

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

R. Scalenghe et al.

scalenghe@unipa.it

Received and published: 11 May 2011

Biogeosciences Discussion 7:9009, 2010 Interactive comment#1

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. Remove “secondary” information and the side story (Olsen P), this will simplify the presentation. Response: We rearranged the whole text and the iconography associated according to the suggestion of the referee.

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

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reducing and oxidizing conditions which are most probably not representative from what is happening in the field (or they should demonstrate it). Response: In the field a series of complementary causes smooth the inferences obtained in the laboratory, in particular the effect of temperature and local moisture coupled with the availability of oxygen, nitrate and other electron acceptors (Zak and Gelbrecht, Biogeochemistry 85:141, 2007). The purpose of our experiments is a guide towards field experiment, as is for most lab experiments, and a source of insight for management at planning level. A key conclusion is that relatively short periods of full reduction repeated over time mobilize much more phosphorus than a continuous anaerobic longer period. This fact, in terms of management of overfertilised soils is certainly useful. For instance, in the correct choice of the type of irrigation technique.

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). Response: Done.

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. The increase in solubility of inorganic phosphorus from ignition is correlated positively with the content of organic phosphorus as described by Legg and Black' method (1955). HCl extractable P from ignited values are robustly correlated with total soil P determined by fusion with NaOH ($r = 0.93^{**}$). For this reason a duplication of the columns in Table 1 would not add much in our opinion. These results should also be compared to those obtained from the sequential extraction. Response: It would also be good to see the organic C content of the (non-ignited) samples at the end of the incubation. It would certainly be interesting to have the organic C content at the end of the incubation. Most unfortunately, tough, the experiment finished and the treated samples discarded. It would not be economically feasible (and probably not scientifically

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reasonable, either) to re-run today all the experiments. Would these tests, if concluded successfully, materially affect our conclusions (e.g. Ploegh, *Nature* 472:391, 2011)? 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? The point on stoichiometry is honestly intriguing but since we did not perform any speciation of the organic matter it is quite complicated a discussion of the obtained results in this way. On the other side, being a close system phosphorus does not decrease as total amount but only a shift through different forms occurred and a very strong decrease in total organic matter was not observed.

I know that strongly reduced soils actually store organic matter. . . Response: Organic carbon influences release of P because it acts as the main electron donor, i.e. it favours reduction, and because its oxidation can bring along some P release. The addition of organic matter to the soil might have an influence on the onset and development of anaerobic conditions, in particular low-molecular weight compounds which are more prone to rapid oxidation (Petrizzelli et al., *Soil Science* 170:102, 2005). And, in general, organic matter additions to a flooded soil accelerated the development of anaerobiosis. In any case, the presence of a less degraded type of organic matter, is relatively more resistant to oxidation (Ajmone Marsan et al., *Plant & Soil* 282:239, 2006). Elena Molino et alii on October 2007 at MESAEP symposium presented data on the 'Influence of different kinds of organic matter on soil reducing conditions and on the degradation of phenantrene'. Only short chain carbon molecules, within a monthly period of full reduction ($p_e + pH < 6$), act as electron donor, developing CO₂ as product of the reaction. Redox reactions strongly influence the chemistry of phosphorus in the soil through indirect effects associated with changes in pH affecting the solubility of Ca phosphate and sorption reactions.

Or, alternatively, the decrease in soil organic P observed in this work was due to other processes. Which can be these processes? Response: The release of P to the solution

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during reduction of acid soils has been attributed mainly to the reduction of Fe oxides, which act as a sink for P by either adsorption or occlusion, and to the mineralization of P-containing organic molecules generating organic molecules that can displace P from adsorption sites (Huguenin-Elie et al., *European Journal of Soil Science* 54:77, 2003). Furthermore, the increase in pH, and the consequent reduction of the positive charge of the oxide surfaces, can bring about desorption of P from surfaces that are not yet reduced (Demello et al., *Soil Science* 163:122, 1998). Ca-phosphate mineral formation may enhance P removal as long as the soils remain flooded and Ca and pH levels remain high (Newman and Pietro, *Ecological Engineering* 18:23, 2001).

