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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 #3

Received and published: 25 September 2012

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 dominated 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:

C4281

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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.

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.

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.

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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.

BGD

9, C4281–C4285, 2012

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Interactive Discussion

Discussion Paper

C4283



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.

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!

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