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Interactive comment on "Contrasting physical controls on phosphorus transport to shallow groundwater at the hillslope scale" *by* Maelle Fresne et al.

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General Comments:

This manuscript is well-written and provides a nice study incorporating field data collection, lab work, and modelling. The manuscript adds to the ongoing discussion in the literature regarding subsurface P transport in context of its impact on surface water quality. I think the primary unique contribution of the manuscript is the discussion section, which pulls together the field data and model results together with other concepts from literature.

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Discussion paper

Reply: We thank the reviewer for his positive and constructive comments. However, on reflection and following recommendations from Reviewer #2 we have opted to re-focus the modelling component on water flux only.

Specific comments:

Abstract:

L21: This implies that the model was for P transport instead of a conservative solute. This should be clarified.

Reply: We have modified the sentence to clarify that the model was for water flow only on page 2 lines 26-27.

L25: I think the key here is that the model for a conservative model showed the temporal dynamics of solute transport, and when we combine that with our existing knowledge of P sorption/precipitation mechanisms (time for sorption to occur, preferential flow bypassing sorption sites), then we can arrive at conclusions about why P transport is attenuated at the MS location.

Reply: We have added a sentence specifying the implications of the variations observed in soil physical/hydraulic properties, and the subsequent water flow behaviour, for P attenuation on pages 2-3 lines 36-37. We have also modified the abstract and have added some values referring to these differences on page 2 lines 29-35.

Introduction:

L54: For hydrology of hillslopes and the influence of preferential flow (e.g. soil pipes), Glenn Wilson may have some helpful references.

Reply: Thank you for providing additional references, we have included them in the introduction section on page 4 line 73: - Wilson et al. (1990) who concluded that preferential flow from hillslopes through macro- and mesopores was the predominant stormflow mechanism; - Wilson et al. (2017) who showed that soil pipes provided hydrologic connectivity between upper hillslopes and catchment outlets when perched water tables were not well connected.

L95: Object 3 appears to be redundant with Object 1.

Reply: We have modified the objectives and we have deleted the objective 3 on page 7 line 133 and in the discussion section to incorporate the implications for management in the two other discussion sections.

Materials and methods:

L172-177: The way it is currently written, it is unclear how you determined soil volume from disturbed soil samples. I think you are trying to say that you started with an undisturbed soil core, measured the volume of the soil core, removed stones (coarse gravel?), and then calculated the difference between the total core volume and the volume of stones. Is this correct? Also, it might be helpful to say something like, Stones above x mm diameter were removed so that bulk density was determined on the soil (< x mm).

Reply: We took one additional soil core per site and depth and the soil core was then directly destructed and analysed to be able to quickly have bulk density data (as the other cores were only analysed for bulk density after the Hyprop work) and to see the variability between sites and depths and also determine soil PSD and texture. We have modified the sentence on page 11 lines 210-211, have modified the paragraph and its organisation on pages 11-12 lines 216-233 and have stated the size of the stones removed to clarify this.

L192: Clarify how Ks was determined, e.g. measured by the Hyprop? Also, was the unsaturated K curve determined from the Hyprop data? (I realize that some of this is in S1, but K is such a critical factor that it would be helpful to provide a little more information here.)

Reply: We have added more details (while avoiding too much description which is provided in the supplementary materials) in the manuscript on page 12 lines 240-242 to clarify the procedure for the determination of soil Ks.

L200: Either here or in the introduction, it would be good to cite some of the other literature that uses Hydrus to simulate P transport from the soil surface to the water table.

Reply: We have added some references to models available for P transport and to previous work modelling P transport through the unsaturated zone using Hydrus in the introduction section on pages 4-5 lines 75-86.

L215: The soil core that had the best fit between measured data (Hyprop) and the RETC model was selected, right? So, for a given location (e.g. MS), did the chosen soil core have a Ks that was representative of replicates (e.g. comparable to the median)? If the chosen core had a Ks much higher or much lower than the median Ks, that might be a concern in terms of how representative the Hydrus results are. [Ok: : :now I see the data in S3 and S4. For DS, for 30-35 cm, would it be better to pick the replicate with Ks = 396 cm/d (second lowest Erms) rather than using the highest Ks = 2892?]

Reply: We agree with this comment; soil Ks was very variable between replicates and the choice of the Ks value based on the best fit to the model may have not been the best choice to get the most representative value. We have now chosen a Ks value more representative of the sampling area (for DS 30-35 cm and for MS) based on the median value (descriptive statistics, including median values, are presented in Table 3 in the results section on page 19). We have clarified this on page 14 lines 279-282. We have re-ran all the models with the new physical values to account for water flow only and have modified the results section on pages 17-18 lines 339-364 and in Table 4 on page 22.

L228: It would be good to mention that sorption is a significant component of P fate and transport, and that, though it isn't included in the model, it will be part of the discussion.

Reply: We agree with this comment and have added that water flow was influencing P attenuation processes (sorption) on page 15 line 303.

Results:

L330: When the GWL was above the ground level, was this due to a high stream level, or stagnant ponding disconnected from the stream?

Reply: It was due to high stream level.

L330: Also, this situation wouldn't be consistent with the Hydrus BCs, i.e. atmospheric for the upper boundary, and free drainage / zero pressure gradient for the lower boundary (typically assumes a deep water table). Did the time of the Hydrus simulations include times when the GWL was above ground? If so, this should at least be acknowledged (e.g. boundary conditions were violated x% of the time: : :).

