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Biogeosciences Discussions

Interactive comment on "Alkenone isotopes show evidence of active carbon concentrating mechanisms in coccolithophores as aqueous carbon dioxide concentrations fall below 7 µmol L^{-1}" *by* Marcus P. S. Badger

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Thankyou to the reviewer for their kind words about the manuscript and the contribution it represents. I am happy to include the suggestions the reviewer makes in a revised manuscript. A common theme in both reviews received is the need for greater and more detailed introduction of carbon concentrating mechanisms in haptophytes, I am happy to include a new section in the introduction covering this, and this will allow further discussion to be added later in the manuscript as well. Alkenone producers in the modern ocean are relatively well known, and I will include a discussion of this, as

well as a discussion of what is known about past alkenone producers.

In this manuscript I left much of the discussion of the possible causes for the breakdown in the relationship between epsilon p and dissolved CO2 short, as although this work does suggest where and when in the ocean this breaks down, and it makes sense that CCMs would be at play, it is perhaps beyond this work to confirm that. I can however expand this discussion. The strength, I believe, of this work is the power of combining the multiple records and treating them the same. Unfortunately, not all published records have suitable indictors of upwelling and runoff (like BIT), so this level of analysis may be beyond this work and a fruitful avenue of further information. In a revised manuscript I can include what information is available, especially with records to depth and distance from the coast.

The sites are in relatively diverse settings, with ODP sites 925 and 999 considered open ocean sites, and the analysis I show suggests that it isn't one site or another which performs better, rather the [CO2](aq), as can be seen from Figure 4. The diversity of sites will be clearer once I include water depth, which I can add to Table 1.

Similar to the lack of BIT for all Sites, few have nitrogen isotopes available, which precludes a full assessment. The findings of Zhang et al (2019, 2020) are an interesting avenue for further developing the proxy, but I would caution that some of those analyses start from the premise that you can use Pleistocene records to re-calibrate "b", and my analysis would suggest that for many of these records there are times when alkenone isotopes are no longer sensitive to atmospheric CO2 changes, which may require a reassessment of some of that work.

CCMs are indeed quite complex, and I am happy to add futher nuance to the discussion of them, my point with "relies on the assumption of a purely diffusive uptake of carbon" is that the proxy as currently applied is far too simplistic in the low [CO2](aq) situations where CCMs may dominant, assuming as it does that CCMs are not important. I can revise this statement.

There was a problem with the zip file which contained the supplemental data which as been resolved.

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