

Interactive comment on “Stochastic generation of spatially coherent river discharge peaks for large-scale, event-based flood risk assessment” by Dirk Diederer et al.

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Thanks for the opportunity to review this interesting and well-written paper. The modelling of event dependencies on a continental level is indeed highly relevant both scientifically and for the purpose of insurance and flood management.

The authors present a sophisticated approach to address this problem. However, the paper has several major issues that should be addressed before it can be published. The main issues are (1) the definition and validation of events; (2) the limited and highly technical description of data, modelling steps and results, which seem at times a high abstraction from the stated purpose of the paper (i.e. flood event analysis for

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risk assessment); (3) the description of the implications, uncertainty and potential use of the results; and (3) the limited reference and comparison to previous studies in this field. Please see more detailed comments below.

Abstract: The abstract is unconventional with many references. It also does not clearly state the problem (why is the event-scale analysis needed?) and the implication/use of the results (it stops at outputs). I suggest to review and revise accordingly. The references should be better incorporated in the main body of the paper to address the lack of integration in literature (see other comments).

Context: the Abstract clearly puts this paper in line with current continental-scale flood modelling work that is based on statistical return period analysis rather than event identification (Ward et al., 2013; Alfieri et al., 2014; Dottori et al., 2016; Vousdoukas, 2016; Winsemius et al., 2016; Paprotny et al., 2017). However, the Introduction does not mention these previous modelling exercises and how this paper will differ/improve on those. I suggest to revise the introduction to make this context in existing literature stronger. Similarly, in the main text of the paper, the authors fail to compare their methods and results to other scientific work. For example, how do the correlations compare to those showed in previous studies (e.g. Jongman et al (2014); Timonina et al 2015, Risk Analysis), some of which are mentioned in the abstract but at no other stage in the paper?

Events: The definition of an event is indeed challenging, as explained in section 1.2.1 and 1.2.2. However, I feel the authors should use a consistent definition and use of the term 'event' (or various definitions thereof). In section 1.2.2 the authors introduce the use of 'block' and 'dynamic' events, but the later chapters mainly use 'local', 'pan-European', 'atmospheric' events. Indeed, the word 'block' and 'dynamic' are not featured beyond section 1! See also the next two comments.

River basin events: In the description of the computation of river basin events I miss a critical discussion of the hydrological/hydraulic model. Since the analysis is conducted

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using a model instead of observation data, the upstream-downstream correlations (i.e. river basin events) are obviously more strongly dependent on the model than on reality. How are the correlations affected by this model and what are the implications? Is it even realistic to do this, or does such upstream-downstream analysis simply result in backward-engineering of the discharge model? This should be incorporated in section 3.2 and result discussion.

Pan-European events (section 3.3, Figure 5): I have an issue with the interpretation of the concept of pan-European events by the authors. The way I understand it from the methodology, Pan-European events are simply identified by finding one local peak and opening a 21-time window, during which all high discharge values across Europe are incorporated. However, simultaneous high discharge does not make an 'event'. Where does the geographical boundary of an event stop? Would simultaneous high values in Germany and China be considered an event? Or two high rainfall events in one month caused by two separate low pressure systems in different parts of Europe? In my opinion it would be important to incorporate a concept of correlation or at least the identification of a single weather/atmospheric pattern that links the discharge values, to eliminate individual coincidental simultaneous occurrences (i.e. after Section 4).

Multivariate model: This section is extremely cryptic and very hard to read or relate to the rest of the paper. While trying to relate the model to the identification of events based on discharge (which I presume is the aim), one finds that the words 'event' and 'discharge' are not mentioned a single time in the entire section 4 (apart from once in the figure caption)! It is currently very hard to understand what the inputs, processing and outputs are. I suggest to revise and align it strongly with Section 3.

Validation: it is not clear to me what 'synthetic' and 'observed' data is used for validation. It would be important to validate the outcomes of the multivariate model with fully independent datasets, i.e. actual event data (preferably); recorded atmospheric patterns; observed river discharge data; etc. Currently it seems much of the validation is done within the model, which would make it very hard to de-couple the internal work-

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ings of the hydrological model from the results (e.g. the strength of intra-basin correlations are a given, since the hydrological model is the basis of the analysis). Similarly, there is no discussion on potential regional and temporal differences in model precision (for example due to weather patterns, snow melt, data availability, river size, and other real-world aspects). As such, the validation, just as the model description, remains highly abstract. This section needs substantial strengthening and, once again, needs to actually reflect on ‘events’ to prove that it does what the paper aims to do.

Results and use: The paper abruptly ends after the technical multivariate model section, and the conclusion section is only a few lines. It remains unclear to the reader what the exact result is of this study and how it can be used. In the abstract, the authors claim that their study is needed to analyze insurance portfolios. Can it? How do we use the model for probabilistic risk assessment? How would that be better than currently available models, and what are the uncertainties? Which regions does it work and which does it not? It would strongly benefit the paper to add a section on the resulting data, its applications and limitations.

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