Hydrol. Earth Syst. Sci. Discuss., 10, C501–C504, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C501/2013/

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 #3

Received and published: 26 March 2013

General comments: 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. In this context, the approach presented in the AWR2012 paper mainly addresses the change of scale question, from the point to the hillslope scale, for the Horton runoff generation process. Their methodology is close to the one proposed by Massuel et al. (2005), except that they provide the full hydrograph, rather than the runoff coefficient as in Massuel et al. (2005).

I have also some concern about the way the study is presented and conducted. 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. 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? 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. 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? 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?;

The paper addresses important questions in hydrology. However, the way the study is conducted and presented, some missing information (see below) weakens the message and finally, the conclusions do not appear to be supported by the results. A major revision of the paper is required before a possible publication in Hydrology and Earth Systems Sciences.

Specific comments:

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

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

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

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?

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

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?

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?

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

HESSD

10, C501–C504, 2013

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?

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.

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

References FAO, 1998. Crop Evaporation - Guidelines for computing crop water requirements. 56, FAO, Rome. Massuel, S.; Cappelaere, B.; Favreau, G.; et al., 2011. Integrated surface water-groundwater modelling in the context of increasing water reserves of a regional Sahelian aquifer, Hydrological Sciences Journal, 56(7), 1242-1264

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