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Hydrology and
Earth System
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Discussions

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. Please also note that the uploading system in HESS limits the length of characters for the figure caption. Thus, figure caption below the figures itself may not be completed. The reviwer is kindly asked to refer to the full figure caption provided in the rebuttal text.

General comments:

RC: The paper presents a tentative validation, using in situ data, of a closure relationship proposed in a paper published earlier by the same authors in Advances in Water Resources (AWR) (2012). The authors present their work as a contribution to the Representative Elementary Watershed (REW) framework, where closure relationships are defined between zones (unsaturated, saturated, etc..). However, the presented approach does not rely on a sub-catchment discretization, but on “Geomorphologic response units”. This concept is closer to the Hydrological Response Unit (HRU) concept rather than to the REW one.

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. In the previous paper, we defined a function (i.e. closure relation) calculating macro-scale fluxes, more particularly for the infiltration and Hortonian overland flow using macro-scale boundary conditions and state variables. The macro scale was chosen to be the representative support unit of a hillslope or geomorphological feature. 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 functional relations were defined using an extensive synthetic data set of rainstorm characteristics and representative units with different geometries and physical properties, aiming to have a closure relation that is generic and to avoid identification of closure relation’s parameters in an ‘ad-hoc’ manner (i.e. calibration for specific catchments). 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) 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.

Specific comments:

RC: 1) The authors say that they derived change of scale relationships in the AWR2012

(summarized through their a, b and c parameters). The AWR2012 paper also provides relationships between those parameters and the hillslope/rainfall characteristics. A test of the relevance of the approach would require a no-calibration approach such as the one presented p.1776. The introduction of the calibration of the Ks parameters weakens the demonstration.

OC: We agree that a model requiring calibration can be considered worse compared to a model that does not need calibration. We, however, do not agree that calibration weakens our study. We would like to note that we do not calibrate the relationships between the scaling parameters and hillslope/rainfall characteristics. These are considered to be part of the proposed closure relation itself, required to upscale from the local scale parameters to retrieve the fluxes at the scale of a geomorphologic response unit (GRU). They do not need to be calibrated, as the closure relation presented in our AWR2012 paper is assumed to be generic, and not defined (or calibrated) ‘ad hoc’ for a catchment. We do, however, calibrate an input parameter of our closure relation, selecting the main input parameter, the Ksat value. This is because it is notably hard to measure local-scale Ks values in the field. Most catchment studies perform a calibration of the Ksat value for this reason. We evaluate in our manuscript two situations. One is the situation where no discharge measurements are available for calibration, relying on tabulated Ksat values (Rawl et al., 1982). The other one is when discharge measurements are available, allowing calibration of local-scale Ksat values. These two situations often occur in rainfall-runoff modelling. In our opinion, this provides a thorough evaluation of the closure relation.

RC: 2) In addition, several other parameters are hidden in the authors model, in particular those related to what is called “forcing and boundary conditions of the REWs”: the parameters of the interception model, the evaporation calculation, etc.. Also, the choice performed in the runoff routing module may impact the shape and timing of the hydrographs. To what extend the specification of the parameters of those modules impact the final results and the discharge simulation? Could the calibration of the Ks parameter compensate for deficiencies in those components of the model?

OC: We agree that the original manuscript does not properly address the sensitivity of the model to changes in other parameters. A similar comment was made by reviewer 2 and 4. So we propose to include a short subsection in the revised manuscript containing a sensitivity analysis. Results are shortly presented here. Sensitivity of model behaviour is investigated and evaluated in terms of changes in total discharge volume as a result of changes in 5 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 R3.1 and R3.2. 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 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). It can be stated that the parameters governing model forcing have impact on the discharge simulation; however, our comparison between the proposed closure relation and the benchmark will not be largely affected by the choice of the parameters used in the forcing. Neither is our calibration of Ksat significantly compensating for incorrectly chosen parameters in the forcing.

Insert Table R3.1 in ‘hess-2013-27-supplement.pdf’ here

Insert Table R3.2 in ‘hess-2013-27-supplement.pdf’ here

RC: 3) The proposed benchmark model is also quite simple: it assumes the validity of the Green and Ampt model at the scale of the whole hillslope and it neglects the travel time to the network. These hypotheses are strong and the benchmark model appears quite simple. So the fact that it leads to poor results should be expected.

