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Hydrology and Earth System Sciences Discussions

Interactive comment on "Sensitivity of point scale runoff predictions to rainfall resolution" *by* A. J. Hearman and C. Hinz

Anonymous Referee #1

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This is a well structured paper with a clear objective, consequent modeling approach and detailed description of the results. The subject of investigation (effects of different rainfall intensities on surface runoff production) is not really novel, but - to my knowledge - the combination of soil properties (depth, infiltration capacity, conductivity) and storm intensities has not been presented before in such a rigorous way. The model is a rather simple and straight forward one. It is well suited for this kind of sensitivity analysis. However, I doubt that it can be applied successfully with real world data, because it reflects only vertical 1D processes and does not account for spatial variations of soil properties.

The most positive aspects of this paper are:

- Combination of soil properties (depth, infiltration capacity, conductivity) and storm intensities into this model based analysis of surface runoff generation.
- clear presentation and distinction of surface runoff generation processes and soil water storage
- analysis of a high amount of different storms (each group with similar statistical properties) and the statistical interpretation of the results
- “scaling” of the different spoil properties and storm intensities and the derivation of some sort of generic results

The weaknesses or shortcomings of this paper are:

The authors speak of “runoff generation” throughout the paper. This is not correct. They look on surface runoff generation processes only. In all climates (even in West Australia), subsurface runoff processes are also part of the total runoff. So they should substitute “runoff” by “surface runoff” at any appropriate location in the paper, in particular in the title of the paper.

The paper analyses infiltration excess and saturation excess processes by a 1d-approach. I think, for infiltration process, this is ok, even though the authors should acknowledge and briefly discus the possible effects of macropores as a catalyst for infiltration, thus possibly reducing infiltration excess by order(s) of magnitude.

However, for saturation excess, this is a very crude assumption. Saturation excess is - at least partly - a process which is triggered by the groundwater conditions. This is not a point scale and not a 1d-vertical process. In contrast, groundwater dynamics is a basin (landscape) scale process which is reflected by a rather clear organised variability in the landscape. This means, that the occurrence of saturation excess is merely controlled by the (subsurface and surface) topography, and the groundwater level.

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I accept, that the model approach used by the authors can not reflect such dimensions and the spatially organised variability. Thus, I recommend to be less general and more cautious in commenting the results related to saturated excess. One should emphasize that this process is only partly (to a minor degree) controlled by the actual soil conditions (hydraulic conductivity and vertical soil drainage rate) but more by the subsurface topography and by the groundwater conditions.

Being more cautious in the interpretation of these results might yield in different conclusions about the possible role of very shallow soils (100 mm depth of sandy soils did not generate any surface runoff at all), or of the initial soil moisture content. E.g., if talking from 100 mm soil depths, the lower boundary condition is most important. It there is an impermeable or little permeable layer, sat excess may occur even frequently for sandy soils. If the lower boundary is highly permeable (as assumed here), then it is misleading to speak of a 100 mm soil depth only. In that case the controlling system feature is the lower boundary.

In some parts, the presentation of the results is a bit lengthy, e.g. sections 3.2.2 and 3.3.1. There, I feel, is a chance to shorten the paper by about 2 pages.

To conclude, I think this is a valuable and consequently structured and presented paper which should be published after revisions as suggested above. In particular, one should refrain to the term surface runoff, and be more cautious concerning the sat. excess processes. I understand if one distinguishes between different possible causes for sat. runoff generation, the scaled and non-dimensional approach presented in this paper, including the combination of different processes, will be not feasible in that consistent way any more. But that's the tricky nature of runoff generation in real world conditions.

Some specific remarks to the text:

Page (P) 3520, line (l) 17: what do you consider being "as simple as possible" ? Remember, that good old Einstein added the words "but not simpler"

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P 3520, l28: you mention “point scale”. This is correct and this is on of the most important assumptions of the whole study. I feel it would be good to add this scale even to the paper title.

P 3522, l9: You may elaborate a bit more how you can derive the drainage coefficient from the soil data.

P 3523, l4: Wolfram, 2005 is not listed in the reference list

P 3523, l22: you may add some return periods (occurrence probability) for this kind of storm depths and intensities. Is 600 mm occurring once in a decade (in statistical terms) or once in a millennium in your region ? In my region, this never occurs.

P 3527, l19/20: sentence difficult to understand

Pages 3528 - 3534: can be shortened. You may include 1 or 2 tables where you can summarise the findings, instead of explaining them verbally.

P 3540, l17: include the Volume of the journal

References: You may include some more references about rainfall intensity impacts on Hortonian runoff from the early works from Woolhiser and his group

Table 2: add return period fro West-Australia (your region)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3517, 2006.