

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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Received and published: 28 July 2009

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

(1) The introduction part of the manuscript could be strengthened if also the significance of iron, as an important element involved in the cycling of P in marine sediments, is introduced.

We will include the importance of iron in P cycling in the introduction.

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(2) Figure 1 and 4 as well as table 2 are redundant and could be explained and integrated to the relevant text parts. In general, the amount of 7 tables and 8 figures is too much for this manuscript.

We will remove Table 1 and 2, and Figure 4 in the revised version of our manuscript. All other tables and figures are essential for our description and discussion.

(3) The figure and table legends are in part a bit sloppy. They should have a concise but descriptive legend which describes the content figure/table in more detail, e.g. Table 1: What is meant with surface carbon and CaCO₃? Table 1 will be removed.

What does the number (. . .-3 and so on) following the GeoB sites means (same for Figure 2)?; Table 4: What are the mean values are based on (total core length)? The number 9510-3, or rather the ending is an essential description of the core, because during field campaign several cores were retrieved at one site, e.g. MC GeoB 9510-3, GC 9510-2. Table 4: the mean values are calculated as the sum of concentrations of each sample over core depth divided by the number of samples.

Figure 2: The legend is not consistent with chapter 2.2. The authors should be more specific according to results obtained from squeezer and rhizons (e.g. GeoB 9510).; Figure 3: The caption is too short and for better comparison same scales for x-axes should be used.; Figure 5 and 6: All symbols and abbreviations used in the figure must be defined (e.g. Fetot, PCDB, Fetot-CDB, symbols for TOC and C:P ratio). We will rework and clarify it in the revised version of the manuscript.

(4) As no oxygen profiles were measured by the authors, expected penetration depth of oxygen and literature data should be added to support the discussion about the relevance of oxygen in general and the potential zone where re-oxidation processes may occur (chapter 4.1).

Oxygen penetration depth is estimated to be about 1 cm (Wenzhöfer, F. and Glud, R.N., 2002. Benthic carbon mineralization in the Atlantic: a synthesis based on in situ data

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from the last decade. DSR I 49(7): 1255-1279). We will add this information in the revised version.

(5) The authors mentioned in chapter 2.2, that results obtained from squeezer samples were underestimated caused by high sensitivity of ferrous iron to oxygen. I can not follow this argumentation because samples were operated under anoxic conditions in a glovebox (p. 5377, l. 13). However, squeezer samples are not used for further calculations and discussions and thus, results and method should be omitted.

Of course, our argumentation is a little bit unclear, but we assume that the concentration profiles from squeezer samples are an artifact, possibly the result from oxygen contamination during core handling or later during pore water extraction in the glove box system. For this reason, to demonstrate the obvious discrepancy between both extraction methods, we show both squeezer and rhizon pore water results in Figure 2.

(6) The authors gave no methods for drying and milling of sediment samples (see specific suggestions). However, if sediment samples were dried and milled under oxic conditions, I have concerns that sequential extraction of sediment samples gives authentic values for remaining pore water P and reducible/reactive Fe-bound P as ferrous iron is very redox sensitive and co-precipitation of P may occur. Thus, the first would be extremely underestimated and the latter would be overestimated. I would suggest weakening your statements or add supportive data and references.

The samples are oven-dried (at 105°C) and ground (agate mortar). We are familiar with the aforementioned problem (cf. De Lange, 1992. *Marine Geology* 109: 115-139); this would not significantly change the interpretation in the scope of our study.

(7) I think it would be worthwhile to consider the implications of the findings beyond this study site. Would these results apply to all marine sediments? Are there any implications for the C cycling in the sediment or the P and Fe cycling in the overlaying water body?

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These are interesting questions! In general budget calculations are possible when focussing on both pore water and solid phase data, and mixing processes are a site-specific component.

Some specific suggestions for improvement are given below (usually by page: line):
5377:9 Please provide more information about filtration of water samples (e.g. pore size of filter material). The pore water was retrieved through 0.2 μm cellulose acetate filter.

5377:12 correct spelling of "squeezer" (check whole text!) We will correct it in the revised version.

5378:15 What do you mean with $\sim 0.4 \mu\text{m}$? Circa 0.4 μm , $\sim 0.4 \mu\text{m}$.

5378:16 The wet bulk density is calculated by mass of wet sediment divided by volume of wet sediment and values for sediments and soils are typically above 1. Please check your calculations. We will check our calculation.

5378:22 Method for GeoB 9510 and 9519 is given in the text but how did you examine the solid-phase speciation of P for GeoB 9518? We will clarify it in the revised version.

5378:29 Please provide more information about drying and grounding the samples (e.g. temperature, oxic or anoxic conditions). We will clarify it in the revised version.

5380:15 There is just one visible maxima of phosphate over depth in Fig. 2, respectively. We will change it in the revised version.

5382:2 A discussion why the ^{14}C age determinations do not display a linear increase in age with increasing sediment depth is missing. That the ^{14}C age displays reversal gradients is related to non-local transport.

5382:20 The writing needs to be improved (. . . produced in the C. . .)! Text is missing; we will include 'produced in the euphotic zone'.

5384:7 How did you measure the oxic zone and what was its distribution? Cf. (4)

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Table 3 change “reducible” to “reducible” We will change it in the revised version.

Table 6 Why did you calculate the diffusive fluxes for both C:P ratios? As presented in Fig. 6, only 350 would be representative for your sites. We wanted to show the expected diffusive flux derived from average marine biomass versus the calculated Corg:Porg ratio obtained from solid phase data.

Figure 2 As mentioned before, results obtained from squeezer samples did not reflect the in situ porewater concentrations and should be omitted. Caption: A solid line was used to represent pore water concentrations of ferrous iron. We will change the figure caption.

Figure 5 replace solid lines for Fetot-CDB with dashed lines We will replace the line in Figure 5.

Figure 7 part B is redundant as you never refer to Catot data in the manuscript We will remove Figure 7B.

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