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Interactive comment on "What is the P value of Siberian soils?" *by* F. Brédoire et al.

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1 Letter to editor and reviewers

Dear editor, dear reviewers,

we are grateful to the overall positive comments you had on our manuscript reporting the phosphorus status of south-western Siberian soils.

We propose to take all of your comments/suggestions into consideration. Below are the details of our propositions for each item pointed out.

Sincerely.

The authors

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2 Comments on referee 1 report

The reviewed manuscript present data on P stocks and bio-availability in soils of Siberia. Since this area is going through rapid climate change, and since little is known about the P availability in these soils, this is an important contribution. The authors also made a literature search and compiled data on P status (measured in similar methods) in soils around the world. This enabled them to put the Siberia data in context. Overall this study seems sound, and I recommended publishing it after some minor revisions, detailed below.

1) The manuscript will benefit from English language editing. Especially the introduction and the discussion in which some of the sentence structure, and some of the words use, make it hard to read.

RESPONSE: We propose to rephrase some sentences. Also, the modified manuscript will be corrected by a qualified native English speaker.

2) The title is confusing. P-value is usually used for statistics. 'P status' will be much better description for this work.

RESPONSE: We propose to precise the title as following: "What is the P value of Siberian soils? Soil phosphorus status and availability in south-western Siberia and comparison with a global dataset"

3) Page 19823, line 23, remove humus which is not well defined and replace by organic matter. Same throughout the text. See: Lehmann, Johannes, and Markus Kleber. The contentious nature of soil organic matter. Nature 528.7580 (2015): 60-68.

RESPONSE: We fully agree, we will change "humus" by "organic matter".

4) Since a lot of effort was put into compilation of global data, it might be a good idea to present this data by maps with colors representing P concentrations (and insets of areas of interest). This is of course if there is some spatial pattern. Another option is to

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present this global data by plotting it against variables it is correlated with, like pH etc., together with the soils from the current study.

RESPONSE:

1. We are pleased to see that such a global data compilation provokes your interest. However, we believe the presentation of the global distribution of P pool concentrations is out of the scope of the article we propose to *Biogeosciences*, which focuses on our Siberian soils. A thorough analysis of global datasets, notably with maps of the global distribution of P pools, is in preparation by Augusto et al. for *Global Ecology and Biogeography*.

2. About your second proposition, we again think it is not the really in the scope of this article to seek for correlations between P pools and soil properties at the global scale. The study of the environmental controls on P availability is a program in itself. A recent publication reports global relations between the availability of inorganic P and soil properties (Achat et al., 2016).

3. However, we think that better characterizing Siberian soils is a good idea. In practice, we propose to extend the comparison of our Siberian soils with the soils of our global data compilation by adding details on commonly measured soil properties. This could be done by adding the Figure 1 (see at the end of this document) to the supplementary material. As a consequence, some comments on this Fig. would be added in the Results section of our manuscript. This would enable to better situate the Siberian soils among the other soils of the data compilation. Most of the soil properties presented in this new Fig. do not have a strong impact on inorganic P availability (Achat et al., 2016) and are not likely to improve the Discussion section. Nevertheless, and still to describe better Siberian soils in contrast to other soils, we propose to improve the paragraph of discussion about Al and Fe oxides (subsection 4.3) with explicit reference to Achat et al. (2016), and comparison with their results. Also, the soil C:P and N:P ratios, presented in

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the new Fig., could be rapidly discussed in regard to the global ranges provided by Cleveland Liptzin (2007) and Xu et al. (2013).

