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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.

Anonymous Referee #2

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General comments:

The authors submitted results of a study investigating controls on phosphorus (P) transport from soils to groundwater by application of the one-dimensional transport model Hydrus 1D. The manuscript is well-written and comprehensible in applied methods. Processes influencing P transport in soils and export to ground-/head water were investigated before in many studies for different ecosystems, but still is highly relevant in research because understanding of P mobilization, accumulation, and translocation within soils is incomplete. Therefore, the transfer from observations into adequate modeling approaches too is still a challenge. In this regard, the basic approach of the presented study to combine water and P fluxes in a model to simulate P transport under different boundary conditions is very interesting. However, I have serious concerns about the lack of consideration of biogeochemical understanding of P cycling, especially in the modelling approach presented and in the interpretation and discussion of the study results. My main concerns are: (1) to treat P as conservative tracer in the model and therefore the lack of consideration of any biological or chemical controls on P transport. It is well-known in literature that P behaves not conservative in soils. (2) to ignore preferential flow P transport, whose importance has been shown in recent studies. (3) the lack of validation of model results both by means of own observations and literature data. (4) to discuss spatial and temporal variability based on results of only 1-year observation with 1 value per month and on results of a 1D-model with exactly 1 vertical flow process (matrix flow) at two single points on a hillslope. Nevertheless, I appreciate the approach of investigating soil physical controls on water movement and this should be, in my opinion, the central theme of the paper. This is the crucial requirement to simulate solute fluxes in a next step, by including solute-specific information on processes in soils. For the named reasons, I recommend rejection of the manuscript in the present form, but recommend resubmission with a change focus (water fluxes).

Detailed comments:

Line 35: Introduction: As the physical controls on P transport are derived from modeling results in this study, I miss a (short) description of available tools and why the chosen Hydrus 1D might be suitable for this purpose. There are some interesting reviews available regarding models for P transport, such as Lewis & McGechan (2002), Vadas et el. (2013), Radcliffe et al. (2015), Qi & Qi (2016), Pferdmenges et al. (2020) – just to name some of them.

Line 86: not clear what is meant with '...with pressure assumed to be from GW P pathways.' Please rephrase.

Line 107: Sources for information shown in Table 1 should be named (when not gained within the presented study). Include information on classification system for soil type (FAO, USDA, etc.) and drainage class (what is 'well drained'?)

Line 111: 'transect of multi-level piezometers' How may piezometers per slope position were installed (one each in DS, MS, and US)?

Line 113: 'shallow piezometers' implies that there is more than one per slope position What means 'shallow' - the piezometer screening depth (line 114) are quite deep from the pedological point of view. Is 'screening depth' also sampling depth for monthly taken samples? How this screening depth of 4-7 m for DS fits to the average GW levels which are surface-near (line 129). Please clarify.

Line 205: Figure 3: Why the 'unequal'-sign is needed between DS and MS column? The information left to the column make clear that they are related to specific physical soil parameters. Thus, it is clear that they are not equal. Why upper slope (US) was not modelled? Why 10% dispersivity of solutes? Why not 30 % or 50 % - so, how you determined that value? As far as I understood, this has nothing do to with the solute itself and its chemical properties but is more a theoretical approach. As different elements and compounds which are translocation through soils behave quite different considering interactions within seepage water and adjacent soil, neglecting element-specific processes is a rather rough approach. Or asking the other way around: Does it make any different if I replace in your results the phosphorus by nitrate (or any other solute). The model is a nice approach to simulated vertical water movement, but as it contains no biogeochemical information, it cannot be applied for any solute in my opinion. It may be enable to simulate rough estimate for really conservative tracer, such as chloride and to a certain extent also nitrate, but phosphorus (and dissolved reactive P too) is not a conservative tracer and this is well known in literature (also line 226 ff). P is highly sorptive in soil matrix – and the model simulates matrix transport only. The role of preferential flow for P transport was mentioned in the introduction. Was this somehow included in the modelling approach or would it be possible with Hydrus 1D?

Line 211: The model was parameterized for soil depths down to 55 cm, right? So this is also the depth for which P breakthrough was modelled (results in Fig. 6 and Table 3)? Please clarify. You should also explain how you will conclude from modelled solute transport in 55 cm to GW solute concentrations in several meters' depth. There is a gab where a lot can happen depending on deeper soil properties, geology, etc.

Line 230: Based on which information you defined the initial concentration of 10 mmol cm-3, which is around 310 μg L-1. How sensitive is this value for model results?

