First of all, authors would like to thank again referee #1 for the second review and the positive feedback on the reworked version of this manuscript. We acknowledge the time invested and are grateful for it.

Overall, we have focused this new version of the manuscript around the three recommendations suggested by the editor: "(1) the different observations from the traps and the sediments warrant a more careful interpretation and, (2), clearer information about the statistical modelling needs to be provided. Overall, the manuscript would benefit from a more focused discussion of significant patterns only".

Find here an abstract of the main changes that have been carried out on the manuscript and the answers to referee #1 second round of comments:

- Strong statements regarding the comparison between sediment trap and sediment cores (I.e.: strong calcification reduction, massive calcification loss, etc) have now been replaced by more conservative statements. Moreover, the possible role of selective dissolution as a possible factor impacting the calcification trends at the beginning of section 5.3 has been improved.
- Both the abstract and the conclusion now are more concise over the effect of environmental parameters on the different species calcification.
- GAM calculations details have now been included in section 3.6.
- Sections 5.1 and 5.2. are now more focused on significant patterns. Sentences that discussed or described non-significant trends have now been deleted as much as possible in order to keep the text lighter, specially around the chlorophyll-a and nutrients concentration effect on calcification, where a lot of non-significant trends were described. We also reduced and simplified as much as possible different sentences across the manuscript to keep it lighter.
- Some changes have been added in section 2 in order to better suit the map description.

R1-C1: Difference between sediment trap and sediment shells.
The authors find that sedimentary shells are consistently heavier than shells in the sediment trap and attribute this (primarily) to a direct effect of ocean acidification, i.e. that shells in the sediment trap are less heavily calcified than their sedimentary (older) counterparts.
Whilst this interpretation seems to make sense, it seems to go against the direct observations from the sediment trap, which show no clear relationship between calcification intensity and carbonate system parameters on seasonal time scales (Fig. 4) and no, or species-specific, response on interannual time scales (Fig. 5). So why would the three species respond in the same way if they show no, or different (even opposite in case of truncatulinoides) responses to more long-term change? It seems unlikely and is not explicitly explained in the manuscript. It is alluded to in the section about response and time scales, but in my opinion different observations from the traps and the sediments warrant a more careful interpretation and wording such as "reduction in shell calcification" (L1086) should be avoided.

R1-R1: Authors agree. All the "strong statements" have been adapted and a paragraph at the end of section 5.3. has been added. It reads:" In summary, here we propose that a combination of a global scale process such as ocean acidification and a regionally amplified trend such as SST increase may be responsible for the MBW patterns differences between the pre-industrial and post-industrial to recent Holocene. However, the analysis of seasonal and interannual trends showed that the influence of the parameters is species-specific and varies across the time scale studied. This implies that other mechanisms may be affecting MBW trends and therefore, should be taken into consideration when interpretating these results."

R1-C2: Additional modelling using GAMs.
It seems like these models have been sort of stuck on, rather than properly incorporated in the manuscript. Some of the results are even presented in the discussion, whereas they are perhaps better placed in the results.
That said, the main reason why I suggested GAMs was not just because it's a flexible method that allows to model non-linear relationships, but in order to model the calcification intensity as a function of seasonal and interannual variation (i.e. MBW = s(day of the year) + s(year), see e.g. https://fromthebottomoftheheap.net/2014/05/09/modelling-seasonal-data-with-gam/). GAMs are not the only method that can do this, my point was to model the calcification as a function of both seasonal and long-term variability. I don't think the authors have actually done this and hence remain unconvinced that the seasonal and interannual trends are statistically significant and consequently worth modelling further. So please provide clearer information about the statistical modelling rationale and approach and if two modelling approaches are warranted, integrate them better in the flow of the manuscript.

R1-R2: Authors agree that the GAM description in the methods was short. We added more details to the GAM description in section 3.6. It reads: "The influence of a suite of environmental variables upon MBW_{area} was assessed using General Additive Models (GAM) (fitted using the *gam* function from the *mgcv* R package). Due to data limitation, the GAMs could not be fitted to multiple independent variables, so potential effects of interacting environmental variables were could not be assessed. Each model tested the dependence of the different MBW upon a single independent variable: month or year, to evaluate seasonal and interannual trends; the flux of each species, to test effects of ecological variability; and a suite of environmental variables to determine impacts of various aspects of ocean chemistry on the calcification. Smooth functions of these measured quantities were used as the single independent variable within the GAMs, which were fitted using the default settings of the *gam* function: a Gaussian family and identity link function; and the GCV.cp smoothing method. GAM results quantified the significance of the effect of each independent variable upon MBW." The GAMs have been modelled as suggested by reviewer #1, as a function od day/month/year… However, due to the lack of daily data concerning the calcification, a daily modelling was not considered as reliable, therefore the modelling has been done on a monthly basis, the highest resolution possible without losing to much data coverage.

