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Interactive Comment

Interactive comment on "Hortonian overland flow closure relations in the Representative Elementary Watershed Framework evaluated with observations" *by* E. Vannametee et al.

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

RC: The flow model is calibrated by a simple fitting procedure described in a recent paper (2012) in AWR by the same author. In the current manuscript the calibration procedure (testing model performance for a range of calibration factors that scales the Ksat value) is tested for three subsystems of a real world catchment.

OC: We would like to emphasize that we are not testing an ad-hoc calibration procedure on empirical data. The previous publication (Vannametee et al., 2012) proposes a new approach for deriving Hortonian infiltration and runoff fluxes representative for geomorphological response units (GRU). The approach proposed was an upscaling technique that derives hydrological fluxes representative at GRU scale from local-scale infiltration parameters, boundary conditions, and the 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 to any response unit, as it performs scale transfer using upscaling functions. Part of the closure relation was derived by fitting (or calibrating) components of the closure relation against a synthetic data set, aiming to establish relations between variables in the scaling functions and GRU properties at the local scale. Thus, the closure relation can be applicable under extensive conditions of rainstorm characteristics, geometries and properties of GRUs. In this paper, we evaluate the same closure relation (without any modifications or calibration of the relation itself) by using the observational data set of rainfall and discharge in three small catchments. This is done for the case without calibration, and for a case with calibration. We do, however, not calibrate the relations in the closure relation itself (i.e. the generic closure relation presented in the previous paper). Instead, we calibrate the local-scale KSat value, which is an input parameter of the closure relation.

RC: the work is not on REW modeling but follows approaches that more aim to define hydrological response units (HRU) as used in SWAT modeling, for instance. [. . .] The reviewer concludes that definitions on an REW and on closure equations are not respected. As such the work in this manuscript should not (or cannot) be linked to the original work on the REW approach. The work should be placed in the context of HRU modeling.

OC: A number of reviewers criticize our manuscript for placing our work in the Representative Elementary Watershed framework. We agree that this is an important issue. In our previous paper (Vannametee, 2012), we provide details on the approach used in identifying a generic closure relation, which is evaluated against empirical data in this paper. We made the account that the closure relation had to include functional relations between the geometry (length, slope) of the support unit, local-scale parameter values, and macro-scale fluxes because macro-scale Hortonian overland flow fluxes are highly dependent on flow path length and spatial variation within units (e.g., Karssenberg, 2006; Van de Giesen et al., 2011). This relation was represented by a function with three conceptual parameters. The rationale behind the use of Geomorphological Response Units (GRU) as the control volume is that GRUs represent areas of hydrological similarity from which a set of uniform (i.e. lumped) parameters describing the averaged unit characteristics (i.e. geometry and physical properties) can be easily defined. Furthermore, our study area is relatively small (i.e. 15 km2). It is possible to derive the representative units at a scale smaller than sub-catchments by field observation.

In hindsight, our approach does not exactly fit in the original REW framework. This is mainly because, as noted by the first reviewer, we do not consider the entire set of conservation equations for momentum and/or energy, but only focus on the mass balance component. More importantly, we use point-scale parameter values to derive the macro-scale mass balance fluxes. And, our representative areas (geomorphological units) are also defined in a somewhat different manner compared to the original REW framework. So we agree with the reviewers that our work might be better placed outside the REW framework. As our study is more related to work on upscaling, it might indeed to make a connection to the concept of Hydrological Response Units. We would like to note here, as a friendly remark, that, approaches in defining closure relations proposed in our previous study (i.e. using local-scale parameters, local-scale variation , or geometry of macro-scale units) have been placed in the framework of REW modelling by Lee et al. (2005, 2007) and Zehe et al. (2006).

In the revised manuscript, we propose to make the following changes: 1) As our approach essentially defines the representative units according to geomorphological features, we will replace the term 'REW' with 'Geomorphological Response Units' (GRU)

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throughout the manuscript, as suggested by the reviewers; 2) we will rename the manuscript title to 'Hortonain runoff mass-balance closure relations using observable watershed characteristics: application to the geomorphologic response units'; 3) As a consequence, we will revise the introduction, by shortly summarizing the theoretical framework presented in the previous paper, indicating that our work is related to Hydrological Response Units and how it is different from the original REW framework. We will modify the content in the Discussion section as well to make our work fit into the new context.

RC: This reviewer has major concerns with the claims that a fitting procedure that essentially only tests for a suitable 'calibration factor' is effective. In most cases presented in the manuscript the Nash Sutcliffe coefficient is below a level (<0.6) that generally is considered to be the minimum value to accept a simulation result.

