

Interactive comment on “Glacial-interglacial variability in ocean oxygen and phosphorus in a global biogeochemical model” by V. Palastanga et al.

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[. . .] The main problems are the lack of mechanistic insight, data constraints, or testable predictions. The model is used to calculate things, and the results of the calculations are presented with little discussion for the underlying processes. Although interesting, I am left uncertain as to whether the results have any relevance to reality. I would suggest a thorough rewrite of the paper that focuses on mechanisms, data comparisons, and testable predictions, prior to consideration for publication in Biogeosciences.

We regret that our manuscript has given this impression, especially given that one of our major aims is to increase the mechanistic insight in changes in coupled P, C,

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Fe and oxygen dynamics in the ocean. Also please note that this model study builds on an earlier study (Palastanga et al., 2011) focusing on the pre-industrial and future ocean and we purposefully did not repeat many of the details on the sedimentary P cycle, data constraints for the reference case etc. These all firmly ground our model “in reality”. Regarding the glacial period and sediment data: we make use of all of the existing core records that we know of. However, one of the major conclusions of this study is also that observational data are too sparse. More core records of C and P in open ocean settings are needed if we truly wish to understand the dynamics of P in the glacial ocean. To address the comments of the reviewer, we have carried out a major rewrite of the manuscript

Specific comments

[This model was designed to look at the P cycle - and therefore, the most important results are the changes in the P cycle between the different simulations. The removal of P from the ocean is messy - it can be removed in multiple phases, which have complex relationships with the redox and biotic conditions near the sediment-water interface. Therefore, the discussion should really focus on these removal processes, which is very novel.]

Indeed, this model simulates the redox-dependency of P burial by including the process of Fe-P formation/dissolution and enhanced recycling of P relative to C from anoxic remineralization of organic matter in the sediment. It is therefore of most interest to look at the coupled changes in oxygen and the phases P burial in the simulated glacial scenarios.

We have expanded the discussion of the results focusing on P removal to the sediment

[In contrast, I don't think these simulations should be used to try and calculate the pCO₂ changes - pCO₂ varies as a function of many things, as shown by scores of other papers, and I think that a proper CO₂ budgeting is outside the scope of this paper. As such, most of the first paragraph in section 3.2 can be removed.]

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We have removed this paragraph as suggested.

[It is stated that there is a deepening of oxygen minimum zones in some of the simulations. However, this is not shown. Horizontal average profiles could be helpful here. There should also be some discussion of why the oxygen minima deepen - it is not clear to me why this would occur.]

In the LGM simulation with inputs of C and P from shelves, the deepening of the Atlantic and Indian Ocean tropical oxygen minima relative to preindustrial conditions is associated with increased productivity at the surface in those regions; the model parameterization for sinking dead organic matter (POC) does not include lateral advection, therefore, increased POC remineralization in the water column leads to enhanced oxygen depletion.

We have now included a figure showing the deepening of the OMZs including an explanation.

[p 4825, 2nd par: Why is the Fe input discussed in both this paragraph and the previous paragraph? I don't understand the distinction of 'particulate Fe'.]

We distinguish between dissolved Fe, which we call simply "Fe", and particulate Fe, which we refer to as Fe-oxides. The inputs of particulate Fe and the sediment Fe dynamics were not included in previous biogeochemical ocean models of the Fe cycle. We describe the assumptions we make to set the inputs of dissolved Fe and particulate Fe in the model in two separate paragraphs mostly to stress what was new in this model approach.

However, we think that it is a good idea to reformulate the text and describe all inputs of Fe in the same paragraph.

[It looks like the 'POC' in the 'POC' experiment actually includes P (Table 2), in which case it should actually be called POM (particulate organic matter). This is important, since it's where the additional P is coming from!]

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We agree with the reviewer, POC is changed to POM.

[As stated on p 4827 (last lines), 'there are almost no constraints on the flux of PP from shelves.' Nonetheless, there needs to be some discussion of where the prescribed flux came from, rather than thin air. The prescribed POC fluxes should also be better discussed, rather than simply deferring to Tsandev 2010.]

