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Interactive comment on "Will climate change increase the risk of infrastructure failures in Europe due to heavy precipitation?" *by* Katrin M. Nissen and Uwe Ulbrich

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We would like to thank the Anonymous Referee 2 for his/her constructive comments on our manuscript "Will climate change increase the risk of infrastructure failures in Europe due to heavy precipitation?''. In the following we give a short and first reply to the referee's comments. A point-by-point reply will follow with the actual revision process after the decision of the editor.

My main concern is about that main objective of this paper. According to the end of the introductory section, the main objective of this work is to identify heavy precipitation events in the future under climate change conditions, taking into account not only the frequency of occurrence but also the size, duration and severity of the event on

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multi-daily, daily and sub-daily time scales. [...] On the other side, according to the manuscript title, the main objective of this work is a kind of risk assessment for infrastructure failures due to heavy precipitation events in a future changing climate in Europe. [...] Is the use of these parameters in the events identification algorithm sufficient enough to ensure the concept of risk? Is it common in approaches evaluating risks associated with extremes? Authors could further support with arguments and why not with references, how the application of this methodology is sufficient for risk assessment, in order to support the title of the submitted paper.

As the other reviewer also pointed out, the title of our manuscript raises expectations that are not covered by the contents. We agree that the article indeed does not contain a risk assessment. We think that the best solution for this problem is to change the title of the article as a complete risk assessment has never been the purpose of this publication. As a new title we suggest: "Increasing frequencies and changing characteristics of infrastructure threatening heavy precipitation events in Europe under climate change".

Authors present their main conclusions together with discussion. I suppose this is done to support by comparing with other works, their findings. But conclusions fade in that way and the only retained massage is that heavy precipitation events are predicted to substantially increase by the end of this century according to worst scenario (RCP8.5). A finding also concluded by other works according to the discussion part. Reorganise or rewrite your conclusions so as to give prominence not only to your main findings but also to the originalities of this work.

We agree with the argumentation of the reviewer and will split the discussion and conclusions section in a revised version of the manuscript.

In section 2 (Data), authors could justify briefly why they chose the ensemble simulations for RCP8.5 and RCP4.5 and not the best scenario for instance (RCP2.5).

This is due to data availability. The number of simulations conducted by the mod-

elling community gradually increases over time. Most modelling groups ran the simulations with the more severe scenarios first. When we conducted the analysis for the manuscript only very few simulations were available for the RCP2.6 scenario. Even now there is only one simulation available with 3-hourly output of precipitation. In order to obtain robust results with a multi-model ensemble we decided to concentrate on the scenarios with sufficient data. We will add this explanation to the manuscript.

In section 4, (lines 19 – 24) is the precipitation severity index (PSI) defined in this work or has it already been used? If the later is the case, a reference should be added.

This index was newly developed for the current study.

The "present day climate" analysis apart from the information that provides, plays the role of validation of the results (daily and sub-daily 10-year return values) issued from multi-model ensemble after applying the proposed methodology, since they are compared against respective values derived from gridded observational data (E-OBS data and ERA-Interim reanalysis). Though authors state in section 2 (page 3, lines 1-2) that '... The ability of the EURO-CORDEX models to reproduce mean and extreme precipitation has been analysed by Prein et al. (2016) in a recent study ....', I miss a statement at the end of section 5 about the reliability of the thresholds (10-year return levels) resulted from the ensemble dataset. This comment is relevant to the next one. In section 6, the climate change signal in multi-model ensemble is investigated through comparison of heavy precipitation event characteristics (frequency, size, severity and duration) during the two future periods (2021-2050 and 2071-2100) with the historical period 1971-2000. Authors should clarify in the text which results/values are used for the reference period (1971-2000), the ones issued from observational data (E-OBS and ERA-Interim reanalysis) or for reasons of consistency, those resulted from the multi-model ensemble?

In section 6 we compare results of 3 different periods of simulated data. The reference for both scenario periods is the "historical" period. It is produced using observed (1971-

2000) greenhouse gas forcing. This will be clarified in a revised version. The ability of the multi model ensemble to capture the 10-year return period threshold is discussed in section 5 both for daily and sub-daily precipitation events. Even though the model ensemble seems to capture the spatial variability of the threshold fairly well, the differences to observations (which can partly be attributed to the different resolutions of the data sets, and potentially to the use of hydrostatic rather than non-hydrostatic models) are too pronounced to use the observations as a reference for the calculation of the climate change signal.

Further analysis on seasonal variation of events frequency (counts) as well as changes in size and severity of events is limited to specific areas (noted on figure 6d and only for ensemble outputs forced by RCP8.5. Did authors conduct a similar analysis for RCP4.5 ensemble simulations? If this is the case, are there findings that worth mentioning?

We decided to show the results only for the stronger scenario as we wanted to limit the number of figures. In a revised version of the manuscript we will add a statement about the results for the other scenario. The qualitative change is the same. It is however somewhat weaker. The change between the historical annual event count and the RCP8.5 scenario shown in the figures is about 30-40% stronger than the change in the RCP4.5 scenario.

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