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Discussions

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 modeling. 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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Specific comments:

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

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.

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

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

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). . .

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.)

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.

L215: The soil core that had the best fit between measured data (Hyprop) and the

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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?]

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. . .

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

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. . .).

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

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?

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.

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).

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.

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.

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

L447-476: This paragraph is excellent.

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.”

Technical Corrections:

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

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

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

L138: Figure 2 is excellent!

L185: I suggest replacing “modeling phase” with “subsequent flow and transport modeling”

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

L296: the bars for infiltration are difficult to see.

L306: “maximum rainfall” should be “maximum rainfall intensity”

L313, Figure 5 legend: “maximum rainfall” should be “maximum rainfall intensity”

L318: Is “peak” the peak infiltration or peak SMD?

L322: What is “ED”? I don’t think this has been defined yet.

L408, Figure 7: It would be helpful to add labels for “High GWL” and “Low GWL”, e.g. right after “(a)” and “(b)”.

S2, Eq. 6: what are the units of h (cm?)?

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