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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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General comment:

We would like to thank the reviewer for the constructive comments. 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

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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.

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

RC: 1) Is it an assumption that the Hortonian flow is the only runoff generation mechanism in the catchments under study? No baseflow contributes to the catchment discharge at all?

OC: Although our study focuses on the modelling of the Hortonian runoff process, we do not make an assumption that Hortonian runoff is the only runoff generation mechanism in this area. Saturated overland flow and baseflow contribute to the stream discharge as well. To take this into account, the quick flow component (i.e. Hortonian runoff) was separated from the total discharge (page 1782: section 2.2.2). Only the quick flow component is used in the evaluation of the closure relation. In the hydrograph separation, we chose the inflection point that occurs the earliest after the rainstorm has ceased to ensure that the hydrograph component obtained are generated by Hortonian runoff only (page 1782: 18-22). We will indicate this clearer in the manuscript.

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RC: 2) It is stated in the paper that “based on this relation, the scaling parameters can directly be estimated from observable REW characteristics and measureable boundary conditions without the need for calibration of conceptual parameters.” I am not sure if the properties and boundary conditions at the “REW”-scale, except for the geometries, such as the hydraulic conductivity, infiltration capacity etc. can be observable with the technology currently available, although such properties at the point-scale can be measurable. Even the “REW scale” is often arbitrary depending on the choice of discretisation of a catchment to REWs.

OC: Unfortunately, the sentence is not clear, and we will improve this in the revised manuscript. Our closure relation (described in Vannametee 2012) does, indeed, have inputs at the scale of the geomorphological unit. These are geometry of the units, and precipitation (assumed to be uniform). In our opinion, these can be measured relatively easily. However, input parameters relevant for Hortonian runoff mechanism (e.g., saturated conductivity, suction head), are local-scale values. The rationale is that these parameters can be directly measured at the local scale (e.g. using rainfall simulators). According to the approach used in identifying generic closure relation in our previous paper, local-scale parameters were used to establish the relation between the scaling parameters. Thus, the local-scale measurable properties can be directly used in our closure relation. The results in our study support this claim (page 1791, line 13-17). We would like to refer to our replies to the comments of reviewer 2 and 3, where we showed that the calibrated Ksat and local-scale measured Ksat values in our study area are quite comparable. So it seems we agree with the reviewer, but our manuscript was unclear at this place. We will improve this in the revised manuscript.

RC: 3) It is assumed that the “L” and “M” catchment are independent from each other. If looking at a larger scale, these catchments can be the REWs of the parent catchment of them. Then how could we say that there is no dependence between them? This assumption is not convincing if no data to support/justify it.

OC: This assumption is made regarding the surface water component. The catchment

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L and M are delineated according to the watershed divides. Geomorphological units situated between these two sub-catchments are divided into two independent units that drain towards different outlets. We agree with the reviewer that these two sub-catchments are not totally independent if we consider the groundwater flow, but the focus here is on overland flow.

RC: 4) It is subjective to state the model performance as good with those low values of Nash-Sutcliffe values.

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 to compare the two models.

- We present results without calibration (a typical ungauged catchment simulation) or with 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). After calibration, however, a number of events have quite high Nash Sutcliffe efficiency values, 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 stud-

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ies 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.

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