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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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p. 347, lines 12. There is still no certainty about the impact of climate changes on rainfall regimes. I would suggest to be more cautious with this sentence, or to provide references about that.

–> References added. Also, the sentence referred to expected land-use changes in the future and not only to climate change.

p. 351, lines 5-10. The description of the LISFLOOD model is not well written

–> The focus of the paper is not on the LISFLOOD model, since the approach could probably be used with similar models as well. The description aims to provide the main aspects of the model and more detail can be found in the indicated literature. The wording of the text has been reviewed and slightly modified.

p. 351, line 23: Could you justify the use of a 1 km2 grid?

–> The 1km grid was chosen because the JRC holds all necessary maps to setup the LISFLOOD model on a 5km and a 1km resolution for the whole of Europe. A 5 km grid is considered too coarse to simulate flash-floods. A 1km grid may still be coarse for flash-floods, however, compared to the input data resolution, a much finer grid would probably not bring added value. Also, the authors have set-up several other regions and applied the methodology to other case studies in Italy and Slovenia.

p. 351, lines 26-29. The model has been set up at the regional scale, without any calibration. Available discharge data were only used for verification, which is a very instructive task. However did the authors try to calibrate the model and compare their threshold warning system using a calibrated and a non calibrated model?

–> Yes, the authors have calibrated the model based on the available data, which were scarce in particular for the sub-basins. Also with calibrated parameters the model tended to underestimate the observed discharges and the differences in threshold exceedances were only small. The reason for this is in fact the very coarse meteorological input data rather than the model performance. Again, the authors want to focus here on an approach for ungauged river basins and therefore do not want to include this analysis in the paper.

p. 353, line 11 “can be assumed to be small” instead of “can be assumed to be little”

–> ok

p. 354, lines 7-17. The methodology consisting in defining exceedance thresholds both from data and model results is interesting and avoid to use absolute values of simulated stream flow for warning. It is argued that the method allows to compensate for systematic over- or under-estimation of stream flows. However, do the authors use

HESSD

5, S277–S282, 2008

Interactive Comment

some criteria to determine if the simulated discharge distribution is close enough to the observed one for their method to be applicable? It would be interesting to evaluate what would be the performance of the proposed method for instance with a random rainfall field, in order to get a bottom line for the method performance.

–> We thank the reviewer for coming back to the point of testing the efficiency of the methodology. This is in fact, the purpose of this paper, namely to see if the proposed methodology of threshold exceedence produces positive results in terms of hits and false alarms. We will make this clearer throughout the text.

The authors, however, do not see the added value in testing the method with random rainfall fields. Particular in the case of steep terrain the response of the model is largely dependent on the rainfall distribution and random rainfalls will result in random output.

p. 355,lines 1-4. The argumentation provided here is not very clear

–> The authors agree that the sentence is confusing and have deleted it as it has been said before.

p. 355, lines 14-19. In order to fully understand what is the model performance, more details about thresholds should be given in the results section. I would suggest the authors to provide, for each catchment, the values of the observed and simulated thresholds and to show the observed and simulated discharge distributions. It would provide the reader with a better view of model performance.

–> The performance of the “model” in this case depends to a very large degree on the quality of the input data. For the input data from the comparatively coarse synoptic network, some indication of model performance is given in figure 5 through the scatterplots and the correlation coefficient. Figure 6 indicates the performance of the threshold exceedance within the river network (with surface areas from 190 to 476 km2). In fact, figure 8 gives an example in the Gard of the relation of thresholds derived from observed and simulated data series as well as an indication of the performance of

HESSD

5, S277–S282, 2008

Interactive Comment

the model with high resolution rainfall data as input. Showing these figures for all stations would result in too many figures and provide more information,The figure captures were not very explicit and this is now better described.

p. 356, lines 11-14. The authors compare their threshold approach with a traditional simulation of discharges and the warning using specified discharge thresholds. The authors mention that their simulations tend to underestimate high discharge values. Could they compare their approach with the more traditional one to better show the value of their method?

–> Please see the answers given to Referee 3 on the same subject.

p. 356-357, section 4.2. On the September 2002, higher rainfall resolution (hourly data) is available. The authors compare the discharge simulations with the observations. They show that, even with a better rainfall inputs, the model still underestimates the discharge. On the other hand, the threshold method shows that higher warning thresholds are exceeded and that a warning with a lead time of about 24h could have been emitted. They show the power of the methods, despite the deficiency in the simulation of stream flows. In a second step, the authors, show that, even with a coarser rainfall (high resolution weather forecast over a 7 km grid) warning would have been emitted and the severity of the storm could have been anticipated. Some indications on the nature of the forecast is missing (for non specialists of meteorology): does the forecast provides the accumulated rainfall over the next 12 hours period (in this case how is the cumulated rainfall disaggregated?) or an hourly simulation of the next 12 hours, which can be used as input of the model?

–> The resolution of the DWD data is described clearly in section 3.2. It is hourly data provided every 12 hours for a period of 48 hours lead-time. For clarity, the authors have added in section 4.2 again that the temporal resolution of DWD data is hourly.

p. 358-359, section 4.3. Finally the authors presents an evaluation of their threshold method using a 6-month period of weather forecast. Could they give more precision on the way the forecast is used? Does the forecast provides hourly rainfall fields for the next 12 hours? Is the model reinitialised when a new forecast is provided?

–> The temporal resolution of the DWD forecasts, its lead-time and the frequency of issuing forecasts is clearly described in section 3.2. In the text we have clarified how the model has been initialised for each forecast.

p. 359, line 9. As before could the authors provide the values of the observed thresholds?

–> Observed thresholds have been added in table 1 and also listed together with the simulated thresholds in figures of the representative stations.

p. 359 lines 17-18. The sentence is not clear

–> Ok, reformulated

p. 359 lines 23-25. The authors argue that false warning are less important than missed warning. We can agree on that, however, if there is too much false warning, it is probable that people will no more trust the system and that they could not take care of warnings when a real event occurs.

–> We have made clearer in the text that the statement refers to flood forecasting experts and not the public, in which case the discussion should be different.

p. 359, section 4.3. The authors could also insist on the fact that a good weather prediction of rainfall is a prerequisite for any warning system. They show that the forecast was giving high rainfall northern than what was really observed and that it might be critical for the efficiency of warning: if warning is emitted on the wrong catchment, the system would collapse.

–> Ok, added at the end of the section: The study also highlights again that the usefulness of any flashflood warning system depends very much on spatially and quantitatively good precipitation forecasts.

HESSD

5, S277–S282, 2008

Interactive Comment

p. 360 lines 24-26

–> Modify the sentence (see comment above)

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