

## ***Interactive comment on “Hortonian overland flow closure relations in the Representative Elementary Watershed Framework evaluated with observations” by E. Vannamettee et al.***

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We would like to thank the reviewer for the comments. Please find below our response (OC) to specific comments by the reviewer (RC), indicating changes proposed to the current manuscript.

RC: This paper is a follow-on from Vannamettee et al (2012). In reality it is no more than an application of the ideas presented in the earlier paper. It simply applies the ideas presented there to a specific watershed. In that sense, it provides no conceptual advance, but merely purports to tell us if the methodology proposed in the earlier paper

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works. The main problem with the paper is that the answer to the question "Does the methodology work?" has to be yes, since it is constrained to be so.

OC: The paper referred to (Vannamettee et al., 2012) proposed a new closure relation for deriving the Hortonian infiltration and runoff fluxes at the scale of geomorphological response units (GRU). The approach was basically an upscaling technique that derives hydrological fluxes representative at the GRU scale from local-scale infiltration parameters, boundary conditions, and geometry of the geomorphological response unit. The upscaling method proposed in the previous publication is generic in the sense that it is assumed to be transferable and applicable to any response unit, as it performs scale transfer using upscaling functions. The method proposed was, however, purely based upon (and evaluated against) an extensive synthetic data set of rainfall-runoff responses generated with a physically-based model. In our opinion it is a logical next step to evaluate the performance of such a theoretical concept using empirical data. This is done in this paper, using a new data set that has not been presented elsewhere. We present a set of model scenarios that give a proper and objective evaluation of the proposed closure relation, by comparing it with a benchmark. The benchmark is a simple lumped model, simple lumped rainfall-infiltration-runoff model without considering the spatial processes and scaling effects of the GRU in runoff generation. However this model is not much simpler than what is often used in the large-scale model when no information is available on the geomorphology within catchments.

RC: If you calibrate a model against a dataset for the same catchment you would be quite unlucky if it did not work reasonably well (that said the Nash-Sutcliffe statistics are not that impressive!).

OC: We agree. The evaluation of the proposed closure relation is however not done in absolute terms but by comparing the proposed closure relation with a benchmark model. This is done both for the uncalibrated case and the calibrated case. In the calibrated runs, the benchmark model is calibrated as well, thus its performance is also improved by calibration. We show that for all situations (uncalibrated and calibrated),

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the proposed closure relation outperforms the benchmark model regarding the Nash Sutcliffe efficiency and total discharge volume.

RC: Any model that is calibrated is no longer a physically based one, since by calibrating you have, in effect, thrown the physics out of the window. So, philosophically, the paper is unsound. However, these authors are not alone in this practice, and it may be unfair to castigate them for a widespread philosophical failing.

OC: We are sorry, but we disagree with this statement. There are two approaches to find parameter values for a physically based model. One is direct measurement of parameters at a support size that corresponds to the support size required for the physically based model. The other one is by inverse modelling (calibration), also resulting in parameters representative at the support size of the model. In both cases, the physical basis of the modelling is reduced to a certain extent, because one will never be able to provide (either through direct measurement or calibration) model parameters representative for the support size used in the model, for each support unit (each x,y location, each time step) in the model. We will always have to make additional assumptions (e.g. that parameters are uniform over certain areas). So, yes, we do 'throw physics out of the window', but only to a certain extent, and this is also the case when model parameters are directly measured in the field.

As a side note, parameters in well-known physical models or theories are estimated by inverse modelling as well. For instance, take the purely physical model describing the movement of a pendulum, which has acceleration of gravity as a parameter. The acceleration of gravity can be estimated by measuring the period of swing of the movement, using the pendulum model, which is thus an inverse modelling approach. This approach was a standard way of measuring the acceleration of gravity until other techniques appeared.

RC: More specifically, however, I think the paper skips over very important issues that need to be addressed, even if the philosophical weaknesses are overlooked. Funda-

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mentally, the paper is about Hortonian runoff, yet there are no data from the watershed to back up the assertion that Hortonian runoff actually occurs.

OC: We agree that we should have included in the original manuscript evidence that Hortonian runoff occurs on a regular basis in the study area. We have empirical data that strongly support the occurrence of Hortonian runoff. The rainfall intensity is up to 0.1 m in 5 minutes, particularly in summer (page 1786, line 11). This intensity far exceeds the average infiltration capacity of the soil surface measured with rainfall simulations (see below). More importantly, we observed Hortonian runoff ourselves (i.e. runoff occurring due to ponding quickly after the start of a rainstorm, in conditions and at locations without a fully saturated subsoil) in the study area during yearly field measurement campaigns over the last 15 years.