OC: We agree that the benchmark model is somewhat simple, although many large-scale models (where our closure relation could be applied, too) neglect the travel time over hillslopes as well. To evaluate results for a more sophisticated benchmark, we have modified the benchmark model to account for the runoff travel time within REWs. This is done on a lumped basis by assuming that the runoff travel time from REWs is invariant. The manning’s equation is used to calculate the runoff velocity within REWs (assume n=0.04, runoff depth=2 mm). It is assumed that all REWs have equal slope length (i.e. to avoid imposing the scaling element in the benchmark model), which is the average unit length calculated over all REWs. The runoff-travel distance within REWs is set as half of the unit slope length. The results in the figure below (Fig R3_1) show that the within-REW runoff travel time is about 25 minutes. Although the hydrograph lag time is somewhat improved, the peak discharge for the uncalibrated benchmark model is still far too high compared to the observed hydrograph

Insert FigR3.1 here

Fig R3.1: Hydrographs (Q, m3 h−1) modelled using the original (red) and revised benchmark closure relation C* (blue) compared with the observed discharge (obs, black) for an event on 17 June 2010. Rainfall intensity (Rt , mmh−1) is shown on the secondary axis. E* are the Nash-Sutcliffe indexes for the benchmark closure relation. Left panels, without calibration; right panels, with calibration.

Calibration of the improved version of benchmark closure relation (Fig R3.2) yields similar results to the results included in our manuscript. The calibration factor does not change for the L and M catchment, while the optimal is found in S catchment with a somewhat smaller calibration factor. It can be concluded that the performance of benchmark model is still not significantly better even when the travel time within REWs is considered. This is because the process description used in the benchmark model is not appropriate (due to the lack of a scaling component).

Insert FigR3.2 here

Fig R3.2: Comparison between median of the Nash-Sutcliffe index (E*) calculated from events used for calibration (y-axis) as a result of different calibration factors (x-axis) for L and M catchments together (top), and S catchment (bottom) for the original (black) and revised (red) benchmark closure relation C*

RC: 4) The Ks a priori values are derived from pedo-transfer functions, which are known to be uncertain and are seldom representative of in situ conditions. In addition, the Rawls et al. relationships, used in the paper, were developed using soils from the USA. To what extend are they valid for the soils of the studied catchment?

OC: We agree with the reviewer. In the revised manuscript, we will include a comparison between Ks values observed with a rainfall simulator and values used in the model (Rawls et al., 1982). 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. The measurement error might be 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 instead of using the measured Ksat. Table R3.3 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 mostly due to measurement error). The values from Rawls et al. (1982) were indeed somewhat on the low side. For the benchmark, the calibrated values are much higher than the values 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 because the closure relations already contain a scaling component to account for the scaling effects (page 1791, line 13-17). Also, these results confirm that our proposed closure relation outperforms the benchmark, as calibration gives local scale Ksat values that fall within in the range of those measured in the field, compared to the calibrated benchmark. We will include the Ks values measured with the rainfall simulation in the 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 R3.3 in 'hess-2013-27-supplement.pdf' here

RC: 5) Some important information is missing in the paper, in particular the range of values of the a, b and c parameters; the uncertainty on the measured discharge; the choice of Ks as calibration parameter:was a sensitivity study conducted to determine the most sensitive parameters?;

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. In order to give response to the reviewer, we have investigated the model sensitivity to examine the effects of model parameters (i.e. vegetation parameters, moisture content, soil parameters) on the simulation behaviour. Details of model sensitivity are shown in the reply to the comment 2 above. We will include the sensitivity analysis section in the revised manuscript. Please note that model sensitivity is assessed based on 5 rainfall events due to time constraints in compiling the response.

For the range of scaling parameters a, b and c including uncertainty in discharge measurement, please refer to the replies to the specific comment 3 and 7 respectively (see below).

Specific comments:

RC: 1) Abstract: Avoid references in abstract or provide the full reference.

OC: We will change it.

RC: 2) p.1774 lines 4-5. The authors say that the model consists of two components, but there are actually more than 2 components.

OC: The reviewer is correct. The model consists of 4 components in total – 2 components for runoff parts; 1 component to calculate the net rain; 1 component to estimate the soil initial condition. We will change the text accordingly.

RC: 3) p.1776 lines 18-21: could the authors provide some information about the characteristics of the a, b and c distributions?

OC: Statistics of scaling parameters a, b, c are shown for an event on 17 June 2010 in Table R3_5 as an example. Parameter a exhibits a relatively large degree of skewness compared to parameter b and c. Statistics of scaling parameters slightly change after calibration, as a result of changing Ksat. In our opinion, the statistics do not provide essential information and thus we propose to leave this out from the revised manuscript.

