

Interactive comment on “Sensitivity of atmospheric CO₂ to regional variability in particulate organic matter remineralization depths” by Jamie D. Wilson et al.

Anonymous Referee #4

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The submission by Wilson et al explores the sensitivity of atmospheric CO₂ to regional variability in particulate organic matter remineralization depth in the ocean. In doing so, it ties together two sets of important previous findings: those showing that remineralization depth is a key lever on global climate, and those showing that remineralization depth varies significantly between regions. The manuscript uses some interesting methods, such as breaking interior nutrient distributions into components that circulated from different surface regions. It also reaches some interesting conclusions, including the important role of the Subantarctic region, and the fact that regional changes in remineralization depth might be closely approximated by a corresponding uniform change in the global flux profile. However, I have two major concerns that

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mean I cannot support publication of the paper in its current form.

First, I am concerned that biases in the circulation model might skew the results and have not been properly acknowledged. Can the authors state whether they are using a new version of the MITgcm 2.8degree circulation model, or the same one that has been used since the early OCMIP era? Previous studies (Dutay et al. 2002; Doney et al. 2004) have identified some significant shortcomings of this circulation model that might impact the relative importance of different regions in the current study. Not least, the model does not produce deep water along the Antarctic coastline as it should, and instead produces deep water at around 50S. This would shift deep water formation from the Antarctic to the Subantarctic regions defined in the current study, and give the Subantarctic region unrealistic leverage over interior nutrient distributions. It could be the case that the circulation has been reformulated since those studies and this bias corrected. If that's the case it is important for the authors to demonstrate this, to reassure readers like myself who have reservations about that model. The simplest way to show this would be to calculate ideal age in their model, and plot a meridional cross section through the Pacific. They should be able to show a tongue of young water subducting right along the Antarctic coastline and spreading northwards along the seafloor (not a tongue of young water penetrating the deep ocean at 50S). If they are indeed using the old, biased circulation model, this should be acknowledged in the text where the significance of the Subantarctic region is discussed. Either way, a figure like the one I suggested should be included as a supplementary figure either to demonstrate that the circulation model is robust, or to make readers aware of potential biases introduced by the Southern Ocean watermass structure.

Second, I am confused as to why a paper focused on sensitivity of atmospheric CO₂ does not use a model that resolves the carbon cycle. Instead, they model only the phosphorous cycle and relate it to carbon cycle changes using a relationship derived from prior modeling studies. The authors state that this is to avoid the computational expense of simulating the carbon cycle. But that would only require the addition of two

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tracers – DIC and Alk, and a single value for a well-mixed atmospheric CO₂ concentration. This should therefore only double the computation time, and given that transport matrix method is being used (where efficient Crank-Nicolson time-stepping methods can be applied), this does not seem preclusive. And even then, they needn't include the carbon cycle in all 200 of their simulations, only enough to redefine the statistical PO₄ vs. CO₂ relationship from their own model. This would at least keep their study self-consistent, rather than relying on previous results from different models. If the authors are not able to do this in the current study (which would be preferable), they should again acknowledge more clearly the caveats of their chosen method. In Fig. A1, it is obvious that different models yield different relationships between these properties. Fitting just the Marinov et al. results would lead to a much shallower relationship, but those results are not strongly weighted because they contribute fewer data points than others. It would seem more reasonable to fit the relationship for each previous study separately, and propagate that uncertainty into their CO₂ estimates.

Minor points

Figure 3. I think a bar plot would be better suited to show this data. This is a key result of the paper, and a quantitative comparison between regions and production methods would be simpler in a bar format.

Figure 5. This figure is a little overcomplicated, as evidenced by the fact that a fair amount of the text (not just the caption) is devoted to explaining what it means. How about showing these results as a color matrix instead? Region in which b is varied down the rows, region in which we are looking at the preformed PO₄ along the columns (or vice versa), color shows the regression coefficient. I know this is contrary to my previous comment about bars being more precise, but I think precision is less important here than the need to reduce complexity. The color matrix would allow the reader to pick out “bright” rows or columns as indicative of important regions.

Section 3.2 and Figure 6. I'm not sure why the results in Figure 6a are shown? The

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authors acknowledge that simply averaging the b values is not the correct way to quantify the global-mean remineralization profile, and then attempt to correct for it in panel b. But why show an obviously incorrect result in the first place? It seems like the correct way to define a “global mean b-value” would be to construct a global-mean (area-weighted) organic matter flux profile, and then fit the Martin relationship to that.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-509>, 2018.

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