

Rebuttal letter

On behalf of all coauthors I would like to thank the Editor the time devoted to read and comment on our Manuscript and their suggestions to improve it. Below is a point-by-point reply in green, explaining the changes that we have made accordingly.

With all the abbreviations I found it a bit difficult to keep track of everything, if you see a way of improving this that would be highly appreciated. We have substituted FS by “fine silt”, CS by “coarse silt” and C by “clay” in text, figures and tables. We would like to keep the abbreviation for the sample sites so they are comparable to a previous published work (Ausín et al., 2021) based on these very same samples and related size fractions, but we have written the extended name whenever possible.

This is also a little bit due to the tables format, which is partially a problem because of the portrait layout. One of the things I directly started looking for was a comparison between the results of the bulk sediment and a weighted average of the different sediments fractions and it took me a while to find this. I feel this could be emphasized a bit better, possible by adding the weighted average to the graph as well as the table? We have added this data to figures 2 and 6. I also think this should hold true not only for the amounts of alkenones, but also the ages, right? It should, but we would need the age of all the fractions contributing to the bulk to estimate the abundance weighted age, otherwise the result is not comparable. Unfortunately, purification of alkenones for radiocarbon dating was only possible in some of the size fractions, but not in all of them in any of the sites. I think there is one sample where the alkenones from the bulk sediment were much younger than the alkenones from any of the sediment fractions? I could be wrong. In SMB and NAR the bulk is much younger than the related fractions, but it should be kept in mind that there are two other fractions for each site that we could not measure (clay and fine silt in SMB and clay and coarse silt in NAT). According to the ^{14}C age of the OC hosted by each fraction (Ausín et al., 2021), clay is always the youngest fraction. Given ^{14}C age in alkenones and OC from the same sample is comparable (Fig. 8), it is expected that ^{14}C age of alkenones hosted by the clay fraction are also younger, shifting the average weighted ages to younger ages, and likely leading to fraction-weighted average ages comparable to bulk ages, as a small contribution of young material (high ^{14}C content) has a large impact on samples containing little ^{14}C . For the ^{14}C data I got a bit confused by “bulk” in table 3, I think, this is still the alkenones extracted from the bulk sediment, so completely different from TOC. I think you were correct, but if you can make that more instantly clear to the reader, I think that would help. The “bulk”-weighted average comparison is a really good indicator for the quality of the results. Indeed, these are the radiocarbon results of the alkenones only. We have stated this more clearly in the Table header.

I don't think you should write an extensive M&M section, however a few more details would be nice. Alkenones were identified by GC based on comparison with a standard, it would be nice to have some idea of the GC column type and length that was used. The samples were quite marine so I don't expect alkenones derived from freshwater (Type I) haptophytes, but they make an C37:3 (and other :3s) different from the “typical” marine one. On a typical apolar GC column these C37:3s cannot be separated from each other, for instance. Not a

problem for these setting, I would think. Multiple alkenone producing haptophytes do not produce coccoliths, be careful with the comparison between the two. We used 2x60m VF-5MS columns (120 m total) that allow good separation of C37 alkenones. We have added some information on the characteristics of the column and the standards used.

You indicate additional cleanup of alkenones for ^{14}C measurements, however, I did not see any indication on how pure they actually were, I think? We only measured those samples that had a purity >90%. This information has been added to the text, but on average, our samples had a purity >96%.

Some minor comments, I found the sentence starting in line 242 to 244 not clear. At BER, we obtained a cold bias of $6^\circ\text{C}\pm 0.6^\circ\text{C}$, but as we did not use surface or core-top (0-1 cm) sediments, but sub-surface sediments whose planktic foraminifera are 900 yr old, it could be argued that the large temperature bias is due to the comparison of SST during two different periods. For this reason, we need to emphasize our results can be still discussed, as the bias between annual-mean and core-top sediments from that exact core is still 6.6°C based on previous studies. Our sentence now reads: The large and cold bias observed at Bermuda Rise, could be due to the use of sub-surface sediments (2-5 cm; foraminifera ^{14}C age= 900 ± 50 yr) rather than surface sediments. Nevertheless, alkenone-SST from the core-top (0-1 cm) of the exact same core [Nao Ohkouchi et al., 2002] also leads to a -6.6°C (cold) bias.

