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Hydrology and Earth System Sciences Discussions

HESSD

6, C3289–C3294, 2010

Interactive Comment

# *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.

**C. Gascuel-Odoux et al.**

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Major comments

Reviewer comment

1.) P6934L3 (and section 4.4): What is known about the spatial distribution of the hydraulic conductivity (and drainable porosity) from these "intensive soil studies"? What information on saturated hydraulic conductivity is available from slug tests in the piezometers or from soil cores or from other soil information? Please give more background information about the observed spatial variation and how much variation



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Interactive Discussion

Discussion Paper

there is. Which of the 4 models used in this study would ‘fit’ the observed data best or would because of the variability/uncertainty all models fit the data equally well?

Response

We do not know the spatial structure of hydraulic properties. Slug tests have been performed in three piezometers (PG2, PG4 and PG5). Estimated saturated hydraulic conductivity at these three locations do not exhibit a spatial distribution but any rigorous analysis of the spatial distribution of Ks would require estimations at much more locations in space. But this is very difficult to perform on this site as well on many other sites. To establish a spatial structure, it would be necessary to get 50 slug tests and more, which is impossible. Intensive soil studies concern the soil (upper than 1 m) but not weathered layers (deeper that 1 m), as it is generally the case, due to the difficulties to measure them in such material. Therefore, we cannot chose a-priori between the proposed four models. But we know that spatial variations are probably present and within the modelling process, we could estimate the spatial structure by fitting the model to the water table with different structures and to infer the best one. This explanation will be added in site description section.

Reviewer comment

2.) P6938L21: It is unclear why a constant soil depth was used in the model. It is mentioned several times in the manuscript that the soil depth is variable and varies between 0.5 and 1.5 m (e.g. P6934L15, 18-19; P6946L7; P6947L23). It is also mentioned that soil depth variability can have a large influence on modeled flow and water table responses (e.g. P6932L17). However there is no mention or discussion of how this uniform soil depth has influenced the results of this study. Would water table responses be better simulated if a variable soil depth was used? There should be at least a discussion of the effect of variable soil depth and how it may have influenced the results of this study.

Response



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Similarly, the soil depth is well known (between 0.5 and 1.5 m. . ..) as indicated, but the depth of the weathered layers, i.e., the border between weathered and fissured bedrock is similarly, poorly known and difficult to investigate. Therefore, the depth of 10 m corresponds to an approximate depth, deeper than the water table to not influence it, and averaging few observations. This remark will be included in study site section. We did not perform sensitivity analysis on soil depth, considering that this study has been done previously (Saulnier and al. as mentioned in the text and reference list).

Reviewer comment

3.) P6939L13: How was hillslope discharge calculated from the stream discharge? Was the hillslope contribution assumed uniform along the stream-length? What about contributions from the riparian zone/stream channel interception? Was this a convex/ concave/planar hillslope? I expect that hillslope contribution to the stream is highly spatially variable and that there are thus differences in both magnitude and timing of hillslope flow. How did this assumption influence the results? At a minimum it should be discussed how these uncertain estimates of hillslope discharge influenced the model results and model calibration and how this influenced the results of this study.

Response

Assumption was made that hillslope contribution was uniform along the stream network length. As a consequence, the specific stream discharge measured at the outlet was considered as the specific discharge of the study hillslope. The uncertainty arising from this assumption are pointed out in the discussion. The hillslope present a constant slope (Fig. 1) except a small plateau (after PG6), and a small convex domain (PG1 to 3) which correspond to a riparian zone (PG1 to 3). It correspond to a headwater catchment, which present similar hillslopes with the same land-use, so that we can assume that the hillslope contribution is uniform along the stream-length, including the riparian zone, and differences in both magnitude and timing are small. This will be discussed in the discussion part. This is a strong assumption, but this is the only way to connect water table depth and discharge. This assumption is being used in the literature since measuring hillslope runoff is very difficult or impossible.

Minor comments

1.) P6933L19: Did they simulate the water table for just one position? Clarify. We simulated the water table for all position but we focused on the difficulties to simulate it in the upslope.

This will be added.

2.) P6934L10-11: What was the precipitation from 1994-2004? Was the ET measured of calculated from the water balance?

It would be better to have P and PET measurements over 30 years, but we have P over 30 years, but PET only over 10 years (both measured). This information will be added.

3.) P6935L7: How many transects are there in total? Discuss here why this transect/ hillslope was chosen (earlier than P6936)

There are 3 transects in the catchment and we have chosen that one because 1) already simultated without spatial structure, 2) a linear slope without any slope effect and hedge row (disrupt in the slope, which could interact with topography. This information will be added.

4.) P6936L20-22: This sentence is not clear and does not correspond to equation 1 since equation 1 still has an exponential decline component as well. Rewrite to “we included a constant hydraulic conductivity at deeper depths”?

Agreed.

5.) P6938L12: What was the resolution of the DEM? How was it obtained?

DEM with a 10 m grid size, plus manual measurements (digital Laser total station) on this hillslope. We will add this information.



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6.) P6939L21-22: Give the dimensions of the hillslope earlier (either in the site description or on P6936L12) (and/or shade the area in figure 1)

Agreed.

7.) P6940L13: Rewrite this sentence as it does not describe equation 5. Replace "difference in the average level (D)" by "average difference in the water level (D)"?

Agreed

8.) P6940L15: Why were only 3 of the 6 wells used? Why these 3? Explain. Without an explanation is seems like you are hiding something.

This is explained and detailed by the response given to the first comment of the first reviewer.

9.) P6941L20: What period was used for the validation?

This is described in Molenat et al. (2005) and can be added here

10.) Figure 2: Show the discharge in figure A as well. Replace "time" on the x-axis by "day since xxx" or is it "Day of Year" or replace the axis labels by actual dates. The caption does not fit the figure as more than "two" wells are shown and "discharge" is not shown.

Days will be added. The comments and replies are similar for both reviewers.

11.) Figure 3b: Typo on the y-axis "factor of variator"

Agreed

12.) Figure 4: Caption does not fit the figure as 6 wells are shown instead of 5. Label the x-axis.

Days will be added. The comments and replies are similar for both reviewers.

13.) Figure 7: Insert an x-axis. There seems to be some wrong data (sudden drops in



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water level – was the recorder temporarily lifted out of the piezometer?) for pg 1, pg3 and pg4. Was this data excluded from the model calibration and the calculation of the efficiency?

Days will be added. Yes there are few wrong data points which have been excluded from the model calibration and the calculation. They will be corrected here.

Minor editorial suggestions: This paper has been reviewed for English: we got an American Journal Experts Editorial Certification. But these propositions to improve style and/or understanding will be obviously all included. Thanks to this reviewer for them.

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