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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Flood design recipes vs. reality: can predictions for ungauged basins be trusted?" *by* A. Efstratiadis et al.

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We would like to thank Salvatore Grimaldi (henceforth SG) as well as the anonymous reviewer, for their positive evaluation and their encouraging comments, and the fact that both find our contribution useful and attractive.

In his review, SG discusses three interesting issues, to which we reply in turn:

1. Concerning Section 2.1, 2.1.1, 2.1.2, and 2.1.4 I agree, indeed the concepts of these sections are in line with [2, 3] where we underlined similar critical issues. I would add, eventually, the importance of the "subjectivity" concept we introduced that is different to the concept of uncertainty. Indeed, in my opinion, it is important that a hydrological analysis would provide similar results if it is developed by two practitioners

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in two different moments. The runoff coefficient, since it is given as a range of values, can provide different results just for a different feeling of the analyst. This can be problematic in practice.

To our opinion, subjectivity is mainly a consequence of uncertainty, which is intrinsic characteristic of hydrological models. Evidently, uncertainty (and thus subjectivity) can (and should) be reduced (but not eliminated), by providing both physically and statistically consistent conceptualizations, formulas and parameters. This is the central message of our article. On the other hand, it is essential to maintain some flexibility, allowing hydrologists to take advantage of their experience (and – why not – intuition) in the selection of the most appropriate modelling tools and parameter values. In this context, we find rather positive giving a range of feasible values of a certain parameter (e.g., the runoff coefficient) rather than a single "recommended" value. Such a recommended value hides the prevailing uncertainty in describing the underlying process.

2. Concerning Section 4.1. Authors mention that the continuous approach is still not preferred to the event-based one. I would emphasize this aspect. Indeed, the advantages of considering continuous frameworks are multiple: among others, it is possible either to perform uncertainty analysis (as mentioned by the authors in the conclusion - lines 23-24) and, above all, to have a reasonable estimation of the design hydrograph volume, that is dramatically underestimated in the event-based approach.

The underestimation of flood volume is due to the improper representation (if not ignorance) of the interflow-generating mechanisms in most popular event-based models. The key role of interflow, which produces a significant fraction of flood runoff, is thoroughly discussed in the paper (mainly in section 4.2). In the revised version we will pay further attention to the consequences of ignoring the soil-related processes to the outcomes of a flood study.

3. Concerning the Section 4.3. In order to better evaluate the results shown in Figure 1, it would be useful to include in the paper all the related information (net rainfall figure,

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adopted parameter values like lambda, CN, the watershed DEM, the IUH shape, etc.). I am curious to see if the behavior underlined by the authors is also due to some sub-steps of the event-based approach. Indeed, since the SCS-CN is wrong when it is applied at sub-daily resolution and it provides an underestimation of the net rainfall peaks, I expected to see different hydrograph shapes.

In Fig. 1 we present certain preliminary results from four representative flood events in two catchments, in order to highlight the major problems with the use of the SCS-CN & SUH approach. This is a subset of dozens of flood events from several study basins that have been analyzed in the context of a shortly concluding research project (http://deucalionproject.gr/). The essential information about the basins and the related monitoring infrastructure is provided in the referenced article by Efstratiadis et al. (2013), while a synoptic explanation of the methodology (which is based on a calibration approach) is given in section 4.3. In the revised paper we will provide some additional information, although full details about this research will be presented in a forthcoming paper.

Regarding SG's last comment, the over- or underestimation of the peak flow depends mainly on the synthetic unit hydrograph (SUH) that is employed to propagate the generated effective rainfall to the basin outlet. In our analyses, we made use of an empirical SUH having a linear rising branch and a nonlinear falling one (Michaelidi et al., 2013). In order to preserve full consistence with the major assumption of the SCS-CN method, i.e. the dominance of the overland (Hortonian) flow, we set its duration equal to the time of concentration of the basin. In that manner, we considered that the entire effective rainfall flows over the basin surface. In fact, the response time of the basin is much longer, since most of runoff flows through the shallow soil. However, by implementing the erroneous assumption of overland flow, thus setting the duration of the SUH equal to tc, we forced the model to calculate too narrow hydrographs, thus providing too high peaks.

In the forthcoming paper mentioned above, we will propose appropriate formulations

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of the SUH that preserve much more accurately both the volume and the peak flows of the hydrographs observed in a number of flood events across all study basins.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 7387, 2013.