Our choice for the input of PP is based on a transfer of 0.09 TmolP/yr of total reactive P from shelves to the open ocean at glacial maxima as estimated from Ruttenberg (1993). As the flux of POC we prescribe implies an input of organic-P of about 0.04 TmolP/yr (Table 2), the input of inorganic-P is constrained to be about 0.05 TmolP/yr. We tested the sensitivity of the results to fluxes of PP within 0.02-0.04 TmolP/yr, and we decided to use a low value to avoid very large increases in the P inventory.

We explain this point better in the section. We also expand the references and discussion on the prescribed fluxes of POC.

[The names of simulations: I do not think any of the simulations should be called the 'full LGM', since I don't believe it is yet possible to simulate the full LGM. Please call them LGM, LGM+POM, LGM+PP and LGM+POM+PP.]

We agree with the reviewer, the names of the simulations are now changed.

[Model-data comparisons should be quantitative. Please remove all instances where model simulations are said to 'compare well', 'show agreement', etc. If possible, please plot data constraints (e.g. the Moore and Braucher dissolved Fe data, the Kohfeld export production changes) with the simulated fields.]

Quantitative comparisons are not always possible given the sparsity of observations and the coarse resolution of our model. We have chosen to compare our model results with the observed distribution of dissolved Fe (Parekh et al., 2005), and where possible we have made a quantitative comparison between model and data. Note that when describing their model for the coupled Fe and P cycles, Parekh et al. (2005)

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use qualitative comparisons as well. To improve the comparison between our model results and the observed patterns we will ask permission to include Figures 1a and b from Parekh et al. (2005). Similarly, model trends in export production can be shown together with global paleoindicators of productivity at the LGM (permission needs to be asked to reproduce the corresponding figure from Bopp et al. [2003] or Kohfeld et al. [2005]).

[Figure 6 shows some very interesting patterns, however I do not feel they are well explained in the paper. Would it be possible to dissect these changes in reactive P in terms of changes in accumulation rates, and differences in the forms of P, as well as the drivers behind the changes?]

We agree with the reviewer and expanded Figure 6 to include the changes in each phase of reactive P. We extended the explanation of these patterns in the text in relation to the influence of each LGM forcing (circulation and dust) and induced changes in productivity (Figure 3 expanded) and deep-water oxygen.

[Have the authors considered how reduced weathering fluxes due to lower temperatures and greater glacier coverage might impact the results? Could this negate the shelf effect?]

We haven't looked at scenarios with changes in weathering fluxes. There is for example, an open debate on whether the mobilization of detrital-P into more bioavailable phases may have increased in glacial periods, and thus influenced global primary productivity (e.g. Föllmi [1995]). The data analysis from Föllmi et al. (2009) suggests a substantial increase in the global flux of bioavailable P during the first few hundred years of deglaciation phases. We have added a reference to this work when discussing our choice of forcings for the LGM simulation.

On the other hand, the glacial scenarios from Tsandev et al (2008) and Tsandev et al (2010) include a decrease of 10% in the weathering flux (associated with the change in the hydrological cycle and not in the land weathering regime) and this effect alone

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leads to a less than 10% reduction in primary productivity. Therefore, we don't expect that including such a decrease in the riverine input of P in the LGM simulation with inputs of particulate C and P from shelves would affect our conclusions on the trends in ocean productivity, oxygen and P burial.

[Could the lack of a N cycle impact these results in any obvious way?]

Nitrogen availability could potentially limit primary productivity as well. However, current evidence suggests that phosphorus limitation may have been widespread in the glacial ocean (Pichevin et al., 2009; Nature). If that is truly the case, the lack of an N cycle will not greatly impact our results. We have now included a statement about the N cycle in the text.

[I think the most useful addition to the manuscript could be clear testable predictions from the model. Are there any results here that could be used to design studies of sediment cores, to falsify or support the ideas advanced here?]

The model can provide insight on how P burial rates may have changed in open ocean regions in glacial periods, as well as on the relative influence of each of the LGM forcings on P burial changes. For example, along the southern South Atlantic/Southern Ocean the model predicts an east-west change in P burial rates, which is related to enhanced dust deposition during glacial times. Model predictions of P burial are also valuable over the North Atlantic, where there are very few records of P burial on glacial-interglacial time scales; for example the large increase in reactive P burial predicted on the eastern and western sides of this basin could orientate key areas for future sediment cores.

We have added a few sentences in the conclusion section in relation to this suggestion.

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