1. *[Section "2.2. Methods"] It seems that the critical issue of assumption of loss in each cell being independent from each other is not addressed. In the proposed approach, it is implicitly assumed that flood related losses within a given cell are only related to the "hazard rate" and the "vulnerability" of the cell. This assumption neglects the possibility of the exposures in a cell being affected by the losses in neighboring cells. Losses related to exposures in a cell may be triggered due to cutting of critical access paths or lifelines of the cell. In such cases, significant losses may occur in the cell even when the hazard and vulnerability of the cell itself is very low. It seems that the proposed approach does not take into account this phenomenon. This simplification is acceptable for a preliminary investigation however its potential limitations should be stated in the manuscript.*

   The reviewer is right. The loss of lives or injuries in each cell does take into account vulnerability, hazard and exposure of that particular cell. This is called direct costs, which is the focus of this study. To our understanding the referee suggests taking into account the cutting of critical access paths or lifeline (roads, electricity, etc.) into risk estimation, which implicitly enters into indirect costs estimation where vulnerability or exposure of one cell affects another cell. This could be done by adopting a more dynamic and network oriented approach which is out of scope for this article. We definitely can mention this in the discussion.

2. *[Page 5, Table 1] Values provided in Table 1 represent the opinions of 4 experts on the performance of the EWS. Values provided in this table requires some additional justification. The likelihoods reported for the performance of the baseline case (i.e. 24% and 75%) in terms of "scope" do not sum up to 100%. Is this due to a typo? Furthermore, it is stated that "improved" system refers to a theoretical system with maximum performance. Assumption of maximum performance, is a major one. Justification for this assumption should be provided.*

   There is a 1% in the green area that is missing and this is a typo.
   We agree that the assumption is a major one however we have considered a theoretical situation to prove the benefits of EWS as a very relevant nonstructural risk mitigation measure for the type of receptors under study. So, in our view, what is important is the marginal change from a situation A to a situation B in the spectrum of goodness of the EWS, and not point B itself. For example, we could argue that the same "% of improvement" at the lower end of the spectrum could return even more benefits (e.g. passing from no EWS to a decent one).
   Finally, even if right now 100% improved EWS might not be very realistic, in coming years it could be with the improvements in technology, better computers, models, data collection etc.

3. *[Page 8, Line 32] It is stated that a large data set is utilized in the training of the network. This large dataset is reported to be generated from the expert panel results. The bootstrap sampling technique is reported to be utilized in the generation of the large data set. In order to train the network properly, causality*

characteristics of the generated data should match that of the original data. Authors should provide a discussion on how well this could be achieved in this study.

We agree that this part was confusing. We rephrased in the following way:
"The data provided by this panel of experts were used to create a large representative dataset. This dataset was used to train the BNs with bootstrap sampling, so that the contingent probabilities in the learned network approximate the causal structure and probability distribution of the original sample. The dimension of the dataset allowed the use of the PC learning algorithm, a well-established constraint learning algorithm named after its authors, Peter Spirtes and Clark Glymour (Spirtes et al., 2000)."
We will add further details in the discussion.

4. *[Page 12, Line 8] It is stated that output of the BN is expressed as a probability distribution per cell. In Figure 4, the parameters of the distribution (i.e. mean and coefficient of variation) are presented for the case of probability of injury (per 50m2). However, the distribution function itself is not specified in the manuscript. The distribution function should be specified clearly. If the conventional normal distribution is assumed to apply to probability of injury and the minus one standard deviation values are evaluated, negative values are obtained. For example, for the district "24 - Werd" the mean probability of injury is reported to be around 5.8% and the coefficient of variation is around 1.23. In this case, minus standard deviation is obtained as - 1.3% (i.e. 5.8%-1.23*(5.8%)). This result contradicts with the fundamental axioms of probability theory.*

The distribution function is not specified because it's different in each cell as result of H and V conditions and the contingent probability table of the trained network. Being them empirical and discretized it is wrong to assume a normal distribution. Having said this, a high CV like 1.23 (deriving from a SD of 7.134 by definition*), should be interpreted like a measure of relatively high dispersion that says that while the expected value is 5.8% that value could also be close to 0. A negative value is evidently meaningless and the data used for the training don't have negative values neither the discretization has produced negative intervals.

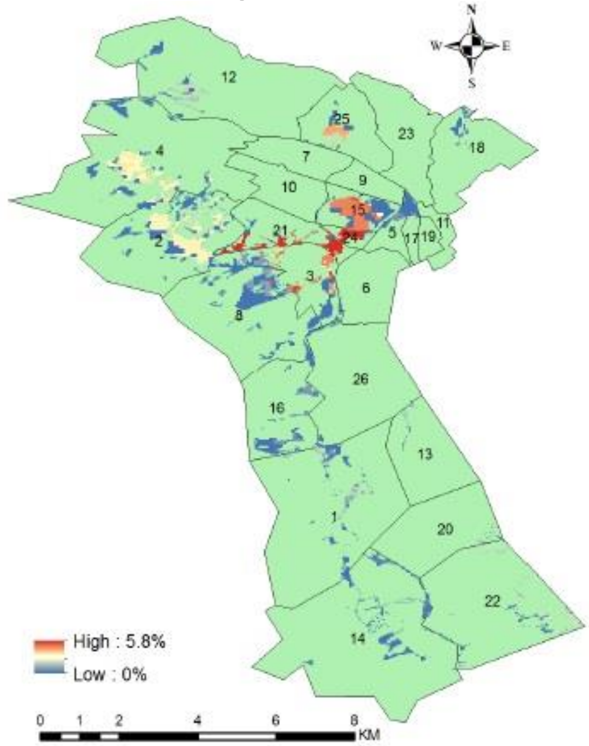
* the SD is computed as

$$\sigma = \sqrt{\sum_{i=1}^{N} p_i (x_i - \mu)^2}, \quad \text{where} \quad \mu = \sum_{i=1}^{N} p_i x_i.$$

where p is the probability of one state (x)

5. *[Page 13, Figure 4] In Figure 4a, the numerical value is only provided for the case of probability of injury being "High". The corresponding value for the "Low" is missing. It should be reported as well.*

We have corrected figure 4a. See the attached.



6. *[Page 8, Line 28] The authors correctly note the possibility that the relationship between the risk and the exposure being nonlinear. In the present study, a linear relationship is assumed to perform a preliminary investigation. It would be highly useful for the readers, if the potential drawbacks of this assumption are stated. For the cells with highly dense exposure (e.g. high density of people, densely stored valuable goods), the probability of injury estimates provided by the experts for moderate exposure conditions may be exceeded. This may lead to underestimation of the potential risk associated with such high exposure cases.*

   We will emphasize this in the discussion. Thanks!