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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.

D. L. Achat et al.

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General comments from the authors

The authors thank the four referees for their useful comments and suggestions. We will prepare a revised version of our manuscript and will include all the corrections the referees proposed (see replies below).

Anonymous Referee #1

General: This paper addresses the effect of vegetation and climate in contrasting forest ecosystems on the phosphorus status in soils. The study is comprehensive and includes a number of different soil measurements. The data are interesting but the paper lacks clarity and would need considerable work to improve the readability. There is a major concern with regards to the aim of the study and how this is approached. The study includes four plots at two different locations. Three of the plots are in one location and includes three different vegetation types and the fourth plot is in the second location with contrasting climate. Comparisons for climatic effects are restricted to two plots without any replication and this is also valid for the three vegetation types. The statistical comparisons are thus restricted to comparisons between the different plots but it can hardly be used for generalizations with regard to vegetation and climate. For this, the authors would have needed replicated sites of the different vegetation types and climate conditions. The current design only provides replications within the plots. Neither can interactive effects between plant species and climate be studied; the latter is suggested by the title. The last part of the title is thus misleading. The current focus of the paper does not match the design of the study and conceptual figures such as fig. 5 in the paper are not justified by the data in this study and this is also true for a large part of the discussion. The data itself is, however, interesting and deserves publication. Especially, the more general relationships across all soils are interesting (for instance Figs 2 and 3) and the approach to detail the depth profiles is a strength of the study. I would recommend the authors to focus their paper on these findings (aim 1, line 7) i.e. differences and similarities in the depth distribution of P across the four sites. I can, however, not recommend the paper for publication in its current stage. Below are some more specific comments/suggestions. Reply (1): We agree with the general comments with regards to the aim and the design of our study. So, we will focus the revised version of our manuscript on the first objective (i.e. "P status across soil depths and site conditions, relationships with soil properties, decomposition/recycling processes, organic matter quality and microbial activity"). We will delete the second objective ("effect of soil depth and plant species"), most of Fig.5 and the second part of the title. As suggested by Referee #1 below, we will only explain in the Discussion section that the differences between the plots may be related to differences in vegetation or climate. We will reduce this part in the Discussion and we will revise our conclusions. In addition, we will delete section 4.2.3. “Implications of vegetation redistribution and local climate change”

Specific comments: Introduction; In general, the intro needs more focus towards on the main aim of this study i.e. phosphorus in the soil. The current intro lacks focus and jumps between different topics throughout the text. Almost a third of the intro is related to climate change and C sequestration to motivate the study. There are, however, few studies from boreal forest ecosystems that indicate that these systems are P limited. Instead most fertilizer studies show that boreal forests primarily are N limited. Why would “P scarcity” be a major issue in the type of ecosystems described in this paper. This need at least to better supported in the text and by relevant references. The aims of the study is with regard to the role of vegetation and climate not relevant since the design of the study does not allow more general tests of these effects (see above). You can only speculate in that the differences you see between the plots are related to differences in vegetation or climate but not test them. Reply (2): As explained before, we will change our focus (focus on objective 1). We will revise the introduction and will reduce the number of topics. In particular, the section on the effects of vegetation, climate and future climate changes (lines 5-20 p6368; lines2-6 p6369) will be deleted. Instead, we will develop the section on the possible P limitation in relation to chronic deposition of atmospheric N to better justify our study.

Material and Methods; Overall, the MM section is many times unclear and the structure can be improved. 2.1 I would separate soil sampling and the site description. Give more detail on the description of the soils, especially for the different soil horizons; for most readers the soil classification terminology won’t help much. The general description (3.1.1) and Table 2 could be moved to this section. I would also structure this paragraph so that you start with climate followed by vegetation and soils. The soil sampling is somewhat unclear. What do you mean with representative zones? How big

