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Interactive comment on "On the value of high density rain gauge observations for small Alpine headwater catchment hydrology" *by* Anthony Michelon et al.

Anonymous Referee #1

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This was the first time I was involved as a reviewer for this manuscript. The aim is to identify the best rain gauge network setup for runoff predictions. The topic is suitable for the journal and of interest for the community; the manuscript is well-written. However, I do not recommend a publication at its current stage. There are a few major comments listed below, which have to be addressed before the manuscript can be recommended for publication. More specific comments and some technical corrections follow afterwards. My overall recommendation of the manuscript is major revision.

Major comments:

1. The title states "value of high density rain gauge observations for . . . hydrology". I'm

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struggling with this holistic formulation. Indeed, the value is “only” (please don’t get me wrong here) based on prediction of RC and deltaP/Q. While a realistic estimate of these characteristics is valuable, the uncertainties resulting from the final network with 3 rain gauges for these two criteria is not shown and should be added in a later version of the manuscript. In general, I’m missing the runoff peak as important characteristic in the manuscript. Maybe the authors can involve it/comment on it why it was not considered. Also, although the analysis is designed mainly for discharge estimation, results should be also interpreted in terms of rainfall (e.g. resulting areal rainfall (extremes) for different rain gauge network densities, spatial rainfall characteristics,. . .).

2. Based on the comment before, the impact of the rain gauge network densities (and rain gauge locations) on the runoff is not analysed. In the additionally uploaded comment the main author states a rainfall-runoff modelling would go beyond the scope of the study. I do not agree with that and recommend this modelling approach to analyse the impact on the resulting runoff itself instead on single runoff statistics. To attribute the spatial rainfall variability, a distributed rainfall-runoff model would be the best solution.

3. Also, I was wondering why is there not a consistent number of events analysed throughout the manuscript. I understand that there are always measuring issues and maybe some observations are questionable, but then please remove them at the beginning. There could be one number of rainfall events considered and one subset of them for discharge analysis, but at the current state results from different subsections cannot be compared with each other due to the different populations of considered events.

Specific comments:

L25-27 It should be mentioned here again that this issue is related to mountainous areas and is not a problem in general.

Fig. 2 I don’t see the additional worth of showing Fig. 2 and recommend to leave it out,

especially since it is included in the supplement as Fig S2 as well.

L90 “average elevation” Please change to mean or median, depending on how you determined the “average” value.

L117-118 The construction of the rating curve is not interesting for the manuscript and can be left out, also the elements regarding its construction in the supplement.

L154-155 The term interpolation is not suitable in my opinion due to the rainfall generation mechanisms behind. I suggest “areal rainfall is generated after Bernoit et al. (2018a) by constraining actual observations at rain gauge locations”. The authors should give a less brief explanation, since in the cited manuscript different versions are applied for rainfall generation (three versions due to different covariance models) and it remains unclear for the reader, which model is used for the current study. Why did the authors choose this rainfall generation instead of a regionalization approach as kriging (maybe with altitude as additional information), inverse distance weighting or Thiessen polygons. The latter is chosen later in the manuscript nevertheless due to computational efforts, so why not for the whole study? Was it the authors intention to add an uncertainty analysis

L154-163 The authors should bring this argument in context with the catchment concentration time.

L165-166 The location of the line chosen for the splitting of the catchment seems to be chosen arbitrary. Would a line constructed perpendicular to the main flow direction of the river (or even better, not a straight line but following the lines perpendicular to the isohypses to separate flows exactly) lead to more representable results, since the catchment is then split into a real upper and lower part? Or (thinking the other way around) does it not matter at all and the splitting line could be also drawn from South to North as long as both parts have the same area?

L211-215 I suggest to move this paragraph to the beginning of section 3.3.2

L217-218 The authors declare volume and lag time as “the two key characteristics of streamflow reaction”. I do not agree with that. The most important characteristic is peak flow, followed by volume and then lag time and flatting behaviour. Even if all characteristics are considered equal important, the authors should state why peak is not considered in the study. If there were attempts to include peaks which did not work, the authors should state so as “lessons learned” in the manuscript.

