

Clim. Past Discuss., referee comment RC1
<https://doi.org/10.5194/cp-2021-76-RC1>, 2021
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Comment on cp-2021-76

Tom Dunkley Jones (Referee)

Referee comment on "Parallel between the isotopic composition of coccolith calcite and carbon levels across Termination II: developing a new paleo-CO₂ probe" by Camille Godbillot et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-76-RC1>, 2021

This study presents an informative new data set of coccolith size-fraction oxygen and carbon stable isotope values across Termination II. The study interval is a useful target to test the behaviour of size-specific coccolith stable isotope data as it can be directly compared to good proxy SST (Uk37) and foraminiferal $\delta^{13}\text{C}$ records from the same cores, as well as ice core records of atmospheric pCO₂, from which some estimation of local [CO₂]_{aq} can be made.

The coccolith size-fraction isotope data look very good and is hard-won through the micro-filtration technique. I have two concerns / suggestions I would like the authors to consider and one request.

First, the framing of the paper, from the title and abstract, is about "developing a new paleo-CO₂ probe". I have no problem with a discussion of [CO₂]_{aq} as a potential (major) control on small-to-large coccolith stable isotope vital effects, as shown in culture studies and suggested by long-term, multi-million-year trends (e.g. Bolton & Stoll, 2013), but the strong framing of this study within the context of pCO₂ proxies causes problems for both the interpretation of data, for the reader and potentially the wider community. For me, this is exactly the sort of study that should be being undertaken - carefully examining the data and responses of coccolith vital effects within key size fractions. The data (to me) show that there is likely some influence of [CO₂]_{aq} on the isotope differentials large-small, but that there are other factors potentially at play. The authors do a good job in trying to explore what these might be - either other physiological controls (e.g. growth rate, CCMs) - or a divergence between their estimated local [CO₂]_{aq} (from ice cores) and what might actually have been, due to local changes in air-sea disequilibria. So, although they do have some good proxy records to compare against their data, it leaves open a question of whether "mismatches" (assuming there should be a relationship) between [CO₂]_{aq} and large-small vital effects are due to physiological or oceanographic confounding effects, or both. This is a problem if the paper is setting out to make a solid contribution towards a new pCO₂ proxy (no major step forward in quantifying a robust pCO₂ - coccolith isotope relationship), but is less so if just trying to gather good data and understand how these size-specific coccolith isotope records actually behave in practice and especially across a range of temporal scales and magnitudes of pCO₂ change - i.e. do the trends observed over long (Ma) timescales actually work over short (millennial) timescales. In particular,

how do the high CO₂ Miocene-Pliocene worlds compare to the Pleistocene for coccolith isotope records, where there's clearly an issue for alkenone $\delta^{13}\text{C}$ behaviour (Badger et al. 2020). In this last context, Figure 5 is a very nice compilation and comparison between the available records. So, I would strongly recommend reorienting the paper towards the best interpretation of the data you have rather than trying to reach for a CO₂ proxy which isn't there (yet).

Second point, and related to the first, is that I'd like a more detailed consideration of the underlying driver of your change in the large-small coccolith isotope offsets, especially for carbon. The raw data - Figure 2 - shows a $\sim 1\text{‰}$ negative shift in small coccolith $\delta^{13}\text{C}$ across Termination II whereas the large fraction hardly changes. The *G. bulloides* record trends slightly positive. In this instance, I can't see how this can be explained other than that the vital effects in the small cell sizes are increasing across TII - i.e. going further from equilibrium calcite - as CO₂ rises. Whereas the large cell sizes change less. OK, this gives you a reduction in large-to-small vital effects, as you'd expect with increasing CO₂, BUT it's the small coccolithophores that are driving this change, not the large cell sizes that should be most limited and sensitive to changing $[\text{CO}_2]_{\text{aq}}$. With no major change in local $\delta^{13}\text{C}_{\text{DIC}}$ (based on *G. bulloides*) how do you account for this big shift in the small coccolith $\delta^{13}\text{C}$? This seems to me to be at the heart of understanding this record but is never really addressed.

Finally, a request. Although there are lovely SEMs of the size splits in the SI, and I'm sure that all the splits look equally lovely, it would help enormously if you could provide some assemblage composition data for some selected representative samples through your record - both species composition and coccolith size distribution. I know this is some extra work, but at the moment it's impossible to properly compare data from coccolith separates like these from different studies (using different methods) unless there's reporting of what is actually being measured. This kind of quantitative assemblage data would also allow better comparisons between studies and across timescales - e.g. knowing that we're comparing small (of defined size range) retics to small retics. Your samples look quite confined in their taxonomic composition - and I'd want to document that (and get others to do the same) - so that we can spot issues if a (90%) *Calcidiscus*-rich assemblage is compared to a (50%) *Calcidiscus*-rich assemblage.

Line 29 – would recommend rephrasing, especially the use of “overtakes”

Line 63 – lower case “a” after the colon.

Line 64 – late Miocene not Late Miocene – informal division.