Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-356-SC3, 2020


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

**Joep van Dijk**

joep.von.dijk@gmail.com

Received and published: 20 October 2020

Many thanks for the kind and swift response to my comments, and the answers to my questions.

I am also intrigued by the results as displayed in Figure 10 of the manuscript. Before I continue, I want to emphasize that I am no expert on the coccolithophore ep-based pCO2 proxy. I hope that I can first and foremost be a helpful objective new participant in this very important field of climate research.



Regarding the Wilkes et al. (2019) framework; I agree that nutrients, lights and CO2 availability all play a part, and that it may indeed be accurate that CO2 availability plays a dominant part in times when CO2 availability drops to the lowest concentrations known in Earth's history. That being said, I do want to exert caution. There is a merit in the statistical approach undertaken in this study, but please do not forget the other pieces of information in the separate data-sets. The relationship between the b-factor and phosphate concentrations is - after all - problematic, as stated in the study (e.g. Zhang et al., 2020). In short, this relationship may not hold for every phytoplankton community, at every location on the planet. For example: yes, phosphate concentrations are greater in the Peru upwelling region, but so is algal growth, and thus light availability at this location may exert a significant control on ep, perhaps more so than [CO2]aq. In this context, I think the study can benefit from a discussion of the Wilkes et al. (2019) framework, if only to get all its readers on the same page.

After reading your reply, I also think that the exclusion of the Manop C site could use some more discussion. I find it interesting that the entire data-set seems to be offset by a constant number. Is this truly due to a change in CO2 equilibrium? Or are other factors playing a role here? Are oceanographic frontal changes to blame? This brings me to what I wrote before. When I look at the data-set from Site 619, it seems to agree quite well with ice core pCO2 reconstructions, even though it seems to have witnessed times with [CO2]aq below 7 umol/L. I know that the whole point of your study is the statistical approach, but a little more discussion on the sites itself; perhaps in combination with the Wilkes et al. (2019) framework, could be useful.

To summarize, yes, the good fit in Figure 10 could indeed be related to the activation of CCM below 7 umol/L, but it could also not be. Some more discussion of why it could not be would be good I think.

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