations grouped?

L249: add SNW after G. rubber

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).

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Sampling at the end of spring could thus easily have missed them.

§5.2: please separate more clearly what are new results and what is existing knowledge.

L366: '... ranges as the ...'

L367: '... and it was also found ...'

L368: what characteristic of inflata shows this correlation? I don't follow this conclusion.

L377: 'In accordance with...' and '... Atlantic station...' (there is no causal link and only one Atlantic station).

L378: there isn't a station completely dominated by a single species, please reword.

L439: what is meant with the SNW is statistically significant? What was tested, with which confidence interval, using which test?

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?

L478: the distributions are significant? Please reword.

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.

L511: reword '... heavier average weight-diameter relation... '.

L515: reproduction? Not calcification?

## References

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