Insert table R3.4 in'hess-2013-27-supplement.pdf' here

RC: 4) p. 1777 line 1: could the authors provide some rationale for the choice of the parameters in their benchmark model? The assumption are quite strong. Are they realistic?

OC: The scaling parameters in the benchmark model are set to create a closure relation without a scaling component. In the benchmark model, a zero value for a will result in no ponding fraction after the event; b = 1 implies no storage in the geomorphological response unit; c = 0 implies no lag response in runoff. In other words, the benchmark closure relation does not account for the spatial processes in runoff generation; and runoff is instantaneously discharged. Although this results in a simple model, similar assumptions are commonly made in the coarse-resolution grid-based models. In such models, delay in runoff generated on hillslopes is neglected or combined with delay in small streams, which is taken into account in our study (also for the benchmark) (e.g. Yu 2000). We have shown an evaluation of a somewhat more sophisticated benchmark closure relation in the reply to the general comment 3 above. Including the runoff travel time within units does not significantly change the final results.

RC 5) p.1778, line 4: what is the sensitivity of the model response to the choice of the Manning coefficient?

OC: The Manning's coefficient has direct effect on the routing component. Therefore, we evaluate the model sensitivity in terms of shifting in the time of peak discharge. A Manning's coefficient of 0.02 and 0.05 are chosen to represent the possible range of values for streams in our study catchment. Details of model sensitivity as a result of changing manning's n are shown in Table R3.6. Compared to the hydrograph of the standard run (n=0.03), a reduction of the Manning's coefficient to 0.02 results in accelerating the hydrographs about 2-9 minutes. The hydrographs are delayed up to 25 minutes when the manning's n is increased to 0.05. The delay in hydrograph also increases with the catchment size. The shape of hydrograph is the similar to the standard run. The benchmark closure relation is more sensitive to the change of Manning's coefficient compared to our closure relation. The manning's coefficient only has small effects on total discharge volume and peak discharge because this parameter does not determine the abstraction flux. We will include a short description of the sensitivity analysis in the revised manuscript.

Insert Table R3.5 in'hess-2013-27-supplement.pdf' here

RC: 6) p.1778: the author use the Thornthwaite potential evapotranspiration (PET) which only depends on air temperature. Did the authors compared this formulation with reference evapotranspiration formula of Penman-Monteith (FAO, 1998)? In addition, PET is valid for a vegetation which is supposed to be a well watered grass and crop coefficients are generally used to derive the PET of different vegetations. In particular the catchment contains forests and agricultural fields, for which this modulation is quite important. To what extend the choice of their PET and AET calculation impacts the initial conditions of their model and, consequently, the simulation of the events?

OC: We agree that PET estimated using Penman-Monteith equation (FAO, 1998) is the most accurate. However, the Penman-Monteith equation requires a large number of inputs that are not available in our study area. Oudin et al. (2005) have shown that for lumped modelling, simple PET models are capable of giving good estimates of PET, comparable to values calculated using the Penman-Monteith equation. During setup of the model, we evaluated a number of simple PET models and selected the one that gave an estimate of yearly PET close to the observed PET in the study area. This turned out to be the equation by Thornthwaite (Xu and Singh, 2001). For completeness, we present here the comparison of monthly PET calculated using four equations - Thornthwaite, Hamon, Blaney-Criddle, and Romanenko (Xu and Singh, 2001). Figure R3.3 shows that PET calculated using the Hamon model is relatively low (542 mm in 8 months), while Blaney-Criddle and Romanenko methods overestimate PET (757 mm and 877 mm in 8 months respectively). Thus, PET from the Thornthwaite equation is chosen as the best guess (602 mm in 8 months).

The effects of different evapotranspiration models of monthly PET on the soil moisture dynamic are investigated (Fig. R3.4). The results show that differences in soil moisture content between the different models is below 25%, and is considerably lower in the wet period. The sensitivity analysis in the reply to the general comment 2 shows that, with 25% change in soil moisture content, the runoff volume changes about 5% for our closure relation (Table R3.1). This change can go up to 13% for the benchmark closure relation (Table R3.2). It can be concluded that using different methods in estimating PET does not have remarkable effects in the discharge simulation because the model is not very sensitive to initial soil moisture content.

In the revised manuscript, we will shortly mention on how the Thornthwaite equation was chosen to represent PET in our study area. We will also indicate in the discussion of the sensitivity analysis (to be incorporated) that the choice of the evapotranspiration equations does not have a major effect on the comparison of our proposed closure relation and the benchmark because the sensitivity to soil moisture is relatively low.

