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Discussions

Interactive comment on "Evaluation of Low Impact Development and Nature-Based Solutions for stormwater management: a fully distributed modelling approach" *by* Yangzi Qiu et al.

Anonymous Referee #2

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General comments

The paper describes how a hydrological model is used to forecast the effect of implementing a number of LID/NSB practices in a model of an urban area. Such studies have been performed before (as reviewed in the introduction). The authors identify two shortcomings of such previous studies, namely (1) that they do not (or only in a limited way) consider the spatial variation of land covers in urban areas, and (2) that they do not consider the spatial variation of rainfall. The study then presents a model setup with distributed land cover information and spatially distributed rainfall, and finds that changes in the land cover (implementing porous pavements, rain gardens, or green

roofs, or combining all three of these) result in reduced runoff rates and volumes.

My primary concern with the paper is that it does not present a new contribution to scientific progress. The current paper only uses a somewhat more complex model to arrive at similar conclusions as earlier papers. The paper does not actually address what the effect of using a fully distributed modelling approach is, in relation to the two shortcomings of non-distributed approaches identified in the introduction. (E.g. does the fully distributed model give significantly different results than other approaches? If differences are found, can these be attributed to the distributed land cover data or the distributed rainfall data? Is the magnitude of the differences between the methods so large that it justifies the use of the more demanding (in terms of data and user effort) fully distributed approach, in relation to the uncertainties that will exist in any forecast?) For the model to be a useful forecasting tool, it would need to be shown that it provides more accurate and precise forecasts than simpler methods. The paper does show that results vary between three different subcatchments; however, since these subcatchments appear to be 1-3 km2 large (if the exact sizes are given I missed them), this does not help demonstrate any added value of a model with a much finer (10 m) discretization.

The three main conclusions of the paper do not seem useful or valid to me:

1. "The results illustrate that implement LID/NBS practices can significantly reduce the urban runoff." This has been reported many times before, and it is unsurprising, given that this is the behaviour that would be expected of the model (and in fact this is what LID facilities in models are designed for), so this is not a particularly interesting finding either. The forecast effects of the LID measures are not validated against any measured data. Although I understand that this would be difficult to do, this does not change the fact that, without such validation, it cannot be judged whether the proposed approach is actually a useful forecasting tool.

2. "In the whole catchment, each LID/NBS scenario is more effective in two stronger

but short events.” I do not understand what is meant here.

3. “In the sub-catchments, the significantly different hydrological responses of LID/NBS scenarios indicate that their performance is influenced by the coupling effect of variability of spatial distributions of precipitation and land uses (e.g., the rainfall amount, rainfall intensity, proportion of LID/NBS practice).” I do not think this is supported by the results of the paper. It seems obvious to me that different subcatchments with different characteristics will respond differently to the implementation of LID measures. The effect of representation of spatial variability of land cover and precipitation is not actually tested in the paper.

To me, it does not seem feasible to address the issues above in a modified version of the manuscript, as it would rather require a whole new study. (As outlined below, there are also some more specific shortcomings/question surrounding the chosen methodology.) Therefore I recommend this manuscript be rejected by HESS.

Specific comments

The manuscript would require good language editing, as it currently contains many grammatical errors.

L72-73: if some research does use more detailed data (as this sentence states) then this is the most relevant literature to be reviewed in the introduction, yet no references are given!

L127: the resolution of the DEM is coarser than that of the model. This limits the value of having the model at that resolution, and it may also lead to problems with the surface runoff module. Is there no higher resolution DEM available?

L133-138: Using only three sampling points for soil classification may be too limited. Although it may be appropriate for the deeper soil layers, studies have shown that urban areas have a high degree of spatial variability in the top layer of the soil and/or the infiltration capacity. Combining a fully-distributed model with uniform data runs the

risk of getting the worst of two worlds, i.e. lots of work to set up the model, but not actually using more information than coarser modelling approaches.

L163-L178: the proposed scenarios assume that LID measures are implemented on all suitable surfaces, is this realistic? When LIDs are assumed to be implemented on all usable surfaces, the comparison between their effects on runoff may not be that useful, since other relevant factors (e.g. installation cost, operating cost, social acceptance, other physical limitations) can be different for the different LID scenarios.

L172-L175: although steep roofs are typically unsuitable for green roofs, green roofs may have gentler slopes.

L180-186: The proposed modelling approach is rather detailed, but it is evaluated only based on peak flow and total runoff volume. Is such a detailed modelling approach really needed/justified if these rather simple metrics are the quantity of interest? Alternatively, are there other relevant metrics where the benefit of the detailed approach would be clearer?

L204-206: although the non-calibrated model appears to function for the first two events (figure 8), there is a major deviation for the third event. Wouldn’t it be better to calibrate the model further so that it better simulates the catchment behaviour? After all, the model being accurate for the current situation is a fundamental requirement for putting faith on its forecasts of the effect of changes in the system. The NSE values for the third event should be checked as (based on the graph) they appear to be quite high given the large deviation during the last 20-25% of the event duration.

L212: A model result with a NSE close to 0 is not “credible”, see e.g. Moriasi et al (2007). After all, 0 is the score that would be achieved by the average of the observations, which is not a particularly strict (albeit a commonly used) benchmark.

Moriasi, D. N., J. G. Arnold, M. W. Van Liew, R. L. Bingner, R. D. Harmel, and T. L. Veith. ‘Model Evaluation Guidelines for Systematic Quantification of Accuracy in

Watershed Simulations'. Transactions of the ASABE 50, no. 3 (2007): 885–900. https://doi.org/10.13031/2013.23153.

Technical corrections

The scale of the y-axis in the graphs leaves a lot of empty space. It would be better to use this space to show the data in more detail, as it is now difficult to see differences between the different lines.

Given the major shortcomings of the paper outlined above, I will refrain from spending too much time on small issues here.

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