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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #2

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The manuscript presents a case study on Hortonian overland flow generation and simulation. 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. The authors suggest that closure relations are developed for the Representative Elementary Watershed approach and that the model performance, in general terms, is good.

Although the work is well written and the approach appears to add to the discussion on the development and applicability of REW closure relations this reviewer has major

concerns. First, 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. In this particular case such response unit is defined to only generate and to simulate Hortonian overland flow. After reading the early works and literature on the REW model this concept largely deviates from the basics and principles the REW approach is based on. 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.

This reviewer has major concerns with the claims that a fitting procedure that essentually 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. 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). 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. 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. 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 modeling 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. 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). Does the optimized calibration factor change value when the time step increases or decreases? Since only very rapid responces are simulated this requires some more attention.

Few minor observations.

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”.

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

Descriptions should be added on value ranges on Nash-Sutcliffe (Eq. 20) and the relative volume error (Eq. 21) so to indicate what can be considered a ‘good or fair’ performing model.

The authors should better clarify what they actually mean by Horton overland flow (generation) since descriptions mix with descriptions on quick flows (see e.g. page 1786, line 8) that may result from other processes as well.

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