Climate of the Past Discussions

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Reply on RC2

Camille Godbillot et al.

Author comment on "Parallel between the isotopic composition of coccolith calcite and carbon levels across Termination II: developing a new paleo-CO_2 probe" by Camille Godbillot et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-76-AC2, 2021

We thank you for accepting to review the manuscript and for your comments that will enable us to improve our manuscript. Please find the responses to your comments below:

In the following, I develop several points that require more detailed explanation and several minor points

The first point concerns the discussion about the coccolith Δd13C$_{small-large}$ over the studied interval (Termination II) (mainly Section 3.3.2):

The relatively acceptable statistical correlation between [CO_{2aq}] and Δd^{13}C$_{small-large}$ (Fig. 4) relies on 2 points with low Δd^{13}C$_{small-large}$ and relatively high [CO_{2aq}] values. When looking at the downcore records (Fig. 3), these 2 points correspond to the H11 interval. This interval indicates noisy isotope values (Fig. 3). How robust is it? (if these 2 points were removed, the statistical correlation would probably be less significant): can you comment on this?

If we remove the two points mentioned for a moment, the statistical correlation between CO_{2aq} and both $\Delta^{13}C_{small-large}$ and $\Delta^{18}O_{small-large}$ remains significant (**Figure 1, enclosed**). We chose to keep these points in the study because we found no evidence that these samples were more contaminated than other levels by fragments from either the adjacent fractions or foraminifera.

Some information is needed about isotope measurements on the different size fraction : are they based on replicates ? (triplicates ?)

We are aware that running small aliquots of foraminiferal assemblages (<15-20 specimens) may lead to biases. Unlike foraminifera, the ≈ 80 µg of coccolith fractions we run for isotopic analyses integrate an appreciable number of coccoliths and thus we do not routinely run them for duplicate analysis. For the sample we did run for duplicates, however, the standard deviation fell well within the standard deviation determined from the different NBS19 values ($\delta^{13}C$ = 1.95 ± 0.05, $\delta^{18}O$ = 2.20 ± 0.1 ‰ VPDB), which is known to be a homogenous standard material. In any case, we are confident that the magnitude of observed changes between our fractions and adjacent levels is much higher that the reproducibility of our measurements.

The second point concerns the downcore isotope records: -some information is missing why d^{13}C of large is more stable than d^{13}C of small coccoliths; -another

particular feature is the stability of the $d^{13}C$ *bulloides* record; even if it is not the main topic of the study, reasons why this former record is stable over Termination II needs a comment (since it is not observed in other $d^{13}C$ records from other planctonic species).

This remark, pertaining to the *G. bulloides* $\delta^{13}C$ reference was somewhat made by Reviewer 1, which we hope we satisfactorily addressed. We would specifically like to add the following:

Two observations can be made on the variations in $\delta^{13}C_{\mathit{G.\ bulloides}}$:

- The amplitude of the $\delta^{13}C_{\mathit{G.\ bulloides}}$ change across the interval is 1.16‰ for the samples considered (**Figure 2, enclosed**). It appears stable because this change is smaller than for the total $\delta^{13}C$ change of the 2-3 µm coccolith fraction (a 1.84 ‰ change).
- Neither the $\delta^{13}C_{2\text{-}3\mu m}$ nor the $\delta^{13}C_{5\text{-}8\mu m}$ parallel the change in$\delta^{13}C_{\mathit{G.\ bulloides}}$ (**Figure 2, enclosed**) nor the δ^{13}CDIC we derived from it:

These two observations show that the foram and the coccolith record are disconnected from each other over the interval. We interpret these results to indicate that the coccoliths and *G. bulloides* record variations in $\delta^{13}C_{DIC}$ of different water masses (see section 3.2. of the manuscript and response to RW1's query). The discrepancy between the foram and coccolith record can be accounted for by distinct documented living depths, as *G. bulloides* is found to live between 70-100m depth, below the preferred living depths of coccolithophores (Rebotim *et al.,*, Biogeosciences, 14, 827–859, 2017). Thus, we are *a posteriori* of the opinion that the uncertainties pertaining to the inorganic reference make it difficult to conclude as to which of the 2-3 µm or 5-8 µm fraction is responsible for the observed changes in the $\Delta^{13}C_{small\text{-}large}$ and $\Delta^{18}O_{small\text{-}large}$. We would like to stress that, as a result, we do not base our interpretations on the isotopic composition of the forams.

The third point is a general comment. The results of this study support findings that the isotopic composition of coccoliths (for different size ranges) is sensitive to CO_2 concentrations at the glacial/interglacial scale. However, even if in Sections 3.3.3 and 3.4, different factors that could imprint the coccolith vital effect are addressed, the conclusions about the use of this proxy as a paleo-CO_2 indicator are slightly too optimistic. It should be mentioned that there are still a number of issues to be clarified (effect of productivity, stratification). (…)

As replied to Reviewer 1's comment, we understand that the phrasing of the conclusion might be a little optimistic in light of the issues with the transfer function that were discussed in the manuscript. Although our results indicate that [CO_2] might exert a first order control on coccolith differential vital effects, we agree that better knowledge on variables such as productivity (which has an effect on cell physiology) and stratification (which has an effect on air-sea disequilibrium) is needed before we can define a robust transfer function between these two parameters.

We have toned down this proxy with an explicit mention to the issues that you identified as a complicating factor of the prospective proxy. The need for constraints on cell physiology, and on air-sea disequilibrium can easily be incorporated into the conclusion of a revised version of the manuscript.

It would be interesting to compare these data either for another Termination or another more distant site of events affecting oceanic conditions.

This is true and definitely belongs to a longer-term and cocco-community approach! We hope that the present study will stimulate such studies from other teams.

Some minor points :

-[CO_{2aq}] calculation : mention the impact of salinity uncertainty on the estimation

Uncertainties on salinity estimates were already included in the calculation of [$CO_{2\ aq}$] (at line 219 of the manuscript). A ±1 psu conservative change in salinity across the interval leads to a maximum ±0.05 µmol.kg^{-1} uncertainty on [$CO_{2\ aq}$].

-What is the temporal resolution difference between atmospheric pCO2 records and SST reconstructions in core MD37?

The Antarctic pCO_2 records (with a mean temporal resolution of 760 yrs across the studied interval) were matched to SST records which have a mean temporal resolution of 1 kyr. We did our best to fit the two.

-in relation with section 3.3.3 : do you have you access to the coccolith counts/assemblages over the studied interval ?

Please refer to our response made to Reviewer 1 in his third comment. But a short answer is that we have not generated absolute or relative coccolith abundances because coccolith size in our approach matters more than the size of the coccolithophore taxon that mineralized it, although the two correlate. Therefore, we checked our coccolith fractions for signs of recrystallisation and contamination from larger coccolith fragments or foraminifera fragments rather than for assemblage changes.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-76/cp-2021-76-AC2-supplement.pdf