

Dear Dr. Carolin Löscher,

thank you very much for your support during the review process of our manuscript. We have incorporated all suggestions for minor additions and changes in spelling and style in the manuscript made by the reviewer Dr. Ralf Schiebel in the attached pdf file. In the following, you can find our detailed reply to all further raised issues and how we address them in the revised version of the manuscript.

The line number used to indicate which comment we are addressing refers to the line numbers in the previous version of the manuscript. All line numbers that refer to how we handle changes in the new version of the manuscript refer to the updated version.

Kind regards,
Franziska Tell (on the behalf of all co-authors)

First of all, we thank Dr. Ralf Schiebel for the feedback on the revised version of our manuscript "Upper ocean flux of biogenic calcite produced by the Arctic planktonic foraminifera *Neogloboquadrina pachyderma*."

Following the recommendation of moving the comparison with the surface sediments out of the results section, we deleted the paragraph in the result section (l. 358-362 in the previous version of the manuscript). Our intention behind the figure 11b was to show that *N. pachyderma*, the species focused on in the study, is not the only species present in the research area, especially in the more southern areas, which is relevant to evaluate their contribution to total CaCO₃ fluxes. As we still see the comparison with the abundances of *N. pachyderma* as relevant for the interpretation of the calculated mass fluxes, we propose to keep figure 11b in the manuscript, as well as the reference to it in the discussion section (l. 577-579).

We agree that it is interesting to look into the relevance of our study for paleoceanography. However, since this is not the main aim of our study, we consider it a distraction from the main results. In the interest of simplicity and brevity we therefore refrain from adding new components to the discussion at this stage.

I. 7: in what sense? Do you believe that plankton is ever challenged?

Our intention with this phrase was to express that the Arctic Ocean is currently mainly inhabited by a small group of plankton and pelagic calcifiers that is adapted to the conditions of low temperatures, high sea ice concentration and low light availability at least during winter months. We understand that the wording was not very precise, and therefore rephrased the introductory sentence as follows (l.7-8):

"With ongoing warming and sea ice loss, the Arctic Ocean and its marginal seas as a habitat for pelagic calcifiers are changing, possibly resulting in modifications of the regional carbonate cycle and the composition of the seafloor sediment."

I. 24: water temperature also in the Arctic is increasing, not decreasing

We are sorry for the confusion in this sentence, which was meant to explain the general physical properties of solubility of CO₂ increasing with decreasing water temperatures. To make clear that this general statement is not related to the changing climate mentioned in the sentence before, we have rephrased the paragraph as follows in the revised version of the manuscript (l. 23-26):

"The world's oceans play an important role in the global carbon cycle, which is at present strongly influenced by anthropogenic carbon emissions (Friedlingstein et al., 2019). The solubility of CO₂ in water is dependent on temperature, being higher at lower water temperatures. Therefore, on a global basis, the oceanic take-up of atmospheric CO₂ is especially high in the colder Arctic Ocean (Steinacher et al., 2009; Miller et al., 2014)."

I. 39: if 2.3 mg is 4 %, then 7.9 mg cannot be 34%, but should be about 14 % or so

We see that this numbers in combination might be confusing, as they represent a number of samples where the CaCO₃ flux varies, and hence the contribution of planktonic foraminifera to the total flux. To focus on the aspect most relevant in the introduction, we have changed the sentence and only mention the overall range of the contribution of planktonic foraminifera to total CaCO₃ fluxes of 4-34% now (l. 37-39).

I. 68: you may take this out, because we do possibly not know the tests without crust that may have been dissolved before arriving at the seafloor or in the surface sediment

We agree and have deleted the part of the sentence that includes speculation and only state that encrusted shells are more resistant to dissolution (l. 67-68).

I. 229: 124 m on average

As it is written in the beginning of the sentence, the given value of 124 m is the median value. We see that the sentence is not formulated to make this clear, so we have rephrased it as follows in the revised version of the manuscript (l. 228-229):

“Based on the calculation after Lončarić et al. (2006), the median BPZ is situated at 124 m water depth.”

I. 240 / Table 4: please make table wider to allow numbers plus parentheses to be written in one line

The current format of the table is restricted to the fact that the manuscript needs to be submitted as a text document in portrait format. We will ensure that in the final version, all columns are wide enough to be readable.

Fig. 3: you may briefly comment on this line in the figure caption (to explain that it includes one data point? one sample?)

This is right, the line at 600 m represents the result from a single sample, as it was the only sample taken at that depth. We agree that a short comment on this in the figure caption could help the readers in the understanding and interpretation of the figure, and have added the following sentence in the caption (l. 256-257):

“The line at 600 m depth represents the abundance of empty shells in one single sample, as sampling in all other stations did not reach to that depth.”

