

Review “A GIS based urban flood risk analysis model for vulnerability assessment of critical structures during flood emergencies”

Dear editor, dear authors

It was my pleasure to review this very interesting manuscript. In the paper, the authors present a novel combination of assessments of direct losses, mortality risk, critical infrastructure and emergency operations. The manuscript is very well-written and makes a good contribution to the field of disaster risk assessment.

Nonetheless, I have some critical remarks related to the methodologies, content and structure of the manuscript, which should be addressed before the article can be accepted for publication in NHESS.

Major remarks

1. The scope of the paper is not quite clear to the reader. In the introduction, the authors bring up a range of flood risk analysis tools, including direct damage models and infrastructure disruption methods. The authors note that these ‘traditional standard based approaches’ (what does this mean?) fall short. The reasons that are proposed why they fall short include the fact that ‘dependences and interdependences’ are not included. The authors seem to focus on the fact that road networks and dependencies need to be analysed properly, and see it as their main aim to provide a new methodology that does include such things.

However, if we read the rest of the paper, the authors present a very integrated approach to flood risk assessment (which is interesting). The result section mainly highlights the quantitative results for direct damage and fatalities, and the results for roads and emergency operations are not covered to great quantitative detail.

In addition, the authors state that they focus on direct effects of floods. However, road closure and network effects are textbook examples of indirect effects.

I suggest that the authors provide a more clear scope to their introduction, that should make clear what the current status of integrated risk assessments are (e.g., the HAZUS-MH and Multi-Coloured-Manual models include many of these steps – why are they not enough?) and what this paper is adding. They should stick to definitions and risk assessment literature as is outlined for example in Meyer et al., 2013 and Merz et al., 2010. For clarity, I would suggest to combine the last two paragraphs of the introduction, which now both state the main aim of the paper but in a different way. This would help the reader to grasp better what the paper is doing.

2. I suggest combining sections 2 and 3. For the reader it is very difficult to understand the methods and framework (section 2) without having any information on the case study. For example: can the depth-damage curves that are used (section 2) be applied to the Italian case-study (section 3)? What are the land-use and population data (section 3) that are used to make the fatality risk functions and direct damage estimates (section 2)?
3. The quantitative estimates and validation of the results deserve more attention. The authors currently provide some maps, but no tables of total estimates. On page 2423 they state that ‘The validations performed by comparisons with the case study illustrate the reliability of the

model'. However, this is not backed by the data. Is this based on Figure 8 alone? More evidence should be provided here, and a clearer validation of the results should be attempted.

Specific remarks

1. The concept of 'vulnerability' as used in this paper should be described. The authors use it in a very different way than the damage models they are citing – usually, vulnerability is seen as the susceptibility of the exposed elements (e.g. depth-damage functions represent vulnerability). In this paper, it is seen more as a measure of total impact, I feel, which is OK, but should be made clear.
2. Abstract: add a sentence that makes clear how current approaches fall short, and how this study contributes.
3. p. 2406, l. 21: twice 'significant'
4. p. 2406, l. 20-22: focus explicitly on floods (not 'disasters' in general) and provide some global literature, e.g. Jha et al. 2012 (see below).
5. P. 2407, l. 6: what is the 'traditional standard approach'? Please explain..
6. P. 2407, l. 13-14: it's HAZUS, not HAZUM. Jongman et al. 2010 does not exist (it's Jongman et al. 2012) and is in any case not the correct reference for the Damagescanner model. Check for the correct base reference. And why capitalize all damage model names?
7. P. 2407, l. 17-23: this section is very vague. What exactly do you mean? If you mean that most damage models (based on depth-damage) don't account for indirect effects, that is correct. So that means that you will take indirect effects into account, right? But in several places you emphasize that you provide a framework for direct risk assessment, how does that match?
8. P. 2408, l.2-3. Damage models DO evaluate the degree of physical damage to roads and infrastructure.
9. P. 2408, l.26 onwards: here you start describing what the added value of your paper is. However this is not clear. Is it that you base your model on ' [...] an accessibility and reliability analysis of the road network'? At this point, you should make very clear what you do; to what extent you look at direct and indirect effects; what the results will be; and why it is better than what is already out there (which you described earlier)
10. P. 2409, l. 5: what do you mean with 'dependencies and interdependencies'?
11. P.2411: I would suggest making the hydrological modelling a separate section. Right now it is unclear to the reader what kind of data you used (which discharge, how you computed the '1/30' etc. Also the population and loss of life should have more details. Following my earlier remark (see 'major remarks') I think the methods and data sections should be combined, so you can clearly state which census data you used, which population data, etc.
12. P.2412, l.3: which population data? Source?
13. P.2412, l.11 onwards: which socioeconomic indicators? How is flow velocity incorporated? How does this matter in your case study?
14. Section 2.1.1: which land-use data is used? What source? What's the case study?
15. Section 2.2.1: so, roads are only closed to emergency vehicles because other vehicles block the road? This implies that emergency vehicles can actually operate at water depths of 1 metre, i.e. depths below the height of a car. Is this true? And valid?
16. I think multiple elements are missing in the equations 1 to 4.
17. Section 2.2.2 is overrepresented in the methodology section (which may not be bad) but is not much reflected in the results section. All output indicators that are defined in equations 1 to 4 do not come back as results in the end. Why is that? If they are not relevant, can't you leave them out?

18. P. 2421, l. 14-27: so flow velocity is not incorporated. What does this mean for the fatality estimation? Isn't it a factor there? Also for this section: how is inundation calculated? Is there an assumption on breach locations or levees?
19. Section 4: some basic results are missing: how many people are modelled and observed to be affected? What is the mortality ratio? What is the modelled damage? It would be great to have a general results table.
20. P. 2423, l.15-20: what validation results? Figure 8? How does this support the outcomes?

References

Jha, A. K., Bloch, R., & Lamond, J. (2012). Cities and flooding: a guide to integrated urban flood risk management for the 21st century. World Bank Publications.

Merz, B., Kreibich, H., Schwarze, R., & Thielen, A. (2010). Review article "Assessment of economic flood damage". *Natural Hazards and Earth System Science*, 10(8), 1697-1724.

Meyer, V., et al. "Review article: Assessing the costs of natural hazards--state of the art and knowledge gaps." *Natural Hazards & Earth System Sciences* 13.5 (2013).