

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 #1

Received and published: 11 January 2018

This paper proposes a methodology for the analysis of catchment hydrological behaviors during flash floods, based on the introduction and comparison of several hypotheses in a distributed hydrological model. This topic is of broad interest for the hydrological scientific community, and is fully relevant in my opinion for a publication in HESS.

However, in its current form the paper suffers from a lack of detail and explanations on several aspects (calibration procedure, explanations related to some figures, ...), causing difficulties for a detailed understanding of the research content. The presentation of sections 4 and 5 should particularly be improved in my opinion (and maybe

[Printer-friendly version](#)

[Discussion paper](#)



organized in a slightly different way) to facilitate the overall understanding of the results and related analyses. The paper well illustrates the difficulties in the interpretation of modelling results, due to equifinality issues and lack of internal observations to confirm the nature of the main hydrological processes. Therefore, even if some solutions to cope with these difficulties are proposed here, I think the conclusions relative to the catchments behaviors (section 5.6) should finally be relativized and presented in the discussion section as the most reasonable assumptions, provided the modelling results obtained here.

Specific comments

- The abstract is very short and could be slightly more detailed.
- References would be welcome in section 1.2
- The description of the calibration procedure (section 4.1) and of the metrics for evaluation (section 4.2) are not sufficiently clear in my opinion, and should be improved:
 - Please indicate how the “confidence intervals” are obtained for observations ($y_i \pm 2\sigma_{y_i}$?) and also for modelling results. This should clarify why the uncertainties ranges mentioned in the text (respectively 20% for observations and 10% for modelling errors) are consistent with eq.(6) and eq.(7)
 - please clarify the reason why the metric used for evaluation (QmedINT) is different from the one used for calibration (DEC) ?
 - the definition of the “acceptability zone” should be provided ($y_i \pm 2\sigma_{y_i} \pm 2\sigma_{mod}$?)
 - the “a priori” and “a posteriori” modelling errors are not defined. This clearly limits the interpretation of figure 11 (see hereafter).
- The presentation of modelling results (section 5.1) could also be enhanced:



- I think the separated presentation of each metric (overall hydrograph, rising limb, high-discharges, recession) does not help to give a synthetic overview of results. It seems that three main situations can be distinguished here: clear hierarchy (Gardons and Salz), contrasted hierarchy (Ardèche), or no clear hierarchy (equifinality, Hérault). These three situations could be illustrated based on a common analysis of all the metrics.
- Explanations in section 5.1.4 are poorly supported by figure 10 in my opinion. Please try to clarify this section and figure.
- The analysis proposed in section 5.1.5 is also difficult to follow based on figure 11, which does not well illustrate in my opinion the differences in models behaviors. Figure 11 indeed is difficult to understand: ϵ_{DDEC} is not defined, the definition of prior and posterior errors is again missing. Moreover, is not clear why the width of the acceptability zone does not vary with y_i (not consistent with equation (7)). Please try to clarify this section and figure.
- Section 5.1.6: I think this interpretation on catchment behaviors arrives too early here. I think it would be better to put this in the discussion section, and to present these analyses as plausible assumptions, according to the modelling results.

- Section 5.2 may be renamed in a more explicit way, such as: "Analysis of relevance of the internal hydrological processes simulated". It could include both considerations on proportion of surface runoff (current section 5.2), and detailed analysis of velocities and water contents in the case of Hérault (current section 5.3)

- Conclusions: I don't' really agree with two of the conclusions:

- "For each catchment, the best performing models were those where results reflected the available knowledge and observations on the overall hydrological functioning of the catchments ...". Actually, it seems that very limited information is



available on the real hydrological behavior, excepted maybe for the Gardon where detailed measurements were performed. Therefore, I would rather conclude that the modelling results help to draw consistent assumptions on hydrological behaviors, that can in some (rare) cases be confirmed by the existing knowledge and local observations.

- “distinction in hydrological revealed between the catchment of the Gardons and the Ardèche may explain that taking into account the spatial nature of precipitation in a flash flood forecasting method results in an improvement only on the Gardon and not on the Ardèche . . .” I think this conclusion is not really supported by the content of the paper. Moreover, another explanation could just be a difference in the rainfall spatial variability, which seems to be more pronounced in the Gardons catchment for climatic reasons.

Technical corrections

- Section 2.2:
 - The reference Ministère de l'Ecologie (2015) just corresponds to an URL, which could be added directly in the text.
 - “These measurements were calibrated by forecasters at the French Flood Forecasting service by monitoring a network of rain gauges . . .”. Sentence not clear, please reformulate.
- Figure 5: are θ_s and θ_i really the current and initial water contents respectively? Shouldn't rather θ_s be the saturation water content?
- Section 3.2, description of the modelling principles: the equations (1) to (4) and description of variables should be placed in the text with reference to figures 6 and 7
- Section 4.2: σ_y and σ_{mod} rather than Σ_y and Σ_{mod}

- Section 5.1.5: “the variation interval of the modelling errors”: I don’t really understand, please define this.
- I finally suggest to check the overall quality of English.

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

HESSD

Interactive
comment

[Printer-friendly version](#)

[Discussion paper](#)

