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Reply on RC1

Thibauld M. Béjard et al.

Author comment on "Calcification response of planktic foraminifera to environmental change in the western Mediterranean Sea during the industrial era" by Thibauld M. Béjard et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-711-AC2, 2022

First of all, authors would like to thank referee #1 for such a precise and helpful review of our work and its positive feedback on the value of our data. All observations and ideas provided have helped to greatly improved this manuscript. We hope the answers and information provided here would respond to what was demanded.

Here we detail the answers to the questions asked and all the changes and corrections applied to the document in order to integrate the changes suggested by referee #1.

Here, **R1-C** stands for referee #1 **Comment**, and **R1-R** stands for authors **Response**. Referee #1 comments are marked in italic.

As a lot of changes have been proposed, find here a quick abstract of what we think are the main changes that have been suggested:

- Statistical tests and analysis have been added. First, in order to tests the difference and independence between SBW, area and MBW, a square chi test has been carried out. Also, in order to test the differences between the sediment trap and sediment core calcification data, a non-parametric Mann-Whitney variance test has been used. Finally, a General Additive Model has been developed in order to plot seasonal and interannual patterns and for trying to identify which environmental parameters are the most likely to affect calcification on different time scales.
- The ^{14}C ages of the core-tops and sediment cores has been calibrated to calendar years BP considering the marine reservoir effect and the local reservoir effect. This has been done using the CALIB 8.2 online program. Results showed that 2 of the core-tops previously presented as pre-industrial, are in fact dated from the industrial period. Therefore, discussion around global change and ocean acidification from the pre-industrial time scale has been now adapted in order to integrate these new dates.
- A clearer link has been made with the ecology of the species. For that, a new section has been added in the material and methods focusing on the life cycle of the three species studied here. The seasonal calcification discussion now focuses more on the ecology of the species.
- The pre/post-industrial discussion now also acknowledges other mechanisms than global ocean acidification. Parameters such as temperatures variations, interannual flux-variations and oceanographic changes are now discussed.
- The figures have therefore been adapted in order to integrate the changes suggested.
- All data used for this work will be provided according to the Copernicus data policy.

Main comments

R1-C1*: Please show the raw data (individual measurements, not the mean values) and the shell fluxes before showing/analysing the intra and interannual variability. At first I was under the impression that the entire time series was analysed, but I think the authors only analysed selected samples for each species (and not the same samples for each species).*

R1-R1: The entire time series has been analyzed, however, not every sample presented an enough amount of planktic foraminifera individuals to carry on the weighing and measurements. We therefore analyzed all sediment trap samples that contained enough *G. bulloides*, *N. incompta* and *G. truncatulinoides*. Sometimes, these 3 species were abundant enough in the same sample, and in some other samples, only one or two of these species were abundant enough. That it's why we could not have and constant calcification pattern for all the three species in the sediment trap, due to the material limitation. The amount of individuals needed for our analysis was described in **section 3.4.** This problem was not encountered in the sediment core material, where individuals from all 3 species were abundant enough and that's why we have the same number of samples analyzed for all 3 species there. The amount of samples for each species analyzed in the sediment trap is displayed in **Table 2**, in the **Total samples**column. On top of that, a table has been added in the supplementary material with all the samples analyzed from the sediment traps from each species, with their corresponding date, SBW, MBW, area and number of individuals.

R1-C2: *To the best of my knowledge Rigual-Hernandez et al. (2012) presented shell fluxes >150 µm. How do the shell fluxes in the specific size fractions analysed here relate to those? In other words, how was the flux-weighting done? Based on size-specific shell flux data, or by assuming that the shell size remained constant and that weights for the calculations of the weighted averages are independent of size?*

R1-R2: Yes, Rigual-Hernández et al., (2012) presented shell fluxes from the >150um fraction, and that data is the one used in this study. No new flux data has been generated here and the flux-weighting done for the comparison with the sediment core data has been carried out with Rigual-Hernández et al., (2012) data assuming the uncertainty associated with >150um fraction used. In order to clarify this point, a sentence has been added in section 3.4., it reads: "**The flux data used for these calculations corresponds to the >150 um for each species after Rigual-Hernández et al., (2012).**"

R1-C3: *Please provide the depths of the analysed sediment samples. I assume that the dated sediments have also been analysed, but it's not obvious from the text. Why were the only replicate analyses done on the core top (PLA CT)? What was the spread?*

R1-R3: The depth of the sediment samples analyzed as well as the sediment samples dated are now inserted in **Table 1**. A sentence has been added at the end of section 3.6., it reads: **"Planktic foraminifera present in the dated samples that were not selected for radiocarbon dating were also analyzed following the methodology described previously**".

