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Biogeosciences Discussions

Interactive comment on "Benthic phosphorus and iron budgets for NW-African slope sediments; biogeochemical processes and the importance of bioturbation" *by* K. Küster-Heins et al.

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We thank Richard Jahnke for his careful reviewing of the manuscript, and the detailed and constructive comments. All comments will be taken into consideration and we try to answer at best. Please find our statement below.

Specific Comments: p. 5382, l. 5. In the discussion of the 14C age determinations, the authors dismiss the fact that the data do not display a linear increase in age with increasing sediment depth to bioturbation and scatter. Two of the cores (9510 and 9518) actually display gradient reversals (i.e. younger ages below older ones) which cannot be attributed to random particle mixing but are either due to measurement uncertainty

or to non-local transport processes. It would be better for the authors to provide a bit more thorough discussion and whether non-local transport could alter their interpretations.

We agree with this comment that 14C ages display a clear reversal gradient. In the revised version of our manuscript we will include this important statement in the result section. However, we can exclude uncertainties in age determinations, because the precision of the applied method is ensured. Our model results will not change our conclusions significantly if sedimentation rates that are for instance 20% different would have been used.

p. 5383, l. 20. While I have no criticisms of the discussion provided for cores 9518 and 9519, the authors do not discuss the results from core 9510 where the pore water P maximum is significantly above the main Fe pore water maximum. For consistency, all three cores should be discussed.

We assume the pore water P maximum from core 9510 is an artifact, probably resulting from pore water extractions by squeezing sampling. This points to the obvious discrepancy between both pore water sampling techniques. For this reason, we are not able to give a reliable discussion on pore water chemistry in terms of P and Fe for site 9510 (cf. Table 6). Only nitrate, ammonium and alkalinity data are acceptable for our scientific discussion.

p. 5384, l. 25-26. The authors state that there are no indications of non-steady state conditions or movement (I assume vertically) of the redox boundary. As mentioned above, the 14C results cannot be interpreted simply with continuous sedimentation and simple random particle mixing. In this manuscript there is no other supporting information such as MnO2 distributions. The authors should provide the evidence upon which their 'steady state' and 'no significant movement of the redox boundaries' statements are based.

Unfortunately we don't have data for MnO2. However, we will clarify our steady state

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assumption in the revised version.

p. 5386, l. 17. The authors suggest that there have been temporal variations in terrigenous input while suggesting steady state previously. I suspect that these are not in specific conflict but represent different scales. The authors may wish to provide a length and time scale of there 'steady state' assumption.

Of course, this is an important point, which is apparently not comprehensible in our manuscript. We will revise and explain this issue in more detail.

The technical issues identified will be corrected.

Interactive comment on Biogeosciences Discuss., 6, 5373, 2009.