OC: We agree that the Nash Sutcliffe efficiency values are not very high. In our opinion, however, they are still acceptable mainly because:

- Our study mainly aims at comparing the proposed closure relation with a benchmark model. The proposed closure relation clearly outperforms the benchmark model regarding the Nash Sutcliffe efficiency (both without and with calibration), as well as for total discharge. Nash Sutcliffe coefficients are acceptable for a comparison between the two models.

- We present results without calibration (a typical ungauged catchment simulation) or by calibration of only one parameter (KSat) and neglecting seasonality in vegetation. By calibrating a large number of parameters and including seasonality, lower values of the Nash Sutcliffe efficiency would be found, but as mentioned above, it was not our aim to find optimal Nash Sutcliffe efficiency values. We mainly aim at comparing our model with a benchmark.

- The median Nash Sutcliffe efficiency values without calibration are indeed low (Table 5 in the original manuscript). After calibration, however, a number of events have a

Nash Sutcliffe efficiency value above 0.6, when using the model that incorporates our upscaling technique (Figure 5A, closure relation C).

- It is notably hard to perform event based hydrograph prediction for small catchments, because errors due to spatial variation in boundary conditions, model parameters or processes are hardly averaged out, as is the case for large catchments. Other studies on events for small catchments find comparable low values of the Nash Sutcliffe efficiency (e.g., Meng et al., 2008).

We agree however that the performance is relatively low. We will thus weaken our statements somewhat in the revised manuscript. In our opinion, $E > 0.6$ is probably too high in our case. We would rather propose $E > 0.4$ as a threshold to consider as good performance. A term 'satisfactory' will be used to describe the prediction with E between 0-0.4.

RC: The fact that a calibrated model works better than a non-calibrated model for a benchmark is trivial but does not add to the conclusion on the good discharge simulation results (e.g. page 1793 line 22).

OC: With the sentence at page 1793, line 22, we tried to summarize the results, and we agree with the reviewer that referring to our simulations as 'good' is exaggerated. The main message we wanted to convey is that the benchmark closure relation has no capability in reproducing the discharge, even after calibration. Our proposed closure relation is in all ways superior to the benchmark closure relation. This is because the process descriptions in our proposed closure relation are better formulated (i.e. incorporating the scaling component in the model). We will make this clearer in the revised manuscript.

RC: Actually, in the manuscript there are a lot of paragraphs and phrases that suggest good performance but such is not supported by performance values. I refer e.g. to page 1786 (lines 18-24) that actually indicates poor simulation results. Also in Table 5 unrealistic E and EQcum values are shown but possibly the description of this table is incorrect.

OC: We agree with the reviewer that we sometimes overstate the quality of the closure relation. We will change this in the revised manuscript. At page 1786 (line 18-24) we mainly want to state that the proposed closure relation outperforms the benchmark closure relation both regarding the Nash Sutcliffe efficiency value and the error in the cumulative discharge volume. This does not mean that the proposed closure relation is good in absolute terms. We will rephrase the text. The values in Table 5 are correct, and not unrealistic. The extremely low E values are due to considerable overestimation of runoff, particularly occurring when using the benchmark closure relation. Furthermore, this table shows the statistics of evaluation criteria (E and errors in discharge volume) for the closure relations without calibration. Statistics of evaluation criteria for the closure relations after calibration are shown in Table 8. The evaluation criteria are considerably improved and fall within the realistic ranges.

RC: Somewhat misleading are also the results in Figure 6 that only show single best simulation results. In the opinion of this reviewer the authors also should not claim that the calibration procedure is robust since they only present results for a single case.

OC: The reviewer may have the impression that Figure 6 in the original manuscript is somewhat misleading, but this was really not intended. As the paper has to be concise, we included only one figure with hydrographs in the paper, so by definition this can never provide a balanced representation of all the results. We decided to select one event, and to show this event both for the uncalibrated and the calibrated case, as we want to evaluate our closure relation for both situations (uncalibrated and calibrated). For the uncalibrated case, the selected event happens to have a below-average performance in terms of Nash Sutcliffe efficiency value, while for the calibrated case, it has indeed an above average performance. We could have selected another event, which might have been an event with above-average performance for the calibrated case. Please note that some uncalibrated events have a Nash Sutcliffe efficiency of 0.8 (Fig 5A in the original manuscript). In this rebuttal, we provide the results for a number of

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other hydrographs (see Fig R2.1, R2.2, R2.3, R2.4 below), and we propose to include these as appendix in the paper.

Insert Fig R2.1, Fig R2.2, Fig R2.3, Fig R2.4 here

Fig R2.1 Hydrographs (Q, m3 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 (Rt , 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 R2.2 Same as in Figure R2.1, for an event on 7 April, 2010.