R1-C4*: Details on the statistics are scarce. What tests were used for what reason? Are they applicable (the data appear log-normally distributed) and are predictor and response variables really independent (Fig. 2, shows area against area normalised weight).*

R1-R4: Corrected according to suggestion. **Section 3.6**. now describes only the statistical

analysis. Concerning the difference between SBW and MBW, Beer et al., (2010) methodology was followed. The main idea was just to prove that our MBW values were not correlated with the morphometric parametric they aim to be corrected for. To further support this observation, we performed a chi2 independence test and the results are discussed in **section 4.1.**However, and as asked in the following comments, statistical tests have been added to the study. In particular, to test the difference between MBWs from the sediment trap and the sediment cores, a non-parametric Mann-Whitney test has been used. This non-parametric test assesses whether two sampled groups are likely to derive from the same population by comparing the medians of the different datasets. A p-value of <0.05 has been used in order to consider the median of two datasets significantly different. This test has also been used for the seasonal and interannual trends.

Additionally, a description of the the GAM model requested by reviewer #1 in the following comments has also been included in **section 3.6.**

R1-C5: *Along the same lines, what is the meaning of the error bars in the various figures? In Fig. 3 it seems that they reflect the uncertainty (standard deviation?) of the sample averages for each month. My most important point is that this hides a large part of the variability (see Table 2) and I recommend to show the individual values (i.e. for each measured shell, normalised to average weight). In addition, how were samples assigned to a month, by start date, mid date, or by taking into account the collection interval? And are the mean and the standard deviation appropriate summary statistics for these data that are far from normally distributed? The same applies to Fig. 4? And in the latter figure, are the mean values flux-weighted?*

R1-R5: Corrected according to suggestion. The error bars, which reflected the standard deviation, from figure 3 and figure 4 have been removed. Also, now the individual values available from each month and each year are now plotted, along with the already plotted mean values. The samples were assigned into their corresponding month and year taking into account the exact collection date. This is now specified in **section 4.2.** before **Figure 3** and in the caption of **Figure 4**.

R1-C6: *Why do none of the environmental variables have error bars? This is especially relevant for the analysis of the seasonal patterns*.

R1-R6: Corrected according to suggestion. Error bars have been added. As specified below and in the GAM description, only the carbonate system parameters have been left in the seasonal and interannual figures as they could not have been included in the GAM.

R1-C7: *What is the rationale to infer linear trends in Fig. 4? And are the calcification intensity values flux-weighted? And how much of the variability is due to changes in the shell flux?*

R1-R7: Linear trends were initially included in Figure 4 to illustrate the evolution of the calcification of the three species analyzed during the 12-year record. In the case of *G. bulloides* and *N. incompta* the trend was flat, excepting the last 2-3 years. In the case of *G. truncatulinoides* our data suggests a clear increasing pattern in shell calcification that was clearly illustrated with the linear trend.

However, we agree with reviewer #1 in that the calcification response does not necessarily has to be linear and consequently the linear trends were removed in the new version of the manuscript. In order to improve the interpretation of the interannual variability, GAM has been employed. As described below (see R1-R9 next section), the GAM could not include the carbonate system parameters, therefore these parameters have been maintained in this figure. These values are not flux-weighted, as they just aim to show a measurement of the degree of calcification during the high productivity period of

each year.

R1-C8: Please provide the data in accordance with the Copernicus data policy: https://www.biogeosciences.net/policies/data_policy.html (preferrably also for review)

R1-R8: Corrected according to suggestion. All supplementary data and figures used in the discussion will be provided according to Copernicus data policy. Also, the environmental data, raw calculations of SBW, morphometric data, MBW calculations and sediment core weighing will also be provided.

Additional points:

R1-C9*: Separate analysis of intra and inter-annual variability. The authors first analyse the intra- annual variability in calcification intensity and then proceed with the inter-annual. What this means is that some of the interannual variability is incorporated in the composite seasonal cycle, perhaps obscuring or suggesting trends that are not there. Why not model the calcification intensity as a function of month/day of the year and time? This will allow obtaining more meaningful estimates of the seasonal cycle and the long-term trend and can for instance be done using GAMs.*

R1-R9: Authors agree with this observation. In order to model both calcification patterns, a GAM has been designed as suggested. This GAM has modeled the variations on a monthly basis and has extracted the seasonal and interannual cycle for both the calcification and the environmental parameters selected. Details about the GAM development can now be found in **section 3.6**. Previous correlation data are still presented in a reduced format (see comment and response R1-R35 in the next section). This correlation data have been used in the GAM in order to constrain the relation between the parameters and refine the analysis. This GAM has been used for both the seasonal/interannual patterns and the correlations with the environmental parameters. Due to the limited availability of carbonate parameters data for our study period, the GAM could only be performed with the Planier data (MBWs, fluxes, chlorophyll-a and SST), and the environmental data from DYFAMED site (Salinity and nutrient concentration). The carbonate system parameters, as excluded from the GAM, are therefore presented as a combination of correlations and linear models. The results and figures generated by the GAM are now included in the new version of the manuscript while details of the calculations can be found in the Supplement.

