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Hydrology and Earth System Sciences Discussions

Interactive comment on "Effect of the spatial distribution of physical aquifer properties on water table depth and stream discharge in a headwater catchment" *by* C. Gascuel-Odoux et al.

Anonymous Referee #1

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The paper tries to generalise the spatial distribution of soil-regolith hydraulic properties in a small basin, using a physically-based hillslope hydrology model, constrained on catchment discharge and average water table, and assessing the quality of the distributed water table simulations by comparison with the observations in a transect of piezometers. Diverse relationships between topography and aquifer hydraulic properties were tested.

General comment: In general terms, the paper is clear and, although the model is not original and the results are not very promising as no clear improvements in the

simulations of the water table were obtained, it may be useful for other researches to design how or how not similar issues may be approached. A more detailed analysis of the data before model application, some more details and discussion on the model calibration process and more concise discussion and conclusion would be welcome.

Specific comments: It seems indeed that the six piezometers analysed (figure 7) can be grouped in two classes with apparently high correlation between the graphs within the groups. The three fist graphs show that "water table rise occurs rapidly with each rainfall event" but after the events, the water table drops with similar velocity and remains very stable between events (particularly for PG3): it looks much more like a stream stage record than a piezometer record and some more comment on this behaviour would be welcome. Yet, the setting of the measuring instruments should be better described, so the occurrence of positive values can be understood by the reader. On the other hand, the three last graphs show a much more common behaviour for shallow aquifers, but with very quick rises of the water table during the events, suggesting both a fast recharge and low values of drainable porosity: on the figure 2, PG6 recorded a rise of about 3 meters subsequent to a precipitation of about 90 mm, so the change in volumetric ground water content must be smaller than 3%. May this be the behaviour of a fissured rock?

Looking to the figures 1, 4 and 7 as well as the results in Molénat et al. 2005, I wonder if the apparent spatial variation in aquifer properties might be simulated just as the result of changes in depth, given the different depths active in downslope and upslope situations. The relative better success of increasing K upslope might be an artefact caused by the use of an inadequate model for variation in depth. Yet, the model is claimed to include a depth function for drainable porosity, but no details on this function are given, nor are tests with different parameters of the function reported.

It is unclear which parameters were included in the Monte Carlo calibration: tables 1 and 3 include only 7 parameters whereas the text states 8 parameters for the model. Yet, the parameters for the spatial variation are not included in these tables.

The mathematical form of the range of variations of the water table (R) is inadequate. Originally this was a quotient, defined between 0 and the infinity and centred in the unity. The logarithm of the quotient should be used instead of the subtraction of a unity in order to "normalize" and obtain a metrics similar to D but avoiding the clear asymmetry of R in the figures 5 and 10. Dist-R measurements would also be then mathematically correct.

The discussion is wordy and includes some opinions not based on the results. The authors seem to have too much faith on the capability of the model for simulating the internal functioning whereas it is well known that many sets of internal processes may give the same results as used for the calibration, and that not all the relevant processes may be simulated by the modelling approach. As an example, 64% of the precipitation is evaporated in the basin, so spatial variations in transpiration may be also relevant in the dynamics of the water table.

Technical comments: - some temporal graphs seem to start in October ("water year"), but this is not stated in the text or captions.

- page 6936, line 21: "we included a term for constant hydraulic conductivity with depth in our model".

- page 6942, lines 16 and subsequent: this is not clear in the graph and must be rewritten after modification of the mathematics of the R variable.

- Tables 2 and 3: "with slope position" does not include the description of the up or down direction.

- caption of table 2: Mean error is D. Mean D and Mean R.

Fig 1: PG1 is not stated in the map.

Fig. 2: the caption does not correspond to the figure, and this figure was already published in Molénat et al. 2005.

Fig. 4: this seems to be the simulated water table, but this is not stated in the caption. A similar figure with the observed data should be included for comparison.

Fig. 5: it is difficult to understand why PG2 simulations appear in the positive values of R for "no variation" whereas they appear in the negative values in the rest of the graphs. Is this an error or do you have an explanation? This figure must be redrawn and reinterpreted after the correction of the mathematics of the R variable.

Fig. 7: Include an X axis. Please, clear the artefacts from the observed values. It is unclear which these simulations are: best fits for discharge? best fits for average water table? best fits for the simulated piezometer?

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