I. 297: significant?

Yes, in samples from PS93.1, where the number of samples enabled statistical testing, the difference between empty and cytoplasm-bearing shells in the calcification intensity is significant. In the other samples, statistical testing was not possible due to the low sample amount, which is why we remained vaguer in our wording. We changed the paragraph as follows to clearly show where statistical significance is present (l. 297-301):

“Shell weight and calcification intensity of non-encrusted shells are lower than of (heavily) encrusted shells. Similarly, cytoplasm-bearing shells are lighter and have a lower calcification intensity than empty shells (Fig. 6). The differences become smaller below the productive zone. A Welch’s t-test shows that the difference between the calcification intensity of cytoplasm-bearing and empty shells from PS93.1 is significant, both within ($p < 0.001$) and below ($p = 0.004$) the productive zone, with empty shells being always stronger calcified.”

Fig. 7: you could add information to the lines of different color...

We have added colour legends inside the plot to show which line belongs to which sampling location. To still keep readability of the plot with this added feature, we have also changed the arrangement of the individual plots within the figure and changed the colour scheme.

I. 430: how? different sampling devices?

We understand that the sentence was not formulated clearly, so we have rephrased it to show that the methodological reasons resulting in a variability of the calculated BPZ are differences in the vertical resolution (I. 423-425):

“In addition, the vertical resolution of the compiled plankton net profiles (15 m to 175 m within the upper 300 m depth, Table 2), has a marked impact on the precision on the estimated position of the BPZ. Thus, some of the variability in the BPZ position could arise from differences in sampling methods.”

I. 441: that can be tested

In this paragraph, we refer to the uncertainties in the calculation of the BPZ due to different resolutions in sampling and different patterns of change of shell abundances with depth discussed in the paragraphs above. We understand that the current formulation is somewhat unclear. Therefore, we have now added the information on what the resolution differences actually are (minimum sampling interval of 15 m and maximum sampling interval of 175 m within the upper 300 m, as shown in Table 2) in the paragraph above (I. 423-427).

We have also changed the paragraph itself as follows to clearly communicate the robustness of our results (I. 436-441):

*“In summary, the calculated BPZ in each profile is associated with some uncertainty. However, the spatial variability in the position of the BPZ is larger than the uncertainty and hence a real characteristic of the ecology of *N. pachyderma*. The location of the BPZ below 100 m in many profiles and never below 300 m is robust considering the range in vertical sampling resolution (Fig. A1). Explicitly considering the variability in the depth of the BPZ increases the leads to improved estimates of the shell flux of *N. pachyderma* from plankton net samples.”*

I. 498: again, a life span of *N. pachyderma* >100 microns of only 4 days is not possible. Rephrase.

We have rephrased the paragraph here to better draw the attention to the sentences that explain that the calculated low values of a few days of a residence time do not represent the whole life span of the individuals, but the time they stay alive after reaching maturity (I.495-5059):

*“Because environmental conditions can have an impact on shell size and calcification intensity (e.g. Weinkauf et al., 2016), advection could blur signs of OVM if the life span of *N. pachyderma* is long relative to the speed of advection. Even though the residence time is not a direct measure of life span, since it only reflects the average time that foraminifera >90 μm spent alive in the productive zone and hence excludes the time it takes to reach maturity, it can provide a first order approximation. The majority of the estimated residence times is below 10 days. Longer estimates are likely due to lack of precision at low shell counts, but we note that they are not inconsistent with the life span observed in culture (Spindler, 1996). Thus, the median calculated residence time of about four days in our data suggests that the life span of the sampled *N. pachyderma* is either too short to be strongly affected by environmental variability, or that the population size is constant at short time scales and hence unlikely to be influenced by changes in environmental conditions. Therefore, we conclude that the possible blurring of signs of OVM would be rather small, and the lack of a clear trend indicating OVM at all stations can be seen as a reliable result.”*

I. 532: syntax

We have rephrased and shortened the sentence as follows to make it easier to read and understand (I.525-527):

“Next to possible regional differences in shell sizes, a further source of uncertainty arises from different mesh sizes across the compiled datasets, with higher average shell weights at coarser mesh sizes.”

I. 631ff: 10 % of what? Atlantic water? Contribution of what? Values of what?

We have added in the different sentences that we here always refer to the flux and contribution of planktonic foraminifera CaCO₃ fluxes in comparison to total CaCO₃ fluxes (l. 627, l. 629, l. 632).

I. 660: oops, I understood that it's at 300 m water depth, not 413 (i.e., 113 m plus 300 m) ?!

We understand the confusion with the different depth intervals, which are related to different analyses. The median depth of the base of the productive zone is 113 m, and the maximum 300 m. Calculating the flux at 100 m depth results in clearly leads to overestimation because 100 m is almost always within the productive zone where shell concentrations are much higher than in the export zone. To avoid overestimations, we therefore conclude to better use 300 m depth as a value to calculate fluxes when the BPZ cannot be defined precisely, e.g. due to coarse sampling depth intervals.

The second aspect raised here is the loss in flux that is happening below the BPZ. The loss of about 6.6% per 100 m is mainly visible in the 300 m below the BPZ, which itself can be located anywhere, mainly between 113 and 300 m depth. To make clear that those values are not related, and the loss is not the loss within the productive zone, but below it, we have deleted the sentence referring to the loss in the first paragraph (l.651-661) of the conclusion that refers to the loss, and only left it in the second paragraph (l. 662-666) that describes the fluxes below the BPZ as well as the loss in that zone.