R1-C10*: Significance of the trends/patterns. Irrespective of the approach the authors take (although I recommend modelling the seasonal and interannual data together) the authors need to establish whether or not the trends/differences are significant. I am not convinced this is the case for any of the time scales considered (seasonal, interannual and decadal/centennial; with the possible exception of G. truncatulinoides on intra-annual and centennial time scales) and the authors run the risk of trying to explain noise (and derive meaningless conclusions) in the sections where they attempt to explain what drives variability in calcification intensity.*

R1-R10: A Mann-Whitney test has been carried out to test the differences in the medians for seasonal/interannual trends but also for the sediment trap and core-top datasets (see R1-R4).

R1-C11*: Drivers of variation in calcification intensity. I agree with the authors that identifying the exact drivers of calcification intensity is not trivial because of collinearity in the environmental variables. However, there are ways to better handle this problem than assessing the correlation of each variable individually. I can think of multiple linear*

regression or more flexible GAMs.

R1-R11: See R1-R9 in this same section of comments.

R1-C12*: Differences among species. One of the most interesting observations is that different species show different patterns in the calcification intensity (the latter still needs to be demonstrated and I suspect that only G. truncatulinoides shows a significant pattern on seasonal time scales). Right now it is hidden in the text, but it would be interesting to discuss in greater depth and with more focus why this might be the case and make a clearer link to the ecology of the species. The relationship between calcification intensity and shell flux is particularly intriguing in this respect and the authors go some way to discussing this for truncatulinoides (allocation of energy), but not for the other species. I recommend that they do.*

R1-R12: Authors appreciate the point made by the reviewer and agree with this observation. Consequently, a new section has been added to the material and methods, **section 3.3**. entitled: "**Ecology and life cycle of *G. bulloides*, *N. incompta* and *G. truncatulinoides*"**. On top of that, a clearer link has been established with the ecology of the species. In the case of *G. bulloides* and *G. truncatulinoides*, the life cycles and the calcification processes are more or less well defined, however, this is not the case for N. incompta, therefore the discussion surrounding this species is more speculative. This aspects are now discussed in the OGC impact on calcification in **section 5.1.**

R1-C13*: Related to this is the concept of optimum growth conditions and whether shell flux (mortality in fact for sediment traps) is a good measure (perhaps not), which should be discussed in greater depth. Perhaps the relative abundance of a species is a better indication of optimum growth conditions? Or the first derivative of the shell flux/abundance as the fastest growth occurs when the population size increases most.*

R1-R13: Authors agree that the OGC (Optimum Growth Conditions) are a complex index that should be interpreted with caution. In our study we first considered the relative abundances, however, the problem with them is that they are dependent on each other, implying that the increase in one species relative contribution means that other species abundance decreases. Therefore, the use of relative abundance could could lead to missinterpretations of the results. In turn, foraminifera fluxes are independent of each other. That's the main reason we used the >150um fluxes from Rigual-Hernández et al., (2012). In our paper, we first consider shell fluxes as OGC, then the chlorophyll-*a* as OGC (de Villiers et al., 2004) and finally compare this approach with the one that considers the OGC as nutrients (Schiebel et al., 2001). Therefore, the aim of our work is not about which environmental factors determine OGC, but using the main factors reporting in the literature to influence OGC to assess their possible role in calcification. However, we acknowledge that our approach was not clear enough in the first version of the manuscript and, therefore, our strategy to assess the impact of OGC on foraminifera calcification has been clarified in the new version of the discussion. In particular in **section 5.1.** It reads: "**Here, we first approach seasonal shell calcification by considering the Optimum Growth Conditions (OGC). Previous studied have defined these conditions on a wide variety of ways: abundance of foraminifera, the chlorophyll-a concentration and even nutrients concentration (de Villiers et al., 2004; Schiebel, 2001; Schiebel and Hemleben, 2017). Therefore, we aim to explore the impact of these parameters as OGC on the shell calcification."**

R1-C14*: Differences among time scales. Another intriguing finding is that the environmental drivers of calcification intensity seem to vary with time scale. I am not sure if this is a robust finding (see above), but at face value the calcification intensity of truncatulinoides appears positively correlated with temperature and carbonate ion concentration on seasonal time scales, negatively correlated on interannual time scales*

and on longer time scales positively with temperature and negatively with CO32-. If real, why could this be the case? The authors hardly touch one this, but it is interesting as it suggests that different mechanisms than considered might be responsible.

