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

Anonymous Referee #2

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This study presented high-resolution distribution patterns of P fractions and major binding elements at the profile scale (i) to describe the spatial and pedogenetic changes of P distribution during pedogenesis, and (ii) to identify the most relevant factors and mechanisms for these changes. They concluded that even in early stages of pedogenesis, P recycling is a major driver of ecosystem P nutrition, however not as important as in later stages and the stage of pedogenesis in silicate soils, as e.g. visible in degree and state of podzolization, serves as predictor for plant and microbial P nutritional strategies.

I found that their study is interesting, however, I suggest a major revision of the manuscript since the manuscript requires additional data/explanation to support their

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Discussion paper

discussion and conclusion and clarify some data/discussion. Below are my comments and suggestions.

1) They estimated “stage of pedogenesis” of their four soil samples. I wonder if it’s reasonable to compare among soils with different parent material, moisture content or elevation. How were these four sites selected? Explanation of study sites and detailed soil property data are required. What does “mean” represent (i.e. entire soil depth)? What is the sample number (n=?)? P2 L9: I cannot find any data for LUE in the paper by Prietzel et al (2016b). In addition, the soil property data by Prietzel et al. (2016b) for BBR, CON and MIT were different from their data shown in Table 1. For instance, Prietzel et al. reported pH and TP of MIT (surface 0-2 cm) as 3.8 and 1.99 g P kg-1, respectively, whereas their data were 2.9 and 0.72 g P kg-1, respectively. Some soil properties such as texture and clay content should be added in Table 1.

2) They discussed P adsorption mechanisms in acidic soils, yet completely ignored clay content or/and types of clay present in each soil.

3) P5 L4:“The interpolated maps did not reveal a uniform distribution of P in any of the studied soils”. This sentence is odd since no one expects uniform distribution of P in soils.

4) I found Figure 5 very confusing. The x-axis indicates, “stage of pedogenesis and soil acidification”. However, according to Prietzel et al (2016b), the pHs of the soil samples are in the order of BBR (pH 3.1) > CON (3.6) > MIT (3.8) or according to their data, MIT (2.9) > LUE (3.0) > BBR = CON (3.2). Either way, they are not representing the stage of acidification. As I mentioned, I am not sure if they can compare the stage of pedogenesis among their soil samples.

5) Prietzel et al (2016b) estimated ∼65% of total P in the upper layer (0-10cm) of BBR was inorganic P, such as Ca(H2PO4)2 (11%), apatite (11%) and FePO4 (41%). Also ∼40% of total P in the upper layer of MIT was inorganic P, such as AlPO4 (18%) and FePO4 (22%). Yet, Figure 5 shows no inorganic P in the upper layer of BBR or MIT.

Any reason why?

6) P12 L8: How about adsorption of inorganic P onto clays in the upper layer? According to Prietzel et al. (2016b), the texture of BBR ((0-10cm) is silty clay.

7) P12 L24: Effects of root interaction on P transformation in soils should be included when thinking of distribution of forms of P. It will help to add approx. age of trees in each study site. I imagine that when they collected soil samples, they should have observed plant roots in different layers.

8) Figure 2 and 3: I liked the way they showed the distribution patters of TP and different P fractions. However, the range of proportion of each color is not clear. (i.e. what does the range high concentration of P represent?)

9) Table 3: I would like to see actual mean data in addition to the correlation.

10) In conclusion, I suggest adding some sentences to explain how their study can be useful to others and what might be the next step.

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