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Interactive comment on "Spatially resolved soil solution chemistry in a central European atmospherically polluted high-elevation catchment" *by* Daniel A. Petrash et al.

Anonymous Referee #2

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General comments

Petrash et al. present an interesting study on the spatial heterogeneity of soil solution in a central European high-elevation catchment which formerly received high loads of atmospheric pollutants. The topic is highly relevant for SOIL and the data gathered for the study are considered worth being published. However, with respect to the structure of the manuscript as well as the presentation of the methods and the data the manuscript seems to need major revisions. The introduction reflects the history of atmospheric pollution in the "Black Triangle" The last paragraph of the introduction need to be completely rewritten as it contains little information concerning the design of the study, but methodological issues, results and even concluding statements. No objectives neither hypotheses are given in the introduction. Please amend accordingly. The methods section isn't detailed enough to be able to reproduce the approach completely. Information on the vacuum system of the lysimeters is missing. At least information on the applied pressure and if the vacuum was applied permanently or non-permanently should be given. Otherwise, assessments are mentioned (i.e. soil moisture determination), which value for the study remains unclear. According to Figure 1, sampling plots for soil solution and solid soil are up to more than 200 m apart. However, no information is given with respect to the comparability of the respective plots. Please give a rationale for this approach as the results from either solid soil or soil solution are related to the respective slope position. The contents of the results section are structured differently as the methods section. Some parameters, which are displayed in the tables and figures aren't mentioned in the text body of the results section. For soil solution, different units are reported in the text and in table 2. The last part of the results (i.e. page 8, row 6-11) seems to be more suitable for the discussion. The integration of the assessment of P availability into the study appears a little bit weak as no relation to either soil solution or runoff P concentrations is given. Also, a discussion on the role of P availability for tree nutrition is missing. Although discussed in the same sub-section (4.4), a convincing evidence for the "de-coupling" of P availability and organic carbon is missing. At least, an explanation should be given, why a "coupling" of P availability and organic carbon should be expected. The conclusion (5.) contains issues, that haven't been discussed before (e.g. the role of drought and torrential rains, isotope investigations). This should be avoided. The last three points of the conclusions resembles a collection of keywords more than elaborated findings. The chapter on 18O/16O modelling in the Appendix seems not very well integrated in the study. The references are mostly relevant for the study and up-to-date. However, some citations aren't very specific to the referenced issues. The publication of the manuscript can only be recommend after a major revision considering the above mentioned concerns. Specific comments line-by-line are given in an attachment.

Please also note the supplement to this comment:
https://www.soil-discuss.net/soil-2019-9/soil-2019-9-RC2-supplement.pdf

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