Fig R2.3 Same as in Figure R2.1, for an event on 7 September, 2010. Note that observed discharge in the M catchment is not available for this event.

Fig R2.4 Same as in Figure R2.1, for an event on 8 September, 2010. Note that observed discharge in the M catchment is not available for this event.

RC: Moreover there is no verification how rescaled Ksat values relate to field observed Ksat values e.g. obtained by infiltrometer tests in the catchment. This reviewer considers this a weakness since the work primarily aims at detailed modelling of time-space dynamics of the infiltration process so to generate a Hortonian overland flow. A simple reference to (Rawls et al., 1982) is not convincing.

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 R2.1 shows that, for the proposed closure

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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, thus indicating that the benchmark closure relation gives unrealistic Ksat values. These results strengthen 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 R2.1 in 'hess-2013-27-supplement.pdf' here

RC: What is missing is a description how time integration and averaging affected simulation results. What calculation time step is set (or is time step adaption applied) and how is timing of the model affected when the time step changes (this also applies to the generated flow characteristics).

OC: The calculation time step is 5 minutes (page 1774, line 9). This time step is already quite large for an event-based simulation in small catchments. Increasing the time step of the simulation may lead to missing information at peak discharge. Thus we do not consider this issue in the original manuscript. The rainfall data was measured at a 5-minute time step. Downscaling rainfall data to a finer time step may not result in an unrealistic rainfall pattern and will almost certainly not contribute to a better simulation. We propose to leave this out.

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RC: Does the optimized calibration factor change value when the time step increases or decreases? Since only very rapid responses are simulated this requires some more attention.

OC: With a longer time step, the boundary condition of the model (throughfall) is averaged out over longer time step durations, and results will be slightly different. However we do not expect major changes, as long as the time step remains sufficiently short to represent changes in throughfall over short durations.

Few minor observations.

RC: The authors should not refer to a “standard rainfall-runoff model” that is highly subjective. Given the plethora of models the question comes up “what is a standard model”.

OC: We will remove this term and be more specific in addressing characteristics of the model used in our previous study (e.g. 2D rainfall-runoff model assuming deep ground water table)

RC: The authors introduce the “runoff coefficient” (RC) but actually do not describe how the RC is defined in this study. This is surprising since results are reasoned for by considering RC values.

OC: Runoff coefficient is defined as a fraction of total rainfall volume over the catchment area and discharge volume at the outlet. We will indicate this definition in the revised manuscript.

RC: The question if events (and thus RCs) are intercomparable at first is not answered.

OC: The events were observed at the same catchments. Runoff coefficients were calculated with respect to particular sub-catchments. Thus, inter-comparison between events among sub-catchments is possible.

References: Jonge de, M.: Physically-based hydrological modelling of a sub-basin in

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C1176/2013/hessd-10-C1176-2013-supplement.pdf

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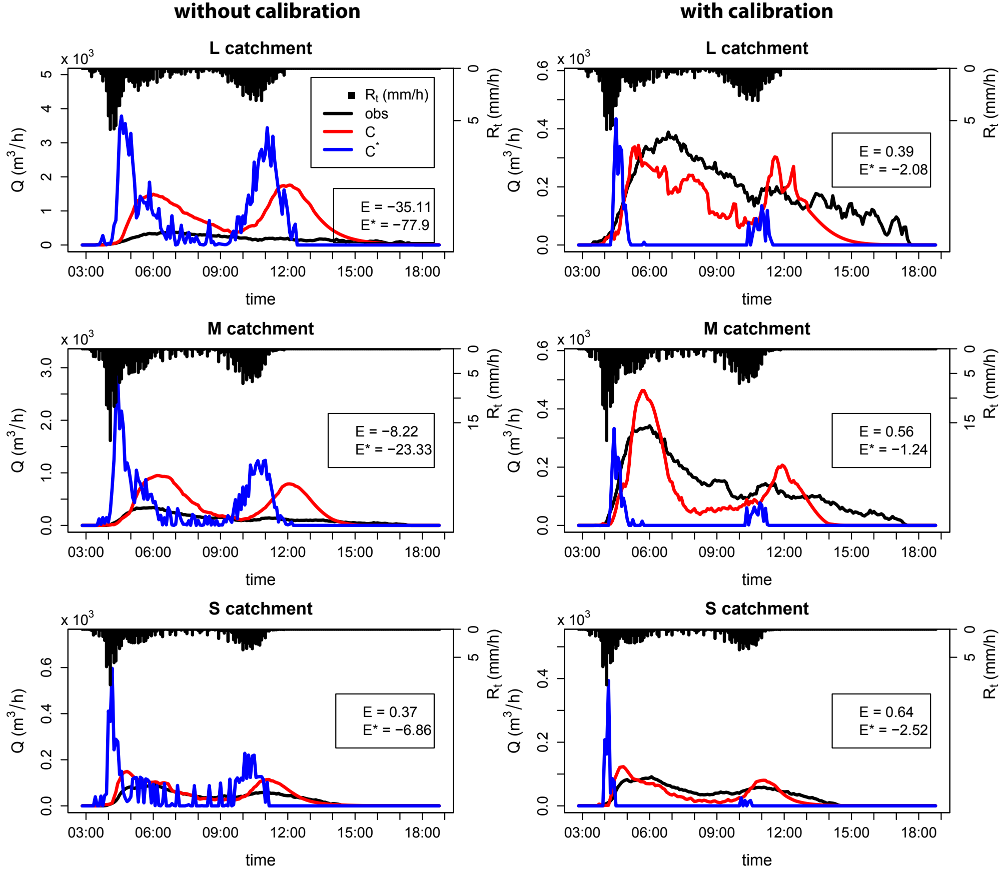