Insert Fig R3.3 here

Fig R3.3 Monthly potential evapotranspiration (PET; mm) calculated using four different methods

Insert Fig R3.4 here

Fig R3.4 Comparison of different soil moisture dynamic as a result of different monthly PET

RC: 7) p.1782, lines 8-11. What is the accuracy of the stage discharge relationship? How many gauging were performed? To what extend the discharges are extrapolated beyond the maximum gauged value?

OC: In the S catchment, a rectangular weir was installed at the outlet. The hydrograph at this catchment was derived using the relations given for the rectangular weir. We measured discharge with salt-dilution gauging methods to determine the empirical parameters in the formula. The streambed and cross-sectional profile at the location where the water height was recorded was fixed (weir construction). Thus, the discharge - stage relation at this location is stable and valid for extrapolating discharge beyond the measured values.

We admit that stage-discharge relations for the L and M catchment are somewhat less reliable, because a weir was not used. The stream cross section at the measurement locations may have changed over time. We constructed a number of possible discharge-stage relations to investigate the uncertainty in discharge, which results in an ensemble of hydrographs for each measurement location (Fig R3.5, Fig R3.6, Fig R3.7). The final hydrograph for each catchment is calculated by averaging the hydrograph realizations that give the best-estimated discharge at the time measurements were done.

Insert Fig R3.5, Fig R3.6, Fig R3.7 here

Fig R3.5 Ensemble of hydrographs at the outlet of L catchment for an event on 17 June 2010. Hydrographs resulting from all possible Q-h relations are shown in grey scale, the final hydrograph is shown in red.

Fig R3.6 Same as Fig R3.5, but for M catchment

Fig R3.7 Same as Fig R3.5, but for S catchment

Table R3.6 gives detailed information about the Q-H measurements. The discharge – stage relations was created based on the information during the low and moderate flow period. It was, however, not possible to perform the discharge measurement during very high flow period due to the limitation of the salt-dilution gauge technique that will become less reliable with high discharge. Thus, it is unavoidable to extrapolate the discharge beyond the maximum-gauged values (about half of the events used in the manuscript). However, discharge extrapolation was usually in 1-3 order of magnitude from the maximum gauged discharge. In the revised manuscript, we will shortly summarize the information provided here.

Insert Table R3.6 in'hess-2013-27-supplement.pdf' here

RC: 8) p.1782, lines 12-15. What is the accuracy of this discharge decomposition method?

OC: The discharge decomposition method used in this study may be subjected to some uncertainties and may be somewhat less accurate than results from more sophisticated runoff separation methods that are known to produce more reliable results (e.g. tracer-based methods). We are confident however that our method gives reliable results, because we used a consistent procedure, and focussed on larger rainstorms. To obtain the best estimation of Hortonian runoff using the graphical method, we always chose the inflection point that occurs the earliest after the rainstorm has ceased. This is to ensure that the runoff component is mainly generated from the Horton process, which is the fast runoff generation mechanism (page 1782: 18-22). Furthermore, we only considered events that generated a considerable amount of discharge (i.e. runoff coefficient larger than 1.5%), which are events for which hydrograph portioning is relatively straightforward. For these events, Hortonian runoff can be estimated with a large certainty and more accurately compared to events with small runoff coefficient (i.e. less than 1.5%) (page 1786, line 4-16).

RC: 9) p.1784, section 2.4. For the model evaluation, the authors only consider the Nash-Sutcliffe efficiency, which is very sensitive to the timing of the hydrographs and possible shift in the maximum. However, as they are looking at events, it could also be interesting to use an evaluation criterion on the simulated volume (or runoff coefficient). It could be a better criteria to assess the validity of the closure relationship as the routing scheme does not consider possible re-infiltration or evaporation in the stream. As a consequence, at the event scale, the total volume at the outlet is the sum of the runoff generated by all the REWs. Some elements are provided about volume and runoff coefficient in Fig. 7, 8 and 9, but the discussion could be strengthened. What would be the results if the volume was used as a calibration criteria?