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are the plots? How did you sample the different horizons besides the samples for bulk density? Were sub-samples from the whole soil layers taken and how? 2.2, 2.3 and 2.4 The separation between these three paragraphs is unclear. For instance, microbial soil properties are included in both 2.2 and 2.4. I suggest that you put all microbial measurements under 2.4 and all physiochemical characterizations under 2.2. 2.3.2 This paragraph is not easy to follow and would benefit by some re-writing. A number of abbreviations are used that are not always clearly defined. For instance the abbreviation iP is used for inorganic P but in the description in 2.3.2 it seems that it refers to phosphate ions, such as Cp and Pw. Why not, for instance, use phosphate in soil solution instead of writing iP in soil solution. The units are sometimes unclear, give the unit for Pr in the text. The mass of soil should be kg and not g in the text if the unit of Pr is mg kg-1 (Fig 1). The definition of Pr can be clearer, why not use exchangeable phosphate since this is what you measure. Later in the paragraph you talk about desorption of the soil solid phase but desorption is a specific process and cannot just be related to the solid phase. In the next sentence you use the term “exchange”. Overall, this paragraph needs some re-writing to improve the clarity. Reply: We thank the Referee for all these suggestions. We will rearrange/rewrite the Material and Method section as proposed. We will used the term “phosphate ions” instead of the abbreviation “iP”, revise the definition of diffusive phosphate ions, and delete the terms “sorption” and “desorption”.

2.4 Did you determine the water holding capacity and how since you added water in relation to it? Give units for abbreviations used in formulas were possible. Reply: Yes, we determined the water holding capacity of the soils. We will add this in the Material and methods section.

Results: Overall, there are far too many subtitles and the section could be condensed and more focused. Reply: We will condense our results and delete subtitles (“Comparison among vegetation types in the Salair mountains” & “Comparison between Salair mountains and lowland site with P. tremula”). We will present the results for the 4 plots in a same part.

Some comments: General, it is not necessary to say “moist composite samples” whenever you mention results were these have been used. You have already explained this in MM. 3.2.1 Is it really justified to extrapolate the Pr values determined from 400 min to up to 1 year; i.e. the linear relationship in Fig. 1. 2. 2.2 Yet another definition appears; explain what you mean with available iP. Reply: We will take into account these comments. The extrapolation of Pr up to one year is justified by previous studies on the method. We will explain and justify this in the Material and Method. We will revise the terms (e.g. available iP . . .).

Discussion I have not given any specific comments since the discussion needs a completely different focus (see above). Reply: We will strongly reduce the Discussion section that focuses on the potential effects of vegetation and climate. We will revise our conclusions and delete section 4.2.3. (“Implications of vegetation redistribution and local climate change”) and most of Fig. 5 (except part 5b).

Anonymous Referee #2

Achat et al. studied the soil P status and the effect of vegetation and climate on P stocks and P availability in soil in Siberian boreal forests. They found high total P storage in the organic layer/topsoils and considerable accumulation of diffusive phosphate ions in subsoils. Vegetation and climate both affected organic matter decomposition and thus, P availability. Although I feel that this is an interesting topic which is of interest to the readers of Biogeosciences, I have major concerns with respect to the experimental design. Therefore, the focus of the study needs rearrangement resulting in a completely rewritten manuscript. Reply: We will change the focus of the study and rearrange the manuscript accordingly (see reply 1 to Referee #1).

Except for the experimental design, the authors produced an impressing and comprehensive data set on the P status of their studied plots. Using this treasure and data published on phosphorus in boreal forests (or ecotones; e.g. Giesler et al. 2002, Turner

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et al. 2004, Vincent et al. 2012), the authors could enhance their conceptual model using an adequate data basis. Reply: We will add these references in the discussion section.

The following more detailed remarks illustrate my decision and might help to improve the manuscript: - 6367/11: The introduction on the mechanisms underlying the role of P for C sequestration must be improved. On the one hand, reduced productivity can be expected in case of P limitation resulting in decreased C sequestration. On the other hand, P limitation might also reduce decomposition potentially associated with increased organic matter accumulation and thus, increased C sequestration. What do the authors suspect? Reply: We will develop the section on the role of P in C sequestration as Referee #2 suggested. It is right: P limitation may also reduce decomposition processes, resulting in an increase in C sequestration in soils. We will add this potential effect in the introduction.

