Biogeosciences Discuss., author comment AC2
https://doi.org/10.5194/bg-2021-188-AC2, 2022

Reply on RC2

Jasmin Fetzer et al.

Author 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-AC2, 2022

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).

Comment author: Thank you for this positive feedback.

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.

Comment author: Thank you for the comment. While writing the manuscript, we also discussed intensively what to include in the manuscript or not. We opted on presenting and discussing also the leaching of the control plots to present a baseline and the relevance of P leaching as this information is rather scarce (as noted by the reviewer). Moreover, the discussion of fluxes in the control plots, documenting that estimated P fluxes at our sites correspond to those obtained by other leaching studies at the same site and elsewhere is needed to interpret the fertilization experiment.

We think that it is unlikely that the KCl addition affected P leaching as chloride is less competitive in sorption than inorganic and organic P forms. Indirect effects on sorption/desorption via changes in ionic strength seem unlikely as the measured electrical conductivity was 63 ± 45 µS cm^{-1} (average ± St. dev for all samples) which in the typical range of soil solutions sampled in forest topsoils. Therefore, we do not expect increased P desorption and fluxes by the KCl addition in organic layers and A horizons. Our assumption is supported by comparable P concentrations and fluxes from our control plots (where KCl was added) to measured P concentrations and fluxes by other groups at the same sites (unpublished, values see responses to (3) and at line 445) and elsewhere (e.g. Sohrt et al., 2019).

- 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 explanations 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?

Comment author: We agree with the reviewer that the broad scope of this study is the advantage and the weakness. Although some findings might be undersold, we opted for presenting a comprehensive view to P cycling in forest soils and think that our study clearly shows so far rarely considered aspects such as the combination of N and P status, seasons, and environmental conditions. We think the manuscript's true novelty is to bring all these factors together instead of slicing the manuscript.

Only this combination allowed us to draw the conclusions that (1): the cycling of P and N may undergo considerable decoupling (indications from comparison of sites as well as fertilization treatments) and (2) that nutrient-poor ecosystems that recycle their nutrients tend to be vulnerable to changes in environmental conditions, such as seasonality, drying-rewetting, as well as external nutrient inputs. These are important contributions to the understanding of the soil P cycle in forests that were discernable only by a complex experimental design as used here.

- 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?

Comment author: Thank you for this comment, this is a fair point. In the revised manuscript, we discussed this more in depth and this is the reason why we added the information about rainfall intensities and annual precipitation in the Methods section (lines 143ff):

"The application rate represents maximum rainfall intensities at the study sites. Rainfall intensities larger than 20 L h^{-1} m^{-2} have been observed once at the low-P site and three times at the high-P site during the last 10 years (Bayerische Landesanstalt für Wald und Forstwirtschaft (LWF) and Nordwestdeutsche Forstliche Versuchsanstalt (NW-FVA)). **The amount of water added with irrigation corresponds to the average weekly**

precipitation at the high-P site and exceeds it by 33% at the low-P site. In 2018, the three irrigations, totaled 60 L m^{-2}, which accounted for approx. 8% of measured throughfall at the high-P site and 16% at the low-P site (cf. Table 2). The two irrigations in 2019 added 40 L m^{-2}."

Additionally, we added mean annual precipitation data in the description of the sites in lines 94 and 100.

These additional information shows that the total addition of artificial rainwater was little compared to annual precipitation, and therefore, did not change strongly the annual fluxes and falls within the amounts of weekly rainfalls.

We compared our data with unpublished P fluxes under ambient conditions from the organic horizons at the same sites during the previous four years. Our fluxes were slightly smaller, which could be due to less precipitation in the studied year than in the previous four years. Therefore, we are confident that our flux estimations based on P concentrations obtained by artificial irrigation are reliable and representative for natural conditions. In the revised manuscript, we provide the comparison to these data in the Discussion and discuss the representativeness as follows (lines 445ff):

"Dissolved P concentrations in the leachates following the experimental irrigation used to overcome site and weather variations corresponded closely to those measured in an adjacent plot receiving natural precipitation. While the annual average concentration in the leachate from the organic layer (only control plots) following irrigation were 0.19 mg P L^{-1} at the low-P site and 0.24 mg P L^{-1} at the high-P site, respectively, those under natural precipitation were 0.35 mg P L^{-1} at the low-P site and 0.18 mg P L^{-1} at the high-P site (K. Kaiser, unpublished data, median over the four previous, much wetter years). We therefore assume that concentrations and fluxes estimated here, are representative for the sites. The TDP fluxes, ranged between 12 and 60 mg total P m^{-2} yr^{-1} across all horizons (Table 3), compare well with the P fluxes measured in other forest ecosystems, ranging from 9 to 62 mg P m^{-2} yr^{-1} (Qualls, 2000; Fitzhugh et al., 2001; Hedin et al., 2003; Piirainen et al., 2007; Sohrt et al., 2019; Rinderer et al., 2021)."