References cited in this response:

- Achat D L, Pousse N, Nicolas M, Brédoire F Augusto L. Soil properties controlling inorganic phosphorus availability: general results from a national forest network and a global compilation of the literature. Biogeochemistry, 127, 255-272, doi:10.1007/s10533-015-0178-0, 2016
- Cleveland C C Liptzin D. C:N:P stoichiometry in soil: is there a “Redfield ratio” for the microbial biomass? Biogeochemistry, 85, 235-252, doi:10.1007/s10533-007-9132-0, 2007
- Xu X, Thornton P E Post W M. A global analysis of soil microbial biomass carbon, nitrogen and phosphorus in terrestrial ecosystems. Global Ecology and Biogeography, 22, 737-749, doi:10.1111/geb.12029, 2013

3 Comments on referee 3 report

General comments: This is a very interesting study on the different forms, stocks and bio-availability of P in Siberian soils. An interesting effort was also made to compare P values obtained on Siberian soils to P values obtained on soils around the world. The question of P availability for plants and microorganisms is a key issue for our understanding of the functioning and evolution of eco- and agro-systems in a context of climate change. I thank the authors for incorporating the comments of the “quick review” in this new version of the manuscript. I consider this article acceptable to be published in Biogeosciences, but with minor revisions.

Specific comments:

Title:
It is a cool name but it is a little bit confusing (reference to statistical analysis) and it does not mention the availably of P for the plants which is, in my view, an important result of your study.

RESPONSE: see our response to reviewer 1. We propose to append a subtitle which should avoid the confusion that both reviewers pointed out.

Introduction:
The vocabulary used and the structure of some sentences make some ideas hard to understand. Two examples:
1- 19821, line 13-15: "These altered physical conditions are expected to modify the composition of the plant communities and the bioclimatic zones of Siberia have been predicted to shift northward and their relative size to change."

RESPONSE: We propose to change for: "Due to these altered physical conditions, vegetation communities may be modified. Projections indicate a shift of the Siberian bioclimatic zones northward and a variation of their relative sizes." More in general, and in line with reviewer one, we will send a modified manuscript out for language corrections. At this point, we may indicate the precise lines you would like to be altered.

2- 19822, line 9: "Also, P fertilization in agriculture is barely, if not at all, practised in Siberia".

RESPONSE: We propose to change "Also" by "In addition" but English copy editing should help rephrase this.

Materials and methods:
— 19825, line 15: "Such soil preparation was reported to affect biogeochemical processes only at a low magnitude (Cernohlávková et al., 2009; Chapman et al., 1997)." I do not understand the meaning of this sentence. Drying largely affects biogeochemical

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processes (for example: mineralization). You should correct or precise this idea.

RESPONSE: Indeed, microbial processes are impacted by drying and our phrase on "biogeochemical" processes is not correct. We propose to replace "biogeochemical" by "physico-chemical" which fits better with what we measured (we quantified only physico-chemical processes, not mineralization). These physico-chemical processes were found to be lowly impacted by sieving.

— What is the influence of the air drying-rewetting and grinding on: (i) the concentration of phosphate ions in solution, (ii) the diffusive phosphate ions at the solid-solution interface, (iii) soils textures (I think that this variable has been evaluated before grinding, am I wrong?).

RESPONSE: First, we precise that soil is not ground for isotopic dilution (only dried and sieved at 2 mm). After checking our MM this may not be so clear so we will explicitly write it in the revised version. Also, to answer your last question, texture was evaluated before grinding.

Achat et al (2012) showed that soil drying at 60 °C for 48 h alters the microbial biomass (cell lysis), and induce a release of inorganic P (as water soluble compounds such as phosphate ions). Therefore, total inorganic P and possibly diffusive phosphate ions are likely to be over-estimated, and organic P to be under-estimated, when measured on samples which have been dried. However, soil drying is part of a standard procedure in isotopic dilution and is usually performed at air temperature, which is lower than 60 °C and minimize impacts of drying. By using the standard procedure, we were able to compare our results with literature and to include them in the global data compilation.

Thus we propose to add the following precisions in the text of the MM: "However, it also permits to reduce/stop microbial processes between sampling and analysis. We took care to limit drying effects using a "soft method" (air drying until constant weight instead of oven drying at 60°C for example). In addition, air drying is a part of the usual protocol of 32P isotopic dilution method (along with the sieving at 2 mm, the use

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of a biocide such as toluene and performing measurements at 20°C). This allows the replication of measurements and the comparison of the results with the literature."

