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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Picturing and modelling catchments by representative hillslopes" *by* Ralf Loritz et al.

Anonymous Referee #2

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The manuscript presents 2D distributed physically-based modeling of water flow dynamics at two representative hillslopes differing in geology, soil, and vegetation characteristics. Authors proposed several scenarios with different conceptualizations of hillslope to analyze first-order controls on soil water dynamics and limitations in current modeling approaches. The manuscript conveys an interesting topic, potentially attracting readers of wide hydrological community. However, several points deserve further attention and need to be clarified/improved.

General comments

1. Rather than a rigorous study on hillslope modeling with detailed data-model comparison, authors set up representative hillslopes built on two perceptual models. As a result, no comparison of spatially dependent variables (although measured) was pre-

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sented. Authors avoided intentionally the first step in modeling. The idea of using the state-of-the-art distributed model for analyzing soil water dynamics in a simplistic representative hillslope segment seems awkward as full potential of the model is not exploited.

2. The hydraulic functioning of the simulated hillslope segment is questioned. Given the vertical height of the 2D flow domain (2 m), maximum soil depth of 1.8 m, and thickness of the soil-bedrock interface (0.2 m), bedrock was excluded at some locations. This hydraulic setting affects deep percolation fluxes across the interface. Furthermore, saturated hydraulic conductivities are too high for soil layer, soil-bedrock interface, and drainage system and too low for bedrock. Can still be laminar flow assumed for near saturated conditions (Richards eq.)? Is there any measurement indicating such high Ks value of the soil-bedrock interface? Can Ks value of the soil-bedrock interface be higher than upper soil? Similarly, bedrock porosity values seem too high, is there any (experimental) justification? Grid size of 1 m used for macropores seems unrealistic.

3. Runoff processes (subsurface lateral flow, overland flow, and deep percolation) and their partitioning mostly depend on hydraulic setup of the hillslope (see point 2). Observed catchment streamflow was compared with the sum of the three runoff component. It would be interesting to see individual contribution of each process to streamflow.

4. Some details of the model were not unveiled in the manuscript. For instance, algorithm of the root water uptake module remains unclear as well as the parameters (not sufficient to refer to previous study). As one hydrological year was considered in the simulations and most of runoff occurred during the winter season (Figure 8), no information was given on snowmelt runoff. More comments are appended below.

5. The manuscript is too long. Some parts can be shorten and condensed (see Detailed comments). Please focus more on hydrology and less on philosophy. Conclusions part resembles Discussion.

6. Discussion should be condensed, there are too many points which are discussed. The results obtained in this study are not related to previous literature, i.e. no deep discussion is provided. Focus on the main aspects of the results. Some discussion parts are too vague and superficial, adding limited value to objectives ("to identify limits in our theories and related physically-based models") and overall knowledge.

Detailed comments

Lines 14-27: This is too long introduction in the abstract. Please shorten.

L16-20: Conceptual models can also be physically-based. The use of conceptual models is incorrect.

L34-6: Not true, internal water storage was not simulated well (see Figure 9).

L79-85: The literature body of recent hillslope 1D&2D modeling applications is somehow limited to a narrow window (mostly of the author's group). For instance, predictions of 2D Richards-based hillslope model with a provision for preferential flow were compared with field data (hillslope discharge and spatially distributed pressure heads) in recent studies.

L110-3: Yes, hillslopes are indeed important in some headwater catchments. However, wetlands/riparian zones may control the runoff generation in other headwater catchments.

L166-8: Is there any source to justify this statement?

L172-4: Add a few references here.

L181-4: A reference is needed here to support the statement on water balance.

Introduction section seems to be quite long, it can be effectively shorten without losing the central messages.

L223-5: Delete "concentration".

L268-70: “. . .perceptual. . .”.

L311-4: Double mass curve is not well suited for studying annual water balance since it relates runoff to precipitation. There is no provision for e.g. storage changes.

L421-4: Delete “a function of”.

L424-6: This is not clear, please explain.

L428-30: The algorithm of RWU is not clear.

L439-42: Any experimental justification for arrangement of the structures? Taken from previous studies on different hillslopes?

L450-3: Define slope angle. Was variable hillslope width considered (total area remains unclear)? The same pertains to Wollefsbach hillslope.

L453-4: Grid size of 1 m seems as a crude approximation for macropores.

L464-6: Boundary conditions are not clearly explained. Does free outflow refer to free drainage BC (with unit hydraulic gradient condition)? Is gravitational flow boundary condition seepage face BC?

L472-3: Ohm*m. In Figure 6B, contour line of 1500 ohm*m is situated in depths ranging from 1.0 m to 3.2 m for 100 m hillslope length. How such spatial configuration could be simulated in 2 m high hillslope segment? Please use the same hillslope segment in Figure 3 as ERT cross-section shown in Figure 6B.

