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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #2

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I was a priori very interested by this work and I found the introduction of the article well focussed and documented. I was a bit sceptic however about the first objective of characterizing "the effect of spatial location of rainfall fields on the timing and amplitude of the hydrological response", based on data analysis only (no modelling) and for such a small watershed. I began to be disillusioned:

in page 6, with (i) the lack of analysis of the spatial variability of rainfall and, e.g. with the implementation of the Thiessen's method for 2-min rain resolution data; (ii) the fact that Figs. 4 and 5 are hardly readable; (iii) the too many references to the supplementary material, starting on line 181 (the general reader will not follow you there ; the article has to be synthetic and "self-contained");

in page 7, with the choice of the spatial rainfall asymmetry index. The shape of the

watershed matters, so why not consider differences in distance and amplitude between the catchment and the rainfall "width functions", as proposed by several authors in the literature; the topography could be included as well in some way, a metrics to be invented, which would be relevant especially in such a high-mountain context;

in page 8, with consideration of the initial wetness conditions as "hydrological response metrics" (while this variable is more on the forcing side), the absence of standard indices on lag times between the hyetogram and the hydrogram (e.g. response time, time of concentration, etc), the way you have determined the runoff volume. Among other points, it is indeed difficult to get an idea of the response time of the watershed, which could drive a basic discussion about time and spatial sampling issues, (e.g. Berne et al., J. Hydrol., 2004, 299, 166-179);

in page 8 with the description of the statistical analysis (pure quadratric regression) while Fig. 6 is based on simple linear regression and the regression attempts presented in Table 2 could have been done with standard multiple regression. Note that, rather than p-values and AIC criteria listed in Table 2, the number of points considered in each regression would be sufficient for the reader to assess the robustness of the inferences. (But more importantly, I doubt that any statistical technique of forcing and hydrological response variables will be able to replace a hydrological model . . .)

In addition, Fig. 6a could have closed rapidly the debate on the spatial variability of rainfall at the scale of this watershed. Heterogeneous events, with significant rainfall, occur once in a while and may impact the flood dynamics; but you do not give any evidence (and in my view there is no way to get it without a model) of this impact in the article.

The cases with runoff coefficients greater than 1 are interesting, especially the July 24th case. Indeed, the rainfall sampling in the steepest part of the watershed is probably deficient and it will be hard to obtain it with raingauges. Is there any hope to integrate some information from the Swiss radar network to compensate for this lack of data in this area, and eventually over the entire watershed?

Therefore, although I recognize that there is a huge field work done, I think the analyses and presentation of this study require some additional effort before the article can be accepted for publication.

With respect to the state of the art presented in the introduction, I may recommend the authors to read (and eventually to refer to) two articles by I. Emmanuel et al. in J. Hydrol. 2015 (531, 337-348) and 2017 (555, 314-322) (which I did not co-authored, I swear!).

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