

## ***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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We thank the reviewer for the detailed comments on our manuscript. We provide detailed answers to all comments below. We will do our best to address all issues raised in a revised manuscript.

[. . .] The introduction is confusing and should be more to the point: Why do you want to study particulate C, P and Fe inputs in addition to the dust input fields used in previous work? Which model results do you want to improve by considering these additional sources? [. . .]

The main hypothesis we wish to test with the model is as follows: would transfer of  
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large amounts of reactive carbon and nutrients from continental shelves to the glacial ocean (either by erosion of exposed shelves or rerouting through river canyons) - as postulated in earlier work -, have significantly changed the ocean's productivity, deep water oxygenation, and the global pattern of P burial?

To test this hypothesis we present a series of experiments with an ocean biogeochemical model of the glacial ocean with and without inclusion of these particulate fluxes. Our work builds on previous box modeling studies by Tsandev et al. (2010) and Filippelli et al. (2007) for the glacial ocean and our own work on carbon (C), oxygen and phosphorus cycling in the present-day (pre-industrial) ocean (Palastanga et al., 2011).

For a comparison of the relative importance of the magnitude of the particulate fluxes from dust and shelves we refer to Section 2.2 and Table 1.

We also would like to emphasize here that with this study we do not aim at a reconstruction of conditions in the glacial ocean; rather, we present a series of experiments with a biogeochemical ocean model of the coupled oxygen, C, P and Fe cycles to analyze their response to certain processes that might have occurred in the glacial ocean. In other words, since the conditions in the glacial ocean are much less well constrained than in the preindustrial one, these simulations should be seen more as a sensitivity analysis than a reconstruction of glacial ocean biogeochemistry.

We have now modified and shortened the abstract, introduction and conclusions sections to make more clear what the aims and conclusions are of this study and how our model approach differs from that used in earlier studies.

Major points

[1] Importance of sediment processes

The study repeatedly stresses the importance of sediment processes. Already the introduction starts with focusing on P burial in the sediments. Most of the discussion, though, later centers on the simulation of water-column oxygen fields. It would be

helpful to clarify from the very beginning of the introduction what the main goal of the paper is. If it is P burial, then the results and discussion sections should present this area in more detail (it does not help that figure 6 is missing in the pdfs I managed to download).

Changes in deep-water oxygenation and P burial are closely coupled on geological time scales (i.e. on the time scale relevant here) because the efficiency of P burial decreases with increased anoxia (see for example the papers by Algeo and Ingall, 2007; Ingall and Van Cappellen, 1994; and Wallmann, 2003). A major goal of this study is to assess how such coupled changes in P and oxygen cycling may have affected ocean productivity during glacial times. While this has been studied before using box models (see the work of Tsandev et al. 2010 that is cited in the paper), there are, as yet, no studies that have looked at this using a biogeochemical ocean model and that thus include spatially-explicit results. We have now modified the introduction, results and discussion section to make this clearer.

Regarding figure 6: we regret that this figure apparently is missing in the printer-friendly version of the manuscript generated by the software on the BG website. Note, however, that this figure is visible in the online screen version of the manuscript and that the other reviewer was able to view the figure.

[From the results presented I could not really infer the importance of sedimentary processes. How different would the results look if P burial was neglected?]

The model we use includes an explicit though simplified description of sediment reactions, burial, and benthic fluxes. Equilibrium is achieved when the input of P through dust input and river input balances the output of P through burial (the only sink of P in the model). On time scales of 100 kyr and more, changes in either continental inputs of P or burial of P (or both) will change the availability of P in surface waters significantly. We demonstrated this already in our earlier work for the pre-industrial ocean (Palas-tanga et al., 2011). In a scenario with extra input of P, while neglecting P burial would

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lead to a too high primary productivity and an overestimation of the area and extent of oxygen depletion.

We have now included a statement to this effect in the discussion section.

[The response of the different LGM experiments to additions of PP and/or POC seems pretty linear. I think that a more detailed analysis is required to show how linearly/non-linearly the different PP, POC, dust supply routes interact. Are there significant non-linear feedbacks that could give rise to a non-linear additive behavior of the various P (and Fe, C?) inputs?]

The response of the model to the addition of particulate material is indeed rather linear. If we chose to deliver an enormous amount of organic C from shelves into the open ocean, the situation would be different: bottom water anoxia would develop and trigger a positive non-linear feedback from preferential P regeneration. However, it is not our goal here to investigate possible non-linear effects in the system; instead we wish to study scenarios that are relevant to the glacial ocean. These involve more moderate inputs of terrestrial material.

We have now included a statement to make this clear in the discussion section.

[2] Importance of particulate Fe The model description emphasizes the separate consideration of "highly reactive Fe" and "particular Fe". From the tables/text it is not completely clear whether all experiments use inputs of both types of Fe (I assume that this is the case). I think that in order to support the above claim it would be appropriate to show the sensitivity of the model's results with respect to the supply of one (or both) iron types.

Since the previous version of the model didn't include the Fe cycle, in Section 2.1 we describe the assumptions we make to set the input of Fe in the model and Section 3.1 describes the results of the new reference run; these basic assumptions remain unchanged in the following experiments, though the magnitude of the Fe inputs may

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vary.

