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Interactive comment on "Simulation of extreme rainfall and streamflow events in small Mediterranean watersheds with a one-way coupled atmospheric-hydrologic modelling system" *by* Corrado Camera et al.

Anonymous Referee #1

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The paper of Camera et al. presents a complete hydrometeorological reanalysis of two high impact events in Cyprus island (Eastern Mediterranean) addressing the challenge of effective reconstruction of such kind of events for small to very small catchments (ranging from 5 to less than 100 km2 in this study). Overall, the paper presents a detailed and complete exercise, which adds another piece to the puzzle, benefiting from the availability of increasingly advanced modelling systems at all scales of analysis. Furthermore, the analysis is performed over an extraordinarily important area for Cyprus water resources, using a considerable set of discharge data and also dealing

(even though partially) with the challenging issue of hydrological modelling in a mountain environment with rock fractures.

I suggest three main improvements to the paper, listed below, and have some other minor comments. I hope my comments are helpful to further enhance the quality of the paper.

- My first main comment concerns the GCM data source (i.e., ERA-Interim). I acknowledge that this study inherits the work done by Zittis et al. (2017), but this global reanalysis is now replaced by the ERA5 reanalysis. This point is important, also given the fact that ERA5 offers ensemble members, which could be very usefully used exactly for the problem analysed (i.e., hydrometeorological chains targeted to small and very small catchments). I ask the authors to deal with this point, of course not requiring new simulations with ERA5, but discussing it.

- Furthermore, I have some concerns about the calibration and use of the bucket model. In general, my idea is that the baseflow bucket model could not be so important for such short-time events. Indeed, the case studies analysed are rather impulsive. Furthermore, I think that the effects of the bucket model are somehow misinterpreted (please refer to a specific comment below). I suggest the authors revise and comment on their choice of calibrating in detail the baseflow bucket model.

- Finally, I believe the authors can go more into details analysing the catchments with rock fractures, which show too low performances that should be increased somehow (please refer to specific comments below).

Minor/specific comments

Abstract: stating that "few studies evaluate the hydrologic performance etc. . . ." is a bit debatable concept (e.g., few with respect to what?). This statement is different from a similar one on L81, where the authors specify that they are referring to WRF-Hydro. I would start the manuscript with a stronger sentence. Furthermore, in the Abstract the fact that 1989 events are used for calibration and 1994 events for validation should be stated more clearly.

L46 (and throughout the text): I would write “As summarized by Rummler et al. (2019)” rather than “As summarized by (Rummler et al., 2019)”.

L85: it looks like the events are much shorter. Including the spin-up period in this time interval could be misleading.

Fig. 1: I suggest the authors focus more on the WRF-Hydro domain, which could be represented with a larger scale (so that also other information, e.g., location of rain gauge stations and reservoirs, can be added). Location of the WRF-Hydro domain in Cyprus island could be shown with another small map in the figure.

Table 1: A clear geological description is ok, but I would also highlight some essential geographical/morphological features, such as area, channel length, etc. Maybe authors can move some piece of information from Table 4 or just repeat it.

L121: the problem of getting a reliable rating curve is rather common. More details about the “appropriate” rating curves used would be useful.

Eq. 6: the variable Z should be explicitly defined

L218: information about average soil moisture would make more sense if information about soil type was provided

L228: 1500 cells should be 1500 x 100 x 100 = 15M m2, that is 15 km2 (it should be better stated explicitly). However, in Table 4 there are some catchments with area lower than this threshold.

L267: at a time

Fig. 4 and elsewhere: to compare the performances of the model system for the two events, probably percent bias and MAE are more appropriate indices

LL320-333: [this comment refers to the main comment about dealing with rock fractures] from this paragraph, it’s not clear if the problem is mainly related to the snow model in the LSM or the not good representation of the geological features. I would favour the second hypothesis, and I think that some test should be performed (and shown) by the authors increasing drainage.

LL335-339 and Figs. 5-6: the Y scale for watershed Mk is not appropriate (much higher maximum value than needed). The comment about watershed ST does not correspond to what I can see in the Figures.

L343: [this comment refers to the main comment about the groundwater bucket model] For Ak, it’s not a problem of baseflow, but of recession, which is typically a problem concerning especially interflow (i.e., quicker contribution than baseflow).

L344: the peak looks not so well simulated in Ak

L68: passing -> moving?

LL372-374: these sentences are confusing, especially if compared with LL351-353, which seem to refer to the same comparison. Not clear what the authors mean when they state that bias “on average increased by 8.6 times”

L395: the three watersheds

L400: decent -> reasonable? Besides, again I don’t think it’s a matter of baseflow

L412: probably, increasing overland roughness coefficient could be also a way for improving interflow and, therefore, the simulation of the falling limb of the hydrograph

LL442-443: please contextualize better this sentence

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