R1-R14: Authors agree with this observation and the fact that other mechanisms could be involved. Therefore a small discussion for each species has been added around the variability of the impact of environmental parameters on different time scales. (). This discussion has been added as a new section, **section 5.4**., it reads: "**environmental parameters influence across different time scales".** Other parameters such as the ecology of the species on different time scales and oceanographic changes are discussed.

R1-C15*: Ages of the core tops. There are several issues with paragraph 4.4 where the calcification intensity from the sediment trap time series is compared to the core top data. First of all, it is unclear what is actually compared. Fig. 6, which is referred to in this section, shows the mean (I presume) values for each sample (cup or depth), not the flux-weighted calcification intensity from the sediment trap time series and the core top data as is suggested in the text (which would mean 1 data point from the trap and one for each core top). This needs to be clarified. It makes of course most sense to compare what the authors write (but they need to explain how exactly they derive the flux-weighted average, right now it is unclear if this is based on the monthly averages or on the entire time series), but the figure shows something else. Notwithstanding the calculation, it is interesting that the differences are so consistent, I think the authors should emphasise that more. Second, the authors have not calibrated the 14C ages to calendar years. This means that the age/durations mentioned in the text are simply wrong. The authors need to correct for the local reservoir effect and calibrate the ages before they can make any statements about ages or rates. I realise that this does not affect the difference in calcification, but it is nevertheless important for the interpretation (as the core tops might on average not be post-industrial, but see below). Thirdly, the 14C ages are average values only. Due to time integration within each sample and because of bioturbation the real ages of the foraminifera vary (Dolman et al., 2021; Lougheed and Metcalfe, 2021). The authors need to acknowledge this in their interpretation.*

R1-R15: We appreciate all the important insights made by the reviewer. Figure 6 shows all the mean values for the sediment trap, however, as written in the text, what is compared in the main text are the flux-weighted values of the 12-year long sediment trap time series with the data from the sediment cores. Following reviewer's suggestions Figure 6 has been modified in order to show the sediment trap flux-weighted values. Secondly, authors have revised this section of the work and calibrated the 14C ages to calendar years. The details of the calibration displayed here can now also be found in **section 3.7**. Radiocarbon ages from the 3 sediment cores were calibrated using the CALIB program (Stuiver and Reimer, 1993) and using the Marine20 curve, which applies a marine reservoir correction of 550 14C years. Additionally, a -165 ± 93 years local reservoir was also considered (ΔR) (Stuiver and Braziunas, 1993). This ΔR was calculated as the average of the nearest 8 points to the sample location from the Marine Reservoir Correction database, whose values have already been corrected for the Marine20 curve. After these corrections, the sample from Menorca (MR 3.1.A 14-14.5 cm) displayed a date of 1557 (rounded to 1560) calendar years BP. So, samples from this setting can be considered pre-industrial. In regard to the calibration on the core-top samples from Planier and Lacaze-Duthiers sites (0.5-1cm), they were also corrected considering the local reservoir correction : 490(± 60) and 460 (±60), respectively. Based on these results the margin of the datations do not allow us to determine with certainty if these samples are pre-industrial. In order to give a rough idea of the time span they could cover, the most recent age accepted for calibration by CALIB 8.2 (i.e. 603 14C years BP) has been used as (and the results are shown in the Supplement) has been used as a reference value (227 cal years BP with a very low possibility of being posterior to 1950 AD) with the same marine and local reservoirs previously considered. Therefore, we can consider that

out samples could be dated anywhere between 227 cal years BP and 1950 AD as some bomb ^{14}C has been found. All of this datations are now available in **Table 1** and in **section 3.7**. As stated before, this does not affect calcification and the trends are the same: a reduction of calcification for the 3 species in the sediment cores compared to the sediment traps. Therefore, we have rewritten the paragraph dedicated to the dating of Planier and Lacaze-Duthiers samples. As a result of all these changes, the new version of the discussion is orientated around global climate change and ocean acidification but it also considers other mechanisms that could be responsible for this calcification decrease. It now also includes the samples that cannot be considered pre-industrial but industrial times samples as a discussion of the changes in calcification during the late industrial to recent Holocene.