Furthermore, how can the effect of potential nutrient limitation on C sequestration be ranked relative to climate change effects? Reply: Our study was not designed to assess this question. Moreover, as suggested, we will not focus our manuscript on climate change. We can only hypothesize that C sequestration is expected to be limited when N or P are limiting (and would not permit the growth increase expected based on higher CO2 levels in the air). We are not aware of any studies for the boreal zone that have quantified this limitation. Recent studies mostly for temperate zones have given indications that C sequestration potential will only occur for the more fertile sites (Loustau et al. 2005).

- 6369/20-23: The authors aim at studying local climate effects. However, the local climate is not well simulated if laboratory (incubation) experiments are used. Based on the methods used for this study, the second hypothesis cannot be tested. Similarly, the effect of the duration of snow cover was not monitored neither were the respective soil variables measured during (or before and after) snow cover. The hypotheses need to be rephrased to match with results. Reply: Following this comment and comments from the three other reviewers, we decided to delete the second objective and the associated hypotheses (see reply 1 to Referee #1).

- 6370/14-26: Climate and soil type differ between the upland and the lowland site. Therefore, any differences in soil P status might not only be caused by climate but also by soil type. Reply: We agree that differences between sites in P status can be related to climate but also soil properties, especially Al and Fe oxides contents and soil pH. The potential effect of soil properties will be explained in the discussion section. Overall soil type is rather similar though, meters deep loess deposits.

- 6371/4-14: What area do the “sites”/”plots” cover? Generally, the authors used too few replicates for statistical comparisons (vegetation effect: 3 x n = 1; climate effect: 2x n = 1). Three replicates per plot represent pseudoreplicates because of spatial dependency within the plot (6377/20-24). Reply: The area of each study plot was 1000 m2. Four plots and only two sites (three plots in a same site) were studied. Statistics enable comparisons between plots but we agree that the number of sites is not enough to conclude on any climate or vegetation effect (objective and hypotheses deleted).

Minor comments: - 6370/18: The soil texture is usually given as proportions of sand, silt (instead of loam), and clay. Loam is a mixture of these. - 6378/9: Description of comparisons in methods (6371/4-14) does not match with ANOVA results in tables: obviously the authors tested all plots against each other. - 6382/13-22: Much more literature is available for comparisons (Cross & Schlesinger 1995, Negassa & Leinweber 2009, Alt et al. 2011) - 6384/26: This statement seems like textbook knowledge and I doubt that it originates from recent literature only. - 6385/1-3: The number of references must be reduced (usually three are enough). - 6390: Some parts are literally identical with the abstract. These need to be rephrased or deleted. Reply: We will consider these minor comments. - Lines 4-15 in page 6371 (“We compared the three adjacent plots in the Salair mountain site to evaluate the effect of vegetation . . . the two sites with P tremula to evaluate the effect of climate . . .”) will be deleted in the revised manuscript because the second objective (“effect of climate and vegetation”)

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will be deleted. Inversely, comparisons between all plots will be maintained. - Other references will be included in the discussion section for comparisons in total P content (page 6382/lines 13-22). - Comment on page 6384/line 26: we will verify whether recent articles are available. - Comment on page 6385/lines 1-3: we will reduce the number of references accordingly. - Conclusion (page 6390): Because the focus of the study will be changed, we will also rephrase the conclusion section. The last sentence ("potential effect of climate changes") will be deleted.

Figure 1: This figure is not necessary to reach objectives and can be deleted. Reply: We think that this figure is useful. Following comments from Referee #4, we will move it to a supplemental document.

Figure 2: How does this figure relate to the objectives of this study? In the text, an additional reference to the next figure places this figure into context. Figure 2 might be deleted as well. Reply: Following the general comments of all reviewers, the revised manuscript will focus on the first objective (i.e. "P status across soil depths and site conditions, relationships with soil properties, decomposition/recycling processes, organic matter quality and microbial activity"). Figure 2 is in line with this objective (effect of soil properties and soil depth; differences between plots). Therefore, we will maintain it in the revised manuscript. However, we will better explain the results and the link with the objective.

Figure 5: Such conceptual considerations require an extensive data set which does not apply for the current study. Based on the restricted data set the conceptual model remains highly speculative. Reply: Following the general comments of all reviewers, we will delete this figure. Instead, we will present all P fractions as a % of total P and as a function of soil depth (inset figure d) for the four plots.

