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Biogeosciences Discussions

Interactive comment on "Phosphorus burial in the ocean over glacial-interglacial time scales" *by* F. Tamburini and K. B. Föllmi

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The authors are grateful for the reviews of Wallmann and of two other anonymous colleagues. We take here the opportunity to answer their comments.

The main point raised by all reviewers is the limited amount of sites that have been studied. As stated in the paper several times, we are aware of this. As stressed by Wallmann, a spatial variability also emerges from our dataset. However, the fact that the interglacial estimate that we present is comparable to previously published values makes us confident with regards to our dataset. Moreover, in comparison to other published studies where a glacial-interglacial reconstruction in P burial fluxes was attempted, this work presents the highest number of sites, whose sediments were all analysed using a multi-proxy approach (mineralogy, organic carbon) and, for what concerns P, a sequential extraction. We think, in fact, that the data published here are

a valuable addition to already existing databases, and will be useful for any future study and model aiming at a better understanding of glacial-interglacial changes in nutrients, productivity and climate.

We also agree on the fact that our reconstruction and the model presented in the paper are limited by number of studied sites and, most important, by the fact that interglacial changes in P input and output are poorly constrained. This remark is valid, helas, for any kind of modelling study. As such, we are aware of the limitations of our approach, but we are convinced that studies like this are still necessary to build up a wider understanding of processes related to environmental change on glacial-interglacial timescales.

The remark by Wallmann on the need of a rigorous statistical evaluation is not quite clear to us. In the case he would refer to a multivariate statistical analysis, in order to evaluate correlations between different P species from different drill sites, we would argue that this would be beyond the scope of this contribution. In the case he would refer to an evaluation of the variability of the data, we have stated in the manuscript that differences between MAR and concentrations differences are not statistically significant. And we have provided in the Supplementary Material all the raw data. In the revised manuscript, we will provide also the analytical errors associated to each extracted P phase.

Reviewer 1 cited the paper by Tsandev et al. (2008) as a study to compare to our results. We were indeed not aware of its publication by the time we wrote this paper. We have now added references to this paper in our revised manuscript, and adapted it accordingly, in taking into account the outcomes of the Tsandev study.

MAR were calculated as the product of 1. P concentration, 2. the dry bulk density (DBD), and 3. sedimentation rates. References to sources of DBD and sedimentation rates are provided in "Supplementary Material".

Table 1 indeed reports average values for Glacial (G) and Interglacial (IG) times. In

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the modelling we assumed that there is a linear change between full interglacial (at 120 ky) and full glacial (at 20 ky), which were represented by the average G and IG P burial flux. As said in the paper, this is an approximation because actual values of input and output are not constrained. Moreover, we have always used burial fluxes for our calculations. When referring to glacial terminations, we discuss our data and the results of the modelling and compare them to changes at single sites, most of which span to MIS 6.

Another remark of Reviewer 1 concerns post-depositional changes in the distribution of P phases in the sediments. About the decreasing trend of organic P and changes in other reactive phases, it is a common assumption that if organic P is mobilized during post-depositional mineralization, the dissolved phosphate may contribute to other reactive pools, such as loosely-bound P and authigenic P, provided suitable conditions in the sediments. If this was the case, down-core changes in these reactive phases would not stand for environmental changes at the time of deposition, but for post-deposition redistribution of P among reactive pools. However, at all locations, the amount of organic P lost during mineralization, does not account for the changes of the other reactive phases.

Interactive comment on Biogeosciences Discuss., 5, 5133, 2008.