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Comment on hess-2021-32

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Referee comment on "Can the implementation of Low Impact Development reducebasin runoff?" by Xinxin Sui and Frans van de Ven, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-32-RC1, 2021

Review of the manuscript entitled: "Can the implementation of Low Impact Development reduce basin runoff?" by Xinxin Sui and Frans van de Ven

In this paper a modeling study on the potential impact of various scenarios of future urban expansion and various scenarios of low impact development interventions in a case study site in the catchment of San Antonio in Texas, USA is presented. The concept of the study is really interesting and within the scopes of HESS journal. The study also involves laborious work and an interesting case study site.

The main novelty of the paper is related to the investigation of the case of a large urban area located at the downstream part of a rural watershed. Several urban expansion scenarios and LID practices application scenarios are investigated. Accordingly, the study could potentially provide interesting information for urban planning. However, the most prominent findings of this study; i.e. that LID may result in increased peaks and that a more compacted and denser urban area may result in decreased peak discharges; are somehow controversial. The reasoning behind them is logical and I can understand that under very specific conditions this could be possible. However, the key question is what is the probability of this to happen? Is it a rare coincidence or a typical outcome for big storm events in the case of a large urban area located at the downstream part of a rural watershed? I believe that the methodology followed in this study cannot safely answer this question for two main reasons.

The first reason is related to the model used in this study. The model in the manuscript is described as semi-distributed. As I can understand by reading the manuscript, in reality it is a simple, cascading buckets, lumped model applied in the entire watershed as a whole with the same lumped parameters values for the entire watershed. Probably the model is described as semi-distributed because it involves different representations for the various parts and processes (rural, urban, LID). The model seems to perform well anyway, even if the calibration and validation procedures are not adequately presented. Accordingly, this model could be adequate for other types of studies. The problem is though that the model (as I can understand) was calibrated and validated only for the current conditions (without LID). The model applied for the LID scenarios involves more components (lags and storages) and the involved parameters values were assumed from the literature, which is really challenging and uncertain. Therefore, given that the model is lumped and empirical, its performance for the future conditions and especially for the magnitude and the timing of the flow components is questionable. Accordingly, the key finding of the study that LID may result in increased discharge peaks cannot be justified by the model results as the concurrence of the runoff peaks coming from the rural and the urban parts of the watershed due to LID could be a coincidence considering the involved uncertainties. For example, in reality parts of the urban watershed may deliver runoff to the river faster and other parts slower depending on the location of the various LID components and the resulted peak will be the mix of spatially distributed processes.

I would also like to mention at this point that while for the justification of the presented mechanisms for peaks increases, the studied events hydrographs are presented in detail in some figures, the performance of the model for the same events isn't clear as only some general performance metrics for the entire time series are presented. Considering the emphasis that is given to the timing and magnitude of sub-event peaks in this study, I would expect a more detailed presentation of the model performance on predicting these events hydrographs. Accordingly, model calibration, validation and performance at event scale should be described in more detail and a figure (and some metrics) presenting model accuracy at event scale is required.

The second reason is that the model is applied for a short period including a small number of flood events that are analyzed to provide the above-mentioned conclusions. Accordingly, it isn't possible to understand if these conditions are typical or limited to the investigated events.

Considering that the title of this paper is "Can the implementation of Low Impact Development reduce basin runoff?" the reader expects that the applied methodology should be able to provide a clear and justified answer.

Another important weakness of this study is related to the presentation of the methodology. There are many unclear parts and some other parts are not described at all. I believe that the model structure and the procedure followed to decide it should be presented more clearly. Most important, the identification of the parameters values for the model describing LID practices should be presented in detail and justified. In the manuscript there is just a mention of some previous studies. It should be also described in detail how the model was calibrated and validated (data used, methods, locations etc). Finally, a paragraph describing the model application, the data used, the modeled events characteristics etc. and the methodology and the steps followed for the analysis of the results is required.

Finally, a general weakness of this paper is that the results and the main findings are not discussed in the light of previous relevant studies in the results section, and most importantly in the discussion section. There are numerous previous studies on the topics presented in this study but there aren't any citations and real discussion in the discussion section or in the results section.

Apart from the above main comments the language includes many errors and should be improved. I am not a native English speaker though and therefore I avoided providing specific suggestions on this matter. The presentation, figures etc. are generally good. There is only a problem with the definition of symbols and abbreviations that makes a little difficult to understand some parts of the methodology especially the related tables.

I am providing some additional specific comments in a commented version of the manuscript.

Based on the fundamental weaknesses of the paper that I explained above I am not convinced that this paper can be adequately improved to be suitable for publication. However, I have some reservations because some parts of the methodology presentation were confusing or missing and therefore there is a chance that I wasn't able to accurately understand what exactly has been done in reality and to effectively evaluate the manuscript. Accordingly, as the concept and the main idea behind this study are very interesting and it involves laborious work, I am recommending a major revision to provide the authors the opportunity to respond to my main concerns and to address the key limitations of this study.

(Please also see the attached pdf file)

Best Regards

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Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2021-32/hess-2021-32-RC1-supplement.pdf