In line 283 “ ^{14}C ages of both types of organic matter”. Corrected.

“Previous authors” is a bit strange, “studies have shown”, “previous publications indicate”. Changed.

I also noticed that after agreeing with reviewer 2 on “warm bias” instead of warmer bias in your answer to the next question you used warmer bias again. Be careful with the revised manuscript. Bad habit, noted.

You mention in your response that there was only one reference with an indication of the amount of alkenones per biomass. In Chivall et al., 2014 (OG) there are alkenones in pg per cell for culture studies, for instance, but I am sure there are more papers that indicate amounts of alkenones per unit biomass. We meant estimates in that (*E. huxleyi*) or other coccolithophore species that produce alkenones. Also, we meant the relative contribution of alkenones to the total organic carbon, which is the data we cite from Prah et al. (1996). But we agree with the Editor that if this data is reported as pg of alkenones per cell of *E. huxleyi* (1.20 ± 0.28 pg/cell; Prah et al. 1996) comparable estimates can be found elsewhere (e.g., Sawada et al., 1996, Marlowe, 1984; Prah et al., 2003).

If I look at figure 5, I seem to see two outliers, BER and NAT, if you remove these your correlation will be much better than 0.26. There is one other sampling site that does fit with the 1:1 line more or less, but all the alkenones are already a couple of thousand years old, if I read the manuscript correctly. They fit accidentally or not that much has changed in this time period. Or, they were synthesized in a place — because a temporal bias might also imply a spatial bias — with a similar SST, this is, little or no SST gradient between place of origin and final deposition. This line of argument is discussed in throughout the text and explicitly mentioned in the conclusions.

I agree with your warning to all of us to be careful, however I think you could end more positively by including recommendations and not only warnings. Now that we know this how can we deal with it? Multiproxy approach, combining foram and organic proxies, etc. I am looking forward to your rebuttal and the revised version of your manuscript based on the reviewers and my comments. We have shaped our conclusions accordingly. We believe further steps include the assessment of particle provenance to evaluate particle transport pathways and whether the entrainment of allochthonous/asynchronous material has or not a large impact on proxy signals. This is because the large impacts observed on alkenone 14C age and concentration do not necessarily impart an equivalent bias in UK'37-SST (as observed at sites SBM SMB and NAM), because the latter also depends on the temperature gradient between the sites (or time periods) of alkenone production and deposition. If this gradient is small or even negligible, it is good news to the Paleoceanographic community. We also agree multiproxy approaches are still needed to evaluate the accuracy of proxy signals and the reliability of derived climate interpretations, specially those combining organic and inorganic proxies and based on proxy carriers that show different hydrodynamic behavior.

References

Ausín, B., E. Bruni, N. Haghypour, C. Welte, S. M. Bernasconi, and T. I. Eglinton (2021), Controls on the abundance, provenance and age of organic carbon buried in continental margin sediments, *Earth and Planetary Science Letters*, 558, 116759.

Marlowei. T., Greenj . C., Neala . C., Brassells. C., Eglinton G. and Course P. A. (1984) Long chain alkenones in the Prymnesiophyceae. Distribution of alkenones and other lipids and their taxonomic significance. *Br. phycol. I.* 19,203-2 16.

Prahl, F. G., Wolfe, G. V., and Sparrow, M. A. (2003), Physiological impacts on alkenone paleothermometry, *Paleoceanography*, 18, 1025, doi:10.1029/2002PA000803, 2.

Sawada, K., Nobuhiko Handa, Yoshihiro Shiraiwa, Akiko Danbara, Shigeru Montani. (1996) Long-chain alkenones and alkyl alkenoates in the coastal and pelagic sediments of the northwest North Pacific, with special reference to the reconstruction of *Emiliana huxleyi* and *Gephyrocapsa oceanica* ratios, *Organic Geochemistry*, 24, 751-764.