OC: Probably the reviewer overlooked this, but the original manuscript does include results for cumulative discharge (Table 5, 7, 8, Fig 7 in the original manuscript). As calibration and validation results were comparable to those found for the Nash Sutcliffe efficiency, we decided not to put a bar plot for cumulative discharge in the main paper. For completeness this is included here as Fig R3.8. The barplot shows the errors in discharge volume for both our proposed closure relation (C) and benchmark model (C*) before and after calibration using the shape of hydrograph as calibration criterion. Calibration of our proposed closure relation on the discharge volume does not yield significantly different results from those found when calibrating on the Nash Sutcliffe efficiency value (Fig 7A and 7C in the original manuscript). We would like to note that calibration on the hydrograph shape will simultaneously result in correct discharge volume, but not vice versa. Therefore, in the original manuscript, we put large emphasis on the calibration aiming at correct response shape. In the revised manuscript, we will provide more discussion on the calibration for the discharge volume as suggested by the reviewer.

For the benchmark closure relation, it is possible to find the optimal calibration factor resulting in the discharge volume correct, but not the shape of hydrograph. This is because the benchmark closure relation does not have the scaling component to simulate the delay in runoff from the REWs. It can be concluded that our proposed closure relation and the benchmark model have similar performance regarding the discharge volume.

Insert Fig R3.8 here

Fig R3.8 Errors in discharge volume in the L, M and S catchment calculated for our closure relation C (top panels) and benchmark closure relation C* (bottom panels). Left panels, without calibration; right panels, with calibration. Vertical dashed lines indicate the median of the errors in discharge volume. Note that plots on the right panel show the evaluation only with the validation events.

RC: 10) Discussion: the authors underline the poor results of the benchmark model, but as this model is quite simple, these poor results may be expected.

OC: We will indicate in the revised manuscript that this is not very surprising to have poor results. We will stress that the closure relation should contain a scaling component to avoid these poor simulation results. Please note that we have evaluated a somewhat more sophisticated closure relation, still giving rather poor results compared to the proposed closure relation (reply to the general comment 3).

RC: 11) Fig. 2. Could the authors provide some names of rivers and villages so that the localization of their catchment could be easier.

OC: The study area belongs to the Buech catchment in the admistrative department of French Haute Alps. Our study catchment is located near the village, Savournon. We will indicate the location in the text.

References:

FAO, 1998. Crop Evaporation - Guidelines for computing crop water requirements. 56,

FAO, Rome.

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

Karssenberg, D.: Upscaling of saturated conductivity for Hortonian runoff modelling, Adv. Water Resour., 29, 735–759, 2006.

Lee, H., Sivapalan, M., and Zehe, E.: Representative Elementary Watershed (REW) approch, a new blueprint for distributed hydrological modelling at the catchment scale, in Predictions in Ungauged Basins: International Perspectives on the State of the Art and Pathways Forward, edited by: Franks, S. W., Sivapalan, M., Takeuchi, K., and Tachikawa, Y., 159–188, IAHS Publication, 301, Mepple, 2005.

Lee, H., Zehe, E., and Sivapalan, M.: Predictions of rainfall-runoff response and soil moisture dynamics in a microscale catchment using the CREW model, Hydrol. Earth Syst. Sci., 11, 819–849, 2007.

Oudin, L., Hervieu, F., Michel C., Perrin, C., Andréassian. V., Anctil, F., and, Loumagne, C.: Which potential evapotranspiration input for a lumped rainfall-runoff model? Part 2 – Towards a simple and efficient potential evapotranspiration model for rainfall-runoff modelling, J. Hydrol., 303, 290-306, 2005.

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

Van de Giesen, N., Stomph, T.J., Ajayi, A.E., and Bagayoko F.: Scale effects in Hortonian surface runoff on agricultural slopes in West Africa: Field data and models, Agr. Ecosyst. Environ., 142, 2011.

Vannametee, 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.

Xu, C.Y., and Singh, V. P.: Evaluation and generalization of temperature-based methods for calculating evaporation, Hydrol. Process., 15, 305–319, 2001.

Yu, Z.: Assessing the response of subgrid hydrologic processes to atmospheric forcing with a hydrologic model system, Global Planet. Change, 25,1-17, _2000.

Zehe, E., Lee, H., and Sivapalan, M.: Dynamical process upscaling for deriving catchment scale state variables and constitutive relations for meso-scale process models, Hydrol. Earth Syst. Sci., 10, 981–996, 2006.