RC: Ks values, which are critical to the modelling, are obtained from Rawls et al but there is no attempt to validate these values for the particular watershed. At the very least a table of the calibrated Ks values that are used in the modelling to compare with the ones taken from Rawls et al must be provided, so that the reader can judge just how physically reasonable the values that yield acceptable model output are.

OC: We have a data set of Ksat values measured with rainfall simulators in a 10 x 10 km area that includes our study catchment, having the same geomorphology, land use, and soils characteristics (de Jonge, 2006). However, measurements were only available for four different geomorphological units. Uncertainty in the measurements is quite high as it is notably hard to measure Ksat with a rainfall simulator. Thus, we decided to use Ksat values from Rawls et al. (1982) for the uncalibrated runs. We agree with the reviewer, however, that it is interesting to compare calibrated Ksat values with the field measurements. Table R4.1 shows that, for the proposed closure relation, the calibrated Ks values are comparable to the measured Ksat in the field (note the high variation of the Ksat values derived from the rainfall simulations, which is mainly due to measurement error). For the benchmark, the calibrated values are much higher than the ones observed in the field and have no physical meaning. These results strengthen

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our claim that local-scale measured values can be directly used as input to the closure relations because the closure relations already contain a scaling component to account for the scale-transfer effects (page 1791, line 13-17). Also, these results confirm that our proposed closure relation outperform the benchmark, as calibration gives local scale Ksat values that fall within the range of those measured in the field, which is not the case for the benchmark. In hindsight, one could argue that we should have used the Ks values measured with the rainfall simulators for the uncalibrated runs. However, we did not do so for the reasons given above. We will include the Ks values measured with the rainfall simulation in the revised manuscript, by shortly describing them in the Methods section, the results section, and referring to these in the discussion of the calibrated runs.

Insert table R4.1 in 'hess-2013-27-supplement.pdf' here

RC: Sensitivity analysis of the model output is missing.

OC: We performed a sensitivity analysis to get impression on how the model behaves with different parameters. It is found that Ksat is the most sensitive parameter, so this parameter is chosen for calibration. Because the sensitivity analysis was not performed in a systematic way, it was not presented in the original manuscript. We will include the sensitivity analysis section in the revised manuscript.

Sensitivity of model behaviour to changes in input parameters is investigated and evaluated in terms of changes in total discharge volume as a result of changes in five model parameters; saturated hydraulic conductivity (Ksat), matric suction at the wetting front (Hf), initial moisture content (mc), leaf area index (LAI) and interception capacity per leaf area (Ic). These model parameters are adjusted by 25% of the values used in the standard runs. Due to the time constraint in compiling the response, model sensitivity is evaluated using 5 events. The results are shown in the Table R4.2 and R4.3. It is shown that Ksat is the most sensitive parameter, followed by Hf and mc. The sensitivity characteristics are quite similar for our closure relation and the benchmark model. The

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model output is less sensitive to changes in parameters used to calculate the forcing of the closure relation, compared to those used in the closure relations (i.e. Ks and Hf).

Insert table R4.2 and R4.3 in 'hess-2013-27-supplement.pdf' here

RC: Overall, the paper asks the reader to take too much on trust. Maybe the model does do as well as the authors claim, but I would like to be able to judge this for myself rather than rely simply on the output statistics.

OC: In this rebuttal, we provide the results for a number of other hydrographs (see Fig R2.1, R2.2, R2.3, R2.4 below) for the reviewer to be able judge the model performance instead of relying only on the statistics. We propose to include these as appendix in the paper.

Insert Fig R4.1, Fig R4.2, Fig R4.3, Fig R4.4 here

Fig R4.1 Hydrographs ( $Q$ ,  $m^3 h^{-1}$ ) modelled using the closure relation C (red) and C\*(blue), compared with the observed discharge (obs, black), for an event on 1 April 2010. Rainfall intensity ( $R_t$ ,  $mm h^{-1}$ ) is shown on the secondary axis. E and E\* are the Nash-Sutcliffe indexes for the closure relation C and C\*, respectively. Left panels, without calibration; right panels, with calibration.

Fig R4.2 Same as in Figure R4.1, for an event on 7 April, 2010.

Fig R4.3 Same as in Figure R4.1, for an event on 7 September, 2010. Note that observed discharge in the M catchment is not available for this event.