Reference cited in this response:
Achat D L, Augusto L, Gallet-Budynek A Bakker M R. Drying-induced changes in phosphorus status of soils with contrasting soil organic matter contents – Implications for laboratory approaches Geoderma, 187-188, 41-48, doi:10.1016/j.geoderma.2012.04.014, 2012

— 19827, line 9: You should give the concentration of toluene not the volume.

RESPONSE: We propose to add the following precision "10 μL toluene mL-1 soil solution"

— 19827, line 11: "This biocide does not affect P biochemical processes (Bünemann et al., 2007)". If you suppress/reduce life from your samples you should inevitably influence P biochemical processes. This information contradicts the idea p19830 line 1: "Since a biocide was added in the suspension, mineralization was stopped and we only measured physico-chemical processes". You should correct or precise this idea.

RESPONSE: Indeed, there is a contradiction in what we wrote. We have to change the term "biochemical processes" by "physico-chemical processes". Those are the processes we were interested in with the isotopic dilution and they were found to not be influenced by biocide (Bünemann et al., 2007).

— 19829, line 1: "In this study, we computed the values of Pr for 1 day, 1 week, 1 month and 3 months." In the manuscript you did not give the results for 1 month, is that important to precise this point in the "material and methods" part?

RESPONSE: indeed, we did not show the values for 1 month to limit the amount of data presented in tables/Figures. We propose to remove all mentions of Pr (1 month) in the text (MM).

Discussion:

The vocabulary used and the structure of some sentences make some ideas/paragraphs hard to understand. Two examples: 1- 19837, line 8-11: "Soil formation processes and soil physico-chemical properties also explain that we observed such "outliers". Despite overall slight effects only, soil content in Al and Fe oxides had some visible influence on soil P at the scale of some soil profiles."

RESPONSE: We propose to change to: "... These "outliers" can be understood considering the impact of soil forming processes on soil physico-chemical properties. At TOM..."

2- 19837 line 22 – 19838 line 2: "The restricted number of significant correlations – between P pools or isotopic dilution parameters (m and n) and soil physico-chemical properties – we identified in our study is not necessarily indicative of an absence of control on the P status. It may simply reflect that the values of the tested soil variables and P pools stand within a restricted range (same order of magnitude or difference of only one order; Table 2 and S4). Soils of the SW part of Siberia are indeed relatively homogeneous. They have developed on loess material deposited during the Quaternary era, mainly during the two last glaciation periods (Chlachula, 2003; Muhs, 2007) and despite some contrasted climate conditions, they have not been sufficiently impacted by diverging pedogenetic processes." This part can be simplified.

RESPONSE: We propose: "The restricted number of significant correlations identified – between P pools or isotopic dilution parameters (m and n) and soil physico-chemical properties – is not necessarily indicative of an absence of control on the P status. It may simply reflect that the tested soil variables and P pools stand within a restricted range (only up to one order of magnitude difference; Table 2 and S4). In fact, Al and Fe oxides, but also C, have noticeable effects when studying data compilations with wider ranges of values (Achat et al., 2016). These restricted range of variation in SW Siberia, despite contrasting soil processes, has probably to be related with the relatively young age of SW Siberian soils, which developed on loess deposits from the two last glaciations in the Quaternary era (Chlachula, 2003; Muhs, 2007)."

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— Paragraph 4.3: You differentiate levels, concentrations and pH by qualifiers such as "high" and "very high". Please be more precise.

RESPONSE: We could precise by adding a few figures/quartiles in parentheses. For example "...high pH (> 6)..."

— 19838, line 20-22: "Contrarily to the other measured P pools in the studied SW Siberian soils, the concentrations of diffusive phosphate ions in the topsoil were not so high, in comparison to global levels (although not being very low; Fig. 4)." This is a very interesting result which deserved to be better discussed in this part.