Fig. 1. Hydrographs (Q, m3 h−1) modelled using the closure relation C (red) and C*(blue), compared with the observed discharge (obs, black) , for an event on 1 April 2010.

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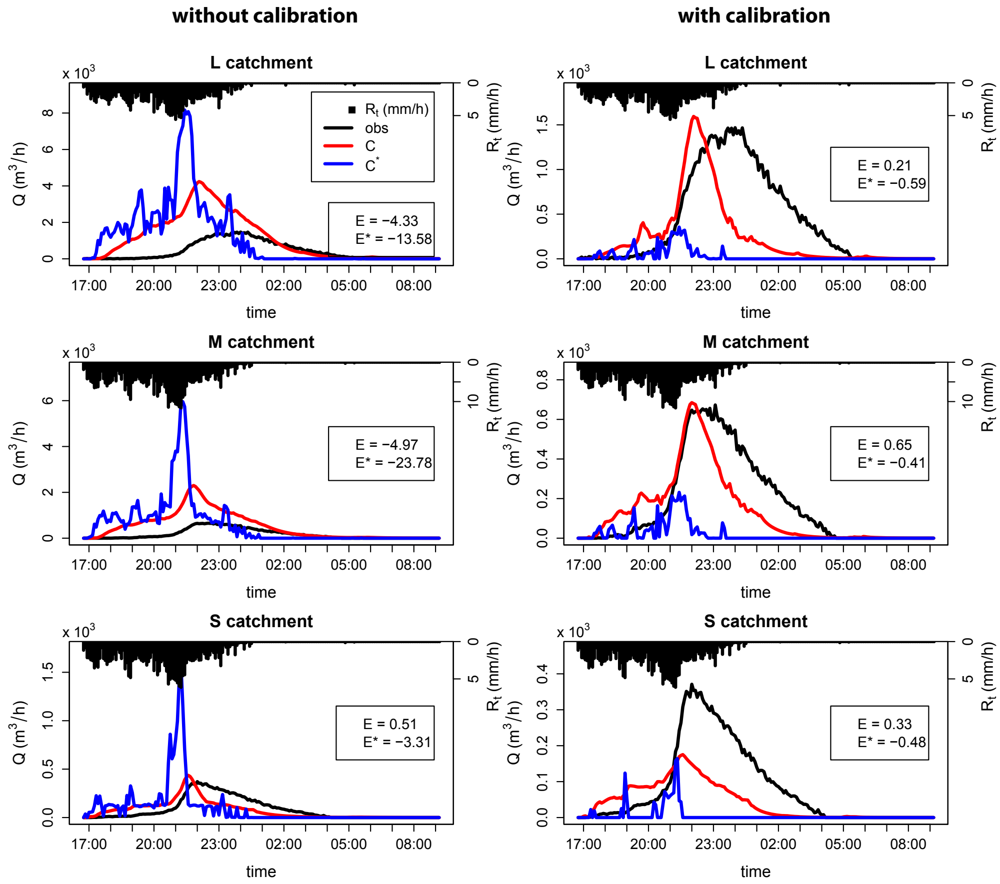

Fig. 2. Same as in Figure R2.1, for an event on 7 April, 2010.

