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Comment on soil-2021-65

Jörg Prietzel (Referee)

Referee comment on "Phosphorus dynamics during early soil development in cold desert: insights from oxygen isotopes in phosphate" by Zuzana Frkova et al., SOIL Discuss., https://doi.org/10.5194/soil-2021-65-RC2, 2021

General Comments:

The paper of Zuzana Frkova et al. addresses a topic of high ecological relevance. Worldwide, glaciers are retreating due to climate warming, liberating vast areas of formerly glaciated areas. Here, mineral bedrock and moraine parent material are converted into soils that support different ecosystems, depending on local climate conditions and bedrock mineralogy and bedrock mineralogy. Current knowledge suggests that these ecosystems during their succession undergo changes of nutrient limitation patterns. It is generally accepted that glacier forefield ecosystems most often with progressing soil development and ecosystem succession change from initial N (and C) limitation into P limitation.

However, at the moment this hypothesis is mostly backed on information gathered from glacier forefields under temperate or cool humid climate, whereas only scarce information is available for cold-arid environments. Yet, climate considerably affects both patterns as well as the pace of soil ecological changes. Therefore, studies on soil systems in non-humid climate regions are of great importance.

The paper of Frkova et al. covers a study on cold-arid climate. It is well written and easy to read. The sites and methods are described accurately enough (suggestions for smaller amendments are presented below) to understand the results. The results are discussed thoroughly, referring to the most relevant literature (again, suggestions for amendments are presented below). However, some additional literature may be added to the discussion, and some additional soil analyses should be performed to corroborate the statements made in the paper. Overall, I liked the paper and recommend **publication after moderate revision** according to my points raised below.

I will waive referee anonymity, because I cited some papers of mine in this review.

Specific comments:

Three major issues should be addressed in the revision of the paper:

- *Assignment of Hedley fractions to soil minerals.* Recent research has shown that the assignment of different Hedley P fractions to specific mineral types is not straightforward, and in specific cases may be completely wrong. See: Gu & Margenot (2021) Plant Soil 459:13–17 (https://doi.org/10.1007/s11104-020-04552-x) and Klotzbücher et al. (2019), J Plant Nutr Soil Sci 182:570-577. https://doi.org/10.1002/jpln.201800652.

Considering this, the authors should be more careful in assigning their Hedley P fractions to specific mineral phases (e.g. "Al- and Fe-bound" phases). This is particularly the case because (i) no support of their statements by other analyses (e.g. P K-edge XANES), (ii) not even any information about the absence, presence, and (iii) no data on contents of different potentially P-sorbing Al- and Fe minerals have been provided in the paper.

I understand that conduction of XANES analyses is probably out of reach for the authors of the paper, but analytical determination of dithionite-citrate-bicarbonate Fe (Fed, estimating Fe present in well-crystallized Fe oxyhydroxides, like goethite) and of acidic oxalate-extractable Fe and Al (Feo, Alo, estimating the Al and Fe present in short-range order minerals and gibbsite) may help to support the assignment of the NaOH-extractable Hedley P fraction to Al and Fe minerals. To be on the safe side, of course, one has to refrain from attributing the Hedley fractions to particular minerals as a whole, and just focus on the different availability of the different fractions to plants and soil microorganisms. If I understand the key message of the paper correctly, this is the main aim of the paper, and attribution of the Hedley fractions to particular mineral phases is of secondary importance.

2) I strongly recommend analysis of some additional soil variables, provided that some sample material is still available. (1) Analysis of dithionite-citrate-bicarbonate Fe (Al) and acidic oxalate-extractable Fe and Al (as mentioned before) would help to clarify the assignment of the reported Hedley P fractions to mineral phases. Moreover, it is a generally important soil variable, and helps to characterize the different soils in the study of Frkova et al. with respect to their stage of pedogenesis. I assume that some Alo and Feo will be present particularly in the older soils of the chronosequence, even though the pH is >7.7 (which normally prevents silicate weathering). This may raise discussions about the sources of pedogenic oxides (see an earlier paper of mine on two glacier forefields in China (also Tibetian Plateau) and Switzerland (Damma): Prietzel et al. 2013, GCA 108:154-171; https://doi.org/10.1016/j.gca.2013.01.029). Alternatively, Alo and Feo also includes organically bound Fe and Al in addition to/instead of mineral-bound Fe and Al – However, this line of argument may disprove the statement made in the paper that the NaOH-extractable P is bound to Al and Fe minerals.

Moreover, I recommend measuring inorganic carbon (carbonate) and the electric conductivity in the different soil samples. The climate conditions at the study sites, as well as the high pH in the investigated soils (7.7 – 8.7) both indicate the presence of carbonate and /or salt accumulation in the topsoil. Additionally, the good correlation between pH and total K in the different topsoils (see Table 1) suggests salt accumulation, which has a strong influence on weathering, soil P speciation, and probably also soil microbial communities and activity. The EC values are a good indicator for salt accumulation, and thus should be analyzed. I suspect that EC values are increased in the studied soils compared to ordinary soils under humid climate, and the investigated soils thus are probably affected by topsoil salt accumulation, which may be temporarily or continuously present at varying levels. If the investigated soils turn out to be affected by salt accumulation due to the arid-cool climate, the influence on weathering, soil P speciation,

soil microbial communities, and activity should be addressed more deeply in the paper.

3) In this respect I recommend reading a recent paper of mine dealing with P speciation changes in cold arid glacier forefield regions of Antarctica (Prietzel et al., 2019, GCA 246:339-362. https://doi.org/10.1016/j.gca.2018.12.001 and the references therein. I have the impression that the environmental conditions in the paper of Frkova et al. and those reported in my 2019 study are quite similar in many (aridity, high UV influence) but not all (seasonality, day length) aspects.

One minor issue that in a soil science paper I would like to see some soil type (WRB) description, maybe also horizon designations for the studied topsoil horizons.

Technical corrections

L29: Please specify soil depth or horizon, where 95% of total P is mineral P

L30: Can you specify the "primary phosphate minerals"?

L34: should read: "becomes" instead of "become"

L149-152: Please specify: Have the analyses been conducted on sieved or in ground samples?

L151: K , Mg, and Ca are not micronutrients

L152: This is "pseudo-total" P rather than total P, because silicates are not completely dissolved by $HNO_3/HClO_4$ digestion and the P bound in silicates thus is probably underestimated.

L160: How can NaOH-extractable P be bound to organic P? Please reword sentence in bracket

L257: The estimation of bulk density should be described in more detail

L259 "0.03 to 0.6" Please add unit also here

L287: Can you estimate average evaporation and a water balance from the d-excee data? Would be nice

L301: Please report mineral P content in addition to percentage

L303/4: Please report percentages of total P in addition to P content data

L321: should read: "Nutrient" instead of "Nutrients"

L325: should read: "total topsoil N concentration"

L348: Should read: "precipitation events" instead of "precipitations"

L377: Important: These environments are much more humid. See my specific comment #2

L379: Maybe change to “which slowed down soil acidification, and prevented…”

L402: Should read: “ Depleted d18Op values have been observed”

L406: Replace “ findings” by “soil features” or “soil properties”

L413: Contributions (of what?). Please specify

L424: Should read: “in low-sorbing sandy soils” instead of “in a low-sorbing sandy soils”

L430: Should read: “d18Op value” instead of “d18Op values”

L467: Maybe add: (alpine environments) with humid climate