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Interactive comment on "Insensitivity of alkenone carbon isotopes to atmospheric CO_2 at low to moderate CO_2 levels" *by* Marcus P. S. Badger et al.

Anonymous Referee #2

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This manuscript presents new alkenone-based pCO2 data along with previously published 11B-based pCO2 reconstructions from the same late-Pleistocene and Pliocene samples. This is a timely comparison and tackles the question of how well the alkenone CO2 proxy as currently applied can reconstruct changes in CO2 near modern pCO2 levels. In an interesting exploration of the alkenone pCO2 data, the authors present novel estimates for the input parameters that would allow the proxy to reconstruct known pCO2 values. This exploration highlights some of the fundamental problems facing the alkenone pCO2 proxy in its current form and will hopefully spark new work to better understand carbon isotopic fractionation by coccolithophore algae and its relationship to CO2 concentrations. The manuscript is well written, novel, and will be of interest to a wide variety of scientists. The figures are generally easily interpreted and

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clear. There are not any major issues with the manuscript and for this reason I suggest to publish with minor revisions. Comments and suggestions for these revisions are described below.

My only substantial comment is that I think the authors should calculate the Pliocene pCO2 values from the alkenone proxy with the cell size corrections added (and do the same for the Pleistocene samples for comparison). I realize this doesn't change the orbital-scale insensitivity in the Pleistocene. But, it does allow comparison of the absolute value and magnitude of change between the Pleistocene and Pliocene windows in both proxies. It will also change the posterior distributions of the input variables for the alkenone pCO2 reconstructions. My sense is that it will bring the b-parameter, ef, and SST posteriors more in line with the priors. The lith size changes are on the order of 150 to 200% higher in the Pliocene with respect to the Pleistocene (it appears from the figure). That is substantial and would increase the estimated Pliocene pCO2 values into the high 300 to low 400 ppm range – very similar to the 11B pCO2 estimates. This may indicate that the alkenone pCO2 proxy agrees in magnitude with Plio-Pleistocene pCO2 changes and thus may be sensitive at higher CO2 levels but not at the very low Pleistocene glacial levels. The authors suggest this might be the case in the conclusions. If they show it is the case with their Pliocene reconstructions it would provide some nice empirical support (and they should mention this in the abstract).

Page 4 line 15 – how were alkenone 13C isotope measurements calibrated and what was the replicate precision and the accuracy (i.e. uncertainty from analysis plus uncertainty in realizing the VPDB scale). (Same comment for p5 line 6).

Page 9 line 20 – The previous paragraph stated that there is some evidence for a reduction in productivity during glacials and if that translates to cell-specific growth rates then it could explain some of the lack of signal in the alkenone pCO2 reconstructions. In light of that observation, the following statement is confusing to me: "This suggests that either our understanding of growth rate effects on CO2(εp-alk) is incorrect, or the estimation of cell size using preserved liths does not capture original cell size

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variations. . .." Doesn't the prior statement suggest that our understanding of growth rate effects may actually be correct?

Page 10 line 25 – This paragraph is quite instructive, nice! One question is how the SST posterior is calculated? For the Pliocene, the SST would also affect the pCO2 estimated by the 11B method. Thus, if one assumes a pCO2 from the 11B and then gets a posterior SST from the alkenone proxy, this different SST would change the 11B pCO2 estimate and thus the alkenone SST posterior based upon the earlier 11B CO2 value is no longer correct.

Page 11 line 5 – ". . .the current understanding of the CO2(ep-alk) proxy is wanting." Yes, the b term may not in fact capture the scaling of physiological parameters or the truly important parameters.

Page 11 line 19 – The sentence starting, "Additionally, CO2 optima. . ." is a bit unclear. I think the authors are saying that different species have different CO2 optima so that CCM effects may vary between regions where different species dominate? But maybe not that. Please rewrite and clarify.

Very nice, concise explication of the issues and a nice manuscript!

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