R1-C16*: Attribution of difference with sedimentary shells. With regards to the line of argumentation to exclude the effect of dissolution I disagree with the authors, particularly on the third point ("if partial dissolution was to take place here, MBWs from the seabed sediments and core tops would be lighter than the ones from the sediment traps" L762-764). (Seafloor) Dissolution tends to affect the thinnest shells first, leaving the sediment enriched in thick/more heavily calcified shells (Berger, 1970 and many more studies since), thus exactly what the authors observe. The difference in calcification intensity thus seems to be in agreement with dissolution affecting the sedimentary shells instead of providing evidence against it.*

R1-R16: Authors understand the point made here and agree with the bibliography provided. Therefore, the hypothesis regarding the possible influence of selective dissolution on the sea floor sediments is not discarded in the new version of the manuscript. This hypothesis is now presented as a possible explanation for the differences between sediment traps and sediment assemblages. However, while the possibility of selective dissolution is not ruled out, this idea is presented as less probable than the other hypothesis because there are several lines of evidence that still suggest that carbonate preservation is optimum in the sediment samples, including:the carbonate supersaturation in the Mediterranean sea (Álvarez et al., 2014) and depth of the sediment cores studied (990 to 2117m) as well as the negligible dissolution of calcareous nannoplankton in the transit from the trap to the floor (Beaufort et al., 2007; Moy et al., 2009), make the authors think that dissolution, although not completely ruled out, seems unlikely here. A paragraph has been added at the of third paragraph of **section 5.3.,** it reads: **"However, it has been described that when dissolution takes place, it affects the thinnest shells first (Berger, 1970), and only the heaviest and more calcified specimens remain. In our study, the specimens from the sediment trap were lighter that the ones from the sediment cores, therefore, this is important to acknowledge as, although unlikely, dissolution cannot be completely ruled out."**

R1-C17: *Similarly, the fourth argument ("SEM observations of all 3 species in samples from the sediment traps showed no sign of dissolution and foraminifera were well preserved" L765) is also unconving. Shells spend orders of magnitude more time on the sediment-water interface than in the water column and dissolution in the water column is therefore less of a problem. The authors therefore need SEM images of the sedimentary shells to prove that they were not affected by dissolution. Ideally, they also include SEM pictures of cracked sedimentary shells to demonstrate that the cleaning was sufficient to remove any sediment from within the shells.*

R1-R17: Authors agree with the point made by the reviewer and will provide additional SEM images of sedimentary shells and broken shells in order to demonstrate the effectiveness of the cleaning technique.

R1-C18: *In addition, can the authors rule out that the difference in calcification intensity is not due to changes in the seasonality of the flux (because of the seasonal variability in*

calcification intensity, it is after all possible to change the flux-weighted mean calcification intensity by just changing the flux; cf de Moel et al., 2009) or due to addition of calcite below the depth of the trap (G. truncatulinoides)? So, even if the difference in calcification intensity between the core top and the sediment trap shell is significant (which needs to be demonstrated first), there are still other options than ocean acidification (based on the seasonal pattern, temperature seems an obvious candidate) that could at least in theory explain the difference. The authors therefore need to provide more convincing arguments to support their conclusions.

R1-R18: Authors appreciate the point made by the reviewer. in order to prove the difference between the different datasets (sediment trap vs sediment cores a non-parametric Mann-Whitney has been included in **section 3.6.** This test assesses the difference between the medians of the different datasets (see R1-R4 in the previous section for more details). Authors agree that changes in other environmental parameters aside from ocean acidification could also account for the observed differences between datasets. Therefore, the end of **section 5.3.** now includes a paragraph that discusses the possible role of changes in other environmental factors (such as SSTs, changes in fluxes and on the oceanographic setting), that could also account for the observed reduction in shell calcification in the three species.

R1-C19*: The authors may want to include the paper by Weinkauf et al (Weinkauf et al., 2016) in their discussion.*

R1-R19: This manuscript is now mentioned and discussed in the manuscript as suggested by the reviewer.

Specific comments

R1-C20: *The title does not seem consistent with the objective as specified in the first sentence of the abstract.*

R1-R20: Corrected according to suggestion. Now the abstract reads: "**The aim of this work is to investigate the response of planktic foraminifera calcification in the northwestern Mediterranean Sea on different time scales across the industrial era**."

R1-C21: *L59: the industrial period started about 170 years ago (although somewhere else in the text the authors suggest it started around 1800 CE).*

R1-R21: Corrected according to suggestion. Now it reads "**defined according to Sabine et al., (2004) from 1800 and therein**".

R1-C22*: L60: unprecedented for what time frame?*

R1-R22: Corrected according to suggestion. Now it reads "**has caused an increase in carbon dioxide"**

R1-C23*: L77: Jonkers et al did not discuss ocean acidification.*

R1-R23: Corrected according to suggestion. Reference deleted.

R1-C24*: L91: "bearing" not "wearing". I think the word depending here is also ambiguous, it may suggest that ecology and feeding strategy evolved first and that the species added symbionts or spines. If anything, I think it was the other way around. Better phrase this neutrally.*

R1-R24: Authors agree with this observation. Corrected according to suggestion.