Anonymous Referee #3

Summary: The authors examined the amount and availability of different forms of phosphorus in multiple soil horizons in upland and lowland sites in Siberia that were dom-

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inated by different vegetation types. They found that total P, organic P, and inorganic P availability tended to decrease with soil depth. Total P did not differ among vegetation types, but was greatest in the upland system. The authors attribute differences among the upland and lowland sites to climate, and attribute differences within the upland sites to dominance by different vegetation types. General comments: 1. I was very interested in this study, as this is one of the few examples of a study that has systematically looked at P availability throughout the soil profile, and has done so in a rigorous way. However, I thought that the "experimental design" was a bit of a stretch. This design was not meant to answer the questions which the authors have based the manuscript on, namely the effects of vegetation and climate on soil P content and availability. Therefore, the discussion is much too speculative, because the results can't really be used to answer the questions of interest. It is reasonable to me to discuss potential causes for results obtained, but the entire discussion is based on speculation rather than measurements. That being said, there is absolutely no reason that the results can't be published on their own. The questions of interest would be how soil P content and availability change with soil depth in multiple locations, and how P is correlated with organic matter content, microbial biomass, and C mineralization. There is no need to speculate about cause. Also, the question of soil depth was lost entirely after the introduction, and is a really interesting aspect of the study. Reply: This general comment agrees with the other referees. As explained before (reply 1 to referee #1), we will focus the revised version of our manuscript on the first objective (i.e. "P status across soil depths and site conditions, relationships with soil properties, decomposition/recycling processes, organic matter quality and microbial activity"). We will delete the second objective ("effect of soil depth and plant species"), most of Fig.5 and the second part of the title. We will only explain in the Discussion section that the differences between the plots can be related to differences in vegetation or climate. We will reduce this part in the Discussion and we will revise our conclusions. In addition, we will delete section 4.2.3. "Implications of vegetation redistribution and local climate change".

2. The depth distribution of fine roots was brought up in both the abstract and introduction as an important reason why it was important to look at the depth distribution of total and available P. However, this wasn't a part of the study, and wasn't really a focus in the discussion, and I'm sort of wondering why roots were such a main part of the introduction. That said, it would be really interesting to know what the rooting depth distribution of the different vegetation types is in order to understand (1) what the root inputs might have been (and how this would contribute to organic matter build-up and P cycling, and (2) how much of the P available at depth could be expected to be reached in response to changing climatic conditions. Reply: The suggestions are very interesting but we did not evaluate fine root distribution in the four study plots. Previous works (e.g. Jackson, R.B., et al, 1996. A global analysis of root distributions for terrestrial biomes. Oecologia 108, 389–411) have shown that fine roots generally occur in surface but also deep soil layers and that studies on nutrient availability should consider the entire soil profile. We think it is important to mention this in the introduction to justify our study. However, we will also focus the discussion section on the comparison between distribution of available P with soil depth and general root distributions.

3. I was somewhat confused about the isotope dilution experiment, in part because the parameters were abbreviated and not redefined upon each use. The results from this portion of the experiment seem to be very important (in terms of plant availability), and it would be helpful if it was explained a bit better. And overall, there is some really great chemistry here (in terms of Al, Fe, and pH) that deserves more attention. Reply: These results will be better explained.

Specific comments: Page 6368, Line 28: Precipitation is singular. Page 6369, Line 1: I think here and throughout the paper, you mean "insulating". Page 6369, Line 6: It would be helpful to keep the sites straight for the reader if you always discussed them in the same order, rather than switching back and forth. Also, once you indicate that you will henceforth refer to "upland" and "lowland", you should stick with this. One line 24 of this page, you revert back to "blackish taiga" and "forest steppe". Page 6370, Line