Reply: We agree with these comments regarding the violation of the boundary conditions that have not been discussed in the manuscript. There was no violation of the boundary conditions at MS as the GWL was deep. Although we chose to use the same boundary conditions for DS and MS, and that there was no violation of the upper boundary condition for R1 and R2 at DS, the upper boundary condition was violated 63 % of the time for R3 when the GWL was above ground. We have stated this in the manuscript on page 25 lines 467-468. Moreover, the lower boundary condition was violated at DS as the depth to GWL was less than 55 cm, this has been stated on page 25 lines 468-469.

L338: Can you report mass balance errors as an indication of how well the model performed?

Reply: Thanks for commenting on this point; good suggestion to indicate the performance of the model that we did not include. We have corrected this on page 25 lines 471-472.

L344, Figure 6: I was initially confused because each simulation is named by a single rain event (R1, etc.), but in Figure 6 it looks like several rain events are included in each simulation. I think the single rain event refers to the rain event when the simulation started, right? But then rain from the weather data was used for the duration of the simulation. It would be helpful to clarify this. If this is the case, should the simulations be named by "injection events" instead of rain events, i.e. to identify the point in time when the solute was injected into the soil profile?

Reply: Yes the "rainfall event" corresponded to the event following the tracer injection but the rainfall continued until the time of last occurrence of the tracer, which includes other rainfall events. With the manuscript now focusing on water transport only, we kept the use of rainfall event as one model was run for each rainfall event, without injection of solute and additional rainfall events occurring (the model was stopped just before the beginning of the next rainfall event).

L344, Figure 6: Also, if the simulation includes many rainfall events, it would be good to explain (e.g. in the methods) why the different rainfall events (R1) at the time of injection are expected to make a difference for solute transport.

Reply: The simulation now only includes one rainfall event.

L352: The data presented focuses on the timing of the breakthrough curve, and does show solute concentration, but does not show water flux or solute flux/load across the bottom boundary condition. One could argue that the total load entering the groundwater is one of the most important factors affecting stream solute concentrations. It would be helpful to add a paragraph discussing this. I assume that the amount of solute mass injected was the same for each simulation. Was the solute load across the bottom boundary condition also equal? After the whole breakthrough curve passes, I would expect the cumulative load to be equal to the mass injected (since it is a conservative solute).

Reply: With the manuscript now focusing on water transport only, we have showed water flow breakthrough at the bottom of the soil profiles in Figure 6 on page 26 instead of tracer breakthrough concentration. We have presented and discussed water flow dynamics in the results and discussion sections.

Discussion:

L390: It would be helpful here to mention that P sorption to soil is a significant factor, and is included in discussing the data, explaining observed trends, etc.

Reply: We modified the models to only account for water transport but have indicated that it's also influencing P attenuation processes on page 28 line 515.

L397: "MS zone: : :suggesting an attenuation of hydrological P transport: : :" This should be spelled out or a reader might miss the significance of it. Since transport to the GW is a longer process (indicated by your Hydrus data), there is more time for P sorption to occur. (Also, lower macroporosity means that more of the P will interact with particle surfaces instead of bypassing sorption sites.) So, in the big picture, your Hydrus simulations may show the same cumulative load of conservative solute entering the GW, but when accounting for P sorption as influenced by the temporal dynamics of your simulation, then it makes sense the P load to the GW would be lower for the MS zone.

Reply: We agree that the implication of soil physical/hydraulic properties for soil P attenuation processes was lacking in the discussion section. We have discussed this point on page 31 lines 602-605.

L429: Clarify: How does lower air fraction help attenuate P transport?

Reply: A lower air fraction means that the volume of water flowing through the soil profile by gravity only is lower, thus attenuating water flow. It may also attenuate P transport by increasing P attenuation processes as there is more contact between soil water and soil matrix. We found that the air capacity was negatively correlated to clay content which retains water strongly.

L447-476: This paragraph is excellent.

Reply: Thank you for this comment.

L506: Similar to my concern with the objectives, the heading for 4.3 (Physical controls on phosphorus hydrological transport to groundwater) seems to be redundant with the content in 4.1 and 4.2. It seems to me that a more appropriate heading, the highlights the unique part of 4.3, would be "Implications for agricultural management."

Reply: We have deleted this section to incorporate the implications for agricultural management in the two previous sections 4.1. and 4.2..

Technical Corrections:

L86: This statement is unclear ("with pressures assumed to be from GW P pathways")

Reply: We have modified this sentence into "with stream P dominantly delivered through below-ground pathways" on page 6 lines 119-120.

L103: "well drained" is a compound adjective and should be hyphenated. Check for compound adjectives throughout the manuscript.

Reply: We have made the corrections required throughout the manuscript.

L131, Figure 1: in the upper-right subfigure legend, should the circle with an X be a solid circle?

Reply: It is the piezometer/borehole/well which is commonly symbolized this way.

L138: Figure 2 is excellent!

Reply: Thank you for this comment.

L185: I suggest replacing "modelling phase" with "subsequent flow and transport modeling"

Reply: We have modified this on page 12 line 243.

L230: Clarify units on the initial concentration (mmol/cm), which are different than units on the breakthrough curves (mmol/cm3).

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