Fig R4.4 Same as in Figure R4.1, for an event on 8 September, 2010. Note that observed discharge in the M catchment is not available for this event.

References:

Jonge de, M.: Physically-based hydrological modelling of a sub-basin in the Buech catchment, MSc Thesis, Utrecht University, 2006.

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Rawls, W. J., Brakensiek, D. L., and Saxton, K. E.: Estimation of Soil Water Properties, T. ASAE, 25, 1316–1320, 1982.

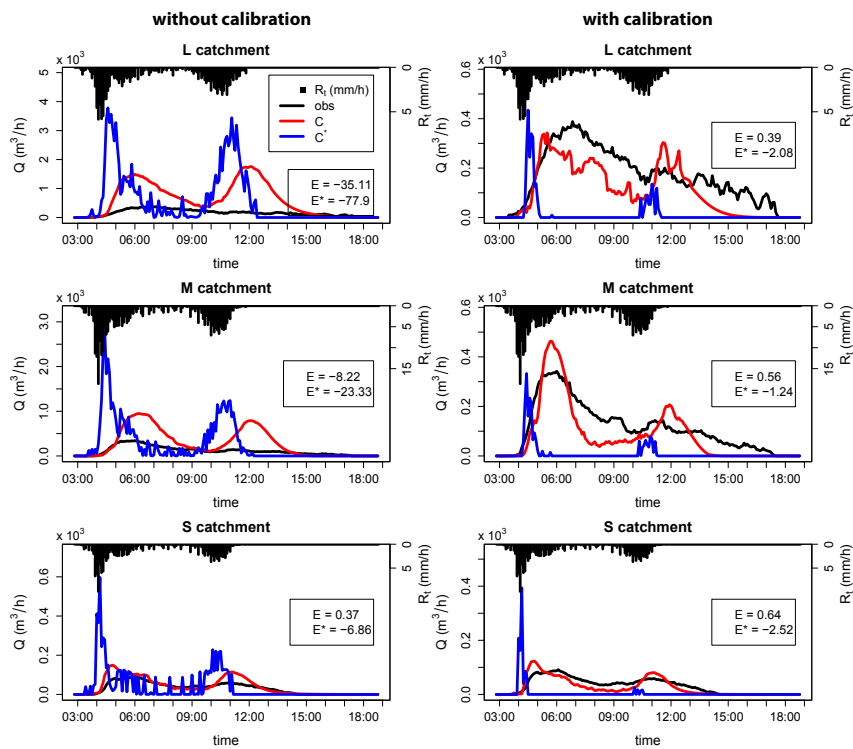
Vannamettee, E., Karssenberg, D., and Bierkens, M. F. P.: Towards closure relations in the Representative Elementary Watershed (REW) framework containing observable parameters: relations for Hortonian overland flow, Adv. Water Resour., 43, 52–66.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/10/C1191/2013/hessd-10-C1191-2013-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 1769, 2013.

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**Fig. 1.** Hydrographs ( $Q$ ,  $\text{m}^3 \text{h}^{-1}$ ) modelled using the closure relation  $C$  (red) and  $C^*$  (blue), compared with the observed discharge (obs, black), for an event on 1 April 2010.

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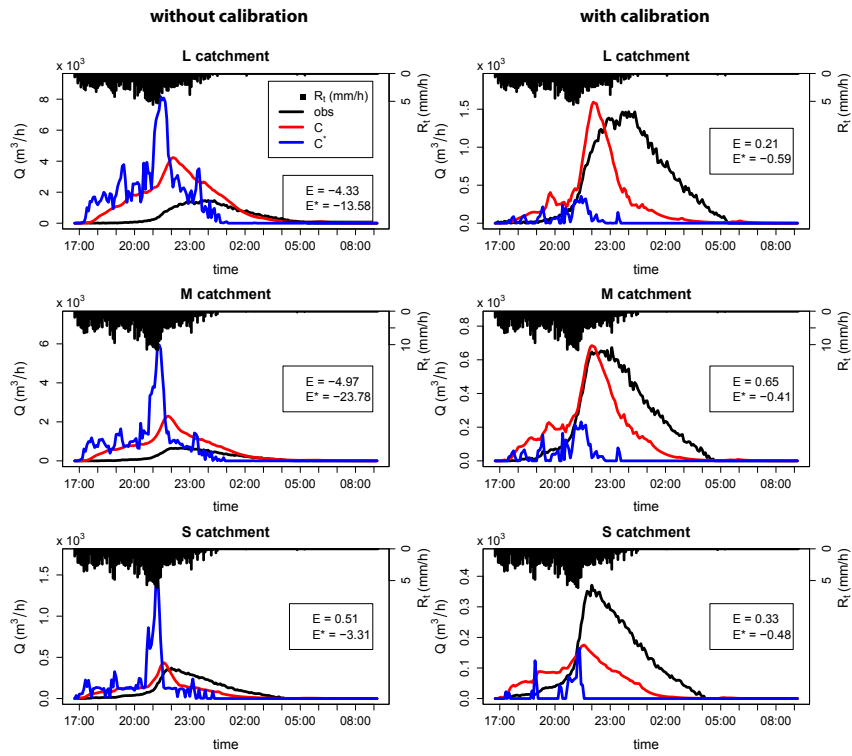