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C1202/2013/hessd-10-C1202-2013-supplement.pdf

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

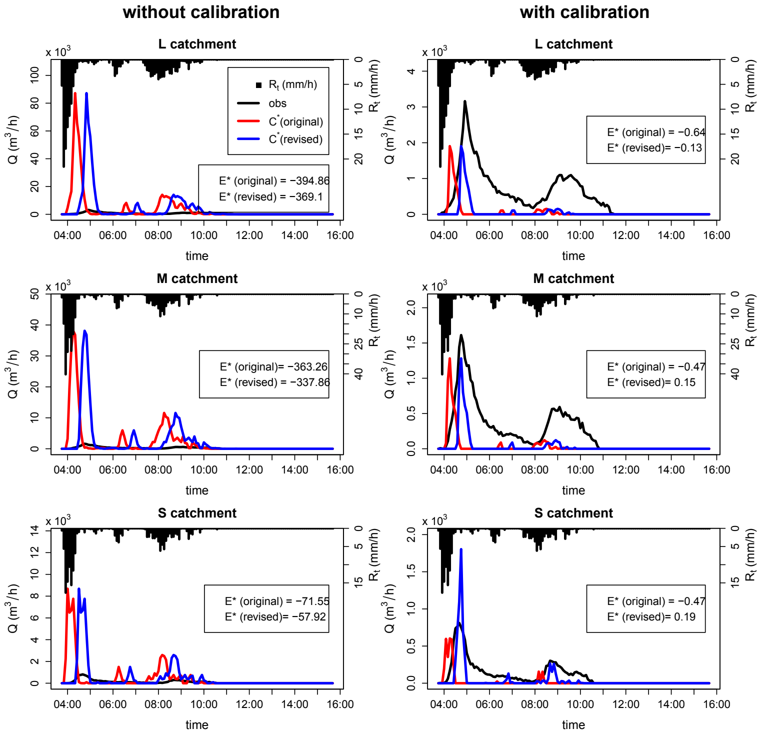


Fig. 1. Hydrographs (Q, m3 h−1) modelled using the original (red) and revised benchmark closure relation C* (blue) compared with the observed discharge (obs, black) for an event on 17 June 2010.

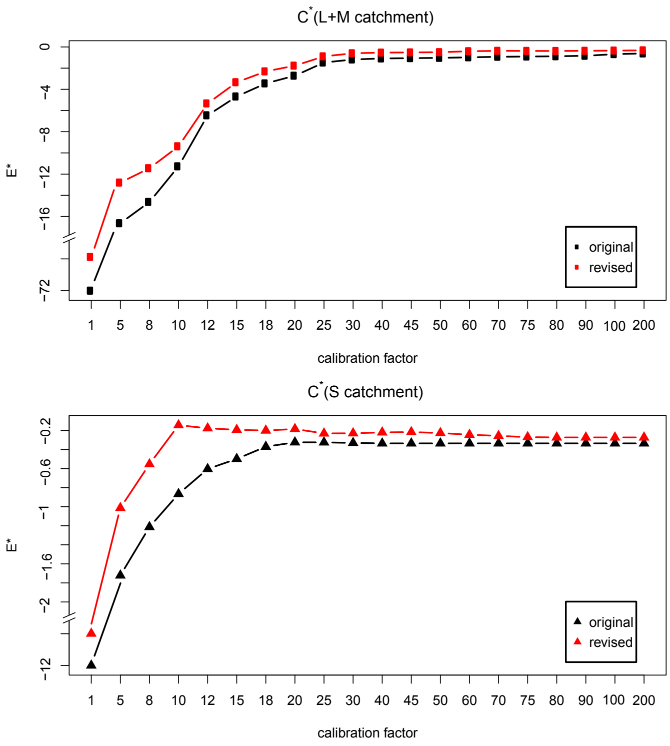
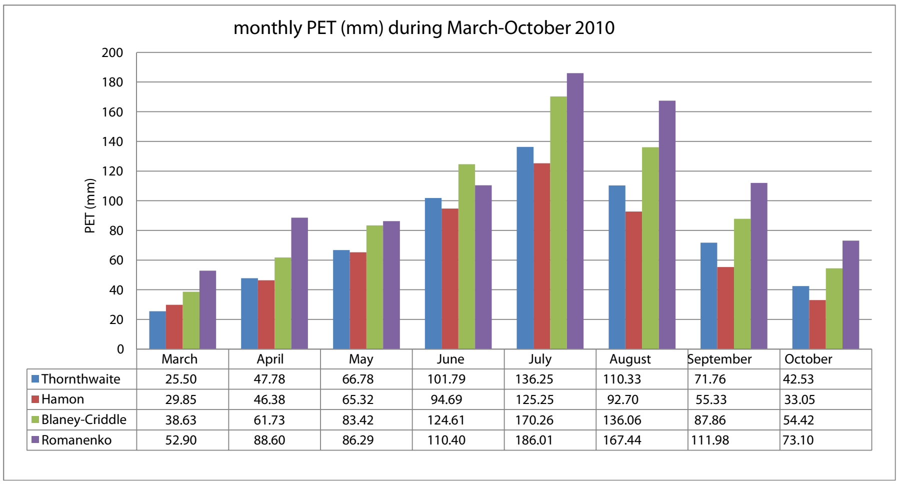