***R1-C25**: L113-124: Is this section needed here? A lot of the information is also presented in the section "study area"*

R1-R25: As this information was not exactly present in the "study area" section, it has been moved there. This part in the "Introduction" section has been shorted and now it reads "**The Mediterranean Sea is a semi-enclosed sea with a high saturation state for calcite (Álvarez et al., 2014). It is often considered as a "miniature ocean" and a "laboratory basin" (Malanotte-Rizoli, 2010), which makes it a valuable zone to study marine calcifiers shell calcification processes."**

***R1-C26**: L124: "MedECC"?*

R1-R26: MedECC: Mediterranean Experts on Climate Change. Corrected according to suggestion, now the acronym is described in full text. Reference and bibliography updated.

***R1-C27**: L129: insert "can" before "provide", short (< 1 year) deployments cannot provide annually integrated fluxes.*

R1-R27: Corrected according to suggestion.

***R1-C28**: L146-193: can this not be condensed to the information that is relevant to the study?*

R1-R928: This section has been condensed but authors felt it was necessary to keep a few sentences about the Northern Current formation. Now it reads :" **The Mediterranean is a semi-enclosed sea and it is considered a concentration basin… The NC largely controls the circulation all over the western and northwestern part of the Mediterranean Sea, including the Gulf of Lions (Millot, 1991) and the Balearic Sea (Figure 1a)**". See comment R1-C6 and R1-R6 for the section that has been moved here.

***R1-C29**: Fig. 1: the line representing NWM lacks an arrow to indicate the direction of the flow. It also seems oddly placed as if it connects Minorca with Sicily.*

R1-R29: The NWM line was meant to show the limits of what is considered the NW Mediterranean. It did not represent a flow or current. In order to avoid misunderstandings, this line has been removed in the new version of the manuscript.

***R1-C30**: L206: "the characterisation and quantification" ... "were analysed" reword*

R1-R30: Corrected according to suggestion. Now it reads: "**Planktic foraminifera fluxes for the 1993 to 2006 period were documented by Rigual-Hernández et al., (2012)."**

***R1-C31**: Table 1: provide reservoir age (R or delta R) and calibrated age (include details on calibration in the methods)*

R1-R31: Corrected according to suggestion. Details on the calibration can now be found in **section 3.7.**

***R1-C32**: L237-241: please be clearer that the number of unique samples processed (Table 2) is lower. Also explain why species were analysed in size fractions, it is not immediately obvious since the size is also measured, and that this may lead to an*

underestimation of the variability in calcification intensity in the entire population of the planktonic foraminifera species.

R1-R32: Corrected according to suggestion. Now it reads:" **However, these numbers represent the total of samples analyzed but unique samples number is lower, as not all the sediment trap samples presented the three species in high enough numbers to perform the picking. The species were analyzed in size fractions in order to estimate the efficiency of sieve fractions and the impact of size and morphometric parameters on the foraminifera weight and calcification,**".

R1- C33: *L242-247: please explain why this number of shells is sufficient to characterise the variability/mean within a sample.*

R1-R33: Corrected according to suggestion. Now it reads*:* **The lowest number of individuals selected per sample was 5 in order to maximize the number of samples available for our study. According to Beer et al., (2010), the higher the number of individuals, the more reliable SBWs are. Here we aim to compare SBW results with a measured weight technique. Measured techniques are acknowledged to be reliable with a lower number of individuals, therefore a minimum of 5 individuals were selected in order to compare the two techniques.** *".*

R1-C34*: L267-270: were the morphometric analyses done on the same shells that were weighted (i.e. the 10-30 shells in specific size fractions)?*

R1-R34: Yes, they were. Added a sentence according to suggestion. It reads **:" These measurements were carried out on the same shells that were weighted."**

R1-C35*: L275: I understand the goal of this normalisation, but please describe better what the difference is between "mean parameter size fraction" and "mean parameter sample". And what is the advantage of this method compared to the area density (they seem highly correlated).*

R1-R35: Corrected according to suggestion. Now it reads: "**Size fraction" accounts for the mean of the parameter (area or diameter) measured in all the sites studied, while "sample" accounts for the mean of the parameter in the sample being measured. The advantage of these measurements is that the resulting MBW is being given with a weight unity (µg), which makes comparable to other studies (Beer et al., 2010).**."

R1-C36*: L288: SeaWiFS started in 1997, please mention this and explain why you analyse composites instead of analysing each sample using the corresponding environmental variable.*

R1-R36: Corrected according to suggestion. Now it reads: "**SeaWiFS measurements started in 1997 and were used due to the lack of direct chlorophyll measurements in the Planier sediment trap deployment site."**

R1-C37*: Table 2: is standard deviation meaningful for data that are distributed like this?*

R1-R37: Authors beleive this parameter is meaningful and appropriate here because it allows a comparison of the variations of the morphometric parameters measured between the different sites. It is also a supplementary argument to show that the use of a narrow size-fraction for analyzing the calcification of planktic foraminifera.

