Review: Revisions of 'Effectiveness of low impact development for urban inundation risk mitigation under different scenarios: a case study in Shenzhen, China'

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The manuscript has been revised and the authors have replied to all my comments. To some extent, the comments have been taken into account. The manuscript no longer puts the focus on the coupled model itself, which I think is good (as outlined in the previous review: there are several such models). Also, some more explanation about the setup of the model and the effects related to risk levels is given. However, I find the discussion of the results is still superficial. There is no explanation about mechanisms that cause the findings of the paper (impact of LID measures). Also, there is now a new measure (pointed out at one point as a focus of the paper), which is not explained and not discussed. I still think that due to that, the scientific content of the paper is rather lean. It is pointed out in the manuscript that the effect of LID measures is large if some measures are taken but not increased much further if more measures are taken. One could consider that as a finding that is worth publishing. However, one does not learn about the reasons behind this and so cannot really judge. With more discussion one could certainly learn more about the effect of LID measures. I think that revisions are needed for publication of the paper.

- Page 1, line 25: 'Urban-Rural' instead of 'Urban-Rrural'.
- Page 3: I the revised manuscript, the focus is no longer on the coupled model, which I appreciate. However, it is written on page 3 at the end of Section 1 that the reason to present the coupled model used here is that other models (and some are listed) cost money. For this reason, open source models are needed. I completely agree to that and if an open source solutions would be presented this would be a very positive aspect. However, I did not find anything about IFMS as an open source code. SWMM is of course well known and it is easy to find. With IFMS I was not successful. If is it an open source model, it would be good to give reference to where it could be found or obtained. If it is not an open source model, the claim should not be made, or rather, it would then not make sense to write much about the need of open source models, as this is not answered in this paper.
- Page 4, line 8: I think the formulation 'useless nodes and pipelines' is chosen not so well. What is a useless node or pipeline? In reality each pipe was built for a purpose, so in principle no part of the pipe network is useless.
- I would suggest to merge Sections 2.3 and 2.4, as both just describe the model that was applied. The algorithm how water is exchanged between the surface and the pipe network is still not clear to me. Maybe it is not so crucial for the paper, as its focus is not any more on the model.
- Section 2.5: I think it would be good to give some explanation why these two measures were chosen. Also, it is not very clear to me what is meant by the percentages.

It is clear that it means that a certain percentage of all possible GR measures (for example) is considered. But at which locations were the roofs chosen that lead to a certain percentage of all possible measures? I am quite sure that it matters where they would be located (effective close to hot spots, less effective further away). Were they distributed equally over the domain?

- Page 6, line 12: How can one conclude that the cause of the high level is due to severe waterlogging? It might be the cause in reality, but the model will only reproduce causes that are in the model. How were waterlogging conditions implemented in the model?

- Section 4.1: I find it still a problem that the influence of storage is not discussed at all. The effects discussed in the manuscript are certainly caused by the storage assigned to the LID measures. Green roofs have a certain volume. If it is full, no more water can be held back. With more volume, more water could be held back. Permeable pavement gives access to the soil, which is a very large storage. But there is an infiltration rate, so that the storage can only be filled with a certain speed. I think a lot of the effects can be explained with this aspect and it should be discussed.

- The finding that 25 percent of both measures are more efficient than 100 percent of one measure can also be explained with the storages assigned to the LID measures, I assume. A certain storage volume is needed to hold a certain flood volume back. So if measures are taken that provide this volume, the measure is fully efficient. If more is provided, it does not add to further mitigation. In the paper one gets the impression that the fact that less percentage of several measures is more efficient than more percentage of one measure is an unexplainable fact that is here 'found'. In general, the results should be discussed more to get to general conclusions.

- Section 4.3: The cost-effectiveness indication is newly introduced and was not part of the first manuscript. I am not convinced by this indicator. It has a unit that depends on the analyzed quantity and the range would go to infinity if no measures are taken. It is a very non-linear function of the percentage of the measures. This makes it difficult to interpret. If it should be used, it needs to be described very clearly what a certain number of the CEI means. The index comes out of the blue and it is nowhere discussed why this is a good index for the effectiveness of a LID measure.

- Page 9, lines 4-5: I do not see how the conclusion can be made that the model can be used for different cities and countries. In principle, of course any model could be used, but if the meaning is just that, the sentence is trivial. I would delete this sentence.