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Answer to Referee 2 comment on "Distribution of coccoliths in surface sediments across the DrakePassage and calcification of Emiliania huxleyi morphotypes" by Nele M. Vollmar et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-105-RC2, 2021

Nele Manon Vollmar et al.

Author comment on "Distribution of coccoliths in surface sediments across the Drake Passage and calcification of *Emiliania huxleyi* morphotypes" by Nele Manon Vollmar et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-105-AC2, 2021

Reviewers Comment #2 (RC#2): The manuscript "Distribution of coccoliths in surface sediments across the Drake Passage and calcification of *Emiliania huxleyi* morphotypes" by Vollmar et al. is dealing with the study of coccolithophore assemblages in surface sediments of southernmost Chile and across the Drake Passage and the factors limiting their distribution, as well as the identification of the prevailing *E. huxleyi* morphotypes in order to evaluate their biogeography in relation to changing environmental conditions. *E. huxleyi* coccolith mass variations have been evaluated. The manuscript is well written and provides novel data concerning the coccolithophore distribution in the surface sediments of the area.

Authors Comment (AC): We would like to thank the reviewer #2 for the comments and suggestions on our paper, which helped us to notably improve it. In the following lines, we address the individual comments and suggestions provided.

There are few points that I would like to comment:

RC#2: Introduction, l. 91. The authors state that they will perform *huxleyi* coccolith mass variations evaluations in pre-industrial assemblages to compare with plankton data. It is not clear if this will be used for acidification effect evaluations? However the dating of the studied surface sediments is not very precise (see comments below), thus it cannot be used as a straightforward evidence for evaluating current climatic/environmental impacts. Please clarify.

AC: We see the surface sediments - as common practice in the literature - as the accumulated mirror of the sedimentation of the last centuries or at least of the last few thousand years, depending on the sedimentation rate and influencing factors such as dilution or resuspension. Since it was not possible for us to carry out our own datings, we relied on published age data, which give calibrated accelerator mass spectrometry (AMS) ^{14}C ages for surface sediments in the work area of about 3 to 4.8 ka BP. As mentioned also to reviewer #1, a newly published paper in which changes in Antarctic Circumpolar Current strength in the central Drake Passage were calculated for the last 140 kyr (Wu et al., 2021) used sediments of one of our stations. The calibrated AMS ^{14}C ages

for the sediments from the piston core PS97/085-3 at 0.5 cm (= 56.5 cm composite depth) and for 20.5 cm (= 76.5 cm composite depth) were 1.13 ka and 6.64 ka, respectively. The real sediment surface was partly missing in the piston core, so that they calculated a recent age for the trigger weight core, but even the surface of the piston core had a reasonable age which allows us to assume that the surface sediment is close to Recent.

We have added this information to our text and we hope to have removed the reviewer's scepticism regarding the sediment ages.

Wu, S., Lembke-Jene, L., Lamy, F. et al. Orbital- and millennial-scale Antarctic Circumpolar Current variability in Drake Passage over the past 140,000 years. *Nat Commun* 12, 3948 (2021). https://doi.org/10.1038/s41467-021-24264-9

RC#2: Material and Methods, l. 132-135. The dating of the studied material is vague. Former AMS datings in near surface sediments of previous studies in the broader have been used and the authors conclude to ages "most likely mid to late Holocene". Do these datings come from the surface samples or from certain depths? Is there a possibility to estimate the age of the nearest of your surface samples to those of the former studies, by using an available sedimentation rate for the area? Please clarify.

AC: This point has been already addressed in the previous paragraph.

RC#2: Also, please use Mid and Late Holocene, as these are official chronostratigraphic units.

AC:We changed this in our text accordingly.

RC#2: 175, it is not clear how many specimens/morphotypes have been analyzed per sample. Is it at least 20 coccoliths?

AC: This question has also been raised by reviewer #1. We tried to measure at least 30 *E. huxleyi* per sample (some of the micrographs showed more than one specimen), but all the countings of the *E. huxleyi* morphotypes within morphogroups A and B were done in an additonal count on top of the routine count (see line 164 f) of at least 100 coccoliths. The mentioned 613 SEM images were used to measure the specimens with the Coccobiom2 Macro and to morphologically describe the 5 morphotypes (335 measurements of morphotype B/C, 134 measurements of morphotype O, 52 measurements of morphotypes A and A overcalcified merged together).

RC#2: 195, this is rather confusing, plankton samples may differentiate from the surface samples (e.g., due to dissolution as you state later on), so why identify type O a potential Type B/C that lost the thin plate when being in the sediment? Maybe clarify even at this stage that "type O" may not totally correspond to the certain morphotype recorded in the plankton, or give it a different, descriptive name that you will keep it all over the manuscript.

