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Interactive comment on "Phosphorus Transport in Subsurface Flow at Beech Forest Stands: Does Phosphorus Mobilization Keep up with Transport?" *by* Michael Rinderer et al.

Anonymous Referee #1

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General remark

The authors present data from sprinkling experiment in three forest sites, performed during two different seasons, where they analyzed water flow and soil solution P concentrations. The paper is generally well-written and easy to follow, and the results are interesting. However, from my point of view the motivation/objective of the paper is not yet properly addressed with the results. This needs to be addressed before the manuscript can be published.

The stated objective of this paper to quantify P losses via subsurface flow (abstract, as well as l. 75 of introduction). This sets the reader up to expect to learn about

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phosphorus fluxes. More information on subsurface flow P losses would indeed be very interesting, also for the land surface modelling community, which is struggling to incorporate P cycling into C, N models. However, in the results no soil P fluxes [g P m-2 time-1] are presented, only P concentrations [mg P L-1 water]. I suggest authors to bring the paper in line with the objectives. Firstly, in the introduction by introducing what are typical soil P stocks in forest ecosystems (see e.g. (Achat et al. 2016; Hou et al. 2018)), and further what are orders of magnitude for P flux losses (e.g. in g P m-2 yr-1) as determined by earlier studies (see e.g. (Vitousek 2004) and othersâĂŤauthors would have to search the literature a bit here). Perhaps also comparing to other P fluxes in forest ecosystems such as dust deposition, rock weathering, etc. This will set the scene for talking about P fluxes in forest ecosystems. I'm guessing that the losses will be several orders of magnitude lower than the stocks, and it will have to be argued why (if?) they are still important.

Secondly, no P flux data is presented in the results. Is it possible to multiply water flow by P conc. to get P flux? Why is this not done?

The discussion should be developed further also. How do the results from this study tie into what we already know about P cycling in forests, and P loss pathways? At the moment the discussion mostly explains the results, but it needs to go further to show readers what has been learned. Again, given the setup of the paper, the focus should be on P fluxes. What do the results mean in terms of fluxes? What do we learn about P cycling in forest ecosystems? Just thinking out loud (authors may choose to follow up on this or not): Apart from the nutrient flush in the first 1-2 hours, P concentrations were relatively constant regardless of SSF. On a methodological note, does this imply that we can (roughly) approximate annual P losses via SSF given the water balance of the site and the soil solution P concentration? What would that imply in terms of annual P loss [g P m-2 yr-1] for these sites? How does that compare to the forest stocks and orders of magnitude that can be expected for other loss and input pathways such as dust deposition, weathering and erosion (Chadwick et al. 1999; Hartmann et al. 2014;

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Tipping et al. 2014; Aciego et al. 2017)?

Specific comments

Title: This is up to the authors, but if they want their article to also reach hydrologists, the title (and abstract?) should be revised. A good portion of the results and discussion as well as the conclusion focus on water flow, which I did not expect from reading the title. E.g. something along the lines of “Beech forest stands sprinkling experiments: effects on sub-surface flow and phosphorus dynamics”

l. 23 Jumping on the “climate change” bandwagon here is unwarranted. There is no discussion of climate change in the article. Also, the data rather show that P conc. is constant and thus only dependent on water balance, right?

l. 29 How much P is in forest soils? How big are these losses?

l. 32 remove period after “SSF”

l. 34 remove period after “nutrients”

l. 45 The way this sentence is written makes it sound like it was done in this study. I suggest to change tense to “has been” or state “in previous studies”

l. 52 add “, USA”

l. 54-62 This is too detailed and should be condensed

l. 66 “In biopores. . .” ?

l. 74 “We performed. . ..to capture potential differences in P fluxes.” However, in the research questions the focus is on dynamics of P concentrations. This should be aligned.

l. 99 231 g at CON is very similar to 209 g at TUT, especially given heterogeneity inherent to soils. I don’t think you can argue that TUT is “less rich in soil P” than CON.

l. 99 So that the reader can put these numbers into relation (is 209 – 678 g P m2 really a large range in P, justifying calling one P poor and the other P rich?), I again suggest

presenting orders of magnitude ranges in soil P stocks in forests (see comment l. 29)

l. 102 Add period before “Bulk”. I stop correcting spelling / grammar mistakes at this point, but there are more in the remaining text. Please proof read the next version carefully.

l. 136 I’m no expert here, but I’m guessing rain water is far from de-ionized. How do you think using deionized water affected the results? Does that need to be discussed?

l. 170-177 Nice setup to let the reader now what to expect, look for and interpret in the results! That’s an example of great scientific writing

Table 1: Please also add pH to the table. pH is an important indicator of soil P forms and dynamics and may be important to explain the results, e.g. the difference between TUT, CON and MIT.

Fig. 1 and others: colors are not grayscale print-friendly

Fig. 2: Very nice overview figure. This makes it a lot easier to understand what was done.

Fig 3. also this is a nice figure. I suggest to move spring before summer. I understand that spring experiments were carried out a year later, and that’s ok since you have the dates there and it can be noted in the figure caption. But it makes more sense to have the plots in seasonal order for interpreting the plots

Section 3.4 It would have been interesting to measure inorganic and organic P as opposed to only total P.

Results section 3.5: multiypling conc. by water flow = element flux. Why not present these data in a section 3.5 “Soil P fluxes”

l. 256-260 (p. 8-9): I’m not surprised that P conc. in the soil solution remains relatively constant. If we consider the very fast turnover time of P in the soil solution of only seconds to minutes (Helfenstein et al. 2018).

l. 300 What about biopores? Is there evidence to suggest that CON and TUT have more earthworms or other large soil fauna

section 4.2 This section could be re-written to make it more focused. At the moment there is a mix of rather trivial findings, such as that P stocks are higher in the forest floor than in the mineral soil, while the interesting things are not discussed in-depth enough. The discussion of P concentration dynamics should be better linked to existing literature, e.g. what is known about turnover time of P in the soil solution and phosphate buffering capacity. Phosphorus-buffering capacity (PBC) is defined as the ability of soil to moderate changes in the concentration of soil solution P (Beckett and White 1964; Olsen and Khasawneh 1980; Barrow 1983; Pypers et al. 2006), and would be interesting to bring in here. Soil solution P turnover, a related concept, has been shown to be negatively correlated with P conc. in the soil solution (Helfenstein et al. 2018), which authors might consider discussing as well. (i.e. the more P in the soil solution (forest floor), the slower the turnover time; the less P in the soil solution (mineral soil), the faster the turnover time.

l. 347 As with the plots, I would take spring before summer.

l. 364 not exactly true that you have six different experiments. It's one experiment carried out on three sites and at two time points.

l. 371 It's quite well known that soil solution P concentrations are lower with increasing soil depth. I would rather focus on novel findings in the conclusion.

l. 372 "it was especially strong. . ." What is it?

l. 373 It is obvious that P concentrations are highest in the P-rich site. Again, the conclusion should focus on the novel findings.

l. 374 "Particulalry high". Please be concrete. How much higher? Are we talking 1.5x, 2x or 10x higher than during the rest of the experiment?

l. 375 – 379 This is interesting and in my opinion the main finding of the study. This

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should be placed more prominently and discussed appropriately.

l. 380 Conclusion not supported by the data. There was no discussion of climate change in the article.

l. 436 “DWD, 2010” please provide complete citation reference

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