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Interactive comment on "Mapping dominant runoff processes: an evaluation of different approaches using similarity measures and synthetic runoff simulations" *by* M. Antonetti et al.

Anonymous Referee #1

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General comments

The paper addresses a relevant scientific question and is well presented. Within the paper, the results of different approaches for the automatic identification of dominant runoff processes are compared. My general comments refer to three subjects (suggestion for minor revisions):

1) In a first step similarity measures are used to compare the reference map, which had been generated by intensive field work (Scherrer & Naef 2003 – SN03-map), and the automatically derived maps. The used mapcurve approach was slightly changed as the SN03-maps were always used as reference maps although Hargrove et al. (2006)

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recommend to change compared and reference map and to use the higher MC scores. In the discussion section you mention "reliability problems of mapcurves". Would the results change (and possibly show less reliability problems), if you would have followed the recommendations of Hargrove et al. (2006)?

2) The usage of PREVAH via differing model parameterizations representing different DRP-maps is an interesting strategy. However, I am not sure if the synthetic runoff simulations really reveal what they are expected to. You argue, that strong model assumptions had to be made, e.g. the assumption of completely saturated catchments, and that thus a calibration against measured runoff would be meaningless. As the parameter sets were chosen within realistic bandwidths, you expect the model results to be meaningful. However, to my opinion a validation of the model results with the aid of measured runoff values (e.g. for events with high antecedent soil moisture contents) would still be important in order to ensure that the chosen model parametrizations work well. I would thus recommend adding a model validation procedure. If no measured runoff values are available, other validation strategies should be used (at least such simple approaches like envelope curves, comparison runoff coefficients. . .). At the moment, the PREVAH model results just show differences between different parametrizations, the proof is not yielded yet, that the model works right for the right reason.

3) Not surprisingly, among all three automatically derived maps, the map SF07 shows the smallest differences to the reference map SN03 as the approach of Schmocker-Fackel et al. (2007) shows – among the three automatic approaches - the strongest resemblances to the approach of Scherrer & Naef (2003). The identification of the DRP after Scherrer & Naef (2003) strongly depends on detailed field investigation of soil profiles. The approach of Schmocker-Fackel et al. (2007) also strongly relies on – naturally less detailed – soil information (soil map of Zurich 1:5.000 with information on soil-water regime as described in the method's section), which is scarcely available in the same quality outside of the canton Zurich. Missing information may be calculated

by a method of Margreth et al. (2010, literature source not easily accessible). Thus applications of the approach of Schmocker-Fackel et al. (2007) outside the canton Zurich might show different results. I would recommend referring to this data restriction in the discussion section.

Specific comments

p. 13259, line 21 f.: To my understanding, the topographic wetness index allows to identify areas prone to saturation overland flow (although a lot of publications show problems of the accuracy of this method). Areas with low topographic wetness index values must not necessarily be areas prone to Hortonian Overland Flow.

p. 13264, line 2: Can you please explain why the reference maps do not take the rainfall characteristics into account although the Scherrer and Naef-method contains different decision trees depending on rainfall characteristics (Scherrer 2006)?

p. 13264, line 11: Why did you use 1.2 m? How sensitive are the results to the choice of this threshold value? For the approach of Gharari et al. (2011) a sensitivity study was carried out for the choice of HAND thresholds. Would this also be useful for the threshold used with the Müller et al. (2009)-method?

p. 13266, line 8: Is the "corresponding degree of similarity" the same as the "expected similarity E"? In this case the usage of the same wording would support understanding.

p. 13267, line 21: I would omit the word "fully" and just name it a "distributed model". Comparing the size of the catchments and the PREVAH-Gridsize of 500 m as well as regarding the way of implementation of the Runoff Type-information, I would not regard the model to be fully distributed.

Figure 12: Which model parametrization (4.1?) led to the simulated runoff of SN03, SF07 and MU09?

p. 13274, last sentence of the discussion: This suggestion is without doubt appropriate. But is it really suggested by your findings (with regard to the general remarks 2)?

Table A3: Different writing of theta leads to confusion. Please explain the subscripted numbers (presumably they are referring to the runoff types).

Technical corrections

Fig. 1 and 2: if possible, using the same scale would be helpful

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Interactive Comment