Minor comments:

R1-C3: L50 "likely to influence" make the abstract more informative and describe in which direction calcification is influenced, not just that it is. Similar remark e.g. for L57. Please check throughout the text (e.g. conclusions).

R1-R3: Authors agree with the changes proposed here. The abstract has been updated and now reads: "The comparison of these patterns with environmental parameters revealed that calcification appeared to be species-specific". These changes have also been added in the remaining parts of the manuscript.

R1-C4:L89: Hemleben et al seems an odd citation here.

R1-R4: Authors agree. Citation replaced by Davis et al., 2017, Figuerola et al., 2021 and Orr et al., 2005.

R1-C5:L129: this section comes out of the blue and stands on its own.

R1-R5: Agreed. The sentence has been integrated in the next paragraph where the study zone is quickly introduced.

R1-C6: L205: single sentence paragraph. Can this not be integrated better?

R1-R6: Authors agree. Corrected according to suggestion.

R1-C7: Table 1: perhaps replace "out of range" with "contains post-bomb material" or something like this. That makes it immediately clear what the age could be (out of range could also be too old).

R1-R7: Authors agree. Now it reads: "bomb ^{14}C".

R1-C8: Section 3.3: I am surprised that the results of Rigual-Hernandez et al about the seasonality of the different foraminifera species is not mentioned here. It seems important for the optimal growth conditions later on. Consider Rebotim et al (Rebotim et al. 2017) for an extensive study of depth habitat and (Rebotim et al. 2019) for an analysis of calcification of truncatulinoides throughout the water column.

R1-R8: Corrected according to suggestion. Now, seasonal abundances of each species are described at the end of each paragraph dealing with the ecology of the species. Also, the suggested citations have now been included.

R1-C9: L290: odd citation for the presence of symbionts. Better use Takagi (Takagi et al. 2019).

R1-R9: Citation updated.

R1-C10: L334: perhaps add how much weight is lost in relative terms.

R1-R10: Authors agree. Corrected according to suggestion.

R1-C11:L633: Fig. 5 instead? Is Fig. 4 mentioned above?

R1-R11: It is Figs 3 and 5 actually. Fig. 4 was not cited above, this has been corrected.

R1-C12: L778-781: Since reproduction means death for planktonic foraminifera, the peak in the shell flux (dead organisms) is more likely to indicate the time of reproduction. Why is the migration to shallower water needed? I like the previous inferred energy allocation part, why is that not fine as an explanation? It could be discussed in the framework of the optimum growth conditions.

R1-R12: The sentence about the migration has been removed. It was originally discussed in order to complete the energy allocation theory, explore the reproduction theory and expand the ecology part of the discussion. Finally, considering the referee #1 comments, the sentence around the water-column migration has been deleted.

R1-C13: L816: since ch-a concentrations from remote sensing only pertain to the surface layer it would be better to write it like that and not make statements about the entire photic zone, which extends vertically.

R1-R13: Authors agree. A sentence has been added to clarify this, it reads: "Also, note that the chlorophyll-a data presented here only represents the conditions in the surface layer."

R1-C14: Fig. 4: why not show the results of the GAMs here in and in Fig 5?

R1-R14: Authors see the point made here. The reason why the GAMs have been put in the supplementary methods is to avoid having complex statistical figures in the text and to promote figures in which recent trends are easier to observe on recent years. Also, with the new reworking of the manuscript, the focus has been put on reducing the discussion and text around non significant trends, and were are not sure including the GAMs would suit that strategy. However, if considered necessary, we would include the GAMs figures in the article.

R1-C15: L1064: "since the industrial era and/or the late to recent Holocene". Still true?

R1-R15: Authors see the confusion. Yes, it is still true, but the sentence has been reworded. Now it reads: "pre-industrial times to post-industrial and recent Holocene."

R1-C16: Fig. 6: are the flux-weighted average values for incompta correct? They suggest really high fluxes for light shells, something I don't see in the figure in the supplement.

R1-R16: The authors acknowledge that the flux-weighted values for incompta look odd, but the values and the calculations have been double checked and they look correct.

R1-C17: L1199: I don't doubt the existence of multiple reproductive strategies in planktonic foraminifera, but it's unclear to me how this could influence the calcification intensity. I don't think this has been demonstrated, or suggested by anyone. Why end the manuscript like this?

R1-R17: Authors understand the comment. The original idea was to explore potential causes of calcification variations that were not considered in our data, but it is true that, for the moment, there is not a study that proves the calcification differences between the reproductive strategies. Therefore, the sentence and references have been removed to avoid confusion.