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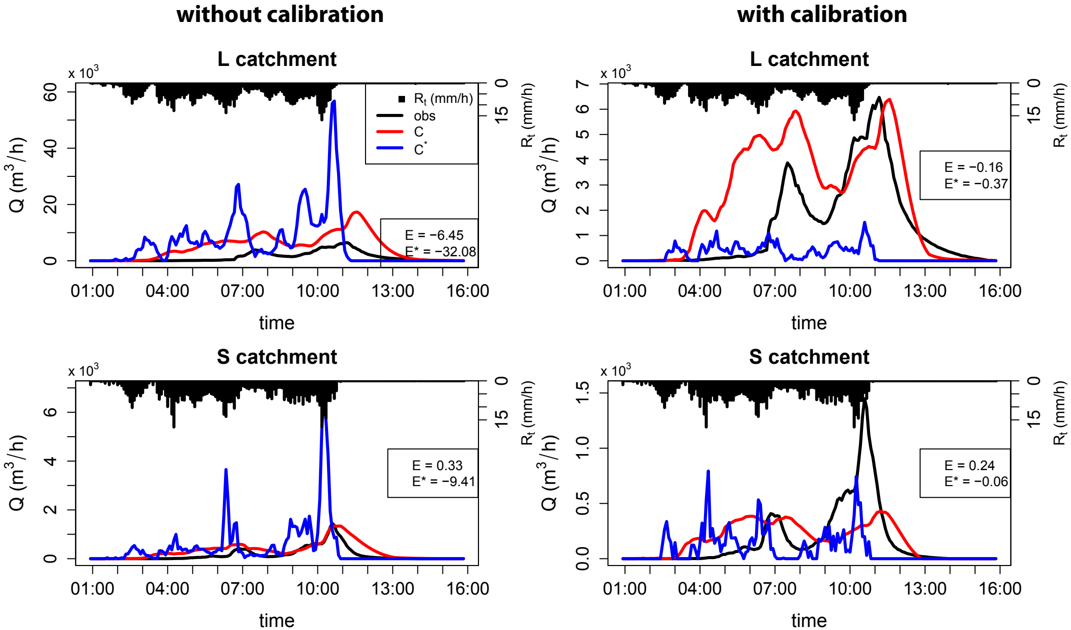

Fig. 3. Same as in Figure R2.1, for an event on 7 September, 2010. Note that observed discharge in the M catchment is not available for this event.

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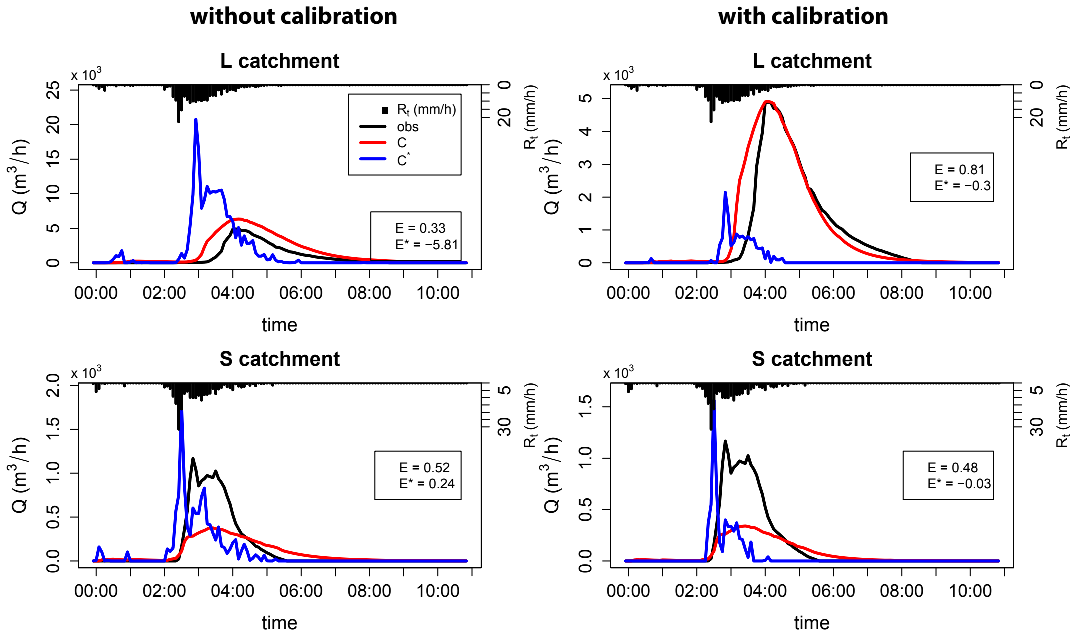

Fig. 4. Same as in Figure R2.1, for an event on 8 September, 2010. Note that observed discharge in the M catchment is not available for this event.

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