The release of P to the solution during reduction of calcareous soils. . . Response: Re-adsorption of excess P in solution by oxides or organic matter may be the reason for the pulsed increase in either inorganic or organic P forms.

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. Response: Multivariate analysis is based on analysis of more than one statistical variable at a given time while factor analysis is used to uncover the latent structure of a set of variables. Both of them are frequently stalled either by the dimensionality of the system or by the number of observations. This is the case of our dataset: only twelve soils and temporal based observations. The groupings realistically sortable are those from principal components analysis, the simplest of the vector-based analyses which, by multiplying the matrix, remain proportional to the original vector grouping our dataset into calcareous (E1, E2 and I3), slightly acid (D1, D2, E3 and I2). Discriminant function analysis and clustering, specifically k-means, sort out our twelve groups of observations into the same clusters. Sorry, no more than this.

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

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these two aspects. Response: Done.

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. Response: We modified the text in the light of this comment.

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. Response: Newman S and Pietro K 2001 Phosphorus storage and release in response to flooding: implications for Everglades stormwater treatment areas. *Ecol. Eng.* 18, 23–38.

P 9011 line 6: shift in pH means here a decrease, then say it. Response: The pH of the twelve soils varies from 4.6 to 7.8. then, under reducing conditions narrowed to between 6.5 and 7.2. For acidic soils the increase in pH is due to the reduction of Fe³⁺ species in the soil. For calcareous soils, the decrease in pH of about one unit resulted mainly in the dissolution of Ca compounds. Probably to the increase in CO₂ as well.

P 9011 line 19: what is meant by "under-drainage"? Response: Drainage.

P 9011 lines 21-22: the fact that riparian zones are subjected to flooding is clear, should it really be mentioned? Response: They are subjected to periodic flooding, this is the key factor of soil P release.

P 9011 last line: we will have also more erosion, leaching, run off. . . these are also important processes for P losses. Response: Done.

P 9012 Last sentence of the introduction: what does "some general environmental consequences" mean; this does not look very specific, delete. Response: Done.

P 9013 lines 8-10 A 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. . . Response: Done.

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P 9013 lines 10-12 the definition of overfertilisation should be accompanied by a reference. Response: In this paper we consider as overfertilized soils having twice the optimum concentration of available P as estimated using the official analytical methods of its relevant country (e.g. Barberis et al., 1996).

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". Response: As defined by OECD.

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. Response: Freeze drying is preferable to air drying because it is much quicker (24 hrs vs weeks/months), more homogeneous because it does allow the particles to settle and allows the recovery of all the soil material whereas air drying implies scratching dried soil from the vial's wall (a 20 ml vial!). We cannot state, however, that it does not produce artifacts; those that are presumably produced are nevertheless acceptable for our purposes.

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. . . Response: Half a gram of fine earth was placed in a 25-mL pyrex vial within a sand bath at 240°C for 1 hour. In this type of baths, the heat, is transmitted to the vials through the sand that is in direct contact with the recipients. The ignited sample were transferred to a 25-mL tube, using small amount of water to remove any particles adhering to the vial. To the ignited soil in the tube, and to a comparable half a gram sample of nonignited soil in a similar tube, were added 5 mL of concentrated HCl, and heated on a sand bath for 10 minutes (final solution temperature about 70°C). After removing the tubes, we added an additional 5 mL of concentrated

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HCl, followed by standing at room temperature for 1 hour, then adding about 15 mL of water, then centrifuging before the colorimetric measurement.

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. Response: Done.

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 either demonstrates that it is possible (and relevant) or delete. Response: Done.

