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Hydrology and Earth System Sciences Discussions

Interactive comment on "Flash flood modeling with the MARINE hydrological distributed model" *by* V. Estupina-Borrell et al.

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The main objective of the paper is "to develop an operational and robust methodology for flash flood forecasting" (p.3398). The MARINE "hydrological distributed model" is considered by the authors as the basis for the presented methodology. The MARINE model describes processes of infiltration, surface and channel flow. Applicability of the model for "flash flood forecasting" has been demonstrated in the paper by the example of the hydrograph calculation for the only flash flood, which was observed at the 250-km2 Orbieu River basin in November 1999. The catchment properties have been partially assigned in the model by the use of a LANDSAT TM image. Three rain gauges together with the rain radar images have been used to obtain the rainfall inputs. Most of

the model parameters have been assigned a priori, from both measurement data and literature. Two parameters have been adjusted through a calibration procedure against the observed hydrograph. Several hydrographs have been modeled under both different initial conditions for infiltration and different methods of the infiltration rate calculation. The modeled hydrographs have been appeared to be in a rather close agreement with the observed hydrographs that allows the authors to conclude that the MARINE model “is able to reproduce the hydraulic behavior of the watershed” (p. 3420) and the model can be adapted to the flash flood forecasting.

General Comments 1. Leaving aside the problem of spatially explicit quantitative precipitation forecast, it is reasonably safe to suggest that an opportunity of using hydrological model for flash flood forecasting depends on its ability to reproduce spatial patterns of flood generation over a watershed. Due to the nature of flash floods and the fact that a single storm can generate several such disasters, hydrologic models with spatially distributed, physically based structure and parameters are necessary to predict the development of the spatial distribution of flooding. However, despite the fact that the MARINE model is presented by the authors as the physically based distributed model (e.g. page 3420), I could not find corroboration of this fact in the paper. On the contrary, as resulted from the Section 4.4, soil parameters in the infiltration module are assigned as constant values over the watershed. As far as I understand, the only distributed parameter is Manning roughness coefficient for channels but this parameter does not control the spatial distribution of flood generation. Also, the MARINE model (at least in its presented version) does not take into account the spatial distribution of the antecedent soil moisture content which has a pronounced effect on the initial runoff losses. Rainfall intensities obtained from 3 measurement gauges and “some cumulated rain radar images” are distributed over the area by the Thiessen method. The authors write (p.3418) that some tests were carried out (but not presented in the paper) and showed that space-time patterns of the extreme storm over the 250 km2 region are reasonably represented by the rainfall data used. This statement is not evident for me and I suggest to present the results of the tests. 2. The infiltration excess process

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(Horton runoff) is assumed as the dominant mechanism of flash flood generation in the Orbieu River basin. In general, due to the high space and time variability of the watershed characteristics, both saturation excess and infiltration excess processes can be active at the same time in various locations of the watershed. Dealing with extreme storms, the question is to understand what the dominant processes, which control the fast horizontal flows, are? Thus the aforementioned assumption requires more justification than it is done in the paper (p. 3401). For example, I would like to see more detailed description of the soil properties and ground water levels. 3. The authors use the very simple models of infiltration and overland flow but, on the contrary, the detailed model of channel flow based on the Saint Venant equation. Taking into account the influence of space-time infiltration dynamics on flood generation, the reasons for using such structure of the model should receive more consideration in the paper. 4. The only flood is used for the model validation. There is not any quantitative criterion (e.g. RMSE) of the model performance in the paper but a reader can see from Fig. 14 that most of the hydrographs are really close to the observed one. However, this result is scarcely surprising when it is considered that the observed discharges “during the first hours” (p. 3417; how many hours?) of the same flood were used for the model calibration. Be is as it may, the presented results of the model testing are distinctly deficient and these results enable a reader to evaluate the model applicability for flood forecasting. I suggest the authors to test the model by using several other floods in the Orbieu River and to show the results of the testing. If there are no available data to model these floods, then I recommend to compare the calculated and the observed hydrographs of the flood under consideration (Nov. 1999) in different points of the river network. 5. According to the presented formulas for the dimensionless criterions (Section 2.4), they depend on the characteristics of the overland flow. However these criteria were calculated “using the spatially distributed characteristics of the basin and the characteristics of the rain” (P. 3407) only. Please, clarify.

Technical Comments 1.The Abstract section - the main results of the study should be added 2. P. 3403 the words “the model ... used to predict some flash floods” should be

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corrected 3.P.3404 I suggest to remove the phrase “This 1-D module can be replaced by a 2-D river/floodplain model.” Such a replacement is a very complex problem 4. Eq. 9 IP should read iP. 5.Overall the English language should be improved

Overall the manuscript may be accepted after major revisions. Please send me the revised version for a second evaluation

1. Does the paper address relevant scientific questions within the scope of HESS? YES 2. Does the paper present novel concepts, ideas, tools or data? PARTLY 3. Are substantial conclusions reached? NO 4. Are the scientific methods and assumptions valid and clearly outlined? PARTLY 5. Are the results sufficient to support the interpretations and conclusions? NO 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientific (traceability of results)? PARTLY 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES 8. Does the title clearly reflect the contents of the paper? YES 9. Does the abstract provide a concise and complete summary? NOT COMPLETELY 10. Is the overall presentation well structured and clear? YES 11. Is the language fluent and precise? NO 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? NOT COMPLETELY 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? YES 14. Are the number and quality of references appropriate? YES 15. Is the amount and quality of supplementary material appropriate? NOT COMPLETELY

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