21: Does precipitation include both rain and snow, or only rain? Page 6371, Line 16: What do you mean by representative “zone”? Also, it’s unclear whether this means you dug three separate pits? If so, was each pit considered a statistical replicate? How far apart from one-another were they? How big were the plots? Page 6373, lines 6-10: Please define “iP” and “Pr” better. And it would help if you redefined them upon subsequent uses in the results and discussion sections. Page 6377, lines 13-27: It is unclear what was treated as a replicate in your statistical analyses. In fact, the experimental design is a bit unclear as well. Perhaps a figure showing plot layout, etc. would be helpful in this case? Page 6378, lines 4-11: In your results and discussion, please make it clear that the total C, N, etc. values are concentrations, rather than stocks. This should also be clearer in the Tables. Page 6379, line 5: Rather than contents, please refer to C and N as concentrations. Page 6379, line 8: But the stocks of C and N were not significantly higher (statistically) in the lowland site, which is unclear from your discussion. Page 6379, line 17: I don’t think the language “affected by” is appropriate. This makes it seem like you somehow manipulated the soil horizons, which you did not. Perhaps you can say “significantly changed with soil horizon or soil depth”. Page 6380, line 3: Again, please rephrase “affected by”. Page 6380, line 7: Please redefine your parameters here and in the discussion section. Page 6382, line 24: “P is abundant relative to N” is a misleading phrase because N is still 10 to 12 times greater than P. Can you rephrase? Page 6385, line 23: Did you not measure the gravimetric water content of your incubated soil in order to express C mineralization on a g dry weight basis? Did the GWC differ among sites? Page 6386, lines 9-13: You don’t know that vegetation types were the cause of your results, or that higher decomposition rates resulted in your patterns of P availability, because you did not systematically control for or test these variables. Page 6387, lines 0-5: You did not measure litter production, and therefore cannot discuss any direct effects of litter amount in your system. Page 6387, line 13: “insulating” Page 6387, lines 12-13: You incubated your soil under standard conditions, and therefore cannot attribute differences in decomposition to climate. Lines 19-25: It would have been interesting to test your hypothesis about soil temperature and freezing in your laboratory incubations. Line 25: But you indicated earlier that you did not measure soil moisture, and therefore don't know whether it differed among sites. Reply: We will consider all these comments in details and revised the manuscript accordingly. - Page 6371, Line 16: representative zone = homogeneous, mono specific & undisturbed zone. Three separate pits were dug and each pit was considered as a statistical replicate. For each plot, the study area was approximately 1000 m2 and the distance between two pits was approximately 20 m. - Page 6385, line 23: Gravimetric water content of the incubated soils was determined and C mineralization was expressed on a g dry weight basis. The water holding capacity was determined for each soil. Then, all soils were incubated at 70% water holding capacity. The water holding capacity and therefore gravimetric water content of incubated mineral soils were similar between the three plots of the Upland site. However, water holding capacity and therefore gravimetric water content of incubated mineral soils was lower for the lowland site than for the upland site. The water holding capacity and therefore gravimetric water content of incubated forest floors were similar between all plots.

Fig 5: The "d" portion of this figure is the most important and could probably stand on its own. This is probably the most important message of the study! Reply: We will delete figure 5. Instead, we will present all P fractions as a % of total P and as a function of soil depth (inset figure d) for the four plots.

Anonymous Referee #4

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,

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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. Reply: Based on these general comments and suggestions from the other referees, we will change the focus, objective, hypotheses and conclusions of the study and rearrange the manuscript accordingly (see reply 1 and 2 to Referee #1). Also, we will revise the introduction section and the framing. In particular, we will improve the section on the role of P in C sequestration.

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. Reply: We will present all P fractions as a % of total P and as a function of soil depth for the four plots.

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).

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Reply: We will change the title accordingly.

- 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 mplications 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 cosystems. 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,

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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. Reply: We will consider all these specific comments.

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. Reply: We will present all P fractions as a % of total P and as a function of soil depth for the four plots.

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? Reply: It will be done.

Figure 1: Is this figure necessary? I would suggest moving it to a supplemental document. Reply: Figure 1 will be moved 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. Reply: Figures 2 and 3 will be merged to form a single figure.

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. Reply: These results will be better explained.

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 or-

ganic 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? Reply: Based on comments from the other referees, we will delete this figure. Instead, we will present all P fractions as a % of total P and as a function of soil depth (inset figure d) for the four plots. We will try to add connections with microbial activity, decomposition processes and physical-chemical properties.

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). Reply: We will make the corrections.

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

BGD

9, C4816–C4833, 2012

Interactive Comment