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Biogeosciences Discussions

Interactive comment on "Phosphorus status of soils from contrasting forested ecosystems in Southwestern Siberia: combined effects of plant species and climate" *by* D. L. Achat et al.

Anonymous Referee #4

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General comments: The authors present a comprehensive overview of various soil physical and chemical data measured across two forest sites and three broad vegetation categories. They also discuss important hypotheses and conclude with a useful conceptual model. A large amount of data is discussed, based on apparently rigorous analytical methods, offering some unique comparisons of how phosphorus (P) is distributed across different fractions and soil horizons under representative forest vegetation in this region. In particular, I enjoyed seeing discussion of microbial P pools, the role of decomposition processes on the relative organic P content, and available

inorganic P at the soil-to-solution interface. These findings in particular could be useful additions to the literature on forest biogeochemistry.

Despite these positive points, I feel that the paper as currently presented is somewhat disjointed and a bit difficult to follow in certain places. In particular, I found the authors' framing of the paper in terms of the possible implications of soil P status for carbon sequestration under climate change (e.g., due to potential P-limitation) to be a bit tertiary to the actual study design and results. For example, the climate change framing receives considerable attention in the Abstract, Introduction, and Discussion, but is perhaps a little odd given the fairly straightforward interpretation made from literature comparisons that these soils appear not to be P-deficient (pg. 6382, line 21).

While much attention is given to the role of vegetation type and climate on influencing different soil P fractions, I wondered if these were really controlled for in the study design. For example, climate-related variables such as precipitation and snow thickness may certainly be important for soil P cycling, but the authors did not, for example, sample multiple sites across a precipitation gradient to assess this. In that sense, I found the mention of climate and vegetation in the Results and Discussion to be really interesting, but perhaps requiring slight de-emphasis or rephrasing so that it is clearer what can and cannot be concluded based on the results (e.g., the authors considered "broad indicators of local climate variation related to topography", or something to that effect). This further suggests a possible limitation of the climate change framing, as the authors rely on two static and fairly general indicators of climate for the upland and lowland sites, respectively, and do not provide much context for how representative these two sites are for the Russian Boreal forest in general.

However, I do not see the above points as *major* concerns, as there is still a lot of insight to be gained from the results of this study. These points solely relate to how the study is framed, possibly warranting rewriting of certain sections in order to emphasize the importance of specific findings. For example, the study design very adequately addresses how factors such as organic matter decomposition processes and available phosphate ions in the soil solution influence P availability, but these received fairly little attention in the introductory material (and overall hypotheses).

I would find it helpful if the authors considered instead focusing a bit more on the insights gained from their study in terms of the mechanisms controlling P availability to these forests. While I have limited knowledge of microbiology and am unfamiliar with the specific methods used to assess diffusive inorganic P (iP), the investigation of inorganic phosphorus at the soil-to-solution interface appears to be quite novel and important in relation to total P content and the other P fractions considered here. Should the importance of these findings be addressed more clearly and its implications for soil P cycling emphasized? The same point likely applies to the role of decomposition processes on mediating organic P availability, which seems quite important.

Overall, I suggest that the authors consider possible ways to highlight the novelty and importance of their main findings for soil P and physical-chemical properties (Tables 1 through 5) while downplaying the carbon sequestration component. Possible replacement of some of the tables with more synthetic or integrative figures could be helpful to show the breakdown of total P into the different fractions, as well as how these relate to key soil properties such as pH, Al/Fe oxides, organic matter quality, and decomposition rates.

Specific comments: In the title, possibly consider changing the wording after the colon to instead read something like "the combined effects of soil physical-chemical and microbiological properties"? (sensu bottom of pg. 6369) The current phrasing of the title seems a bit broad for what the study actually covers. This point also applies to a few paragraphs in the main text (e.g., pg. 6367, lines 4-28, and pg. 6389, lines 4-25).

Pg. 6367, top of page: Possibly add a bit more context for exactly how the study improves our understanding (e.g., by contributing insight on the 'interplay' of broad local climate and vegetation variables with readily available inorganic and organic P pools?).

Pg. 6367 Line 10: Consider adding "... potential of vegetation" at the end of this sentence?

Pg. 6371, line 6: Please provide a short explanation of why the three vegetation plots were not replicated in the lowland site (e.g., were the forest gap and A. sibirica classes not available there?).

Pg. 6372, lines 23-25: I do not understand why HF was more appropriate based on the explanation here. Please clarify what the equation and R-square indicate.

Pg. 6373, section 2.3.2: Please consider adding at least one sentence describing what exactly "diffusive iP at the solid-to-solution interface (Pr)" represents in terms of P availability for vegetation over time. For example, I am quite familiar with common extraction methods for plant-available or soil-solution phosphorus, but your methods appears to be far more rigorous. Adding some explanation here about exactly what the inorganic P pool you are sampling implies for P availability to vegetation (and at which time scale) would help to relate your findings to a much broader array of studies on P biogeochemistry.