The cycling of highly reactive Fe in the ocean involves both dissolved and particulate Fe, and here we refer to particulate Fe as Fe-oxides. The basic difference between our model and previous models is that we include Fe sediment dynamics and burial of Fe-oxides. The Fe cycling in the sediment is included because Fe-P interactions play such a key role in controlling P sequestration in marine sediments. The inclusion of a burial flux of Fe implies that we must include an external input of Fe to the oceans as well. We consider two external sources of Fe to the ocean and for each of these we make different assumptions. We assume that a fraction of the total input of Fe from dust is soluble (and this is the only external source of dissolved Fe in the model), while the remaining part of the dust input of Fe sinks (as Fe-oxides) to the ocean floor without further dissolution in the water column. For the riverine source of Fe, we only consider the input of Fe-oxides, since inputs of dissolved Fe from rivers are considered to be relatively small, i.e.  $0.1 + 0.1 \text{ Tg y}^{-1}$  (Raiswell, 2006).

We have modified section 2.1& 2.2 and Table 1 to make more clear what assumptions we make regarding Fe inputs in each simulation.

[Also, from the material presented it is not obvious why the input of particulate Fe into the sediment is relevant for the distribution of water-column O and P.]

As we mentioned above, including a parameterization for the precipitation/reduction of Fe-oxides in the sediment is relevant for simulating the dynamics of Fe bound P in the sediment. Because of P sorption to Fe-oxides, the retention (and burial) of P in marine sediments is enhanced when the overlying bottom waters are oxygenated, while under anerobic conditions, dissolution of Fe-oxides promotes the return of P to the water column. Then, a more efficient recycling of P from sediments can affect marine biological productivity, which in turn favors deep-water oxygen depletion and benthic P release, in a positive feedback loop.

We have included an explanation on the link between the Fe and P cycles in the sedi-

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ment and the potential consequences for water column oxygen and P.

[Is it possible to estimate how relevant the input of particulate Fe is with respect to Fe input from hydrothermal vent systems or from continental margins? This might change with glacial/interglacial changes due to sea level change. How good is the assumption to neglect possible changes in these supply routes?]

The input of Fe from hydrothermal vent systems has been estimated at  $1.4 + 0.5 \text{ Tg y}^{-1}$  by Raiswell (2006). This input of Fe will certainly impact the burial of Fe and P in areas surrounding the vents. However, we do not expect this to change significantly over glacial-interglacial time scales. Thus it will not in any way impact the conclusions of our study that we have not included that source of Fe here. We fully agree with the reviewer that inputs of Fe from continental margins may have changed during glacial/interglacial change. We have included this effect by assuming a speciation of 50% Fe-P in the flux of particulate inorganic P from shelves (Fe-oxides are related to Fe-P by a constant Fe to P ratio of 20). However, because estimates of the flux of Fe from continental margins to the open ocean are uncertain even for current conditions, we did not perform additional experiments that explicitly consider an increase in the input of Fe from rivers to the open ocean during glacial times.

[3] Importance of Fe-P sink This process seems to be underestimated in the model (p.4830, l.22). Does it matter, if not why not?

We are aware that the model underestimates the sink of Fe-P. In the present formulation, the formation of Fe-P in the deep sea is tightly related to the input of Fe-oxides from dust, and this flux is chosen to be consistent with the estimate from Raiswell (2006), i.e.  $1 + 0.1 \text{ Tg y}^{-1}$ . We have tested runs with higher inputs of Fe from dust (i.e.  $2 \text{ Tg y}^{-1}$ ) and although these lead to larger Fe-P concentrations in the sediment, the overall relative increase in Fe-P burial flux is very small. Therefore, we decided to use the input of Fe from dust consistent with estimates. Also, because, either for interglacial or glacial conditions, the model doesn't simulate patterns of bottom water

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suboxia/anoxia, having an underestimated reservoir of Fe-P in the sediment shouldn't affect the estimate of P recycling from sediments. We have modified the text to make this clear.

[With Fig.6 absent in my pdf, I cannot really comment on the analysis, but it would be helpful to have a more detailed interpretation of the Fe-P concentration changes in terms of the simulated changes in O<sub>2</sub>.]

We agree with the reviewer and we have modified the text to discuss in more detail the relation between changes in O<sub>2</sub> and Fe-P.

Minor points:

p.4826, l.25. What annual mean dust deposition fluxes are prescribed? glacial/interglacial ones? Both, glacial and interglacial fields of annual mean dust deposition are prescribed to force the model preindustrial and LGM scenario, respectively. We have now specified this in the text.

p.4827,l.2: what exactly is meant by LGM forcings? LGM circulation, LGM dust, LGM Fe oxides? LGM forcings here refers to the LGM circulation field and LGM dust field. We have now specified this in more detail in the text, with a reference to Table 1.

p.4827, l.6 "almost" no grid points with depths < 200m. This is not really a good argument. You could equally well say that there is almost no ocean area with depths < 200m. Why should the model grid underestimate the shelf area? It might equally well overestimate it? The model we are using here is a coarse resolution ocean biogeochemical model (3.5 degree x 3.5 degree) that does not include a detailed representation of the coastal zone. As a consequence of the large grid cells, there are few grids that are truly "coastal" with an average depth of <200 m.

We have now included a statement in the model description section to make this clear at an earlier point in the manuscript.

p.4828, l.24. What do you mean by reasonable agreement? We will include the com-  
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pilation of observational data of Fe in the surface and intermediate waters given in Fig. 1a and b in the paper of Parekh (2005) in our Figure 1 assuming we will receive permission from the publisher. Similar to Parekh et al. (2005) we find that the modeled distributions are in qualitative agreement with the trends in the sparse observations. Given the sparsity of the observations, a more quantitative comparison is not very meaningful.

p.4836,l.12 there are no observations of O<sub>2</sub> in the glacial ocean, all we have are observational estimates. We have rephrased the text.

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