R1-C38*: L376: it would be good to explain the rationale for testing this better and earlier.*

Size and weight are obviously correlated and it is not a priori clear why that is an issue.

The names MBWarea and MBWdiameter are a bit confusing, especially since the unit is microgram. It is interesting to see that there is still a size weight relationship within a narrow size fraction, but not entirely surprising. What are the implications?

R1-R38: Corrected according to suggestion. A paragraph has been added at the end of **section 3.4.**, it reads: "**Correlations between SBW and MBW_{area} against area are displayed in Figure 2. The reason of this comparison is to show the relation between size and weight. In order to avoid the impact of having the bigger specimens displaying the heaviest weight and impacting the mean weight (therefore calcification indicator) of the sample.".** Concerning the implications of having some influence of the area on the MBWarea is not surprising (as stated in the text). However, as the r^2 showed are very low, this influence can be considered minimal compared to the influence of area on the SBW. Therefore the authors consider that it is a better calcification indicator.

R1-C39: *Fig. 2: please try to make this figure clearer. Allow more space between the subplots and show the axes next to the points. Make sure that the decimal separator is a point and not a comma. Add space between genus abbreviation and species name.*

R1-R39: Corrected according to suggestion.

R1-C40: *L409-411: and variability within the size range.*

R1-R40: Added to text according to suggestion.

R1-C41*: L417-418: an R2 of 1seems an artefact. It would only occur if all shells have the same shape, which I doubt.*

R1-R41: It is exponential correlation, therefore area and diameter are expected to be perfectly correlated. It has been done as a quality control for the morphometric measurements. In order to avoid confusion, this sentence has been removed.

R1-C42: *L431: "Mean annual MBWarea and roA values were calculated ... to illustrate the seasonal variability" does not make sense. It seems that monthly values were calculated. And why the average? It does not seem a robust indicator.*

R1-R42: Corrected according to suggestion. The average value has been taken due to the number of values available.

R1-C43: *Fig. 3: (in addition to the comments above): use point instead of comma, add space in species name, add number of observations per month. Why are there no error bars for roA? (and what is the advantage of showing both metrics?)*

R1-R43: Corrected according to suggestion. The number of observations per month as well as the total observations across the time span have been added. The roA plots were deleted because they are not discussed in the rest in the study.

R1-C44*: L445-447: These two sentences mean the same (and I do not agree that all species show clear patterns at all).*

R1-R44: Corrected according to suggestion. Now it reads: "**The seasonal variations in shell calcification differ according to the species".**

R1-C45*: L469: are these values flux-weighted?*

R1-R45: No, these values are not flux-weighted. Here we aim to just present the data from the different years in order to see if the calcification varied on an inter-annual scale. This is know acknowledged in the manuscript, at the beginning of section 4.3., it reads: "**As our aim is to present the raw interannual calcification trends, these values have not been flux-weighted**".

R1-C46: *Fig. 4: comma and space. It is unclear to me why the carbonate system parameters have not been averaged. The linear trends do not make sense, especially for the nutrients and salinity.*

R1-R46: Corrected according to suggestion. This figure has been modified and simplified, keeping only the interannual trends. As suggested in R1-R1 in previous section, a GAM has been carried out and, in order not to be repetitive with the plots, we took all the environmental parameters out of this plot. As donne in the previous figure, the number of observations per year has been added.

R1-C47*: L481: knit-picking, but there can only be one minimum value.*

R1-R47: Corrected according to suggestion. Now it reads: "**Lower values".**

R1-C48: *L485: the increase in calcification intensity is not constant.*

R1-R48: Corrected according to suggestion. Now it reads: "**with an overall steep calcification increase throughout the record"**.

R1-C49*: L491-492: I am sure that the other environmental variables also showed interannual variability. Why treat the carbonate system differently?*

R1-R49: Corrected according to suggestion. At first they were treated different due to the fact that they were only available for 2 periods. The paragraph has been reworked in order to treat all parameters the same. It reads: **"All environmental parameters showed variations across the years. Sea Surface Temperatures… Between the 2 periods for which direct on-situ carbonate system parameters measurements were available".**

R1-C50*: L495-496: This sentence is irrelevant here in the results section. Move to discussion if relevant at all.*

R1-R50: Authors agree with the fact that the sentence is irrelevant and therefore, it has been deleted.