L219-221 Is this criterion developed by the authors or should a reference be cited in this context? How was 1/3 chosen as threshold? This value should be catchment-dependent in my opinion, or not? Please clarify.

L222 Why is this criterion “1/3 of the rainfall amount” more robust than “start of the rainfall event”, although both starting points are linear correlated?

L275 Same differences lead to higher asymmetry values for smaller values. To avoid a misinterpretation (“Interestingly . . .”) Pnorth and Psouth could be normalized by the mean event rainfall amount. This would provide deeper insights, especially since larger differences between both parts cannot be seen in the current approach if they occur for events with high rainfall amounts.

L323-327. I cannot follow the argumentation here. Please explain in detail how you achieve this conclusion and consider at least one or two sentences for each argument.

L330 “to reach a higher RC. . .” Please rephrase, the manuscript is about observations, not modelling.

L341 composites: If there is a differentiation into wet and dry state, how do the authors achieve only one value for each criterion? Are two values estimated (for wet and dry) and then the arithmetic mean is mentioned? Please clarify!

L351-355 It would be nice to have a table with all criteria , where it is stated which one was removed (and why) and which ones were kept. Maybe the information can be added to Table 5 or 6?!

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L354 Again, it feels as the number of considered events changes among all subsections.

L380 What is the reason for IASYM preference in the Southern part? Due to the steeper areas? I would have estimated Northern part, since the hydrograph would have already been smoothed when originated in the South. Please try to find physical explanations to your results.

Technical corrections:

General: Please double-check the abbreviation for “meter above sea level”; I have only seen “m a.s.l.” and “m asl” so far, but not “m asl.”

L155 Benoit et al. 2018 <- a or b? I assume a.

Eq 2, 3, 4 I’m a bit confused what rainfall characteristic is used as input for these equations. Is every raster cell with rainfall used (so I understood it from the text) or only the centre of the rainfall events (as mentioned in Table 1)?

L63 “overlooked” -> ignored

Eq. 2, 3, 4 The term in the numerator should be put in brackets (Eq. 2: “P(..)dHills” -> “(P(..)dHills)”)

L195 DHAND is not a distance as indicated by the D, and in the text the variable is introduced with HAND. I suggest to stick to HAND throughout the manuscript to avoid confusions with the other two “real” distances”.

L202 Section 3.5 includes no network extent description. Is it missing in the manuscript?

L268 317.8 mm – Is it areal rainfall amount sum or sum over all stations?

L268-269 please provide also the mean values, not only the highest and lowest values, so that the reader get a “feeling” for the rainfall events.

L275 again, please don't use the term average, use mean or median to be more concise. Since Iasym can be positive and negative, the median of its absolute values would be worth to show instead of just the mean, since positive and negative values are levelling out each other.

Fig. 5 and 6 For a logical order the figures should show the rainfall events first, followed by the discharge plot.

L279 "One strongly asymmetric and high intensity event" -> "One strong asymmetric and very intense event"

L283 A volume can't be fast (check also for later occurrences. . .)

L288 In the sentence before authors mention that the number of events under consideration are reduced by "1", but here again 48 events are studied (also in the following subsections).

L289 The authors should state what wet and dry networks are. I found it later in the caption of Table 1 in S1, but it would lead to clarifications here. Also, the Table 1 in S1 should be shown in the manuscript, since the written part in Section 4.1.2 is more confusing than explaining for me.

Fig9 "events without reaction are not shown" belongs to part b), not a). Please correct the caption. General: Maybe I missed it, but which temporal resolution was used to calculate the correlation (and other criteria)? 2min as this is the resolution of the rain gauge? Or are values aggregated up to e.g. 1h? This has a high impact on the values of the correlation coefficient.

L339 "absence of correlation". Correlation cannot be absent. Better to speak of low correlation or provide absolute values.

L384-386 "is assessed", "is evaluated" – two verbs, please rephrase the sentence.

L402 "what we previously thought"? What was the hypothesis of the authors before?

L421 “three station network” It would be nice to provide the resulting density here as well as “(general) recommendation”.

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