

SOIL Discuss., referee comment RC1
<https://doi.org/10.5194/soil-2021-65-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on soil-2021-65

Andrew Smith (Referee)

Referee comment on "Phosphorus dynamics during early soil development in a cold desert: insights from oxygen isotopes in phosphate" by Zuzana Frkova et al., SOIL Discuss., <https://doi.org/10.5194/soil-2021-65-RC1>, 2021

Review for Frkova et al., 2021: **Phosphorus dynamics during early soil development in extreme environment.**

Overview:

The paper deals with P dynamics using stable isotope ratios from a series of soil sequences from recently formed soils (0-100's of years) in the wake of glacial retreat in the western Himalayas. The chosen sequences represent different ages of soil development and the authors use these to tell the story of soil development and the concurrent changes in P uptake and utilisation strategies. The authors demonstrate that the organic P release became increasingly important over time and soil development, and that at this site mineral P was still the major P pool even in the oldest soil studied, unlike previous studies.

General comments:

My overall view is that this is an excellent paper and with a few minor revisions should be considered for publication. The authors set out the paper well and it is clear to follow in the most part, there are a few places that the paper could do with a re-read for English, but this is not a major issue and I have highlighted most places below. The authors have included some enzyme activity work which I think will continue to constitute a major element in 18Op papers and rightly so, this is nice to see. They have also modelled the potential inputs to the available P pool which seems an interesting method to understand available P and how his changes through the soil development process.

I have listed my more major comments below.

- Title: I think this needs a slight re-word as it does not actually highlight which environment or the novelty/ breadth of the techniques used in this study.
- Figure 1: I think this figure could be re-worked, it would be really great to see a figure showing the sampling locations clearly, possibly with the old glacial limits as a visual representation of the speed of glacial retreat in the valley. At the moment this is very difficult to visualise from the large area map provided.
- Line 160: Five pools are mentioned here but only 4 described? Did you mean to include Po here?
- 166: I think the working of this is a bit confusing. I read it as some sites only had values of X and Y where you mean some sites don't have values of X or Y. maybe just re phrase to make this clearer.
- Line 175: I would be surprised with a 2Hw precision of 0.1... maybe 1.0?
- Line 179: Please add in the measurement precision here as with above.
- Line 183: do you have any Local meteoric water line data here, maybe from ongoing field campaigns?
- Lines 185-190: some info on sample weights and number of replicate analysis would be good in here as well as the average standard error of sample replicate analysis. Additionally, this error information would be good to see on Figure 2.
- How much of an issue is it for the rest of the interpretation that you only have Po from the oldest site, and how valid is it to assume that Po is the same across all of the other sites (Line 210)?
- Again, how representative are the full range of microbial P values going to be here. I don't question your rationale in using the values from sites M3 and M4 but it would be good if you could comment on how representative you think these values are, especially as you are studying this site specifically as you expect and see major changes in soil and vegetation development leading clearly to different P pools becoming more or less important.
- Table 2: Maybe I'm confused here and some of the data comes from long term averages but where are the fixed values in 18Ow and temp coming from? I think the Table caption needs to make this clear.
- Line 311: this is a little misleading, it sounds like oxide bound P is normally similar to the parent material, but actually from your 3 data points 2 diverge and only 1 is similar. This needs re-wording.
- Figure 2: It would be good to see the theoretical equilibration value for the sites in a similar way to how you show PME or as a shaded bar. This will help visualise how distinct the values are from pyrophosphatase driven equilibrium. This would be good to visualise between sites, especially when in lines 355-370 you discuss this as a potential reason for lower microbial P values between M3 and M4.
- Line 370: significant suggests some statistical significance, if that's the case please quote if not just re-word. It would also be good to get a feeling of the analytical error you expect. i.e. error through chemistry as well as the MS, did you run duplicates at all through the 18Op prep?

Minor comments:

- Line 30: remove the word also
- Line 32: P is defined above as phosphorus, just be clear when you are talking about phosphorus or phosphate, maybe stick to the 18Op that you have used earlier, check

this throughout.

- Line 38: add an "s" to condition
- Line 45: merge refs
- 139 cm⁻³ repeated?
- Line 184: silver phosphate rather than phosphate
- Line 233: merge refs
- Line 308: M3 repeated twice one is M4
- Line 333: remove "with"
- Line 355: remove and lied and replaced with "lying"
- Line 375: remove the word "also"
- Line 475: remove the word also
- Line 476: remove "in the" and replace with "into"