R1-C51*: L500-501: this sentence seems to come too early, you have not yet established that there is a reduction in calcification intensity. In addition, the figure does not allow to infer much about any Holocene trends since there is no time axis.*

R1-R51: Corrected according to suggestion. Now it reads: **"Foraminifera weights analyzed in core tops and sediment cores from the NW part of the Mediterranean (Figure 6) and radiocarbon dating allowed a further insight on foraminifera calcification during the Holocene."**

R1-C52: *L502: how was the flux-weighting done? Using the monthly averages, or the observed data? If the latter, is it not biassed to the times (fluxes) when the observations were made?*

R1-R52: The flux-weighting is described in section 3.4. and is cited in the text. It reads: "**MBWs were flux-weighted. Mean monthly MBWs values from each species were**

multiplied by the corresponding mean monthly flux and then divided by the total annual flux of the corresponding species".

R1-C53: *L506: "in the last 489 years" is not correct, see comments above.*

R1-R53: Corrected according to suggestion. The sentence has been deleted.

R1-C54: *Table 3: what does seasonal mean in this case? Monthly, three-monthly (which months?)? How was significance determined? And is that appropriate? Why provide the entire matrix, it contains a lot of redundant information. And what is the purpose of correlating everything with everything (how relevant is it to show that phosphate and carbonate ion concentration (use correct notation) are correlated)? Simply show what is relevant and discussed.*

R1-R54: Seasonal means monthly in this case, and it has been added in the caption of **Table 3**. Significance was determined using a p-value of <0.05 as stated **Table 3**. Caption, it reads: **"Significant correlations (p<0.05) are set in bold." Table 3** has now been reduced to show only the relevant and discussed values.

R1-C55*: L558-561: this is interesting. Perhaps add some information about the life cycle of the different species here to discuss why truncatulinoides shows a unique pattern.*

R1-R55: Corrected according to suggestion. A paragraph has been added, it reads: "**As described previously (see section 3.3), this species life cycle is complex and it migrates through different depths of the water column. It is thought to reproduce once a year in winter in subtropical waters and it has been speculated that nutrient availability and the lack of predation could explain this strategy. Therefore, our data, that displayed a heavier calcification in autumn/winter for this species, could show adults that have spent time in deeper waters developing a thick calcite crust come to shallower waters in late autumn to winter and reproduce. This coincides with the period in which the other species are less present and therefore, could allow *G. truncatulinoides* to reproduce due to the lack of competition and predation"**. This paragraph has also been extended in order to discuss the ecology of the remaining species (see comment R1-R12). On top, a section has been added about the ecology of all three species: **Section 3.3.**

R1-C56: *Fig. 5: commas. Not all environmental variables are from the DYFAMED site (or the caption of table 2 is incorrect).*

R1-R56: Corrected according to suggestion.

R1-C57: *L591-594: the use of the term OGC seems confused. Obviously, the niche of the species is multi-dimensional, i.e. the species are likely to have food and other preferences (temperature for instance), but it is not likely that OGC are linearly related to any environmental variable (e.g. it may be too hot for a species). OGC occur within a range of environmental variables, and there is hence no "proxy" for it, they can only be described (e.g, OGC are between x and y). Without establishing what OGC actually are, it is therefore difficult to use them as a predictor of growth/calcification intensity (L665).*

R1-R57: In this study, OGC have been first described as species fluxes and then as chlorophyll- a concentration and we aimed to see the differences between these 2 approaches. In this particular case, OGC are defined as chlorophyll-a concentration, but they are discussed as covariation with nutrients could lead to think that the latter are a better OGC indicator. However, a sentence clarifying that the OGC proxy has to be taken with care has been added in order to avoid confusion. It reads: **"Although here we have first described the OGC as species fluxes and then as the chlorophyll-a**

concentration, it is important to remember that the niche and favorable conditions meant to be described by the OGC for each species are multi-dimensional".

R1-C58*: L617-618: if this is true, what would the sensitivity be (how much less calcification with how much more phosphate) and how does this compare with the studies cited?*

R1-R58: Corrected according to suggestion.

R1-C59: *L666: what aspects of seasonal changes?*

R1-R59: Calcification seasonal changes. Corrected according to suggestion.

R1-C60: *L685: presumable Fig 4. Are the trends significant?*

R1-R60: Yes. Corrected according to suggestion.

R1-C61*: L716: figure 4 instead?*

R1-R61: Yes. Corrected according to suggestion.

R1-C62: *L718-720: "the recent SST decrease". Can this not be tested explicitly?*

R1-R62: As this is just a theory around an oceanographic process, it has not been tested explicitly as is not the main focus of the paper.

R1-C63: *Fig 6: commas. What is compared (see above). The different colours for the species are redundant here as they are in separate graphs. Consider changing them and give more distinct colours to the sites to improve the clarity.*

R1-R63: Corrected according to suggestion. The colours have been changed. See coments above for the data that has been changed.

R1-C64*: L797: see Bird (Bird et al., 2017) for bulloides.*

R1-R64: Ressource included in the discussion.

R1-C65: *L859: what is in the supplement and where can it be found?*

R1-R65: The supplement will be furnished in the next round of discussion.

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