Biogeosciences Discuss.,
doi:10.5194/bg-2016-98-AC1, 2016

Biogeosciences
Discussions
Open Access

Interactive comment on "Spatial Patterns of Phosphorus Fractions in Soils of Temperate Forest Ecosystems with Silicate Parent Material" *by* Florian Werner et al.

Florian Werner et al.

florian.werner@wzw.tum.de

Received and published: 16 June 2016

Answers to Reviewer 1:

"1) The authors do not set up why the reader should care about small horizontal variation in soil properties (each of the many samples are taken from a 70x100cm grid). As far as I can tell there was only one grid per site. If the question is how do soils vary horizontally vs. vertically, this sampling design seems unlikely to be informative in any way that is ecologically relevant."

In the introduction of the original version of the paper, we didn't explain the importance of small-scale variation of phosphorus (P) in soils well enough. The small-scale spatial

Printer-friendly version

Discussion paper

distribution of soil variables like pH, metal cations (esp. Ca, Fe and Al) and organic and inorganic ligands can be a crucial factor governing plant root activity (Hinsinger, 2001). In addition, local P depletion, mobilization, and transformation by plant roots results in small-scale (μm to cm) heterogeneity of soil P contents and P fractions (Hinsinger, 2001, Hinsinger et al., 2005). In our study, we performed a combined assessment of three issues: (1) P distribution by grid sampling (we addressed our research question by using a 70x100cm grid, supported by a nested sampling, s. manuscript P3 L13-16), (2) distribution of different P binding forms by wet-chemical analysis, and (3) distribution of explanatory variables. This assessment is of significant ecological relevance, because accumulation of P in soils and specific soil fractions has implications for plant P acquisition. We hypothesized that at sites with poor or intermediate P supply, spatial heterogeneity of P (fraction) contents results in (micro-)sites of P accumulation. At the moment no information can be found on the heterogeneity of physical and chemical soil properties of P content and P fractions at different scales. In the revised version of the manuscript we intend to clarify the aim and the novelty of our study by highlighting the (i) unknown issues of small-scale variation of P, (ii) our research hypothesis and (iii) how both are addressed in our study.

"2) All of the kriging done basically reproduces a lot of what is known, there is strong variation in soil properties (including C and P) with depth, and in very small areas less variation horizontally."

It is known that there is strong variation of C and P with depth and less variation horizontally. However, we do not know of any studies using P fractionation to address and explain small-scale P distribution patterns in soil and the relationship between the distributions of different P fractions with that of other soil variables (e.g. pedogenic oxyhydroxides). We therefore studied the distribution of total P and different P fractions to gain insight into spatial heterogeneity of P binding forms at different stages of pedogenesis (better: podzolization, s. next Author's answer). Geostatistics offered the possibility to produce high resolution maps, not only of total P and the P fractions, but also of important variables for P binding. To the best of our knowledge such maps have not been published earlier.

"3) Any comparison of trends between the 4 sites, which differ in parent material, climate and elevation, is compromised by the issue of pseudo replication. At least from my reading there was only one soil pit excavated at each site, and soils were taken (in exhausting detail) from different parts of one pit. Thus comparison of the sites (e.g. effects of pedogenesis on basalt vs. gneiss) has only one true replicate for each site."

It is true that one soil profile per site was excavated and there is one replicate per site. We described the spatial and pedogenetic changes of P distribution at sites with differing pedogenetic age and identified the most relevant factors and mechanisms for these changes. In our manuscript, we used the term "stage of pedogenesis" to distinguish the "pedogenetic age" of our sites (referring to our sites as a "geosequence"). In the revised version of the paper we intend to replace these ambiguous terms using "stage of podzolization" instead to describe the pedogenetic age of our soils. The stage of podzolization is, of course, affected in combined effects of parent material, climate, elevation, and many more factors. The revised manuscript will clearly demonstrate that our study principally focused on P distribution in four soils (Cambisols) with different stage of podzolization of forest soils with siliceous parent material under temperate climate. We collected a large sample size. However, we did not statistically test for differences between parameter (e.g. OC and Ptot content) mean values of the four different soils. We discussed each site result independently and qualitatively set the site in a framework of "stage of pedogenesis". We did not claim to have replicated the sampling of our site, but we also did not statistically test for significant differences. We therefore argue that pseudoreplication is not a critical issue in our study because the differences between the sites are described and discussed qualitatively.

