

Interactive comment on “The influence of pulsed redox conditions on soil phosphorus” by R. Scalenghe et al.

Anonymous Referee #2

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General comments:

This study is showing that pulsed wet-dry cycles change the amount and/or composition of chemical extractable P forms rather than permanent wet (reducing) conditions in different P enriched soils having contrasting properties. The issue is rather interesting and new not only in the context of expected climate changes. A high frequency of changing redox-conditions in soils can be also found under current conditions just due to direct anthropogenic hydrological changes or even under “non-disturbed” conditions in soil layers affected by natural oscillating groundwater levels. Hence, the authors have to think about in which soil systems a high frequency of pulsed redox-conditions is likely - to my mind mainly in floodplains and drained peatlands. Do these systems have the soils chosen in this paper? Although the paper fits in the broad scope of the journal I

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strongly recommend the authors to submit the manuscript to a more soil specific journal. However, before a thorough revision of the whole manuscript is necessary in order to fulfil the requirements of a scientific paper and to my mind the experiment should be either repeated or it must be really clarified that freeze drying has no impact on P forms like ‘organic P’ in soils considered in this paper. According to my own experiences and also taking the literature into account freeze drying strongly influence microbiota and consequently also the portion of ‘organic P’ (e.g. Hilliard & Davis 1918, Soulides & Allison 1961, Schlichting & Leinweber 2002, Cernohlávková et al. 2009). To say it in a more ironic way at the moment the paper could be also entitled “The influence of freeze drying on soil phosphorus”. The primary investigations described in the Methods are unpersuasive to exclude artefacts like a change of organic P forms, namely of microbial P. In general I am convinced that the chosen experimental conditions are not suitable to mimic the suggested environmental changes. I have to apologize all these unpleasant words, but the authors should take some time to answer the question: Do you really believe that findings of this study can be interpolated to real field conditions? Apart from this the readability and the reliability of the text must be much improved, in particular the result section need a “harmonization” with the discussion or vice versa (see specific comments).

Specific Comments:

1) The introduction is wasting too much time with general well known and also with superfluous things instead of explaining more the crux of the story: why a comparison of soils having different properties would be useful in the context of pulsed redox-conditions. Some of the relevant properties should be named and hypothesis can be deduced then. A very interesting point of the paper is that the same 12 soils were used that experienced continuously reducing conditions before. However, it cannot be the aim of the study to discuss “some general environmental consequences of the loss of P” (P. 9012, lines 19–20). Just note that the “phosphorus cycle and its interaction with other environmental compartments has been the subject ...” (p. 9010, Lines 23–25)

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of research since the beginning of the last century. Some sentences need rewording (see technical comments) 2) Material and Methods. Section 'Soils': I recommend including a table itemising the 12 studied soils and showing relevant properties such as organic matter content, pH as well as the initial P-fractionation data. Section 'Pulsed reducing conditions - . . .': How the vials were flushed with oxygen (P. 9014, Line 4)? As mentioned before, for me it is not clear why the freeze drying is preferable to air drying (P. 9014, Lines 7. . .). Air drying is more closed to real field conditions; the more so as it induce different physical changes as highlighted by the authors themselves. It remains obscure how the initial testing (P. 9014, Lines 10–16) can exclude the likely effects/artefacts of freeze drying. I agree with the authors that 20 day saturation period should be sufficient to reach reducing conditions. For the discussion of the results it might be useful to consider that one soil needs longer than the other (if some detailed results exist). Section 'Laboratory methods': I assume that standard analytical methods were used, if so please refer to this (P. 9015, Lines 5-6). It must be considered that the first NaOH extraction step also remove organic P, the same holds true for the second extraction step. Please indicate how it was distinguished between formerly organic bound P and inorganic P. 3) The result section must be carefully revised. At the moment data presentation it is rather confusing, complicated and also filled with neglectfulness regarding units and terms (see also technical comments). For example, three different units are used to express the MRP concentrations (mmol m⁻³, g P m⁻³ and mg P kg⁻¹). This must be unified and of course standard units should be used. It is also not clear if presented results are related to soil dry matter or not. The first three paragraphs of the result section can be deleted. It was already mentioned in the Method section that all soils reached reducing conditions or if available any detailed results should be shown. In general the presentation of data is not consistent. Why not showing the changes for P forms separated by soil groups as done for MRP. In overall, it remains obscure if presented changes are significant or not. For example I cannot see any significant differences of P form changes in Fig. 3. 4) The discussion is rambling in many places and not always referring to the results. I recommend

