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Hydrology and Earth System Sciences Discussions

Interactive comment on "*HESS Opinions* "More efforts and scientific rigour are needed to attribute trends in flood time series"" *by* B. Merz et al.

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We thank Attilio Castellarin for his valuable comments. Below we address each comment and explain how we revise the manuscript in response:

I would encourage the authors to discuss a little further trend-detection. I am aware that this manuscript focuses on trend attribution, not on trend detection. Nevertheless, in my opinion, the current version of the manuscript may indirectly deliver the message that trend detection is a mature topic, for which objective and "unbiased" procedures are available. I believe that the general considerations raised by authors concerning trendattribution hold also for trend-detection (and perhaps on trend-modelling) too. Biased search and interpretation may also characterize trend-detection studies. Literature on flood and streamflow trend-detection reports contradictory and conflicting

results for the same study areas and on the basis of the same data (see e.g. Villarini et al. 2010 and references therein).

Response: Discussing the limitations and problems of trend detection would be certainly a very valuable issue. However, we think that an extension of our paper (1) could possibly distract from our key message, and (2) would require quite some discussion to cover this topic. To avoid that our paper conveys the message that trend detection is a mature topic, we will add a short statement on the limitations of trend detection.

Still on this point, perhaps the authors could better underline that attribution in itself is not a necessary ingredient of a trend analysis. I believe that robust and objective investigations on the possible presence of trends or step changes in long and reliable observed series (streamflows, low-flows, flood flows, annual precipitation, etc.) that tests the hypotheses on the basis of a number (and not a single) of robust statistical tests (e.g., distribution-free, or non-parametric tests, see e.g. Villarini et al. 2009 and 2010; Pistocchi et al., 2011; Castellarin and Pistocchi, 2011) may provide very useful information for practical purposes, even without a trend attribution. Nevertheless, I totally agree that if one claims a trend attribution, he/she should provide the reader with the three ingredients outlined in this opinion article.

Response: We agree that good detection studies are a value in themselves, and that they may provide very useful information. We will add a statement on the value of sound detection studies in the introduction.

Among the "simulation-based attribution" I would also probably include approaches based on simplified rainfall-runoff models, such as the model adopted by Allamano et al. (2009).

Response: The model of Allamano et al. (2009) is indeed an attractive approach which analytically derives the flood probability distribution based on a clear process hypothesis for high mountain catchments. Given its relation between temperature and flood frequency, this approach might be attractive for studying changes in flood frequency in

HESSD

9, C711–C713, 2012

relation to temperature change. However, the purpose of our paper is not to discuss advantages and disadvantages of different model strategies or even of the data-based versus the model-based approach; the purpose is to point to a deficit in current approaches, namely the neglect of the attribution problem. We feel that starting the discussion on the usefulness of different hydrological modeling strategies for flood trend attribution would open up a new field and would not contribute to our key message.

p.13355 – lines 11-14: I would mention here that the influence of flood retention basins is also strictly connected with the magnitude of the flood event being considered (i.e. the influence becomes negligible when the magnitude of the flood event significantly exceeds the magnitude of the design-flood)

Response: Good example which will be added in the manuscript.

I found section 4.2 to be less operative and practically oriented than the rest of the manuscript. It would be interesting if the authors could provide the reader with some practical indications and examples in this section too.

Response: We agree that section 4.2 is more abstract than the rest of the paper. We will try to make the description of the method clearer. We will add a paragraph on how the optimal fingerprinting approach can potentially be implemented in the detection and attribution of extreme flows. Although Hundecha and Merz (2012) is not based exactly on this approach, there is some conceptual similarity in that both are based on detecting modeled change signals of external drivers on the response variable. Furthermore, we will add a paragraph on caveats of the approach in terms of its applicability in the study of extreme flows.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1345, 2012.