L474-7: Model of van Genuchten assumes zero air-entry value. Thus, alpha parameter is not reciprocal to air entry value. Not clear explanation of the soil hydraulic parameters - list also the parameters of macropores in Table 1. Furthermore, show the (variable) depths of the soil structures in Table 1.

L486-9: I did not find any band generator in Zehe et al. (2010a).

L501-3: Be more specific, what kind of data?

L508-9: Pressure-water content relationship was measured in 0-3 pF range in detail, resulting in large spread of data points. This is not true for smaller pressure head values. These aspects may invalidate the fitted representative curves used in modeling.

L518-22: Boundary and initial conditions are identical for the two hillslopes. Do not repeat the information.

L523: “Model scenarios”

L527-31: Not left boundary (Figure 3CD)? What is the added value of log NSE compared to NSE criterion?

L551: “In VE2.1 scenario, . . .”

L558-9: Instead of “Last not least” use “Finally”.

L565-8: Need to say what parameters were changed and what were kept unchanged. Otherwise it is a black-box.

L581-2: Delete “until the onset of the summer period”.

L582-3: “Summer period was started with ...”.

L585-7: Does this mean that value of the saturated hydraulic conductivity was increased 75 times compared to reference scenario? Reference scenario used already high Ks value.

L593-4: This a harsh break from runoff to 2D saturation distribution, I suggest discussing runoff first and then move to saturation.

L602-5: This can’t be concluded, so far only runoff component of hillslope balance was shown. No comparison on hillslope storage was made.

L607-9: I would not say that in case of Wollefsbach, please see the winter period in Figure 8D. To support this statement, provide efficiency coefficients for both winter and summer period.

L610-2: Is hydrological year 2013 meant here? I miss the point. A1 scenario for Colpach has NSE = 0.84, so why discuss smaller NSE? Instead, scenario A2 for Wollefsbach requires discussion (NSE = 0.26).

L613-5: Not clear what is discussed, Weirbach simulation?

L622-4: Please provide the volume proportions (overland and subsurface flow, deep percolation) of runoff for both hillslopes.

L624-5: Infiltration-excess overland flow was most likely not simulated due to extremely high Ks value of (top)soil. Was the extent of overland flow decreased by further increasing Ks value of bulk soil (emergent structures)? Such short high flow events also could not produce saturation-excess overland flow. Please check.

L631-5: Not clear ". . . terrestrial filter properties . . .".

L666-8: This suggests misrepresentation of the soil profile (e.g., soil layering) as well as misparametrization of the soil hydraulic properties.

L670: This paragraph needs a reference to Figure 10A. Evapotranspiration module is treated as a black-box for readers. We are left unaware what parameters were changed. Was there a difference between potential and actual ET fluxes? Did any water stress occur? Add information on Wollefsbach hillslope.

L690-2: Do not write "it may be", this must be exhaustively explained by the model. Less storage above the interface for steeper hillslope setting would lead to increased storage in bedrock. Note that deep percolation was considered when comparison with observed streamflow was performed. This would also cause a delay in simulated runoff compared to reference scenario.

L708-10: Please make a reference to VE2.3.

L732-5: Please reword.

L737-41: Delete "revealed and" and "matching".

L773-6: Instead of “parallel” use “lateral”.

L778-80: Instead of “in concert with” use “and”.

L788-90: Satisfying match was obtained due to large measurement variability. Many scenarios with different parameter sets would fall within measured soil water content range (even different modeling approaches can provide similar match).

L796-9: Instead of “benchmarking” use “comparison”.

L802-4: Given the comments above, I would hesitate to make such statements.

L810-2: The values contradict the previous statement.

L812-5: I do not agree with this statement, see Figure 9.

L828-30: Check KGE values (see Table 2 and Figure 11).

L884-6: Is VE2.3 scenario discussed here?

L886-9: It may be also due to location of a large depression (considered at the hill-foot region in this scenario). Therefore, these statements are not fully justified by the results.

L908-9: Not clear what are SVAT modules.

L911: The message of this section remains unclear. What is suggested here? The need to use distributed model of representative hillslope with spatially uniform rainfall and pET fluxes?

L912-3: Not clear “. . . models encountered to capture flashy . . .”.

L936: This section is too vague.

L995-7: This can’t be concluded in such a general way. Beside hillslopes, riparian zone may play an important role in runoff generation in some catchments.

L1171-3: Cite this study in HESS.

L1272-5: Improve the reference.

Figure 3: Add dimensions instead of “small section”.

Figure 4: Dimensions are necessary.

Figure 9: No green color found. Stick to Colpach and Wollefsbach.

Figure 10: Relative saturation > 1?

Figure 12: Add hillslope location.

Table 2: Check KGE values shown in Table 2 and Figure 11.

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