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Natural Hazards and Earth System Sciences
Discussions

# *Interactive comment on* "Stochastic generation of spatially coherent river discharge peaks for large-scale, event-based flood risk assessment" *by* Dirk Diederen et al.

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Summary We would like to start with thanking mr Jongman for his thorough and constructive review and apologise for the slight delay in reply.

A number of constructive comments was provided related to clarification and discussion of work presented in this manuscript. We are very happy to incorporate these and think it will improve the quality of the manuscript. A request was made to provide a quality check of the used (modelled) discharge data set. Unfortunately, this would be very hard for us to do (since we are not in possession of the model), so we feel this is out of scope and refer to the authors who generated the modelled discharge data.



First, we will address his points one by one. Second, we will provide a list with proposed improvements. We would be happy to hear from the reviewer, after reading the comments to his points, if we have missed/neglected/misinterpreted any major points.

Referee (B. Jongman), 1. 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).

Response (Diederen et al.): This point was also made by reviewer one and we fully agree. We concluded that the abstract has not pointed readers in the right direction. We will completely revise the abstract to make it clear what the focus of this manuscript is.

Referee (B. Jongman), 2. 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?

Response (Diederen et al.): In this manuscript we focus on the dependence structure.

Ward et al., 2013; Alfieri et al., 2014; Dottori et al., 2016; Paprotny et al., 2017; focus on local distributions (of modelled data, per grid-cell). Vousdoukas, 2016 focus on coastal variables. Paprotny et al., 2017 focus on a different type of dependence, which is related to flood inducing factors (causal, instead of a hydrological model). Jongman et al (2014) do capture correlations of monthly discharge maxima in Europe. They use them to split Europe into different zones in which the correlation is relatively high. However, we look at events in more detail (building them up from tracked discharge waves in different river basins) and are more narrow in our framework (we do not address the entire flood risk modelling chain nor touch upon climate change). We captured the spatial dependence of discharge peaks and do not so much aim to analyse the obtained dependence (e.g. using spearman correlations), but aim to capture the dependence in the best we way can (wave tracking) and reproduce the dependence while generating a large synthetic data set (for which we use the spearman correlation). Timonina et al (2015) seem to focus on the dependence of flood losses, rather than on the dependence of discharge.

We conclude that the aims of these studies are quite different from ours and do not overlap in terms of methodology. The binding factor is the large spatial scale. We appreciate the referee's suggestions for additional references and we will include a discussion on the differences in methodology.

Referee (B. Jongman), 3. 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.

Response (Diederen et al.): The distinction between block events and dynamic events are used to place the studies into context. From the introduction onwards, all events referred to are dynamic events. We will clarify this in our revised manuscript.

Referee (B. Jongman), 4. 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 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.

Response (Diederen et al.): No hydrological nor hydraulic model is used in this study. For a discussion of the quality of the used data set we would refer to the JRC, who produced the data set (Alfieri et al 2014). We agree that the dependence in the modelled data may/will differ from the dependence in reality. However, since we do not have the data of reality, it is impossible to compare, such that we would only be able to speculate. Moreover, we focus on a methodology to capture the dependence structure and retain it while generating a large synthetic data set. In the future, it can be readily applied to data sets that capture reality more closely. If we were to speculate, we would be fairly confident that waves can be tracked in reality and are looking forward to studies that apply wave tracking to data of the (relatively dense) European network of discharge gauges.

We will add in section 2 that the data is model output and, hence, not perfect, but that this is no problem as the focus is on the methodology.âĂČ

Referee (B. Jongman), 5. 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).

Response (Diederen et al.): a) Correlation of discharge across different river basins can be expected, as a result of atmospheric systems spanning multiple river basins. To use atmospheric systems for event identification would require the usage of an atmospheric data set (e.g. precipitation instead of discharge for event identification). This will introduce a new set of problems to deal with (e.g. where a single 'atmospheric event' triggers multiple 'discharge events' or the other way around). To consider atmospheric data sets is out of scope for this study.

b) We would argue that there is no strictly correct way of defining an event, but that it is subjective what an event is. A large number of previous studies exists that use an event identification without spatial delineation (time window methods/'concurrent' or 'simultaneous' methods). For multi-site studies that use event identification methods without spatial delineation, the spatial domain is determined by the sites considered in the study.

c) We agree with the referee that, in the context of the large-scale studies, it becomes evident that event definition without geographical boundaries is not necessarily meaningful in the physical sense.

d) Discharge waves do not span across multiple river basins (by definition). However, atmospheric systems do (point a.). Large systems may be of the order of magnitude of the European continent. This study aims to capture the expected spatial correlation in river discharge as a result of atmospheric systems, even though the events may not physically make sense regarding the discharge.

e.) In summary, to capture spatial correlations across multiple sites may limit the physical meaningfulness of events, which becomes more evident with a larger scale. We



discuss this problem in Sect. 1.2.3 "Handling dynamic events in a statistical event generator", since the choice of a multi-site approach is related to the statistical method available. We will expand the discussion of this issue and provide recommendations for follow up studies of how to deal it.

Referee (B. Jongman), 6. 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.

Response (Diederen et al.): In section 4 we are applying multivariate statistics to a matrix. A step is made from the physical world to the world of statistics, which has its own jargon. We will try to clarify the transition in Sect 3.4 in the revised manuscript.

Referee (B. Jongman), 7. 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 workings 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.

Response (Diederen et al.): The paper is about a methodology to generate a large catalogue of synthetic events that captures the spatial variability found in a short historical time series. Since the focus is on the methodology, we find it acceptable that this is done with modelled data.

We will change the name of the 'validation' section to ' validation of statistical model', and we will emphasise in the text that this is not a validation of the resulting data set, but only shows that the observed is a likely subset of the synthetic with respect to the dependence structure.

Referee (B. Jongman), 8. 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.

Response (Diederen et al.): In this manuscript we present a method to generate synthetic data, which is to be used to force the flood risk model chain. This step is decomposed into two main sub-steps (event identification and description, generation of synthetic peaks), both of which are challenging.

It is different from other studies in that the result is a catalogue of large-scale, synthetic events of discharge peaks which spans across Europe, which incorporates the spatial dependence structure, i.e. are spatially coherent. Such a data set did not exist previously (to the best knowledge of the authors).

To analyse insurance portfolios: 1) discharge hydrographs have to be reconstructed. 2) a European-wide inundation model has to be run (in event-based mode). 3) Inundation data has to be converted to damage/losses using a flood loss model. The result would



be a synthetic catalogue of European-wide events of flood damage (to be used by planners, e.g. (re)-insurance). Follow up work (at least steps 1 and 2) is expected from the Bristol group (Bates et al) within the context of the System-Risk project.

We will elaborate these points in the text and emphasise that it is only the first step in a chain of steps towards a synthetic catalogue of European-wide events of flood damage.

Proposed improvements

a) We will completely revise the abstract as suggested [point 1]. b) We will add references and improve the study's positioning in the introduction [point 2]. c) We will try to provide more clear definitions of the event-type used in this study [point 3]. d) We will aim to improve Sect.1.2.3 "Handling dynamic events in a statistical event generator", in which we discuss the main limitation of the methodology applied in this study (events without spatial delineation) raised by the referee [point 5]. e) We will try to clarify Sect.3.4, where the transition is made from physical jargon to statistical jargon [point 6]. f) We will change the name of the 'validation' section to 'model validation', and we will emphasise in the text that this is not a validation of the resulting data set, but only shows that the observed is a likely subset of the synthetic with respect to the dependence structure [point 7]. g) We will better describe how the resulting data set is to be used in practice [point 8].

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