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 – P_i NaOHCB – P_i CBD – P_i HCl, 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. Response: We would personally agree that the Hedley et al. (1982) latter method might be more accurate in the identification of residual P but we used the method of Olsen and Sommers, (1982). And in the method we used, residual P is not defined exactly. We modified the text according to your suggestions.

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. Response: Routine descriptive statistics and tests were run – with STATISTICA 6.1 sw - so we decided not to detail that in the text.

P 9016 two first paragraphs of the results section. They probably refer to the figure 1 (although it is not said in the text). Response: Done.

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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. Response: We have simplified the figure. Our feeling is that a comparison with the dataset of our previous publication would be helpful to the Reader. The graph, in fact, is not from our previous paper on continuous flooding (see below) but those data are redrawn for comparison at the same scale of the pulse redox conditions experiment. At this scale, the relative huge variations observed in the continuous flooding are smoothed, by far.

You do not show the pe and pH data. Why? I think that it would help understanding the results. Response: It is true that the change in P concentration during a redox cycle depends on a combination of the shifts of redox potential and pH. The rate of decline of pe+pH differs between soils although the general trend is similar and within the chosen period of reducing conditions all soils reach a steady reduced state (pe+pH <6). This is the rationale of the experiment: alternating the steady reducing environment with pulsed oxidising conditions. For this reason, we measured pH and Eh within all vials at the beginning and at the end of individual cycle. Initial and final pe+pH values do not change through the cycles.

Fig 2 is again difficult to understand. What does weekly/seasonal stand for? What are these lines? Which lines are dotted/continuous? Response: We have simplified the figure that refer uniquely to pulsed reducing conditions. Lines indicated ranges of MRP and organic P variation when they are statistically correlated.

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? Response: We have simplified the figure and clarified the text.

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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. Response: We have deleted this figure.

P 9017 from line 26 to line 11 (P 9018) is confusing. Response: Done.

Section 3.4 P 9018, fig 5, table 2: Results are again expressed in a very complicated manner. Why do not you show the 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. Response: We have deleted the whole section 3.4.

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. Response: Done.

The importance of NO_3 is mentioned, but NO_3 was not measured. So, are the results really relevant? Response: N pollution has received more attention than P pollution by EU authorities in the past, probably for its direct implication on human health, which explains that EU directives related to the control of non-point pollution have been mainly focused on N (Ondersteijn et al., *Agric. Ecosyst. Environ.* 92, 283–296, 2002). Promoting realistically the application of mitigation options at catchment scales implies the reduction of both P and N loads in an integrated way. The importance of nitrates is mentioned here as P and N should not be seen as decoupling issue but as common problem in term of management of overfertilised soils.

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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. Response: Actually, it was said (p 9024 line 13) that it changes, not decreases. In fig 3 it can be seen that it decreases at first than increases and finally it decreases again.

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? Response: A wrong management (bio)available P over short time periods may provoke a number of disadvantages from an environmental perspective.

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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? Response: We think that majority of our soils would be expected to experience changing redox-conditions, not only because of the physical location (floodplain) but also in response to structural damage (and poor drainage) resulting from arable cultivation.

Although the paper fits in the broad scope of the journal I 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

