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Biogeosciences Discussions

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

Anonymous Referee #2

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General comments

There are many competing explanations for the glacial-interglacial variations of pCO2. These include changes in ocean circulation and sea ice, and changes in the marine ecosystem. Among the ecosystem changes, it has been suggested that the supply of nutrients (iron, nitrogen and phosphorus) may have increased.

This paper is focused on one of the older variants of the nutrient increase theories: the shelf nutrient hypothesis. This idea has received relatively little attention by much of the paleoceanographic community, perhaps because of a lack of observational targets. This paper comes from the relatively small number of researchers continuing to

pursue the entirely defensible idea, and is, as such, a potentially valuable contribution. However, I feel that the paper fails to develop the idea in a useful way.

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.

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.

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.

- 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.

- 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'.

- 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!

- 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.

- 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.

- 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.

- 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?

- 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?

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

- 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?

Interactive comment on Biogeosciences Discuss., 9, 4819, 2012.