RESPONSE: We suggest to develop 3 hypotheses that are likely, in our opinion, to explain such a result:

- these Siberian soils are young and have probably experienced only a low mineral alteration. Total P stock is high but it is possible that a great part is in primary mineral forms, which are not available on short time scales
- harsh Siberian climate conditions (cold winter but also water deficit in summer for the southernmost sites) may also induce slow kinetics of soil organic matter decomposition. This may contribute to the high organic P concentrations (relatively to the global values, see the Fig. 4 of the BGD manuscript), and organic P would not be a source of available P as important as in other ecosystems
- the relatively low sum of Al and Fe oxides may also limit Pr, by contras to other soils at the global scale

— 19838, line 25-27: "In comparison, our SW Siberian soils have a low sum of Al and Fe oxides (68.44–184.08 mmol kg-1) – and the narrow range of values explains why we found only a few correlations between P pools and oxides – and a very high pH (5.37–7.16, Table S4)". You should divide this sentence in two sentences.

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RESPONSE: We propose to change to: “In comparison, our SW Siberian soils have a low sum of Al and Fe oxides (68.44–184.08 mmol kg-1) and a very high pH (5.37–7.16, Table S4). Narrow ranges of values may explain why we found only a few significant correlations between P pools and oxides.”

Interactive comment on Biogeosciences Discuss., 12, 19819, 2015.

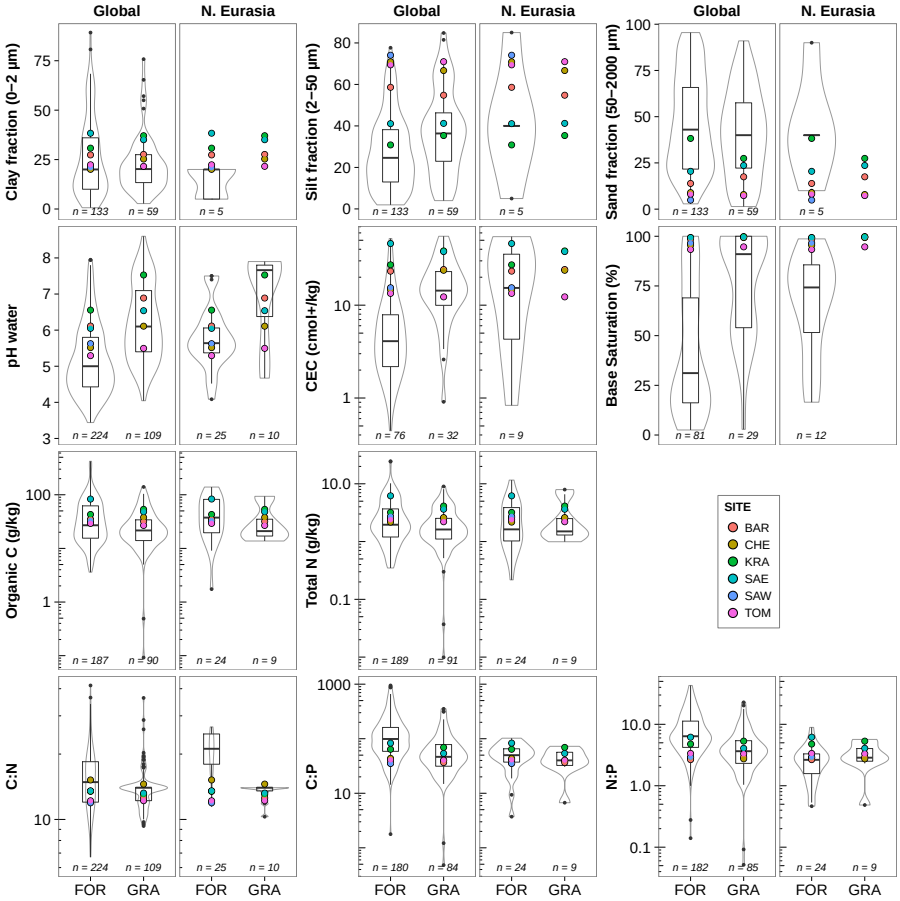

Fig. 1. Comparison of selected soil properties in topsoils (about 0 to –20 cm) of south-western Siberia (colored dots) and different vegetation types (FOR:forests; GRA: grasslands – box- and violin-plots) a

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