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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. Response: We acknowledge the fact that freeze drying could influence biota and as a result organic P. This is true as well, actually, in the case of drying it self. In both cases an artificial lab manipulation affects soil which under natural conditions would, probably, behave differently. In particular, this reviewer takes into account specific literature in which artefact are likely occurring. Soulides and Allison (1961) incubated from 7 to 13 days 50 to 100 grams of soil under up to WAC of water, Hilliard and Davis (1918) melted frozen tubes gradually immersing them in coldwater then incubating at 37°C. Both of them dried the samples but they did not measured Eh so, although a reduction was highly plausible it is not possible to sort out the causes of the observed phenomena. Cernohlávková et al. (2009) sampled in field unfortunately according to ISO 10381-6:2009, a method that provides guidance not applicable to the handling of soil where anaerobic conditions are to be maintained throughout. By the by, the position of the anonymous referee is very harsh and would annihilate the results of any lab experiment. We appreciate her/his sarcasm, as it could nicely fuel new ideas, but we would appreciate even more that the referee suggests what to do to mimic the suggested changes. In years of experience we did not find any better way. One possibility is to go to the field but even then artefacts are inevitable. In ecology, for instance, methods based on litterbags and subsequent linear mass-loss balances, which are seen as the experiments closest to reality, present intrinsic limitations such as underestimation of the activity of (macro) fauna, extrapolation of unrealistic microclimatic conditions coupled with realistic loss of material during field incubation. Nevertheless, this approach is generally considered the more appropriate compromise and results obtained allow significant inferences (Austin and Vivanco, Nature 442:555, 2006; Isidorov et al., Biogeosciences, 7:2785, 2010; Manzoni et al., Science 321:684, 2008). More specifically, we thought it was extremely interesting to have the opportunity to compare soils from very different origins and the only way this

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can be done at a reasonable cost is to sample them and bring them to the lab. We are very well aware that sampling itself, not to talk of sample conservation and preparation, freeze drying –actually any lab treatment – would alter the original soil but we feel this is the best or at least the most accepted, way a comparison between soils can be done.

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? Response: No, we do not believe that nor we intended to do that. Our goal was to simulate conditions that happen in the field and compare soils. Our results and methodology could nevertheless guide a field experiment as is for most lab experiments. Maybe the actual results are not directly applicable but the point is that the experimental design is relatively simple and shows the potential for change. We know areas of fields get waterlogged, especially after cultivation. We also know that organic matter in form of slurry/FYM is added in autumn/spring which would presumably speed up reduction. These were all agricultural soils.

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. Response: Done.

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). Response: Objectives of study is that soils differ in the susceptibility of P fractions to the cumulative effects of short-term, episodic changes in redox conditions. Certain P fractions will be more susceptible to periodic changes in redox conditions rather than under continuous reduction. I wonder if we should try and stress the oxidation stage that influences the P forms.

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Just note that the “phosphorus cycle and its interaction with other environmental compartments has been the subject : : :” (p. 9010, Lines 23–25) of research since the beginning of the last century. Some sentences need rewording (see technical comments). Response: Done.

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. Response: Done.

Section ‘Pulsed reducing conditions - : : ’: How the vials were flushed with oxygen (P. 9014, Line 4)? Response: Done.

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. Response: Done, please see reply to rev#1.

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). Response: Time needed by these soils to reach a certain pe+pH was the subject of previous paper (Scalenghe et al., 2002). Here we pointed at being certain to have reached a value between 5 and 9 for pe+pH to be sure to have included the P release peak.

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. Response: Done.

Please indicate how it was distinguished between formerly organic bound P and inorganic P. Response: Sorry, we could not understand what is suggested here.

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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. Response: We revised the result section. As for the units, we report concentrations in the soil solution and in the soil: samples of solution were collected under the reducing conditions (p. 9014 line 25) and then these are analysed for Fe, Mn and MRP with results given as a ratio in Figure 1. The remaining concentrations are given in mg P per kg of dry soil.

It is also not clear if presented results are related to soil dry matter or not. Response: Done.

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. Response: Done, partially.

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. Response: Done

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. Response: Figures 3 and 4 are meant to represent changes in P forms. Figure 3 reports changes of P forms through time while Figure 4 refers to overall changes, i.e. it compares P forms before and after the whole experiment.

The discussion is rambling in many places and not always referring to the results. I recommend 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). Response: Done, partially.

Until the forth/fifth redox cycle results support the idea “alternate reducing and oxidizing

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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). Response: Done.

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. Response: The pH decreases in these calcareous soils of about one unit resulted in the dissolution of Ca compounds (Scalenghe et al., 2002). In these soils, the trend towards neutrality that occurs when soil is subjected to anaerobiosis resulted in a initial decrease in pH for calcareous and an increase for the others: the range in pH initially was 4.6-7.4 and narrowed towards the end of the experiment to between 6.5 and 7.2.

