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Interactive comment on "Sensitivity of atmospheric CO_2 to regional variability in particulate organic matter remineralization depths" *by* Jamie D. Wilson et al.

Anonymous Referee #1

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The authors explore the sensitivity of oceanic distribution of PO4 to global/regional changes in the remineralization depth, represented by the exponent of the Martin's powerlaw curve. The authors then infer the sensitivity of atmospheric pCO2 based on the previously published relationship between the oceanic inventory of preformed PO4 (or regenerated PO4) and atmospheric pCO2. I agree with the authors that the regional sensitivity can be an important component of the global carbon cycle response to climate change. Although the model experiments and analysis are sound and the paper is well written. I have some issues to be resolved.

Firstly, the model only infers the atmospheric CO2 response based on the published

relationship between the oceanic inventory change in preformed PO4 and the atmospheric CO2 change. The near-linear relationship between the two is only valid if we assume no solubility and air-sea CO2 disequilibrium effects. I acknowledge that the effects might be implicitly included in the empirical relationship extracted from a few previous global studies. However, I am not sure if the same relationship can be applied to the regional perturbation study. For example, any perturbations in the Southern Ocean (e.g., the ACC band where air-sea CO2 disequilibrium is large due to the short surface residence time of upwelled waters) might not lead to the atmospheric CO2 response proportional to the preformed PO4 response. This point can be especially worrisome because the most sensitive regions turn out to be the ACC band in the study.

Secondly, the sensitivity is estimated using the multiparameter linear regression method applied to the two sets of 200-member ensemble experiments where 15 regional exponents are perturbed simultaneously. Although the method seems sound, I wonder why the sensitivity should be quantified in this way? Are there any merits? Would the sensitivity be the same or different if the authors perturbed the exponent in a region at a time, requiring a total of 15 perturbation experiments for one model scheme? The individual perturbation experiments seem a simpler and cleaner way to quantify the atmospheric CO2 response to the perturbation and its relations with the export production in each domain.

Thirdly, the major novel finding is that the highest sensitivity in atmospheric CO2 is to the change in remineralization depth in Subantarctic regions due to high export production and the high connectivity to deep water formation regions. I see the reasoning behind it: The export production should be high because the export will determine how much regenerated PO4 can be affected by the perturbation. The connectivity to deep water formation regions is important because the deep water formation is the main pathway of preformed PO4 to the ocean's interior and the inventory of preformed PO4. However, I am not fully convinced by the authors' finding. Both "nutrient restoring" model and "constant export" model show that the sensitivity of atmospheric CO2 to the remineralization depth change is also high in the “NTemp-PAC” domain (Fig. 3). Yet, the subtropical North Pacific is not a region with high export production nor close to any deep water formation regions. How can it be explained? Similarly, why are the deep water formation regions (i.e., the model NADW and AABW formation regions) not the sensitive regions?

It may be related to the third point. But I don’t understand Figure 5. The sensitivity is normalized to what? What do the authors mean by “mean preformed PO4 in a region”? Is it the surface PO4 averaged over each region or the total preformed PO4 subducted from each region divided by the volume of water subducted from the same region?

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