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Comment on bg-2021-188

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Referee comment on "Leaching of inorganic and organic phosphorus and nitrogen in contrasting beech forest soils – seasonal patterns and effects of fertilization" by Jasmin Fetzer et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-188-RC2, 2021

Fetzer an co-authors studied element fluxes woth percollating water through upland soil profiles. Their work focuses on P fluxes. They compare (a) two sites (high/low P) (b) three different depths (litter, organic layer, A horizon), (c) seasonal dyanmics, and (d) the effects of N, P, and N+P fertilization. The authors aimed for a semi-experimental approach, where heavy rainfall event are simulated at each site to measure soil leachate concentrations under comparable rainfall conditions. Their key findings are that (a) season is the most important determinant of P fluxes, (b) inorganic N and P shows stronger sesasonal variation than organic P fluxes (c) there were surpsiningly small differences in P fluxes between the two sites, but the two sites responded differently to fertilization, in paticularly N+P treatments.

Strength:

This is a timely study addressing a important topic - P dynamics in soil profiles less well understood than C and N dynamcis. The authors used state-of-the-arts methods and their results justify their conclusions. Overall, this is an impressive piece of work that features a fully factorial experiment with 5 independent variables (site, horizon, season, +N, +P) and over 10 measured endpoints (concentrations and fluxes of DIP, DOP, DON, DIN, DOC).

Weaknesses:

1. I think the scope of the expriment is also a main limitation to the manuscript. I cannot get rid of the feeling that the authors tried to do too much in one step here. This has some consequence in experimental design: The authors tried to study both 'background' (unfertilized) fluxes and fertlization effectes at the same time. This made compromises in experimental design necessary like the application of KCl to control plots to compensate for the applied K in P fertilization plots. This raises the question how representative the control fluxes still are for natural conditions.

2. I think the size and complexity of the presented project also limited the degree to which individual results are discussed. Overall, the discussion section remains largely limited to providing explainations for the observed phenomena. I think this undersells the novelty and significance of the presented data. It would be nice to hear not only how the observations can be explained, but also how they changed your conceptual understanding of the soil P cycle? What are the implications of your findings?

3. I think the experimental approach chosen (field measurements but with the same rain event simulation performed at both field sites) and the consequences of these choices need to be discussed more explicitely. How representative are these simulated heavy rain events for 'normal' conditions with much smaller rainfall event spread out over the year? What did you learn about this new experimental approach?

4. Finally, it's not quite clear to me how the annual fluxes were calcualted. I'm assuming that these were upascaled from the concentrations found from the soil leaching experiments perfromed 4x/year? If that's true, I would doubt that the concentrations measured in such experiments are representative for other (less intense) rain events throughout the year. I would also assume that leachate P concentrations vary with the length/intensity of individual rain events, and the length of and conditions during the periods between rain events. All in all, I'm not convinced that the presented data allows calcualting and annual P balance that can be compared in absolute terms (e.g. to deposition inputs).

Possibilities for improvement:

1. I would suggest adding some graphic summary of the main findings (e.g. a conceptual figure).

2. I would suggest removign part of the data. Alternatively (in my opinion, preferably) would be splitting the mansucript into two companion papers (e.g., one dealing with site, horizon, and season; the second with fertilization effects). This would give more space to discuss the novelty and implications of each part of the study.

Minor comments:

I would avoid using the term climate to refer to seasonal dynamics (eg. L518).