Related to the above comment, please consider providing a bit more context on our understanding of the availability of different soil P fractions and at what time scales these might be relevant. For example, one approach for this would be to reference a study (or studies) dealing with synthetic soil P fractionation results, such as the two listed below. A benefit to this is that it would provide additional context for how your results on the relative availability of different P fractions (the diffusive iP versus organic P in particular) fit with broader understanding of soil P dynamics. –Johnson AH, Frizano J, Vann DR (2003) Biogeochemical implications of labile phosphorus in forest soils determined by the Hedley fractionation procedure. Oecologia, 135, 487–499. –Cross AF, Schlesinger WH (1995) A literature review and evaluation of the. Hedley fractionation: applications to the biogeochemical cycle of soil phosphorus in natural ecosystems. Geoderma, 64, 197–214.

In general, there are a lot of terms used in Section 2.3.2. A table providing simple definitions could help.

Pg. 6375, lines 1-8: I found the parameters 'n' and 'r/R1min' to be rather confusing. Please consider adding more explanation in this section about what these mean for understanding P mobility in the soil-water system. In particular, the term "Residual values of parameter n" shown in Figure 3 is not adequately explained, which made interpreting the results quite confusing.

Pg. 6375, starting at line 16: I would find some additional rationale for this exercise to be useful. How does this test fit with your main hypotheses?

Pg. 6379, line 15: Because so many results are presented in the tables for the different P fractions, it would be helpful to know why both P concentrations (mg/kg) and stocks (kg/ha) are important. Are both of these needed, or could one be chosen and then the other set of results added to a supplemental document? Please note that I also found the Results section a little confusing because of some jumping around between "composite"/"profile" and soil horizon sample results. It may be helpful overall to try to make this description a little more consistent, which again could possibly be accomplished by choosing the most pertinent results and moving others to a supplemental table.

Pg. 6382, line 14: Please consider clarifying where these comparisons of "permanent vegetation" are located (e.g., are they comparable northern forests?).

Pg. 6383, lines 1-3: As the authors are likely aware, these issues are commonly discussed in terms of soil "weathering" status / pedogenisis / or soil age. It might be helpful to explain how these podzolic and loess-derived soils fit in terms of weathering stage and the expected amount of total P (see, e.g., Johnston et al. 2003 reference above).

Pg. 6383, line 8 (and elsewhere): there is some discussion about the influence of deeper root systems in being able to access sub-soil P, presumably in the future under

a warmer climate. I would find discussion of how available the diffusive iP that accumulated in the sub-soils would be to the present vegetation and over what time scale more relevant. Has this P essentially 'leached' from the surface horizons and possibly been lost from the forest for an intermediate period?

Pg. 6383, lines 17-26: "This shows that microbiological processes potentially play a key role in [the] P cycle and iP availability...": I found this sort of insight really fascinating and feel that this sort of explanation may warrant greater attention in the Discussion.

Pg. 6384, line 6: This statement is confusing, but I think the authors mean an experimental plot study. Can you please clarify?

Pg. 6384, lines 9-13: This explanation of how inherent differences in soil nutrients were controlled for should probably be moved to the Methods.

Pg. 6389, line 5: One could describe the role of vegetation on decomposition as being "intermediary" or "indirect"?

Pg. 6390, lines 13-15: The authors may consider adding a more impactful statement to succinctly describe their most important findings.

Tables: In general, I found the tabular results a little overwhelming and somewhat difficult to interpret. Replacing some of the tables with a synthetic figure (perhaps something like Figure 4(d) to show all of the soil P fractions in one place) might be helpful.

Table 4: Consider using the same time intervals for diffuse P as in Table 2 (Pr in one week/month/year)? Or choose the most appropriate time interval only?

Figure 1: Is this figure necessary? I would suggest moving it to a supplemental document.

Figures 2 and 3: It seems that Figure 2(a) and 2(b) could possibly be paired with Figure 3(a) and 3(b) to form a synthetic figure that could more easily be discussed.

Figure 2(c) and 2(d) are a bit confusing to me. Perhaps consider describing what exactly these panels show? Also, I found the term “Residual values of parameter n” in Figure 3 unclear.

Figure 5: This is an interesting and insightful figure. I wonder if certain components might be broken down into sub-components (to form a “zoomed-in” view of what your results show, for example, how the various soil and vegetation properties control organic P and inorganic P fractions relative to the total P in soils). I wonder in particular if there is some way to more easily connect the cumulative soil respiration results (Figure 4) to relative organic P content of soils?

Technical corrections: Throughout the manuscript, the word “precipitations” should read “precipitation” (singular).

Pg. 6368, line 28: “are” should be “is”.

There are a few other minor grammatical errors in the text (e.g., a missing “the” on pg. 6381. Line 14).

Interactive comment on Biogeosciences Discuss., 9, 6365, 2012.