Fig. 2. Same as in Figure R4.1, for an event on 7 April, 2010.

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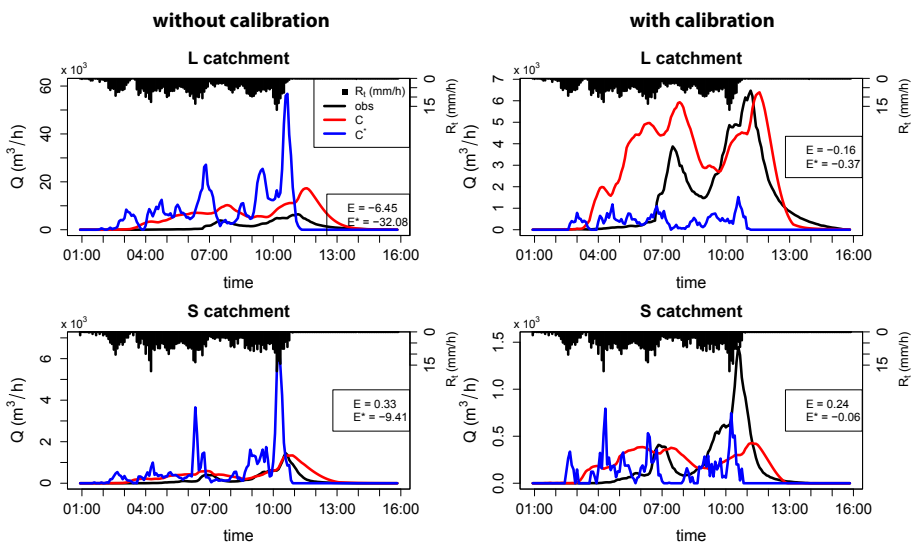
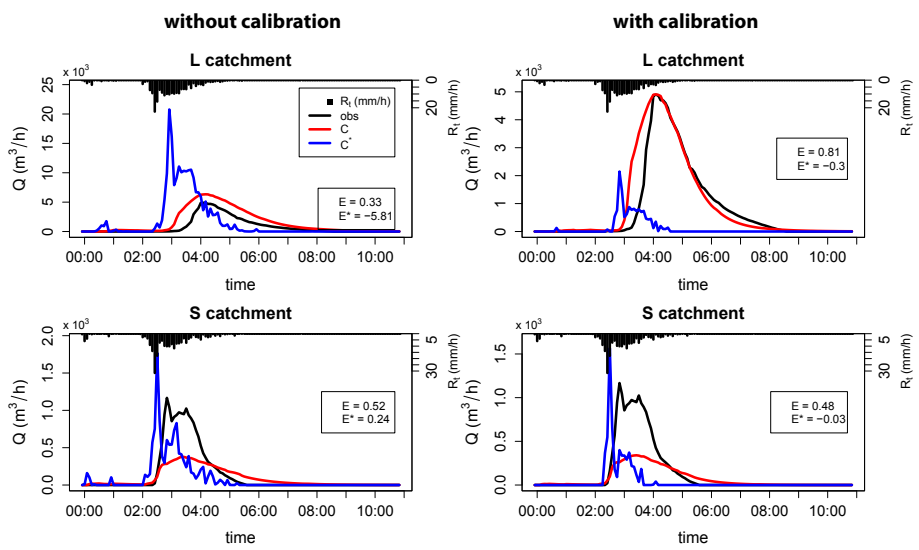


Fig. 3. Same as in Figure R4.1, for an event on 7 September, 2010. Note that observed discharge in the M catchment is not available for this event.

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**Fig. 4.** Same as in Figure R4.1, for an event on 8 September, 2010. Note that observed discharge in the M catchment is not available for this event.