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deleting all speculative parts, e.g. the first section (P. 9019, Lines 11-27 and P. 9020, Lines 1-2). The same should be done with the last part of the discussion (P. 9023, Lines 14-29, P. 9024, Lines 1-5). Until the forth/fifth redox cycle results support the idea "alternate reducing and oxidizing conditions would promote the solubilisation of these (Fe, Mn) oxides . . .pulse by pulse (P. 9020, Lines 14-18", however later on the 'reactivity' of the system seems to be slowing down (see Figure 1). Another general problem is that results are discussed which are not shown, like pH decreases. For me and also according to literature it is surprising that pH was decreasing under reducing conditions, usually pH decrease happens at oxic conditions in acid soils (e.g. Smolders et al. 2006). Moreover, a strong pH decrease leading to dissolution of phosphates in well buffered calcareous soils (P. 9021, Lines 19-23) is rather unlikely even under oxic conditions. For the interpretation of the interesting decrease of organic P pool I strongly miss the impact of soil drying/rewetting as discussed by Turner & Haygarth 2002. In general, the nice idea to choose 12 soils with different properties is more or less getting lost within the discussion. According to the results there are some differences in the soil responses. This should be emphasized and discussed. 5) I recommend to shorten the conclusions or to re-write it. Summarizing the results is not useful (P. 9024, Lines 14-18). For me it is not clear how the (bio)available P should be managed. If the Olsen-P issue is so relevant for this study then it must be already pointed out in the Introduction

Technical Corrections:

P. 9010, Line 2: pulsed wet-dry cycles might be better than "reduction-oxidation cycles"? P. 9010, Line 23: Write: "The phosphorus (P) cycle" and use another word instead of "compartments" P. 9011, Line 4: Delete "more" write "towards a reductive system". P. 9011, Line 5: What are "P-carbonates"? P. 9011, Line 14: Rewrite "The most comprehensive understanding . . ." P. 9013, Line 18: I would use "CAL" for calcareous soils instead of "C" P. 9013, Line 21: "extreme properties" must be reworded P. 9014, Line 9014: Write "chemical P fractions" P. 9015, Line 20: Write "P bound to Ca" P. 9015, Line

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11: "Olsen and Sommers (1982)" is missing in the References P. 9015, Line 25: The standard unit for P concentrations should be 1.6 $\mu\text{mol L}^{-1}$ P. 9016, Line 2: Write: "Fe and Mn concentrations" P. 9016, Line 18: What is meant by Feox/Fed? (Feox/Fered) P. 9016, Line 24: I expect that Eh was measured and then converted into pe, please clarify. P. 9017, Line 1: "The pattern of changing MRP concentrations ..." must be reworded. P. 9017; Line 10: the unit must be changed either mmol L⁻¹ or mg L⁻¹, but consistently throughout the paper. P. 9019, Line 17: Finish the sentence "...influencing P" release? P. 9020, Line 23: Reword "oxidative supersaturation" P. 9021, Line 23: Reword "elevated ionic concentrations"

Figures and Tables are nicely drafted so far, but not always self-explaining. Table 1: If total phosphorus (P) is changing within the experiment the changes in soil organic P should not be expressed as the percentage of total P. Table 2: It must be explained that MRP is molybdate reactive phosphorus. Usually MRP is expressed as a concentration of a solution (e.g. mg/L or $\mu\text{mol/L}$ etc.) If the MRP analysis are related to the soils then another term must be used, e.g. 'water-extractable P'. It must be also clarified if dry soil is meant or not. And, the soil groups must be explained: C (calcareous), SA Figure 1 must be revised: I cannot recognize dotted lines and the separation between weekly and seasonal (left part of the Figure) is not very meaningful to my mind. It must be explained what is meant by cti and ct0. Figure 2: see Figure 1. Figure 3: Some of the P fractions must be explained: CB, CDB. Are these changes significant? Figure 4: Write: Forms of phosphorus (P) and delete stage 4 or explain it, "seasonal exposure" should be also deleted. Figure 5: Changes in Q/I?

References

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