"4) Even if one allows the samples from a single pit to count as true replicates, little can be inferred about the role of parent material, "pedogenic state" or anything else that varies between the sites because so much varies - there is almost 1000 mm/yr

Printer-friendly version

Discussion paper

difference in rainfall between the sites, as well as very big differences in parent material, and differences in temperature."

We admit that it is audacious to develop a detailed description of the changes in P binding forms and P distribution with advancing stage of podzolization (manuscript Fig. 5) based on only one replicate per site. However, Fig. 5 and the comparison of the sites should be seen as a general conceptual model of relationships between stage of podzolization as identified by the distribution of pedogenic Al and Fe minerals and the distribution of different P forms and important P fluxes as derived from our results. We will do more to adequately reason the choice of our sampling sites. Despite the differences, all studied soils were formed from siliceous parent material under temperate climate and are stacked with European beech stands of similar age. The soils showed different pedogenetic age, as seen in stage of podzolization. Pre-studies especially found differing P contents in the beach leaves from these soils. The "sequence of stage of pedogenesis"-Figure is therefore rather a conclusion of our study of P (fraction) distribution, than a rationale of our work. We intend to clarify this issue in a thoroughly revised version of the manuscript.

"Thus in my opinion the study is not appropriately set up to explore horizontal vs. vertical variation within a site, nor to study differences between sites. The major results (that there is large variation in soils with depth) is well known and the mechanisms for this have been explored for decades. The detailed work on P is interesting, but much of it has been published at a coarser scale by the same group."

In the answer to comment (1), we argued that looking at the small-scale variation of P is expedient, ecologically relevant and provides novel information. Main results of our study not only comprise the distribution patterns of total and organic P and organic C (large variation with depth). The detailed work on the distribution of several P fractions and the relationship with the distribution of Alox/di and Feox/di in soils with differing P status ("stage of pedogenesis/podzolization") at smaller scales is essential to explain P distribution patterns in soil. We argue that the mechanisms governing small-scale P distribution are affecting P acquisition of plants and soil microorganisms. The mechanisms are known; however, the detailed description of the spatial heterogeneity of P fractions in temperate forest soils is novel.

"I'm sorry I can't be more positive about this manuscript. I think a more robust introduction that sets up the questions and what is already known would go a long way to helping the reader, but I'm not sure that would over come the limitations of study design that I perceive here. In general I think the introduction could be fleshed out. There should be some discussion of the different ways of assessing P forms (fractionation, NMR for organic P), and what their pros and cons are. If the ultimate goal is to understand pedogenic effects on P availability to organisms, there is a lot more literature that could be cited. If the goal is to see how P forms vary across this particular "geosequence" then I think more material is need to convince the reader that this a compelling question."

We strongly agree with this statement of Referee 1 that the introduction can be improved. In the revised version of the manuscript, we intend to describe pros and cons of methods that are used to assess P forms in soils. Fractionation, as well as NMR and spectroscopic techniques will be mentioned. We did envisage an "ultimate goal": To refine the model of Walker & Syers (1976) for soils from siliceous parent material (1) by determining the spatial distribution of P in soils with different stage of podzolization, (2) by distinguishing more P fractions, and (3) by examining the relations of different P fractions to important soil properties. In addition, we (4) performed analyses for different soil depths which also supplement existing studies (e.g. Frossard et al., 2000, Ferro Vázquez et al., 2014). At last, also the assessment of the patchiness of different P fractions is novel and has not been described before. A revised discussion will go more into detail on these matters, citing more studies.

