

Biogeosciences Discuss., referee comment RC2
<https://doi.org/10.5194/bg-2021-124-RC2>, 2021
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Comment on bg-2021-124

David Archer (Referee)

Referee comment on "Ideas and perspectives: Sea-level change, anaerobic methane oxidation, and the glacial–interglacial phosphorus cycle" by Bjorn Sundby et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-124-RC2>, 2021

This is a valuable and well-written paper clearly worthy of publication. The new idea is to combine changes in methane hydrate stability driven by sea level change to diagenesis chemistry that would release phosphorus to the ocean, stimulating biological production and ultimately possibly the enigmatic decrease in the CO₂ concentration of the atmosphere. Any new mechanism for volatility of the ocean phosphate reservoir is interesting, because of the profound biological impact of the P in the ocean.

I think it would be illuminating to put characteristic depth and concentration scales into Figure 2, to try to constrain how much phosphorus could be mobilized on which time scales. On the shallow end of the depth range, the hydrate stability zone is thin, so the released P would be near the sea floor, where it could diffuse to the ocean relatively quickly. On 100,000 years, the mean free path for molecular diffusion is about 50 meters. But if the stability zone is 500 meters thick, a change at the bottom of the stability zone would be pretty isolated from the ocean. In this way perhaps one could estimate the amount of methane that is properly poised to release phosphorus on a glacial time scale, which would tell you also the magnitude of the potential P dose. Putting characteristic depth and concentration units onto the profiles in Figure 2 would allow you to calculate a diffusive gradient and flux for P, another way to address the same issue, I guess. My hunch is that this depth-restriction of where this process could be relevant on 100,000-year time scales might diminish the potential scope for driving ocean P cycles, but that would not diminish for me at all the value of this paper or its worthiness of publication.

I think it would be worth mentioning, in the background to the ocean nutrient inventory as a driver of atmospheric CO₂, that the extent of this effect should be reflected in the plankton – benthic δ¹³C difference, a measure of the strength of the biological pump. I think this “Δδ¹³C” says that the biological pump is not the entire answer to the glacial CO₂ drawdown. That also does not diminish the value of this paper at all; the proposed coupling between methane and phosphorus cycles should clearly be elucidated, as this

paper nicely does.

Line 105 A citation for the “until recently” would be useful. I note that there is further discussion of this topic later in the paper.

Line 127 My distant recollection is that there is a lot of particulate phosphorus in rivers, stuff that gets scavenged in the estuary double-layer-collapse sedimentation zone. It might be helpful to clarify how this flux fits into the P budget. Could it be that it is this phosphorus flux that gets periodically mobilized, maybe along with biologically-deposited phosphorus? Thinking about this might be complicated by estuaries that exist or not through glacial cycles, possibly changing where the P deposits. Perhaps that would increase the potential P mobilization, if you’re not limited by P that was deposited by primary productivity during the past glacial cycle (which is a very clever idea).

Line 277 I’m not sure I agree that a linear P gradient implies that there is no impediment of diffusion by adsorption onto solid phases. There is clearly a diffusive flux, but it could be linear in a long-term steady state, with a higher concentration of adsorption P going with higher porewater concentrations at depth; the pore waters could still be buffered by the sediment. That might be important for thinking about the time dependence of the phosphate concentration and hence cycles in the diffusive flux. In a more buffered case I guess the relevant depth range for driving P cycles would get shallower.