

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.

Anonymous Referee #2

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General comments:

Extended chemical analysis of porewater and solid material from three sediment cores taken on the continental margin of NW Africa were examined to unravel the local geochemical conditions that control benthic P and Fe cycles and to identify, understand and quantify the processes that affect the (re)distribution of P in surface sediments. Applying a simple balance approach authors were able to show that particle mixing probably due to bioturbation must be considered for the interpretation of the sediment data. The study could be of interest for marine biogeochemist but need thoroughly revised before it is acceptable for publication. The major problem of this study is that applied methods

C1211

not allow a concise discussion of the interrelating processes of benthic P fluxes like sorption, organic matter mineralization, microbial uptake, biotic or abiotic iron reduction, formation of P minerals. Also the influence of bioturbation remains vague due to the lack of any direct evidence (detection of organisms) or involvement of lab experiments. The manuscript would strongly benefit from a re-focusing of the study aims and a condensing or sharpening of the discussion section (see specific comments).

Specific Comments:

- 1) The title of the study is a bit misleading or to promising. It should be considered that benthic phosphorus and iron budgets are just shown for three sediment cores and that biogeochemical processes and bioturbation are not investigated in detail. My suggestion is: “A simple balance approach for the interpretation of P distribution in sediment cores from NW African continental margin” or something similar.
- 2) Although the introduction is written in a concise way it needs to be clarified if the study provides really something new for the scientific community or if it is just an evaluation of known things. The latter seems to be true. 3) P. 5377, Line 24. Since the squeezing method led obviously to substantial changes of chemical composition of pore water chemistry the data obtained by this method should be omitted. Note, that also manganese might be oxidised using the squeezing method (p. 5378, Lines 6–7). Since manganese data are not presented in this study this sentence can be deleted anyway.
- 4) P. 5378, Lines 4–6. For what purpose diluted samples were acidified with 1% HNO₃. Probably, not to measure total sulphur (following sentence) or to repeat nitrate analysis?! This sentence should be deleted.
- 5) P. 5378, Lines 11–12. I assume that the porosity was calculated? And, please indicate the source of diffusion coefficients. “temperature- and substance-dependent” can be deleted.

C1212

- 6) P. 5378, Lines 14–15. It might be useful for the readers to give some information about the used model Explicite. What was modelled (if steady state conditions can be assumed or not?). The cited references seem to be inadequate.
- 7) P. 5379, Lines 2–6. What does it mean: “. . . , good to fair correspondence was obtained for cores 9510 and 9515, . . .substantial difference for core 9518.”? Please give numbers.
- 8) P. 5379, lines 9–10. It should be clarified if the leaching solution was filtered before of P analysis.
- 9) P. 5382, Lines 1–5. What about the precision of this method? Are there any other (non-bio)turbative processes like growing and rising gas bubbles?
- 10) P. 5382, Line 11. Are any oxygen data (oxygen profiles) available from the sediment surface? If not, I would just delete “under oxic conditions”. Since nitrate is detectable in the upper zone “oxic” might be replaced by “aerobic” alternatively.
- 11) P. 5382, Line 13. “The total release of P by this process is hard to quantify”. This also holds true for the other mentioned P releasing processes below. I recommend condensing the whole section 4.1 substantially. All speculative parts should be deleted, for instance the last para in this section: “Our data clearly indicate . . .”. It was stated that sulphate reduction can be neglected (p. 5383, Lines 4–5). Are any sulphate data available for the overlying bottom waters? If so (net) sulphate consumption rates can be calculated by the second derivation of sulphate concentration gradients or by using the computer program Profile V1 (Berg, P., N. Risgaard-Petersen, and S. Rysgaard. 1998. Interpretation of measured concentration profiles in sediment pore water. *Limnology and Oceanography* 43(7):1500–1510.). Since fixation of Iron by sulphide is argued later in the text (p. 5386, Line 13) there must be sulphate reduction or is sulphide coming from deeper layers?
- 12) P. 5384, Line 28. Please specify “moderate time scales”.

C1213

13) P. 5385, Line 1. What a decrease of Corg could be expected in case that bioturbation or other mixing processes are absent, above all at this low levels? Can be also assumed that rising of gas bubbles is of importance in investigated sediment layers (e.g. M.P. Haeckel, B. Boudreau and K. Wallmann, Bubble-induced porewater mixing: a 3-D model for deep porewater irrigation, *Geochim. Cosmochim. Acta* 71 (21) (2007), pp. 5135–5154.)?

14) Section 4.2. This discussion is a bit confusing or long-winded. Please condense and highlight the main points. In particular, p. 5385, Lines 3–23 needs to be revised in order to increase the readability of the text.

Technical Corrections:

P. 5375, Line 9. Replace “P-cycle” with P cycle. Please check also the rest of text to delete the dashes (e.g. P. 5375, Line 19; Line 28, P. 5376, Line 4, etc.).

P. 5376, Line 23 and also in the rest of the text the unit wt% was given for matter contents. Probably it means % of dry matter?!

P. 5377, Line 3. Write “NW Africa” instead of “NW-Africa”, see also the title.

P. 5377, Line 8. Write “in situ”.

P. 5377, Line 11. Write “squeezing” instead of “squezzing”, please check also the rest of the text, e.g. p. 5378, Line 6 and Figure 2 (but squeezer data should be deleted anyway),

P. 5377, Line 17. Delete concentrations behind “alkalinity”.

P. 5377, Line 18. “retrieved” might be replaced with “gained”.

P. 5380, Line 6. Values should be rounded: 23–26 and 30–39.

P. 5380, Line 24. The numeration of single fractions needs to be changed: first: “NH₄Cl-extractable biogenic P”, then “oxide-associated P”, . . .

C1214

Figures and Tables are nicely drafted so far, but it must be clarified for Table 4 if P concentration is related to dry sediment. Figure 2: solid lines were used instead of dashed line for P and Fe concentration profiles, and please indicate the charge of the species: SO_4^{2-} , NO_3^- , etc.. I assume that phosphate is present in the protonated form in your system; SRP (soluble reactive phosphorus) might be an alternative?

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

C1215