Line 288 Table 2: The soil samples were taken in 5-10 and 30-35 cm depth and thus are within the first two horizons you listed in Tab. 2. For the third horizon in DS you assumed same values as in the second. Did you found (in the field survey) that these horizons actually were very similar in soil properties or did you observed considerable changes with depth (what I would expect in the soils you described earlier)?

Line 334: 'comparable to concentrations at MS on some occasions' – It is known from earlier studies that P concentration in different hydrological pathways is highly variable over the year with storm events as one of the main drivers. A monthly sampling strategy can give a rough overview on occurring concentrations but for detailed data analysis values should be handled very carefully.

Line 344: Figure 6: Looking on the temporal development of tracer concentrations, it is not clear to me how exactly the model simulated solute transport. When I understood it right, there is on injection point (e.g. starting with the rain event R1) and then the model simulated the solute curves. During this curves are further rain events – how do they influence solute transport (with additional water input or additional water + solute input or not at all). When I see the modelled tracer concentrations, I am concerned, both with regard to value range and to curve development. There are many studies available, which worked on P transport through soils. You should consider them to carefully validate your results (just as examples: Heathwaite & Dils (2000), Haygarth et al. (1998, 2012, and more), Verheyen et al. (2015)). The peak concentrations of

0.6 mmol cm-3 (or 18.6 μg L-1) seem to be very low and an often observed curve development would be short and high concentration peak directly after or during every rain events (especially after dry periods). The graphs in Fig. 6 confirm my concern above, that the model approach is not suitable to simulate P transport.

Line 348: Same comment as line before: The results are not in any way comparable to results of monitoring studies in literature. The occurrence of tracer (or phosphorus) in the breakthrough depth of 55 cm several days after the rain event is not plausible. Previous observations showed clear 'first flush' effects for P during the rain events (within hours not days!) followed by fast decrease of concentrations (also within the first hours) because of the high importance of preferential flow for P transport. The model results of your study must be validated before interpreting and discussing them!

Line 374: I think you should be careful to discuss 'spatial variability', because a 1D-model with no variation of hydrological pathways (e.g. differentiation in matrix and preferential flow) was applied at two single positions at a slope with no lateral connections (thus, line 378 'along the hillslope' is also not valid). Conclusion on spatial variability are not possible in my opinion.

Line 385: 'favourable soil properties' – consider rephrasing, because P transport to GW is not favourable but should be prevented

Line 417 ff: The section discussion hydraulic properties, but completely neglects some influencing factors which might change these properties. For example, land management affects bulk density, infiltration, and runoff (vegetation cover) as well as soil chemistry (fertilization). Therefore, both water and solute transport through soils depend on many factors which are not included in your study, what should at least be part of a critical discussion of the results.

Line 446 ff: An interpretation of inter-annual variability based on monthly-taken sample for only one year should be handled very carefully. Besides a high variation of seasonal variability from year to year, the within-month variability with rain events as main driver is very high, what was highlighted by some previous studies. However, the influence of anoxic conditions on P release for the downslope position is an interesting aspect.

Line 463: ‘higher soil labile inorganic P’ – no data show, what is ‘higher’? Can you include some values?

Line 478-479: You conclude on soil moisture effect based on one single sample in May, that’s critical.

Line 481: You cannot compare explain GW P peaks with particle-bound P because your samples were filtrated. Nevertheless, the processes you name here are important and therefore should be also considered in models (biological controls, preferential flow, all P fractions).

Line 489: ‘... suggesting that P reaction time ...’ the model can simulate water flux (and P or any other solute is attached to that), thus you could conclude that water flux behaves different depending on type of rainfall event. For P, the model contains no ‘reaction’ routine within the soil matrix.

Line 520: I wonder, why remediation measures are part of the discussion as no land management-dependent effects were considered in the study. In my opinion, the methodical approach is not suitable to derive management measures.

Line 252: Why you assume long-term legacy of P when your soils are well-drained?

Technical comments:

Line 85: change ‘knowedge’ to ‘knowledge‘ Line 118: what is MDL? I think this is detection limit, but all abbreviations have to be explained when firstly mentioned. Line 131: Figure 1: Font size should be increased. Line 230: I assume the unit mmol cm-3 is right here, please prove Line 246: Tables S3 and S4 as well as table 2 include a lot of parameters/symbols. It would be nice for the reader to include a list of abbreviations in the manuscript. Line 322: What is ED? Clarify abbreviation

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