

Interactive comment on “Decision tree analysis of factors influencing rainfall-related building damage” by M. H. Spekkers et al.

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General comments

This paper is interesting, and well written. It provides an excellent addition to the literature on flood damage assessment in urban areas. In particular, the use of an extensive insurance loss database is quite unique, and already this would justify publication. Also, the decision tree approach taken is novel for rainfall-related damages.

However, there are a few points that would require attention, before the paper would be suitable for publication. These concern mostly points of clarification and some textual changes, and I provide some suggestions below that I hope are useful.

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Specific comments

Title: why is only “building” included in the title (implying property damage), when also content is studies? The authors could consider excluding the word building, or including the word content.

Page 2265: here I would include not only the notion that there is little research on urban flood damage, but also that most research has focused almost exclusively on flood damage related to large scale flooding of river basins and coasts. This is on the one hand because the problem at these scales is possibly larger, but also because information and data on impacts from urban flooding is very rare, as well as appropriate methods to analyse these. This justifies more research into this area. The authors should add these considerations.

Page 2266: please change text to “cover the whole of the Netherlands”.

Page 2267: “In The Netherlands, almost all buildings are insured” What the authors probably mean here, is that all privately owned buildings. Other residential buildings owned by corporations are not always insured, e.g. housing unions may be self-insured.

Page 2268: Coverage of water-damage is not really relevant for a bank loan, as a property would almost never become severely damage or a total loss because of rainfall. If a bank requires property insurance, than this is because of the fire and wind cover included in the policy, not because of the water damage cover, as the authors state now in the text on pages 2267-2268. Please correct this.

Page 2271: “The housing stock in the reference year 2011 was used” does this not refer to the National Building Register, from page 2272, rather than to the topographical data? Please check.

Page 2272: Can you please explain here that ownership structure in fact simply consists of the share of homeowners?

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Page 2275: It is mentioned that the property and content loss databases could not be linked. But why would one want to do this? Probably, what is meant that it is not possible to link the two sets of damages to the policyholders, or to the same set of variables.

Page 2276: Please explain why only claim frequency was considered in the global regression analysis. I suspect this was because the claim size regressions are more complicated, as explained earlier in the paper, and as shown by other research.

Page 2277: I am not sure what influence the authors infer from the negative correlation here, apart from that the occupants of the buildings are likely different. Another explanation for the negative correlation between household income and real-estate value and claim frequency for property, is that more expensive houses are better maintained and/or newer (see also effect of age²), and income also is related to better maintenance. The authors later also conclude there may be a relation between real estate value and construction quality (page 2280-81).

Page 2279: Regarding the claim size data: can you please explain how these were treated before concluding that there is no acceptable tree? For instance, how were temporal changes in insurance conditions (maximum cover, deductibles) treated? These may have severe influences on claim sizes, and should be considered before constructing statistical models with the claim size data. Finally, were only average claim sizes considered? Or also the median, for instance?

Page 2279/Table 6: I am surprised that the topographic variables do not play a role at all, especially for content claims. What does this imply for management of rainfall discharge (sewerage), and can the authors include policy implications for reducing damage by improving sewerage systems? It would be interesting to test variables related to the sewer system. Although these data are not available country-wide, a test location to assess the effects, and possible scope for reducing losses by improving discharge capacities. Can the authors please discuss this?

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Page 2281: Another important use of these study results for insurance companies is the better assessment of risk premiums in The Netherlands, and also the quicker assessment of actual damages, and also to raise appropriate capital (not too much, not too little) in the event of large losses.

Page 2283: I wonder whether the authors have a point here (and elsewhere) that the district size in some cases may be too large for this analysis. I do not think this necessarily true. Also, the authors could have analysed this aspect, by including a variable on the size of districts. Now this is more or less speculation.

For instance, could drainage/sewer system variables not have an important potential effect (see also above under my point regarding Page 2279)? Moreover, variables on soil type are not included either. Would sand versus clay soils not also have an influence, as well as for instance the share of soil sealed in urban areas?

Page 2284: I do not agree with the conclusion that claim size effects cannot be assessed because of district size problems. The authors cannot rule out the possibility that a smaller district size (if it was possible to construct these from the data, using 6-digit postal codes) does not lead to more randomness and noise, and therefore similar or even worse results for claim size (and possibly also claim frequency). Also, as a number of variables are not included, in particular related to soil and sewerage (see my comments above), it is not possible to conclude this now. Also in the abstract this conclusion should be revised.

Table 3: Which inflation correction of Statistics Netherlands is used exactly? Consumer price index?

Figure 3, Figure 6: The text in these figures would be better readable if the font size was increased.

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