

# ***Interactive comment on “Subcatchment characterization for evaluating green infrastructure using the Storm Water Management Model” by Joong Gwang Lee et al.***

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Please find author's direct responses for the Referee-1 in Supplement (Responses for Referee-1.pdf).

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Dear HESS Editor,

Before addressing the specific reviewer comments we want to re-emphasize the novel aspects of our research. We arranged a modeling approach using SWMM with the primary consideration of simulating GI scenarios for urban watersheds. Emphasis was

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placed on the accuracy of the physical representation of the watershed landscape with respect to land cover types and their role in stormwater runoff dynamics. Given the nature of GI designs we consider this a necessity for model simulation. This differs from the more common (or conventional) context for which SWMM has served the urban drainage community, that has been to support drainage network design that minimize flooding risk with emphasis on more centralized and larger sized stormwater management structures. This overall objective is well explained in the Introduction. The subsequently presented novel (or innovative) aspects of our research include: 1) the introduction of a new concept, buffering pervious area (BPA). 2) The study of several different ways of parameterizing the subarea hydrologic connectivity within a SWMM subcatchment, of which we present the performance of 6 different options. Rather than applying each option at the full scale for our case study system we used a hypothetical-unit area approach. This greatly simplified the presentation of the differences among the options while still supporting our major findings and allowing us to explain the rationale for recommending one of the options for GI modeling in SWMM (Option 6) that we go on to demonstrate in the set-up of our SWMM model of the case study watershed. 3) The configuration of a land cover based parameterization framework that is implemented through a supporting spatial database. This explicitly accounts for changes in hydrologic connectivity that result from GI scenarios in the model set-up. While we have no way to prove that this improves the hydrologic simulation performance of the model, it does account for changes in the model set-up that reflect more the real changes to landscape hydrologic properties that would have to be ignored all together, or at best, lumped at a relatively large spatial scale to be represented using more common practices for SWMM set-up. Finally, 4) we present the value of flow hydrograph separation (Figure 13) while evaluating GI scenarios. The volumetric separation provides insight to the potential effectiveness and rationale for developing strategies for GI in a small watershed.

One general aspect of many comments we noted from both reviewers but primarily Reviewer 1 was the request for additional details and specifics on the use of GIS and

SWMM software to implement our approach. To keep the paper a reasonable length (for readability), we reduced some of the level of detail presented from earlier drafts and following suggestions of internal reviewers before we submitted to HESS. For example, the use of ArcGIS to clip, buffer, and intersect, we felt that these details are well known to standard GIS practitioners. However, we realize that these may not be as apparent to traditional hydrologic modelers. Therefore, we have prepared a “hands on” compilation of the detailed procedures with screen-shots that will be released separately through the US EPA as a standalone document in the coming weeks. This report can be referenced in the revision.

Our general interpretation of the Reviews is that Reviewer 1 had little issue or concern with the integrity of the approach presented. Conceptually it all made sense to him/her, but an unfamiliarity with ArcGIS and SWMM software made some of the explanations seem ill-explained or confusing. We had to try to strike a balance between providing highly detailed descriptions of software use while not losing sight of the main theme of demonstrating the utility of adopting the approach to SWMM set-up for GI modeling. We note in the address of each of Reviewer 1’s comments where we will amend or adjust the MS to help account for most of their questions.

Reviewer 2, on the other hand, had some concern about the conceptual legitimacy of the approach. He/She was not convinced that the methods we develop and demonstrate have utility over a “conventional” SWMM modeling approach. However, Reviewer 2 did not provide specifics on this qualification of “conventional”, so it made responding directly to their comments difficult. We addressed these the best we could based on our interpretation of their main points of concern. We note that in our presentation we never stated explicitly that adopting our approach would result in better hydrologic performance of the SWMM model. While we do expect this to be the case for GI simulation, specifically, we have no way of actually testing this because we lack data on the effect of GI on hydrology post implementation. What we can say exactly about our approach is that it allows for a more realistic expression of reality in the SWMM model

set-up. This should make the model output more accurate, but again, because we have no way to directly test this assertion we will be sure to 'tone-down' such implications where they may exist in the MS.

One aspect that Reviewer 2 (and Reviewer 1 also had a question about) was correct to point out was the potential inaccuracy in our assessment of what adopting our approach means for the effort placed during model calibration and the scalability to larger systems. Our claims in this regard were based on our experience modeling the case study system, which was relatively homogeneous with respect to topography and land cover spatial distribution. So, our approach accommodates GI scenario analysis and likely will reduce the effort required for calibration, but this depends on how homogeneous the landscape characteristics are in the project area. As Reviewer 2 points out, in many urban systems heterogeneity in topography alone could result in spurious modeling results if our framework was adopted as presented. This doesn't mean that the framework is incorrect, just that a higher order classification of subcatchments would be necessary. This can be accounted for in the configuration of the spatial database, and we will add this to the revision of the MS. Also, if a higher order classification for subcatchments is necessary this means that the parameter considerations during calibration increases beyond the dramatic reduction that we noted as a significant result of adopting our approach in the original version. We will correct these statements, as while they were true for our case study system, they might not be completely true for all systems. Either way, though, it is true that adopting the approach does reduce the number of parameters that may have to be considered during model calibration, which is an advantage. The disadvantage is the level of effort required in setting up a spatial database to support the modeling. Finally, we think Reviewer 2 misunderstood the overall point and discussion of our hypothetical unit area analysis. A few of the questions and comments here seemed to convolute the unit area analysis with the calibration of the SWMM model of the case study watershed after applying Option 6 for its setup. We will try to clarify the differences between these two, largely independent analyses, in their presentation in the MS.

Overall the questions and comments provided by these reviewers will result in several improvements in the MS, mainly by forcing some clarifying statements here and there and some adjustments to figures. They are much appreciated, and we hope that you will still consider the presentation of our research a worth contribution to HESS.

Sincerely, Joong Gwang Lee Christopher T. Nietch Srinivas Panguluri

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-166/hess-2017-166-AC1-supplement.pdf>

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