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Interactive comment

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

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We would like to thank the reviewer for the overall positive assessment of our manuscript and the insightful propositions of improvement. We provide hereafter a detailed response on how we plan to revise our manuscript for each comment. The original comments are in italic, our response in normal font. For cross-referencing, we numbered the comments.

Overall comments

1) The paper aims at highlighting the values of high density rain gauges networks for hydrological purposes in small catchment of mountainous areas. The topic is interest-

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ing and relevant for the community. It furthermore has other potential applications in urban areas which are also small and quickly reactive catchments where rainfall variability has strong consequences. Although quite short (and it should be stressed more clearly that it is a limitation of the study), the data set is relevant.

We will stress this limitation of the study in the revised version.

2) The paper is well presented and easy to read (except for Fig. 8 and corresponding comment).

Thanks for the positive assessment, we will improve Fig. 8 (please see below the answer to the point No. 13).

3) However, I think that the indicators used to characterize the rainfall variability are too simplistic (basically an asymmetry indicator splitting the catchment in two) to enable robust conclusion. The indicators of hydrological behavior also seem quite simplistic. And this is confirmed by the low scores and quality of regressions that are found. I believe that indicators enabling to grasp more precisely rainfall variability and its consequences should be used. I guess that this would enable to highlight more precisely the importance of dense networks of rainfall measurement devices.

The presented indicators can indeed seem simplistic, but we would like to underline that this choice was motivated by the observed spatial rainfall structures and the catchment shape. This led to indicators that evaluate the spatial heterogeneity of precipitation from two perspectives: (a) independently from precipitation location (asymmetry index), and (b) in relation with rainfall location relatively to the hydrological network (D_{HILLS} and D_{STREAM}). To improve the set of precipitation indicators we propose to add i) a new indicator of rainfall variability that characterizes the spatial heterogeneity of precipitations (see detailed comment No. 7 hereafter), and ii) to use “width functions” in our analysis as suggested by the referee No. 2.

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Detailed comments:

4) l.15 (abstract): “the identification of key hydro-meteorological metrics that explain the runoff coefficient and lag times (e.g. total event rainfall, center of mass of the precipitation field)”: depending on the application there could be other indicators as well.

We will reformulate the corresponding sentence to make clear that these are the indicators that we used.

5) Introduction: I believe it would be worth mentioning urban applications. Indeed, there have recently been numerous papers highlighting the need for high resolution rainfall data for these small catchments.

Thanks for pointing this omission out. We did not mention urban applications because we made the assumption that the need for high resolution data in urban applications is much more evident than for rural catchments. We will mention urban applications explicitly in the revised version.

6) l. 155-157: I do not see where is the “steel sponge” on Fig. 3. Could you please highlight it? It might be interesting to test the sensitivity of the results to this issue.

The steel sponge is indeed not visible on the pictures. We will add/pick a picture showing the sponge in place in the revised version. We propose first to verify the effect of the sponge experimentally (using the same experimental setup as the Pluvimate calibration detailed in Appendix A) to evaluate i) the delay caused by the sponge on low rainfall intensities and ii) the amount and timing of the delayed rainfall after the actual end of the rainfall event. If the results show a significant effect, in a second step we will evaluate the significance of theses artefacts on metrics used in this study.

7) Eq. 1: it seems to be a very simplistic indicator of the rainfall variability. Many other have been developed to characterize much better the rainfall variability.

We agree that an indicator for intra-event rainfall variability is missing. We propose to use this indicator presented in Smith et al. (2004):

$$\sigma_t = \sqrt{\frac{\sum_{i=1}^{N} P_i^2}{N} - \frac{\left(\sum_{i=1}^{N} P_i\right)^2}{N^2}} \quad (1)$$

$$I_\sigma = \frac{\sum \sigma_t P_t}{\sum P_t} \quad (2)$$

with P_i the rainfall amount at the station i at the time step t and N the number of stations.

8) Eq. 2: given the fractal nature of river networks, how the river network was determined? i.e. at which resolution was the upstream network not taken into account?

The accuracy of the river network extent presented here is quite exhaustive: streams in the Vallon de Nant appear punctually at well-defined springs; accordingly, we identified the stream channel heads based on these springs (identified in the field). Exceptions are on the east side of the catchment, which is hardly accessible, where the stream channel heads are identified from topographic maps. These details about the river network identification will be added to the revised version.

9) l. 230-231: please clarify of the fast runoff is computed.

For reasons detailed in Section 3.3 of the original manuscript, streamflow events are identified manually from the hydrograph. Thus, an event starts at t_{START} with a discharge Q_{START} and ends at t_{END} with a discharge Q_{END}. A straight line is then drawn between these two coordinates. The integration of the area above this straight line (the area between the straight line and the discharge curve) corresponds to what we consider as the amount of fast runoff during the event. The description of runoff computation will be clarified in the revised version.

10) l 237: I guess it should be a reference to Table 2.

Thanks, it will be corrected.

11) Section 3.5: I am not sure that AIC is needed, if the "corrected" version is also used.

We agree, we will use only the corrected version AICc.

12) Section 4.4: basically, the absence of good models seems to suggest that the indicator used are too simplistic and do not enable to grasp the hydrological behavior.

In our view, the absence of a good model indicates that there is no simple linear relationship between the observed variables and the runoff response. At the same time, the limited number of observed events prevents the use of a more complex model, which is a classical problem in comparable hydrological studies. The revised version will use an additional rainfall indicator. We will furthermore extend the statistical analysis (use multiple regression) and discuss in more detail the results.

13) Fig. 8: I found it difficult to understand what is done. Could you please clarify?

The aim of the sensitivity test presented in Fig. 8 is to evaluate the robustness of best weather stations network results presented in Fig. 7. We performed the same estimation, but removed 1, 2 or 3 out of the 23 events from the data set. Fig. 8 summarizes, for each possible dataset (testing all possible combinations of event), how often a given observational network was chosen as the best network. The more often a given station is retained as being part of the optimal network, the larger the symbol in the figure. In addition, the figure represents how often two stations are part of the same optimal network. The more often two stations are retained together, the wider the line between them. As Fig. 8 shows, for networks of 2 stations, the 2 same stations are the best choice most of the time (with the same holds for a 3-stations network).

REFERENCES:

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