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. Response: Turner and Haygarth (Nature 411:258, 2002) measured the amount of water-soluble phosphorus by extracting soils at field-moisture capacity after drainage from saturation for 48 h. Redox parameters were not measured, apparently. Results are comparable to ours certainly.

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 Response: Done.

P. 9010, Line 2: pulsed wet-dry cycles might be better than "reduction-oxidation cycles? Response: These were actually designed as full reduction – oxidation and again

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reduction cycles. This occurs in nature when water occupies soil porosity then when air replace it. The design purpose must be mentioned firstly, wet-dry is a mean not our aim. It would be very unlikely if describing the true aim of the paper.

P. 9010, Line 23: Write: "The phosphorus (P) cycle" and use another word instead of "compartmentens". Response: Done, it was supposed to be 'components'.

P. 9011, Line 4: Delete "more" write " towards a reductive system". Response: Done.

P. 9011, Line 5: What are "P-carbonates"? Response: Done, it was supposed to be 'P associated to carbonates'.

P. 9011, Line 14: Rewrite "The most comprehensive understanding : : ." Response: Done.

P. 9013, Line 18: I would use "CAL" for calcareous soils instead of "C". Response: C and the other abbreviations have been used in many other papers dealing with these soils and CAL would introduce an element of confusion to the Reader. For this reason we do not accept this suggestion.

P. 9013, Line 21: "extreme properties" must be reworded. Response: Done.

P. 9014, Line 9014: Write "chemical P fractions". Response: Done.

P. 9015, Line 20: Write "P bound to Ca". Response: Done.

P. 9015, Line 11: "Olsen and Sommers (1982)" is missing in the References. Response: Done.

P. 9015, Line 25: The standard unit for P concentrations should be 1.6 $\mu\text{mol L}^{-1}$. Response: Done, it was supposed to be 50 mg m^{-3} .

P. 9016, Line 2: Write: "Fe and Mn concentrations". Response: Done.

P. 9016, Line 18: What is meant by Fe_{ox}/Fed? (Fe_{ox}/Fered)? Response: Done. It was supposed to be Fe_{ox}/Fed. Although fluorite is normally employed in mining with the

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purpose of smelting iron, the ratio Fe/F is a ratio not very helpful as far as we know in redox chemistry of phosphorus. A typographical error is the most probable explanation for Feox/Fed.

P. 9016, Line 24: I expect that Eh was measured and then converted into pe, please clarify. Response: In keeping with the sardonic style of the reviewer, we would be pleased of being informed of a method of direct electron counting suitable for our purposes.

P. 9017, Line 1: "The pattern of changing MRP concentrations : : ." must be reworded. Response: Done.

P. 9017; Line 10: the unit must be changed either mmol L⁻¹ or mg L⁻¹, but consistently throughout the paper. Response: Litres are not included into the International System of Units, so we have homogenised the concentration in solutions in g (or mg) per cubic metre.

P. 9019, Line 17: Finish the sentence ": : influencing P" release? Response: Done

P. 9020, Line 23: Reword "oxidative supersaturation". Response: Done.

P. 9021, Line 23: Reword "elevated ionic concentrations". Response: Done.

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. Response: It is the usual way to compare different soils and compensate, partly, for experimental errors. We do prefer the present way of expression of our results.

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 μ 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 : : . Response: Done.

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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. Response: Done.

Figure 2: see Figure 1. Response: Done.

Figure 3: Some of the P fractions must be explained: CB, CDB. Are these changes significant? Response: Done.

Figure 4: Write: Forms of phosphorus (P) and delete stage 4 or explain it, "seasonal exposure" should be also deleted. Response: Deleted.

Figure 5: Changes in Q/I? Response: Deleted.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C5479/2011/bgd-7-C5479-2011-supplement.pdf>

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

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