- 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).

Comment author:

Methods

Correct, we upscaled the concentrations from the point measurements and multiplied them with measured water fluxes. In the revised manuscript, we rephrase and expand the describing of the approach used for flux estimation in lines 218ff. We are aware that these flux estimates are approximations (as in many other studies where measured element concentrations are multiplied with modelled water fluxes). A continuous monitoring at the 2 sites receiving NxP fertilizer would not have been possible. Nonetheless, we regard them to correspond to other assessments (see last response and Discussion lines 445ff). In the Discussion, we present the number very cautious and present the numbers in a rather conservative manner ("ranged between 12 and 60 mg total P m^{-2} yr^{-1} across all horizons

(Table 3)"; "The P fluxes from the A horizon at the high-P and at the low-P site are approx. 150% and 50% of reported atmospheric P deposition in Germany." Therefore, we are confident that we are sufficiently cautious in our data interpretation.

How representative are the concentrations obtained by artificial irrigation with 20 L m^{-2} h^{-1} for other (less intense) rain events?

Firstly, the amount of water added with irrigation corresponds to the average weekly precipitation at the high-P site and exceeds it by 33% at the low-P site (added at line. 145). We regard this as an amount representative of a higher intensity rainfall event. **Secondly**, in order to have enough organic layer leachate reaching the mineral horizon, also a certain amount of precipitation is needed, especially on dry soils. The amount of artificial rainfall we applied correspond to 60-70% of the pore volume of the soil material above the lysimeter in the A horizon (approx. 1 L of water for the area of a lysimeter (19.5 * 25.5 cm). We therefore think, the applied amount was an appropriate comprise between "representative" conditions and the need to obtain sufficient leachate for analysis. **Thirdly**, P concentrations indeed vary with length of rain events (see reference in lines 142ff: Our sampling procedure represents the "first flush", comprising the majority of P leached during heavy rainfall events (Bol et al., 2016; Makowski et al., 2020a; Rinderer et al., 2020). In terms of length of rain events, there is a decrease in P towards the end due to dilution when P concentrations reach a constant low level (Rinderer et al., 2020). Therefore, most P export happens during the "first flush", which we covered by our experiment. The close match of P concentrations measured here and in the continuous monitoring supports our assumption that we have sampled representative leachates. As mentioned above, this information has been added to the manuscript.

Also, please note that the standardized irrigation allowed a better comparison between sites, treatments, and seasons.

Our annual fluxes are clearly estimates. Therefore, we compared our concentrations and fluxes to other studies that obtained their data under natural rainfall conditions (see comment above). As the concentrations and fluxes were similar, we are confident that our data is a sound approximation of natural conditions and we think it is useful to set it in comparison with other numbers (that are often estimates as well since being based on modelled water fluxes), to judge the importance of the fluxes.

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

- 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.

Comment author: We appreciate your suggestions and constructive thoughts on the manuscript. While writing the manuscript, we also considered splitting, but opted on providing a more holistic assessment of complex ecological interactions under field conditions. We felt that for the evaluation of the fertilization effect, we first have to document and discuss the representativeness of the measured fluxes (varying differently for DIP and DOP at a seasonal scale). Moreover, fertilization effects depended upon sites and therefore, we have to discuss 'site effects' beforehand.

In conclusion, we prefer to keep the manuscript as one.

Due to the complexity of the variables, factors and processes involved (DIP, DOP, DIN, DOP, 3 horizons, 2 sites, interaction of N x P fertilization), we also refrained from providing a conceptual figure, which would be too simplistic. We could instead provide a kind of summary graph as this one:

Please, see graph in the PDF document appended.

Additionally, we tried our best to revise the result and discussion section to improve the clarity of the processes involved and provide a deeper insight into P cycling.

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

Comment author: Thank you for spotting this inconsistency. We changed the term to seasonal conditions (line 552).

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-188/bg-2021-188-AC2-supplement.pdf