

Interactive comment on “Using a multi-hypothesis framework to improve the understanding of flow dynamics during flash floods” by Audrey Douinot et al.

Anonymous Referee #2

Received and published: 2 March 2018

The article presents the test of three versions of an event-based model (MARINE) on Mediterranean catchments in France. The authors investigate the impact of the sub-surface flow and deep infiltration on model response using three modelling alternatives. They try to relate their results to the a priori knowledge on hydrological processes on the studied catchments.

First, I found that the originality of the proposed methodology is not clearly explained compared to existing works. Second, my main concern is that the results and discussion section (section 5) is excessively long and verbose. The authors discuss all the results with great detail, but the reader gets lost in all the information provided (at

C1

least I got lost). At the end, it is a bit difficult to extract the main findings. I suggest reducing the size of this section to highlight the most important results. Besides, I found that the discussion on the link between results and the a priori knowledge on processes remains very qualitative. Though the explanations are sensible, there is no clear demonstration that the results are actually the consequence of the perceptual knowledge on processes invoked by the authors. There are so many possible causes to explain modelling results. I found that the reasons found by the authors only remain hypotheses and should be more clearly presented this way.

I advise major revision.

Detailed comments

1. General: Though the English writing is generally good, some sentences remain unclear. I suggest that the article be checked and corrected by native English.
2. Abstract: The main results are summarized in three lines. I find it difficult to fully understand what was done in the article by reading the abstract only.
3. Sections 1.1 and 1.2: This introduction of the context is interesting but quite classical and does not really bring essential material to understand the work done. I suggest reducing these parts to a few lines only.
4. Section 1.3: This section appears to be mostly centered on the French context. A more general perspective could be given to this literature review.
5. P4, L15-20: Clark et al. (2015a; 2015b) also proposed the SUMMA framework, applicable to distributed models. The authors should more clearly explain what is new and original in the approach they propose compared to these past works.
6. Section 2.1: This section could be presented in a more synthetic way, which would help the reader to more easily compare the study catchments. I suggest not repeating in the text information already contained in Table 1.

C2

7. Fig. 1: For those not knowing France, maybe a small location map within France could be added.
8. Figs. 2-3-4: I suggest grouping these three figures.
9. Table 1: Be clear that QD2 and QH10 are “maximum” discharge. The HYDRO code could be introduced in the table. The meaning of Ls, L and Lsi should be made clear in the caption. Say in the caption that bold values are dominant geology. In column ID, use the same detailed ID as those used in Table 2 for consistency. Not sure “Vogue” is the right spelling.
10. P9, L2-4: The information on flow data availability could be added in Table 1.
11. P9, L5-6: Not sure this QD2 threshold is actually the alert threshold everywhere in France. Though there may be link, I am pretty sure the alert threshold is not determined using a statistical approach, but rather by a local analysis.
12. P9, L5-11: The event selection process ignores all the rainfall events that did not generate high flows, but which would still be interesting to investigate, especially to check that the model is not over-reactive on such events. Was this analyzed in separate work? A few words could be added on this issue.
13. P9, L14: Which FFS is it?
14. P9, L14: What RHEA and CALAMR mean? Any reference?
15. P9, L26: What SIM means?
16. Section 2.2: Maybe I missed something but I did not find information on how the events were split into calibration and validation. Given there are only a few events per catchment, I guess results may be quite sensitive to this selection. This is not commented. Typically, if the authors had reversed the roles of the two events sub-sets (calibration / validation), would results be the same? If yes, this would strengthen the proposed analysis. If not, this may add further uncertainty in the analysis.

C3

17. Table 2: For Qpeak, is it the mean of peak flows?
18. Section 3.1, title: From the description, it appears that MARINE is a model, not a framework.
19. P11, L3-10: Indicate units in brackets for parameters.
20. P11, L8: Write “Module 2 (i.e. subsurface downhill flow)”
21. P11, L12 (and elsewhere): Check the place of brackets around references.
22. P14, last line: “hourly maximum discharge”
23. P15, L17: This point was not fully clear for me. Please explain a bit more.
24. Section 5: As mentioned above, I think the authors should make an effort to much reduce this section. In several sub-sections, the comments detail so many cases that it is very difficult to get a broad picture.
25. Fig. 8: The distribution of mean results over all the catchments together would be useful to add. Is there any version that appears better on average? Please indicate in figure caption that the x axis refers to the catchments ID in Table 1.
26. Fig. 9: Colors are not very useful (especially if the article is printed black and white). Maybe use different symbols instead.
27. Section 5.2: Difficult for me to extract the main points from this long discussion. . .
28. Section 5.3: I was not fully convinced by the usefulness of this section.
29. Fig. 13: Are the simulation shown obtained in calibration or in validation? It would be useful to have the dates instead of the time steps on the x axis.
30. P30, L20 (and elsewhere): I think the term “demonstrate” is not appropriate. The work done here is not a demonstration. The links established between model results and actual processes remain hypotheses in the work, which may simply be more likely than others.

C4

31. List of references: There are several incomplete references. The authors often give two URL; only keep the one for doi. Several titles are in French; please at least add the English translation in brackets, so that the non-French reader can more easily understand the topic of the cited references. I personally find it is not good practice to cite discussion papers if they were not ultimately accepted. The reference "Ministere de l'Ecologie" is strange looking.

Cited references

Clark, M. P., Nijssen, B., Lundquist, J. D., Kavetski, D., Rupp, D. E., Woods, R. A., Freer, J. E., Gutmann, E. D., Wood, A. W., Brekke, L. D., Arnold, J. R., Gochis, D. J., and Rasmussen, R. M.: A unified approach for process-based hydrologic modeling: 1. Modeling concept, *Water Resour. Res.*, 51, 2498-2514, 10.1002/2015WR017198, 2015a.

Clark, M. P., Nijssen, B., Lundquist, J. D., Kavetski, D., Rupp, D. E., Woods, R. A., Freer, J. E., Gutmann, E. D., Wood, A. W., Gochis, D. J., Rasmussen, R. M., Tarboton, D. G., Mahat, V., Flerchinger, G. N., and Marks, D. G.: A unified approach for process-based hydrologic modeling: 2. Model implementation and case studies, *Water Resour. Res.*, 51, 2515-2542, 10.1002/2015WR017200, 2015b.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-710>, 2017.