"Along those lines, given that rainfall differs by almost 1000 mm/yr between sites, and there are different parent materials, it's hard to understand how these can be considered any sort of sequence. Rather it seems to me that it's four sites that have different soils, for a variety of reasons that can not be disentangled."

We admit that the term "sequence" is difficult to keep, even though we have already pointed out that the studied soils share a lot of commonalities (Cambisols, silicate parent material, beach forest, and temperate climate). As discussed in our answer to comment (4), in our original manuscript, we ranked our sites into a general concept of P distribution changes during pedogenesis. We propose to introduce the broader, conceptual model of P (fraction) distribution by "stage of podzolization" in a revised version of the manuscript. The soils CON, MIT and LUE will serve as representative examples for early, intermediate and advanced podzolization, respectively. As discussed in the original version of the manuscript, BBR is a special case of early-stage podzolization with a large capacity to withstand podzolization. A revised manuscript will not use the term "sequence of soils" or "geosequence" any more, but "soils with a different stage of podzolization" instead.

"P1 L12 - what is a geosequence? Perhaps better to explain as you did in the introduction, as a series of sites that differ in P status due to differences in parent material and age."

In the revised version of the manuscript, we intend to rephrase the respective paragraphs according to the previous comments on the difficulty to keep the word "geosequence".

"L22 - I do not think that documenting different pools can be translated into an understanding of the pools from which P is acquired. There can be large pools of P that are not useful to organisms on short timescales."

We agree with the Referee that documenting pools cannot straightforwardly be translated into an understanding of the pools from which P is acquired. Other methods, as e.g. P isotope approaches, would be needed. However, enrichment and depletion zones of total P and/or different P pools as identified for the different soils in our study may give hints about low and high P uptake, respectively.

"L24 - Presumably the pedogenesis you refer to is all fairly early stage, and thus P availability is increasing as primary mineral P dissolves."

Not all studied soils have the same pedogenetic age. The BBR soil has formed from basaltic parent material which has been exposed not earlier than in the early Holocene. CON and particularly MIT have formed from gneiss debris which has been weathered intensively under Tertiary tropical climate. In addition, P availability, as characterized by nutrient P contents of beech foliage, is less at LUE (LUE < CON < MIT < BBR, result from pre-studies). The research project, which our subproject is a part of, intends to study the mechanisms that allow temperate forests on silicate bedrock to sufficiently supply the trees with P. It is true that P availability is increasing as primary mineral P dissolves, but the role of pedogenic minerals and its relationship with organic and inorganic P fractions has not been studied in this detail, let alone put in a broader figure of spatial P pool distribution at different stages of podzolization.

"L25 - I am not sure what is novel about this result. The idea that soil development influences P forms and availability is quite old. What is the novel contribution of this work?"

We agree with the Referee that it is well-known that soil development influences P forms and availability. In the revised version of the paper, we intend to clarify the aim and novelty of our study as addressed in previous answers, particularly referring to the issue of spatial distribution of different P forms with different plant availability.

"P2 L19 - How is soil age determined in this geosequence?"

See previous answers; in the revised version of the manuscript, the choice of sampling sites will be rationalized by the different stage of podzolization and not set in a geosequence.

"L22 - Why is the depth distribution important? Are rooting depths different across the geosequence? If so, how?"

Rooting patterns are affected: We have observations of rooting from the soil sampling campaign which we will include in a revised manuscript. However, our methods focused on the spatial imaging of P fractions in soil. In contrast to earlier studies, we also addressed the horizontal variation of P in soil, which might also affect rooting patterns. We demonstrated that and how P accumulates in the studied soils (both in depth and horizontally). This information therefore has implications on plant and microbial studies on these sites.

"L24 - There is a great deal of literature on the distribution of P in soils, though less about P forms. Steven's work in the early 1970s in New Zealand had a wealth of information about P fractions with depth across the Franz Joseph and other chronosequences. The works from Hawaii (Crews et al, 1995 and subsequent) also has information. I believe Paul Selments has P fractions across the San Francisco Volcanic chronosequence, though I can't remember how much depth information he has. And of course Ben Turner has done a lot of work exploring organic N forms in myriad places and across chronosequences."