Fig. 2. Comparison between median of the Nash-Sutcliffe index (E*) calculated from events used for calibration (y-axis) as a result of different calibration factors (x-axis) for L+M and S catchment



	March	April	May	June	July	August	September	October
■ Thornthwaite	25.50	47.78	66.78	101.79	136.25	110.33	71.76	42.53
■ Hamon	29.85	46.38	65.32	94.69	125.25	92.70	55.33	33.05
■ Blaney-Criddle	38.63	61.73	83.42	124.61	170.26	136.06	87.86	54.42
■ Romanenko	52.90	88.60	86.29	110.40	186.01	167.44	111.98	73.10

Fig. 3. Monthly potential evapotranspiration (PET; mm) calculated using four different methods

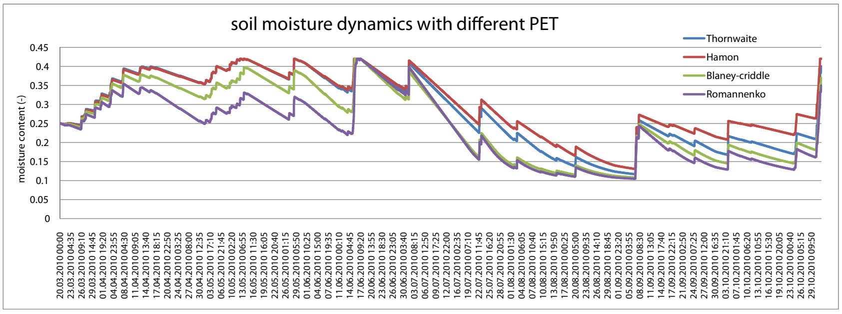


Fig. 4. Comparison of different soil moisture dynamic as a result of different monthly PET

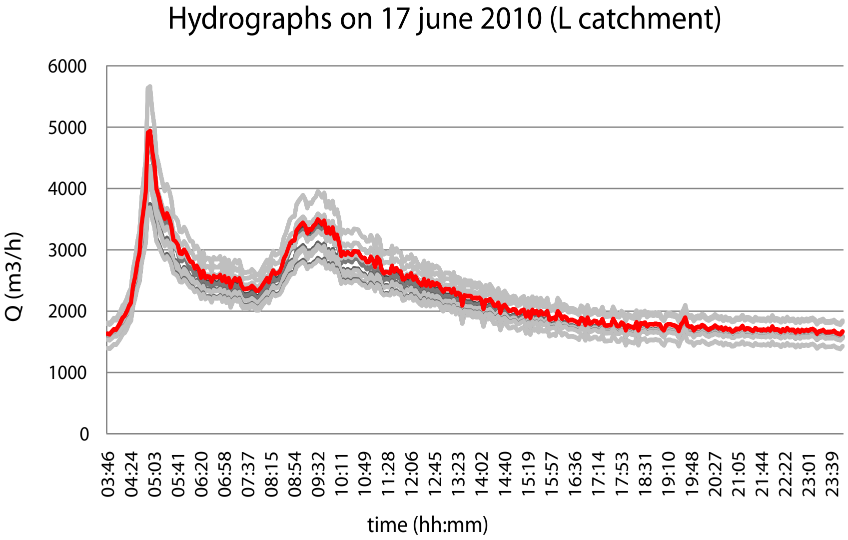


Fig. 5. Ensemble of hydrographs at the outlet of L catchment for an event on 17 June 2010. Hydrographs resulting from all possible Q-h relations are shown in grey scale, the final hydrograph is shown in red.

