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Interactive
Comment

Interactive comment on "The global marine phosphorus cycle: sensitivity to oceanic circulation" *by* C. P. Slomp and P. Van Cappellen

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In this manuscript, Slomp and Van Cappellen present a new model for the global P cycle and use it to evaluate the C and P cycle response to specified changes in ocean circulation rate. The model is presented as an update and improvement over Van Cappellen's highly cited work from a decade ago. The fundamental improvement is the incorporation of boxes representative of shelf environments, neglected in the earlier work. The model also utilizes updated concepts about the importance of bottom-water oxygen levels in the preservation of organic matter in sediments.

In general, I strongly support this line of research and think the authors have identified some interesting and unexpected behaviors that demand further consideration. I do have some concerns related to simplifications incorporated into the model that I hope

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the authors will consider in revision.

A fundamental concern I have is the assumption the authors make that a slowing down of ocean circulation will automatically lead to anoxia. One reason I'm concerned about this is the authors cite the work of one of my former students (which I co-authored: Hotinski et al., 2000; pp. 1598) in support of this notion. Instead, we argue that the rate of ocean circulation in and of itself is not the major control on anoxia. Take the extreme condition, a sudden shutdown of ocean circulation. The supply of O2 to the deep ocean would indeed go to zero, but very rapidly the surface waters would be depleted of nutrients (because that supply is dependent on upwelling associated with ocean circulation, now shut down), so there would be no oxygen demand associated with settling, decomposing biomass. In other words, both oxygen supply and oxygen demand scale with ocean circulation rate, and so this factor becomes unimportant (to a zero-order approximation appropriate for the box model here). Now, the 4-box model presented here apparently tracks deepwater [O2], but I have been unable to ascertain how the modeled O2 itself responds to imposed circulation changes. One thing missing from the manuscript is a table of the differential equations solved; such a table would help clarify this issue, but I think the authors could also spend some time describing the O2 response in their model.

I think the authors also want to a bit more careful in their discussion of the "limiting nutrient" concept. They state that P is limiting to marine biological productivity on geologic time scales (1594) and cite papers such as Tyrrell (1999). Actually, Tyrrell shows what the geochemists have been (sometimes mis-) stating all along, that is that P limits the burial rate of organic matter (i.e., is the ultimate "limiting nutrient") while N or P can limit productivity. To the extent that 99.9% of gross primary productivity gets remineralized, carbon burial is essentially decoupled from biological productivity. So it's unwise to perpetuate the false argument between biologists and geochemists by stating that P limits productivity on geologic timescales. It doesn't, it just limits burial (and even then, according to the Van Cappellen and Ingall mechanisms, a given amount of P can bury variable amounts of C depending on the extent of anoxia and preferential P recycling). I suppose that the authors are equating burial with biological productivity when considered on long timescales, but this is confusing to most people.

In comparing the old and new 1-box ocean models, have the authors removed the fictitious P box with long time constant that they needed (Van Cappellen and Ingall, 1996) to decouple P from O2 long enough to drive anoxia?

Finally, I'm somewhat concerned about drawing definitive conclusions from this study based on the results of this box modeling, because there's no physics in the ocean circulation. I'm particularly concerned about the exchanges with the shelf boxes, and how those might respond to changes in thermohaline circulation of the open ocean. These to me are open questions, so the authors need to carefully couch their conclusions here in terms of "what if" scenarios. Perhaps more sophisticated 3-D general circulation modeling will follow that can provide the physical constraints missing from this work.

In summary, I find this to be a well-done, thought-provoking study and a notable improvement over (highly regarded) earlier work. With modest revision this paper should be ready for publication. Note I have no minor editorial comments; the paper was exceptionally well written.

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