We failed to clarify that we addressed only temperate forest soils from siliceous parent material. However, we still reckon that the small-scale 2-D depth distribution in the four studied soils is novel information. Moreover, the wet-chemical fractionation provides additional information on different P binding forms, which were related to other soil properties/variables with potential relevance for soil P such as OC and pedogenic Al and Fe minerals. We thank the Referee for the valuable mention of studies. The revised version of the manuscript will also cite these studies in the discussion.

"L26 - I'm not sure from this intro how your data differ from Prietzel et al, 2016b. I'm sure they are different, but your introduction does not set up that difference very well. From my reading of that paper they also looked at P and what it's bound to across these sites."

Our study actually complements the study of Prietzel et al. (2016b), for we were able to describe the factors controlling P distribution and binding forms (by correlation and factor analysis) at different scales. The approach in this manuscript focused on smaller scales and on distribution patterns within a soil profile (correlation at different depths), whereas Prietzel et al., 2016b focused on P speciation (methodologically), its relationship with parent material and studying soil horizons.

"P3 L8 - Basalt, gneiss and Pleistocene sand are very different parent materials, so I can understand why they would have wildly different P availability, and might host very different forms of P. However, there is no information given as to why the authors suggest these soils are of different age. In Prietzel et al., 2016b it is said that the 10 sites differ in lithology, but I can find no description of how they vary in soil age."

As already described in earlier responses to the Referee's comments, in the revised version of the manuscript the term "soil age", as referred to as "stage of pedogenesis" will be reformulated.

"L13 - This reads as if sampling at each site took place in a single 10cm square. But in truth it took place in a 70x100cm rectangle at every 10cm intersection. Is that correct? If so this text could be clarified?"

The text is indeed not clear enough. The samples were taken in a 70x100cm rectangle at every 10cm intersection. The revised manuscript will clarify this.

"On a more scientific note, why would a single grid be used to get a spatial variation, rather than many different locations? Some more explanation of why this approach was taken is warranted."

In our study, we focused on small-scale spatial variation within a soil profile (cm to m scale, soil profiles). A larger (stand) scale variation study was performed by another group on the same sites and their paper is in preparation. As already mentioned, in the revised manuscript we intend to introduce the importance of small-scale P distribution more pronouncedly.

Printer-friendly version

Discussion paper

"P4 - The motivation for the geospatial aspect of this paper is unclear to me. Until coming to the statistical analysis section of the methods, I had no idea there even was a geostatistical analysis, and even at this point in the paper I'm not sure what the goal of such a small spatial scale analysis is. I think this points to the fact that the introduction is so short that it does not really set up the motivation for the study or the questions as well as it needs to to bring the reader along."

In the revised version of the manuscript, we will rephrase the introduction profoundly. Among other issues, we will explain in detail the importance of small-scale variation of P (s. previous answers). The geostatistics, however, provides information about the P and other element's distribution patterns which are difficult to grasp from a numerical data frame. The revised manuscript will furthermore introduce the geostatistical analysis and reason its value to our study earlier.

"P5 L4 - I don't think anyone would expect uniform distributions of P or any of its forms, and in general would expect higher concentrations of total P and organic P in the upper soils. So I'm not sure why this is a major result."

We suggest introducing our results by: "As expected, the interpolated maps..." We tried to report all results from total P down to smaller P fractions and sub-fractions (organic, inorganic, dithionite extractable, oxalate extractable...). The discussion focused on all results, including the higher contents of total P and organic P at the soil profile scale (s. P8 L4-13). We reckon that we not just reported the spatial patterns of higher total P contents, but discussed binding forms and distribution of P in soils with different stages of podzolization, which are novel scientific contributions.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-98, 2016.

Printer-friendly version

Discussion paper