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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 #2

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The manuscript presents the application of a variance based sensitivity analysis method to a rainfall-runoff model used to simulate 10 large flash floods at 6 different gauging stations. The temporal evolution of the sensitivity factors of the computed discharges to 5 parameters (so-called TEPADS) is analysed to evaluate the influence of each individual parameter on the simulation outputs during the various phases of a flood event. Such approaches have seldom been documented in hydrology which makes this manuscript attractive but the presented results and interpretations raise important questions needing clarification:

1) The proposed sensitivity analysis is basically a local analysis evaluating separately the effect of each individual parameter (one at a time method, OAT) since only the "first-order variances" or better said the "first-order derivatives" are considered. The authors argue that a simple difference between total variance and the sum of the first order variance is sufficient to evaluate the possible influence of dependencies between parameters (lines 1-6 on page 1395). The terms of the variance decomposition being positive according to the authors. This statement seems totally wrong to me. Covariances may be and are often negative ! Moreover, the obtained result does only hold in the vicinity of the selected optimal set of parameters. This interpretation and the conclusions - the combined effect of the parameters is limited and the model is parsimonious - seem highly questionable to me. The authors should consider the article recently published by Saltelli and Annoni (2010) showing how misleading local OAT sensitivity analyses can be: "How to avoid perfunctory sensitivity analyses" (in Environ. Model. Software).

2) The authors have selected 10 events presented as validation events (p. 1381) but do not describe the model implementation (calibration and validation procedure). The Nash criteria presented in table 4 are striking and will appear as surprisingly high to any hydrologist used to running rainfall-runoff models and this especially for large validation events in the Mediterranean context. An average relative error on the simulated peak discharge of 13% (table 4) for validation events does not fit with common practice in hydrology. This deserves comments and explanations and the authors should at least absolutely explain how they implemented their model.

Minor comments: 1) I personally do not like the term "physically based". If we consider the complexity of the runoff generation processes on watersheds and the role of preferential flows, our extremely simplified rainfall runoff models, even distributed, are a long way from the real physics. I wish this expression could disappear from our common vocabulary. 2) In the same line of thought, the sensitivity analysis of a model does not provide insight into the processes (line 5 of the abstract). It only reveals what

part of the model dominates the considered aspect of its response. This idea should be removed form the manuscript. 3) The mathematical developments in part 2 are very unclear. They are based on notations and terms that are too shortly or even not defined and contains ambiguities. Line 5 : "Omega denotes all possible values of the factors.... let X in Omega be a possible value of the k model input": factors are equivalent to input and to parameters? what is exactly g ? Can the author give its exact expression of g in the considered case study? Please define random balance design, gaussian process emulators or Polynomial chaos expansion (line 15 P 1384). Please give some explanations of the State Dependent Parameter method that could be understood by non specialists: what is hidden under the terms "recursive filtering and smoothing estimation" (l7 P 1384)? 4) The manuscript could be significantly improved if corrected by an English native.

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