AC: Since one of our aims was to compare with the plankton data of Saavedra-Pellitero et al. 2019, we tried to differentiate as much as possible, as the former authors did. Although we could not distinguish sufficiently enough between the types B, B/C (which is an integration of B and C), and C, we could clearly distinguish a type O with open central area or lamella in central area. And from the description provided by Hagino et al. (2011),

this is clearly Type O, and so we named it and kept it this way throughout the manuscript. We explained this in the text and in Table 1. Whether this is just a „potential Type B/C", as mentioned by the reviewer, or an own morphotype, and if this has the same or a different ecological adaptation than the other *E. huxleyi* types (in particular the B- and C-types) should actually be discussed in further investigations. In the end, we cannot rule out that these may be disguised, dissolution-related B / C types, but we believe most of them are real. Nevertheless, we tried to better clarify this in the text.

RC#2: 345-350, is it acceptable to correlate "Mid-Late Holocene" surface sediments with the present day surface waters conditions? This needs to be more documented, e.g., are there evidence (from sediment cores) about the paleoceanographic evolution of the area during the Holocene?

AC: We think this is a common practice, a fair assumption, and we consider it is fully acceptable, particularly when we have provided more publications which have dated a core-top sample used in our study, which were confirmed as of recent age. This information has been included in the revised version. The comparison of coccolith data from sediment traps with those of the underlying surface sediments (e.g., Sprengel et al., 2000; Skampa et al., 2019) shows that the data are similar in species fluxes, assemblages, etc., even if the surface sediments are usually not dated.

Sprengel, C., Baumann, K.-H., Hendricks, J., Henrich, R. & Neuer, S., 2002. Modern coccolithophore and carbonate sedimentation along a productivity gradient in the Canary Islands region: Seasonal export production and surface accumulation rates. Deep-Sea Research II, 49, 3577-3598.

Skampa, E., Triantaphyllou, M.V., Dimiza, M.D., Gogou, A., Malinverno, E., Stavrakakis, S., Panagiotopoulos, I.P., Parinos, C., Baumann, K.-H., 2019. Coupling plankton - sediment trap - surface sediment coccolithophore regime in the North Aegean Sea (NE Mediterranean). Marine Micropaleontology, https://doi.org/10.1016/j.marmicro.2019.03.001

RC#2: 372-374. Overcalcified E. hux specimens (primary calcification) have been also found e.g., in the Aegean Sea, Mediterranean, during the winter season, strongly correlated with low temperatures. It would be useful to discuss the occurrence of these morphotypes in respect to what happens with them at different parts of the world ocean. Apparently your specimens cannot directly prove primary overcalcification as secondary calcite precipitation in the "Mid-Late Holocene" surface sediments cannot be excluded? In the same way that many "type O"s might be originally type B/C

AC: We are relatively sure that we can exclude secondary calcite precipitation in the coccoliths in our samples, as we see no signs for this at all in our SEM based investigation and we also did not detect obvious carbonate dissolution in the same samples.

We suggested that some *E. huxleyi* O types might have been originally B/C type specimens, not many, which is possible because of the fragile nature of the central area platelet / lamella, especially in the southern water masses of the AZ and SZ+CZ. We are rather confident on our observations, especially regarding O type specimens and we tried to make this clear in the text.

We consider that comparing our morphotype coccolith results to other Southern Ocean locations (as we already did) makes more sense than comparing to data from the Mediterranean since it is a completely different setting.

RC#2: 520-525, "type O" distribution is problematic as you also state. It is because what you have counted may not really represent original type O. Could you differentiate in a possible way, the real type O from the dissolved B/C? It would be useful to produce a figure similar to Fig. 11 with all Ehux morphotypes plotted with CEX, to see which of them are more related to dissolution.

AC: If it would be possible to differentiate them, we would have done so. However, that was not possible, and we just suggest that some of the *E. huxleyi* O type might be originally B/C types that are slightly dissolved and thus lost the central area platelet / lamella (but impossible to appreciate in SEM). We cannot rule out that some of them may belong to the B/C-type. We are, however, quite sure that morphologically most of them are actual O-types. And interestingly, while *E. huxleyi* morphotype B/C (including B, B/C and C) dominates over morphotype O in plankton samples (Saavedra-Pellitero et al., 2019) they do not in the studied surface sediments from the Drake Passage. This makes us speculate this morphotype may be highly sensitive to lower calcite saturation state at depth south of the Polar Front (see discussion in lines 579ff).

RC#2: 532-535, again a correlation of Holocene surface sediments with plankton assemblages must be treated with caution as the time interval in between them is not negligible. Also to show any agreement it would be useful to do a statistical correlation between all available mass values, taking into account the different methodologies.

AC: This comment is linked to remarks previously addressed. The newly published AMS ^{14}C ages surface sediments demonstrate, as the aforementioned published dates, that the surface sediments in the Drake Passage seem to be recent.

RC#2: 538-539. The statement is rather vague, when projecting Mid-Late Holocene to the future. Will it be a primary coccolithophore feature? What about potential dissolution? May be additional correlation with potentially available sediment trap data of the broader area would be useful. AC: See previous remarks to comment about the age of the sediments. We consider that hypothesizing about future coccolithophore response to anthropogenic warming, in terms of morphology or dissolution, is out of the scope of our study. There are multiple publications which include modelling work or are based on culture evidence, which are much more suited to make such predictions.