

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

Anonymous Referee #1

Received and published: 4 January 2011

This manuscript describes the effect of pulsed reduction oxidation cycles on soil P for 12 European soils that had been “overfertilised” with P but otherwise that present different properties. Soil samples were incubated for 20 days under reduced conditions and then oxidized during two days 11 times, and different fractions of P were measured in the solution and on solid. This manuscript is addressing a relevant but complex topic. The experiment is well conducted and the results are interesting. The strong decrease in organic P in particular was a surprise. I have some comments which should be addressed before the manuscript can be accepted.

General comments The manuscript is presented in a complicated manner (figure 1, Olsen P, residual P). The reader must spend a lot of time to understand the content of the manuscript. If the authors want this paper to be read and cited they should keep

C4576

the most important information and remove “secondary” information and the side story (Olsen P), this will simplify the presentation. The authors give at several places in the text the impression that their results could be upscaled at soil, or landscape levels in a changing climate. I would be extremely careful with that since they use 3 g of soil per tube and they have ways to set up reducing and oxidizing conditions which are most probably not representative from what is happening in the field (or they should demonstrate it). The decrease of organic P is a surprising but important result of this paper (attention: data in table 1 should be shown with their standard deviation to show their variability). However, before this can be accepted results must be presented in a more convincing manner and they should also be better discussed. Organic P is the result of a subtraction between HCl extractable P from an ignited sample and HCl extractable P from a non-ignited sample, so that changes in organic P could be due to changes in one or to the two terms of the equation. The changes in HCl extractable P from ignited samples and from non-ignited samples should be shown. These results should also be compared to those obtained from the sequential extraction. It would also be good to see the organic C content of the (non-ignited) samples at the end of the incubation. The fact that organic matter acts as electron acceptor is clear, but since the C/P ratio of soil organic matter is at least 100, a significant decrease in organic P by this process should be accompanied by a very strong decrease in organic matter. Was this observed? I know that strongly reduced soils actually store organic matter. . . Or, alternatively, the decrease in soil organic P observed in this work was due to other processes. Which can be these processes? Statistics, presentation of data: The authors do not speak about statistical analyses, and they present their results in different ways (see e.g. figures 2 and 3). I know that their data set is complex but would there be another way to present these data? I would try to carry out multivariate analyses as principal components analysis.

Specific comments The introduction could be structured in a different way. The paragraph beginning with “At a global scale. . .” is interesting but it arrives a bit like a hair on the soup. Similarly As is not really in the scope of the paper. I would delete these

C4577

two aspects. On the other side I suggest that the authors state what they expect from reduction/oxidation cycles, formulate hypotheses based on known processes, and then develop their objectives. P 9011 Lines 3-5: "the shift in redox potential. . . also involves a release of P from the organic fraction", do you have references for that; if yes they should be cited. P 9011 line 6: shift in pH means here a decrease, then say it. P 9011 line 19: what is meant by "under-drainage"? P 9011 lines 21-22: the fact that riparian zones are subjected to flooding is clear, should it really be mentioned? P 9011 last line: we will have also more erosion, leaching, run off. . . these are also important processes for P losses. P 9012 Last sentence of the introduction: what does "some general environmental consequences" mean; this does not look very specific, delete. P 9013 lines 8-10 "Nutrient surpluses occur. . . " : I do not understand this sentence. For me nutrient surplus occurs when nutrient input on a field is larger than nutrient export by plant/animal products. If you introduce losses in the equation, you allow them to occur, and then you perpetuate them. . . P 9013 lines 10-12 the definition of overfertilisation should be accompanied by a reference. P 9013 line 13 the definition of intensive farming is very variable from country to country, so that this should be accompanied with a reference. If it is not possible then delete "intensively". P 9014 paragraph starting line 7: This paragraph is difficult to understand. Freeze drying might indeed be better for the soil porosity but is it really relevant for a soil system at 313K? Probably not. . . Try to reformulate this paragraph saying that freeze-dry was used because it was practical for this experiment and that it did not cause any artifact. P 9015 Lines 8-9: Since changes in organic P are so important in this paper you should describe in more details the method of Legg and Black which might not be known to everyone. How many grams of soil are taken, what is the molarity of HCl, how long does the ignition last. . . What does the term "broadly considered as "organically bounded"" mean. And start the sentence "This assumes. . ." just after the end of the former sentence without starting a new paragraph. This is confusing. P 9015 line 19. I have a problem when one assumes that CBD will allow removing occluded P in aluminum oxides. This assumes that CBD dissolves aluminum oxides. But aluminum oxides cannot be reduced. Please

C4578

either demonstrates that it is possible (and relevant) or delete. P 9015 lines 21-22. Your calculation of residual P is not clear as it does match what Hedley et al call residual P. An extraction with NaOH (+CB) will solubilize inorganic and organic P. The CBD extract might also remove organic and inorganic P. HCl will remove mostly inorganic P. As you are only measuring inorganic P concentrations in the extracted solutions the residual P is total P – PiNaOHCB – PiCBD – PiHCl, and therefore the residual P contains all the organic P. This must be explicit. For instance you might call this fraction "residual and organic P". As the text is actually formulated one could understand that the total P is measured in the different extractants. P 9016 end of the material and method section: there is no section on statistics, please add one explaining which statistical analyses were done and how. P 9016 two first paragraphs of the results section. They probably refer to the figure 1 (although it is not said in the text). But going to figure 1 one sees a part "pulsed" (these are the results of this paper) and a part "continuous" (from Scalenghe et al 2002). Furthermore the "pulsed" part is again divided in two parts "weekly" and "seasonal" which have not been introduced before. I had understood that the pulse sequences were 20 days of reducing conditions followed by one day of oxidized condition and one day of drying. So, what do weekly and seasonal mean? Is the graph of your previous publication really useful here? Please simplify the figure and explain it better in the text so that it can be understood. You do not show the pe and pH data. Why? I think that it would help understanding the results. Fig 2 is again difficult to understand. What does weekly/seasonal stand for? What are these lines? Which lines are dotted/continuous? Fig 3 is clear (except for the comment on P forms given above). What do not you use this representation for the other P forms? Fig 4: What is meant by "collected" in the legend? If I understand it, this figure suggests that continuous reducing conditions and pulse redox have the same effects on PiHCl and on PiCBD, but that the pulse redox leads to more PiNaOHCB and continuous reduction to more residual P. This is what is said at the end of the section 3.3. But the paragraph from line 26 (P 9017) to line 11 (P 9018) is confusing. Section 3.4 P 9018, fig 5, table 2: Results are again expressed in a very complicated manner. Why do not you show the

C4579

changes in Olsen P with time for your different soils? Instead we see a “supposed Q/I” and differences in Olsen P which are difficult to understand. A simplified presentation will allow you to reach the same conclusion: MRP is more sensitive to Olsen P to these changes in redox conditions, but it will be more transparent for the reader. The fact that Olsen extracts forms of P that are available and forms of P that are non-available is known since a long time. Discussion/conclusions This discussion is too long and goes in many chemical details which I am afraid are not really supported by the data (e.g. on Fe and Mn speciation). I would be much more prudent in extrapolating to the field level in a changing climate. The importance of NO₃ is mentioned, but NO₃ was not measured. So, are the results really relevant? The decrease of organic P is as said above extremely interesting but this needs to be clarified. It is said in the conclusions that Ca associated P decreases during these cycles, but I could not see that in the results. Conclusions are drawn on management. . . I do not understand what do you want to say here. . . should we inundate the soils to obtain more available P? How would these redox cycles be linked to risks of P losses?

Interactive comment on Biogeosciences Discuss., 7, 9009, 2010.