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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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Figure 6: The authors could show how vary the forecast performances (in term of Hits, False alarm and Missed alarms) for different values of model efficiency (such as Nash-Sutcliffe efficiency). This may be done for the basins reported in figure 6. There may exist a lower limit concerning the "uncalibration" of the rainfall-runoff model, for which the proposed methodology is not applicable.

–> With the NASH coefficient the simulated discharges are directly compared against the observed discharges. For reasons outlined in the paper, and illustrated by the scatterplots, this comparison will not work in this case because a) there is a big discrepancy between observed rainfall and rainfall input, b) the model has not been cal-

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ibrated to test the performance of the approach in ungauged catchments. Therefore the authors would not like to use efficiencies such as the Nash for the evaluation of the results. Instead, other measures are given, for example the correlation coefficient given in Figure 5 is derived from data of all stations in the catchment. It is obvious that the performance of hits is worst in the Vidourle where also the correlation coefficient is low. In order to make a proper assessment like suggested by the reviewer more stations would be needed than were available for this feasibility study. It is certainly an issue that the authors would like to investigate in a follow up study, and in this case also related to the performance of the approach with regard to the temporal-spatial scales involved.

The authors include here (see attached file) – but not in the paper – a diagram showing the distribution of nash over the catchment scale derived from the uncalibrated model results. It is clearly shown that there is not a clear cut-off value in this case. The two negative NASH values are all situated in the Vidourle.

351:28-29. I think that improvements in forecasting performances could be obtained by the individuation of a methodology to transfer model parameters from gauged to ungauged basins. It is not clear how the parameters were set across the different basins.

–> For this study the same parameters – the default parameters - have been used throughout. This has now been made clearer in the text.

354:26-27. I think that the use of a daily discharge threshold may lead to a high number of false alarms particularly in the case of basins with area lower then 50-100km2 which are the most prone to flash flood. I think that further studies should be planned in order to apply the same methodology to another case study for which hourly discharge thresholds can be extracted.

–> The authors fully agree. Unfortunately for this study these data are not available. This is planned in a follow on study in a different catchment

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359:26. It would be better to extend the 6 months forecasting assessment to at least one year.

–> We agree that the time period should be longer, but unfortunately this is beyond the scope of the project also regarding data availability. The work for this paper was meant as a feasibility study. Longer-term and more detailed studies are planned.

360:23-26. I think that also the number of false alarms should be limited. Even if they can be identify they will be "false alarms" for the vast number of people warned. The credibility of a warning system may be lost due to excessive false alarms.

–> The authors have made clearer in the text that the results from such a system would be used by local flood forecasters only as early warning information in addition to their existing ones and not by non-experts or the public.

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 345, 2008.