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Hydrology and Earth System Sciences Discussions

Interactive comment on "The benefit of high-resolution operational weather forecasts for flash flood warning" *by* J. Younis et al.

J. Younis et al.

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- Introduction: please, check for improving the bibliographical review with comments on other approaches, as for instance, those in the references below. Also, for instance, the authors say on page 348, line 9: "One accepted method is...", and oppose their approach to the one presented in Georgakakos (2006) (lines 26-28). However, what are the other "accepted"; methods existing in the literature and how their approaches compare to the one proposed by the authors?

–> Ok, references relevant for early flash-flood warning added, but not those on now-casting, in order to keep the focus of the paper.

- p.348, ls.14-19: the authors introduce the topic of physical processes at the origin of

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runoff production during floods. It would be interesting if the reader could have more insights into this aspect with a focus on flash floods, the topic of the paper: what are the predominant physical processes in the case of flash floods? Have any catchment experiments or numerical modelling experiments been done on the subject? References to international published papers would be appreciated here.

–> In the previous paragraph we have listed several international journal references on flashfloods and flash-flood generation. The emphasis of this paper is not to explain the processes of flash-floods in detail but on early warning. Therefore the following reference is given for background information: Smith R.E. and D. C. Goodrich: Rainfall Excess Overland Flow, Encyclopaedia of Hydrological Sciences, John Wiley& Sons, Ltd., 1707-1718)

- p.350, l.8 and figure 2: since the authors are comparing rain gauge data with radar data, saying that one is confirmed by the other [this has been rephrased now] and as it is said later on page 352 that calibration based on rainfall data was not used, it would be interesting to have few words on how precipitation amounts from radar data were obtained. Besides, there is a mismatch between the text (says: the 24h rainfall accumulations) and the figure (caption says: 48h rainfall accumulations).

–> The text seems to be clear to the authors and for more detail on the radar data and their treatment we refer the reader to the indicated published literature on this topic. Regarding the accumulation times, there is no mismatch: the text refers to an intensity of 600 mm/24hrs whereas the figure shows 48 hours accumulated rainfalls.

- p.350, l.10 to 20: the whole paragraph is very confusing and needs to be rewritten. The links between the maximum specific discharges presented in Fig. 3 for a family of catchments and the hydrographs presented in Fig. 4 for a specific catchment is not fully examined. For instance, considering that the area is a very heterogeneous one, how far can the catchment represented in Fig.4 be considered representative of the average behaviour of the catchments within in the area? When referring to "estimated peaks" in

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Fig.4, are the authors referring to observed or simulated discharges? It is difficult to see the differences between observed and simulated hydrographs in the figure and maybe it would be more suitable to make reference to the regression-type graph (Q simulated x Q obs in Fig.5). Besides, the caption of Fig. 4 does not correspond to the symbols in the legend. Also, the last sentence lacks precision: what do the authors mean by saying that "this figure" (Fig. 4?) points out the "characteristic size of the watershed affected by the flood for which" rainfall has to be "correctly forecasted"? What is the characteristic size? In Fig. 4 there is only question of a catchment of 262 km2 (and, if the authors are referring to Fig. 3, the catchment areas there correspond to a wide range going from 10 to 2000 km2).

–> The authors apologise for a confusion in figure numbers. The whole paragraph refers to figure 3 and not figure 4 as indicated, which should solve all the issues mentioned above to which the authors fully agree.

p.351, l.28: what are these "default values" of parameters and how were they computed?

–> Ok, this has been made clear in the text

- p. 353, line 8: the French national database is rather known as "Banque HYDRO" and not "Banque d’Eau".

–> Ok, thank you for this correction

- p. 354: In the French database, the 2-yr flood is usually presented for each gauge station in a summary of the main statistical properties of the data collected. Did the authors check if the "Qcritical obs" for the high level corresponded to the 1-2yr flood as it is stated on line 5 that it "frequently" does)? How did they equal or differ? Also, how do the "Qcritical obs" and the "Qcritical sim" compare?

–> Table 1 has been modified in order to include all these information as well as the 2,5 and 10-year return period for all the stations.

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- p. 355, 4.1: the section is not clear. It is called "hydrological regime and calculation of thresholds", but, as results, the calculated thresholds are not shown. It would be interesting to have a table with observed and simulated thresholds, together with some statistical quantiles (2-yr flood, for instance) for each catchment. As for the "hydrological regime", the authors say that the "hydrological regime of the river basins in the Cevennes-Vivarais region is proposed in Fig. 4", which, however just illustrate one river basin. It should be explicitly said if this catchment is representative of others and if the time period in question corresponds to the river regime (if it is stationary). Maybe it should be presented here a graph of mean monthly discharges (over time and space), which is what really describe the "hydrological regime" of a region. Otherwise, what is presented is the discharge evolution for the years 1994 to 1998 for a given catchment.

–> Ok, done

In Fig. 5, one can rather see a particular over-estimation of simulated discharges in the case of the Vidourle catchment, especially for small values of Q. I suggest the authors to rewrite the comments to the graph.

–> Done

Page 356, line 1: what do the authors mean by "the results worsen as the severity class increases"? The number plotted in figure 6 should be a relative frequency (number of occurrences/total number) if one wants to compare the categories among themselves.

–> Ok, reformuled

Page 356, line 8: was this time shift evaluated to check if there is a systematic bias in the timing of the simulations?

–> Yes, this was evaluated and there was no systematic bias

- p. 357, results 4.2: it would be interesting to have the results for the Ardeche catchment (not shown). I suggest to merge figure 2 and 7 in one, and add a figure on more results on the effective early warning over the studied catchments for the flood event

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analysed.

–> Ok, 1 figure for one representative station of all catchments.

- p. 359, line 26: precise here that this is specifically the case for severe, i.e., rare, flood events. - p. 359, line 28: do the authors mean "although the forecasted rainfalls are often too wide spread..."? The sentence is not clear and should be rewritten.

–> Reformulated

- p. 360: in the "summary and conclusions" section, it would be interesting to have some sentences on the uncertainties associated to flash flood forecasting and the limits of the deterministic forecasting approach presented by the authors.

–> Ok, done

Technical corrections: P. 347, line 6: "in French", should appear in the list of references only, and not in the text, especially because this is not the only reference in French cited in the paper.

–> Ok

Figure 1: check for "stream gauges" in the legend.

–> ok

Figure 6: the legend should mention the link between "category" 1 to 4 and the four thresholds.

–> done

Figure 8: the plot is too small and difficult to read.

–> Ok

Sandrine Anquetin, Jutta Thielen-del-Pozzo, Jalal Younis April, 25, 2008

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