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Interactive comment

Interactive comment on "The role and value of distributed precipitation data for hydrological models" *by* Ralf Loritz et al.

Anonymous Referee #2

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This was the first time I was involved as a reviewer for this manuscript. The manuscript introduces an adaptive spatial clustering of hydrologic response units (HRU) to cope with the dynamics of the intermittent rainfall by keeping the model as parameter parsimonious (=model states) as possible in terms of reduction of similar-reacting HRUs. The manuscript is well-written and I enjoyed reading it. The introduced clustering is innovative from and fits into the scope of the journal. I have a few moderate and a number of minor comments, which are stated below. My overall recommendation would be a moderate revision to give the authors enough time to solve the open issues. Since I can only choose between minor and major revision, major revision it is.

Moderate comments:

The manuscript is about the reduction of the spatial model resolution based on the

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variety of precipitation as input signal. I'm wondering if there is not an adaption of the temporal resolution required as well since scales in space and time are not independent of each other (see Melsen et al. (2015) and references therein)? Maybe it's not an issue for the small catchment studied here, but for larger catchments with a small hydrologic variability the numeric stability can be questioned due to the large spatial discretization and the high temporal resolution (e.g. in terms of the Courant-Friedrichs-Lewy condition, Courant et al., 1928). The authors should proof this condition for their model setup and discuss possible issues in the manuscript. An alternative would be to reduce the temporal resolution as well, which would lead to an additional reduction of parameters/computational costs.

The authors have selected two events to show the ability of adaptive clustering. The choice of both events seems to be very arbitrary. From Fig. 4 it seems that the resulting runoff peaks are not representative for runoff mechanisms of the catchment. As far as I understand from P13l8-10 the clustering is carried out manually and not automatically so far, which is the reason why the authors decided for two small events covering only a few time steps. However, I disagree with the hypothesis that “a test on a longer time scale . . . would provide only little more scientific inside” (P13l9-10), which is also not proven by the authors. I rather expect that the reduction of model parameters due to the adaptive spatial resolution is reduced significantly for long-lasting rainfall events causing a direct runoff response over several days as e.g. in Nov 2014, Jan 2014-Mar 2014 and Aug 2014. Another point that can be questioned is snow, which does not cause runoff immediately, but when snow melt begins. How will this be affected/can be incorporated by the adaptive clustering? The impact of more complex events than those analysed in the current study has at least to be discussed sufficiently in the manuscript, although an analysis of more events is encouraged to represent the effect of the adaptive clustering on the variety of runoff responses.

P15l20 The model states are identified by the slope of the resulting runoff curve. However, the slope can be more or less identical for one time step independent of the current runoff situation, e.g. if runoff is reduced in one tile from 25mm to 20mm and in another tile from 10mm to 5mm (which could be the case in a stratiform event with a convective cell inside), the resulting slope is the same, right? So the soil moisture and other storage elements is then "averaged" due to the same model state of both tiles, although both tiles are in completely different hydrologic situations. It would be useful if the authors would comment on that issue or, if I understood it not correctly, clarify the part where I got lost.

Specific comments:

P4l5-8 The difference is not clear formulated at this point. It becomes clearer while reading the manuscript, but should be communicated concisely at this point.

P7l27 Where are the disdrometers located? Can they be used to improve the rainfall input for the reference model to achieve a more realistic uniform areal rainfall? If not, could be an increase of rainfall amounts with altitude improve the areal rainfall estimate? The Roodt station is situated in the raster field with the lowest rainfall amounts (Fig 2) and not representable for the catchment. So any correction has to be done to enable a fair comparison between reference model and model a.

Fig2 Please add rain gauge data to Fig2b) to enable a comparison of all rainfall inputs

P10l2, p11l26 area-weighted -> As I understand the areal mean is estimated by the arithmetic mean of the satellite data. How do weights for different areas affect this estimation? This is not clear for me, please rephrase/add the explanation.

P11l2 sap flow -> Do the authors mean by sap flow the flow in plants? I can't imagine at this point how the authors applied observations like that in the current study. If so, please describe a bit more detailed, since it is not a conservative measure for model validation and hence of great interest for the community.

P12l23 "average distance of each grid cell to the outlet" -> Should it not be the distance along the flow path/flow direction? So it would be possible that the runoff is assumed to stream upwards in some areas of the catchment- Please rephrase or reconsider.

P12l30 “3.2.1 to 3.2.3” -> “3.3.1 to 3.3.3”

P13l2 “wetness state” Please define this term. It sounds as only soil moisture is included without any additional information, but there is more included, right? If not, why not using the term soil moisture? Section 3.3 and 3.3.1 There are repetitions among the paragraphs, please remove them.

P15l4&21 Both thresholds are catchment size-dependent (as the authors state also later). For other applications it would be useful to introduce a catchment size-dependency to derive these thresholds. This is beyond the scope of the study since it demands a multi-catchment analysis, but the authors should add a small sensitivity analysis by e.g. using $\Delta P > \{0.5, 1, 2\}$ mm/hr as thresholds. This is along with a comment I have for the results section later, but I want to state it already here. In the results discussion it is often mentioned, that the number of parameters is reduced between model b and c, there is no figure illustrating it, although I would imagine it would be an impressive plot with y as KGE over x as the summarized number of model parameters per time step (or on average) for one event. Model reference, a, b, c(ΔP>1mm), c(ΔP>0.5mm) and c(ΔP>2mm) would be the points to show in the diagram. I assume model c would represent a break in the curve (KGE not increasing, while number of model parameters do) and the different thresholds would represent the uncertainty of this approach.

Table 1: As fa as I understood the calibration was done only for the reference model, right? Although that seems to be done in a former publication, a brief information about calibration and validation period, objective function and so on is required to interpret the table. For model a, b and c no parameters were changed, so the same parameter set was used throughout the study to enable comparisons? If there was a re-calibration for model c, the reference model and models a and b should be re-calibrated for the events only as well to enable a fair comparison

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Fig. 5: I'm a bit confused here. The authors state P=12 for t=2, but from counting it is P=13 – please double-check (also the number of entries in the following text referring to t=2). Additionally, for t=4 M=3 results from P=2 and S=1 – from my understanding the maximum of model states is max(M)=2 in this case, please double-check.

P27l4-22 This paragraph provides already a good overview of related references. However, from my understanding the reference of Nicotina et al. (2008) concluded that spatial patterns of rainfall are only important for large catchments (8000km^2 in their study) for hourly time steps, the correct estimation of areal rainfall is sufficient for smaller catchments. The authors should review this reference again and check their implementation in the current manuscript. Also, Ogden and Julien (1993) state that only for rainfall with durations shorter than the concentration time of a catchment the spatial distribution of the rainfall matters, for longer rainfall events only the temporal distribution matters. To highlight the importance of distributed models the authors could also look at Krajewski et al. (1991), Bardossy & Das (2008) or Müller-Thomy et al. (2018).

Technical corrections:

P7l23 aggregated -> transformed

P7l27 merges -> is a merged product of

P10l29 afterward -> afterwards

P12l12 Thee is a 3.2.1, but no 3.2.2.

P15l15 ">1 mm/hr" -> "ΔP >1 mm/hr" Please add a variable name here. However, "P" is already used in the manuscript for precipitation bins. P is a general abbreviation for precipitation, please consider e.g. "PB" for precipitation bins.

P15l30 (P>0) -> (P>1)

Citation syntax errors: p4l13, p8l2

References: Fenicia et al. (2011a) is identical to Fenicia et al. (2011b), please double-check.

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