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Hydrology and Earth System Sciences Discussions

Interactive comment on "Characterization of physically based hydrologic model behaviour with temporal sensitivity analysis for flash floods in Mediterranean catchments" *by* P. A. Garambois et al.

Anonymous Referee #1

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General comments: The paper presents a methodology aiming at better understanding the response of a physically-based distributed hydrological model adapted to the simulation of Mediterranean flash-floods. The method is based on a temporal sensitivity analysis of the modelled discharge. The approach is original and, to my knowledge, has seldom been used in hydrology. One of the strength of the paper is that the results are based on several catchments, covering various physiographic conditions and the number of flash flood they use. The results presented by the authors are quite convincing and show the interest of the approach. They highlight the sensitivity of the model

response to the soil depth (and more generally to the soil storage capacity) around the peak, and the impact of the lateral saturated hydraulic conductivity during some recessions. However, I have two main concerns, I would like the authors to address: 1) The approach they propose is a local sensitivity analysis. Therefore, the results depend on the choice of the "optimal parameter set". The authors say little about the calibration phase of their model and refer to a submitted paper on this topic. This point should be a little more developed in the present paper so that the reader is able to evaluate the reliability of the "optimal parameter sets" and how the possible equifinality problem was handled. How are the results susceptible to change if a different optimal parameter set is used? 2) The analysis presented in the paper only addresses the sensitivity to the model parameters. However, especially for flash flood simulation, the input rainfall and its spatial variability is very important. The authors use a combined radar / rain gauges product with 1x1 km2 spatial resolution and 5 minutes time step. What is the reliability of this product? Would it be possible that the large sensitivity of the model to soil depth could be partly due to a misspecification of the rainfall input? Would it have been feasible to also include the uncertainty on the rainfall input in the sensitivity analysis? In conclusion, the topic of the paper is of interest for the readers of HESS and the paper will deserve publication once these comments and the specific comments below are addressed?

Specific comments:

1) Abstract: lines 1-15. p.1377 lines 5-10. There are some repetitions and these parts could be shortened. 2) Some English corrections: p. 1377, line 24 "among others for .."; p.1384, line 6 "were proposed"; p. 1386, line 15 "mainly develops.." 3) p. 1378, lines 12-15 and throughout the paper. Choose one manner to cite the references, according to the journal conventions: in alphabetical or in chronological order, but not a mixture of both. 4) p. 1378, line 21: a reference is missing about BATEA 5) p. 1380. Define the acronyms TEPADS and TIGER 6) p. 1383 Eq. (4): the range of the integrals is between 0 and 1. Does it mean that the parameters are

normalised before the analysis is performed? 7) p. 1386, line 1-3. I would not say that the rating curves quality is good for French catchments, especially when flash floods are concerned. High values of discharge are often extrapolated because gauging high discharge values using standard methods is too dangerous. In addition this sentence is quite contradictory with those of p. 1388, lines 12-15. 8) p. 1386, lines 5-7. Sentence not clear. Please rephrase. 9) p. 1387, lines 18-21. Sentence not clear. Please rephrase. 10) p. 1388, lines 8-10. Sentence not clear. Please rephrase. In addition, I believe that this section is too short and that more should be said about the calibration method, the number of events used for calibration/validation, etc.. so that the reader can judge the quality of the “optimal parameter set” used in the remaining of the paper. Are you sure to have reached the absolute optimum? 11) Figs. 5-8 are mentioned before Figs 3 and 4 12) p. 1388, line 26. What do you mean by “different resonances”? 13) p. 1389, line 8. Table 4 instead of Table 3? 14) p. 1389, line 14. “present observed specific peak discharge..” 15) p. 1390, line 21. You mention that you explore a region around the optimum. What is the size of the explored window? From the remaining of the section, I understand that you tested $\pm$ 5, 10, 15%, which is quite low. Did you tried values as large as $\pm$ 50, 100%? 16) p. 1390, lines 15-16. You mention here the notion of metamodel, but it has not been introduced before. Could you explain a little more, what is the metamodel and how it is estimated? 17) p. 1390, lines 19-23. Sentence not clear. Please rephrase. 18) p. 1392, lines 1-11. All these analyses are not clear for the reader. To be clearer you could arrange tables 3 and 6 so that the events are ordered in increasing or decreasing order of the specific peak discharge. 19) Table 1. Hsol is not defined and its unit is not given 20) Table 3: Does the average has a physical meaning? 21) Table 4: What is Bransdy formula? You could add it to the caption.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 1375, 2013.