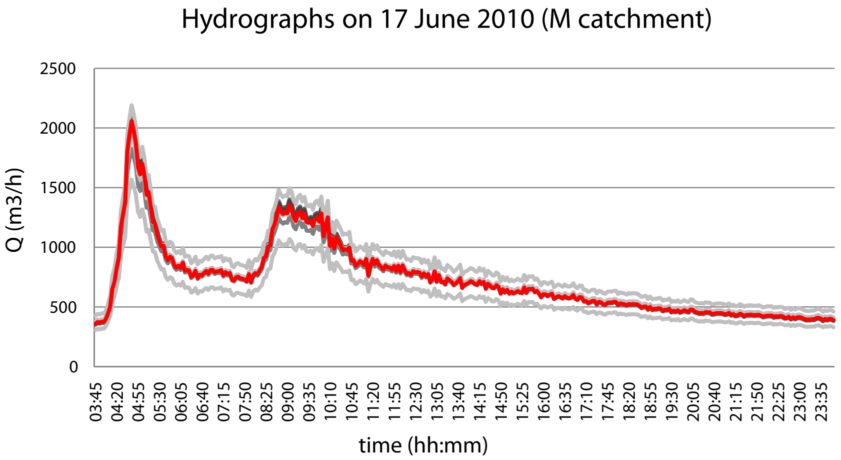


Fig. 6. Same as Fig R3.5, but for M catchment

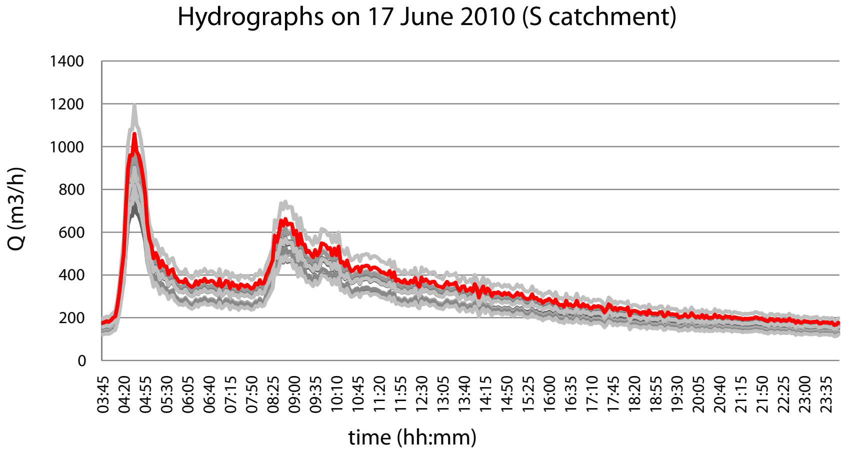


Fig. 7. Same as Fig R3.5, but for S catchment

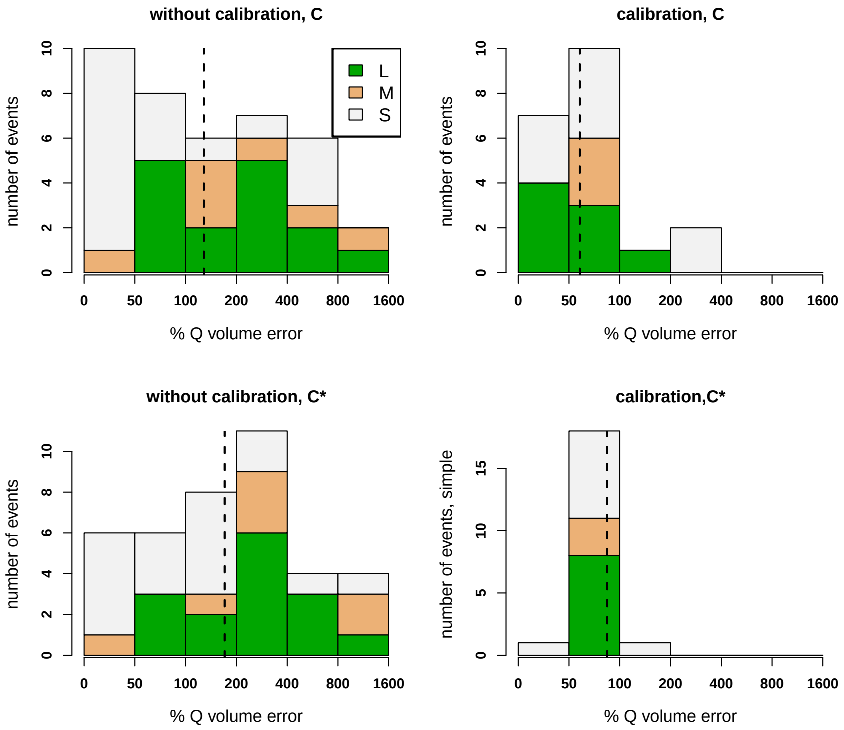


Fig. 8. Errors in discharge volume in the L, M and S catchment calculated for our closure relation C (top panels) and benchmark closure relation C* (bottom panels). Vertical dashed lines indicate the median