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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.

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Response to reviewer 1

Major comments

Reviewer comment

"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?"

Translated in 3 sub responses

o Is there two groups of piezometers? Is there any correlation between piezometers and with stream discharge?

We propose to add a correlation table between the 6 piezometers and stream discharge, calculated on daily data. This table can highlight that the 6 piezometers can effectively be grouped in 2 sets: 1) one group highly correlated to the stream discharge, but not recording stream stage since the flow is still towards the stream, explaining our focus on only one piezometer (PG3) of the group; 2) the other group poorly correlated to the stream and from one another, explaining the choice of two piezometers (PG5 and PG6). This table can be used for the discussion, highlighting that new information, compared to discharge information, can only comes from water table in upslope domain. This graph will be commented as above.

o Why are there positive values of water table depth?

Positive values for water table depth were recorded only in riparian zone, mainly at PG1 and PG2 locations, for short times. The positive values did not exceed 20 cm. This corresponds to flooding of the riparian zone. As a consequence of the flooding, the hydraulic heads in groundwater were larger than the soil elevation leading to record positive values of water table depths.

o What explains the high reactivity of the water table upslope? Is it normal values for a fissured rock?

We propose to add two graphs in the presentation of the site: 1) temporal frequency of water table depth for the 6 piezometers on 4 years, confirming the two sets of piezometers, but also indicating that variations of the water table from PG4 to PG6 correspond to highly different depth along the water year, and therefore layers; 2) the reactivity of the water table depth versus rainfall amount, indicating a linear relationship with decreasing coefficient (from PG4 to PG6, going from 6.4, 5.8, 3.1, respectively), and therefore a decreasing drainable porosity from midslope to upslope, confirming the availability of the chosen values of drainable porosity (3 to 5.5 in the Monte Carlo procedure). PG6 is definitely not in the fissured layers but in weathered layers. This has been identified when drilling the wells. Otherwise slug test have been performed on PG 2, 3 and 4 (See Molenat et al. 2005), and the chosen values are those observed in weathered layers on these wells.

Reviewer comment

"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."

Translated in 3 sub responses

o Details on the depth variation functions on K and porosity? Are these variations constant or not? With slope position? Other model of variation in depth?

The depth variations of k and drainable porosity are both described by decreasing exponential functions, as shown in Eq. 1 and Table 1 for KS, but only by Table 1 for drainable porosity. Each function comprises a parameter defining the change with depth as indicating Table 1. A sentence will be added in the text to describe the variation in depth of the drainable porosity, giving the type of the depth function for it and indicating that its variation in depth is much slower than for Ks.

o Monte carlo on all the 7 parameters? Is there 7 or 8 parameters?

There are only 7 parameters in the model (and not 8): this will be corrected. The Monte Carlo test has been realised on these 7 parameters.

o Correction of the mathematical form of R?

We agree that the proposed mathematical form would be formally more adapted. However, it will not change the conclusions, shifting and distributing the criteria differently, but not changing the comparison between the different simulations. Nevertheless, the proposed mathematical form of R will be indicated in Eq. 6 and criteria re-calculated, and then implemented in Figures 5 and 10, as well in all tables and graphs where R is used.

Reviewer comment

“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.”

Translated in 3 sub responses

o Value of PET during the study period? Spatial variations?

The cumulated values of rainfall and PET over the study period are about 340 and 80 mm respectively. Therefore the recharge and lateral flow processes are clearly dominant. If spatial variations of PET would have to be considered, they would increase from upslope (deeper water table) to down (saturated conditions), and be rather similar from PG1 to 3 (saturated conditions), and from PG4 to 6 (soil at field water capacity). But, firstly, the difficulties are to simulate the water table during the recharge periods (where ET is not an active process) as well during the recession periods (where ET is an active process). Secondly, the simulated values are rather correct in PG4, much higher in PG5 and 6 while ET can be considered to be similar between these three wells. Therefore we think that the spatial variations of ET could exist but could not explain the difficulties in modelling water table depth along the hillslope.

o Other processes except PET not modelled?

Many other internal processes could have been tested (preferential recharge,. . .), spatial variations of others parameters, ect. . .. But the first ones are completely outside of our focus, and the second ones are discussed in the discussion section. We could discuss the conditions at the limits of our system. ET has been discussed previously. We can also discuss the boundary conditions at the top (connexion with an aquifer having boundaries not delimited by topography, other regional aquifer,. . .) and hav-

ing flow draining deeply (deep drainage), The spatial variations of KC (conductivity in depth) have to be mentioned as well the flow pathways from the top the bottom which can deeply influence the spatial variations of the water table dynamics. These two assumptions will be added.

o Wordy discussion?

The discussion could be a little shorter: we will streamline this section. Caution will be carried on the fact that all sentences have to be supported by results. But this reviewer comment is not supported by examples and we do not see that the discussion is not supported by our results.

Technical comments

- some temporal graphs seem to start in October (“water year”) but this is not stated in the text or captions.

Will be corrected on Fig.2, 4 and 7 by adding “from 31October to 31 March”

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

Will be corrected by the proposed sentence

- 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.

The mathematics on R will be corrected as proposed, as well corresponding graphs 5 and 10 other comments and results.

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

Will be added by “m and drainage porosity increasing from downslope to upslope, as previously for Ks, to simulated higher weathering processes upslope (deeper and more intense).

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

This mistake will be corrected.

- Fig 1: PG1 is not stated in the map.

This mistake will be corrected by adding PG1.

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

This is right. But it seems important to present and to comment the observed data. The caption will mention the presence 5 wells data (and not 2), and that the figure has been previously published in Molenat 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.

The caption will mention that the data are simulated ones. The observed data cannot be included in the same graph which would be unreadable. Otherwise, these observed data are just previously presented in Fig. 2.

- 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.

The table indicates negative values for R-PG2 for all the model of the table, which is exact, while Fig. 5 represents negatives values for only one model (no variation) which is a mistake which will be corrected.

- 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?

A X axis will be included. Artefacts from observations will be removed. These simulations are related to best fits for discharge.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6929, 2009.