Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-367-RC1, 2020

Hydrology and Earth System Sciences
Discussions
Open Access

Interactive comment on "A Framework for Automatic Calibration of SWMM Considering Input Uncertainty" *by* Xichao Gao et al.

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Received and published: 4 November 2020

General Comments:

This article correctly identifies a knowledge gap in rainfall-runoff models wherein calibration typically considers uncertainty only in the model's parameters, neglecting the uncertainty in forcing data. Additionally, to the best of my knowledge, tying the DREAM calibration methodology to SWMM using a modified PySWMM API is a new and useful contribution. For these reasons, the research that the author's propose is of great interest to the hydrologic community.

However, the work done in this paper towards addressing that knowledge gap is unconvincing. The authors aim to "settle [the] problem" of rainfall uncertainty being left

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out of the calibration process. Yet, at the end of the paper the problem remains unsettled and their arguments undefended. Deficiencies in the simplistic case study and calibration approach call into question the conclusions that the author's method definitively demonstrates the dominance of forcing data uncertainty in the calibration process. The rainfall uncertainty model itself does not provide convincing evidence for the true magnitude of the rainfall uncertainty issue; their calibrated rainfall model implies an underestimation of rain depth greater even than the worst case described in the literature. Given the implications of the assertions posed in the introduction, and the lack of convincing evidence to support those assertions, the contribution of this research to the hydrologic community is hard to justify.

Specific Comments:

In Eq. 3 the authors offer the Sum of Squared Residuals as a common objective function for minimizing the residual. In the results, though, the authors swapped the SSR in favor of the Nash-Sutcliffe Efficiency Index. Why? The Nash-Sutcliffe Index is well known to have deficiencies for optimizing hydrograph behavior, and the author's calibration attempts in the case study seem to fail both this metric and the eye test. I would recommend using more than one objective function to help ameliorate the drawbacks of calibrating to any one function. The authors admit that the peak flow was poorly captured by the "parameter-only" calibrated model; it may be beneficial for another objective function to be targeted at that hydrograph feature.

Peak Difference=|Peak_sim - Peak_obs|

The poor calibration of a relatively simple case study system does much to undermine the researcher's conclusions on the impact of the rainfall uncertainty. Another option would be to allow for parameters to vary for each subcatchment independently. This increases the dimensionality of the calibration problem 34-fold, but the MCMC methodology within DREAM, and the pre-processing sensitivity analysis, make this reasonably achievable.

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The preponderance of my experience in calibrating SWMM has been focused on the subcatchment parameter set (credit to the authors for identifying the myopia of the community on that front). However, I find it hard to believe that the two options for considering rainfall uncertainty are an additive correction factor and a multiplicative one. This simplistic approach feels like less of a foray into an unexplored frontier of calibrating rainfall-runoff models, and more of just adding another parameter to be calibrated in the same manner as any other.

From my understanding, a narrow posterior distribution for a parameter is evidence of a high confidence in that parameter's optimal value. The conclusion from Fig 4 that "most parameters are approximately Gaussian" is puzzling. Rather than relying on the "approximately" disclaimer, why wasn't a statistical test to demonstrate Gaussian-ness performed.

I remain vexed as to the purpose of Figures 6 and 8. I wish they were explained in the text rather than just referenced. I don't have any clue what constant vs varied mean in either Figure's context. The authors claim that the narrowness of the 95% confidence interval band around the median hydrograph "confirms the existence of equifinality". That seems logically inconsistent. Equifinality describes the phenomenon that multiple parameter sets might yield the same hydrograph. It does not suggest that every parameter set will yield the same hydrograph. If anything, that observation calls into question the initial sensitivity analysis.

The general aim of this paper was to demonstrate that the uncertainty introduced by forcing data is a significant contributor to the uncertainty in system behavior. However, seeking to prove that by showing the improvement of a calibrated model by adding a rainfall multiplier as another calibration parameter presents a catch-22. The better that the initial calibration is, the less impactful the rainfall calibration will seem. The author's conclusions are better supported the less carefully the system's parameters are calibrated. It would be a much more compelling paper structure to compare the calibration refinement from a rainfall uncertainty model vs another weakly studied uncertainty

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source.

Technical Corrections:

To restate a previous question as a suggestion. Consider using metrics beyond the NSE to evaluate the calibrated solutions.

In Section 2.5, the linkage between DREAM and SWMM is described. The steps enumerated don't add any description value to Figure 1. Rather than a regurgitation of the Figure, I'd be interested to see some of the other questions I had be answered in the delineation of this workflow. Such as, is there any difference in the workflow for considering just the subcatchment parameters and the combined forcing/subcatchment parameter sets?

There are a number of capitalization/grammar mistakes. Such as The last sentence in the abstract. Capitalize "The" "3.3 parameter sensitive analysis" should be "3.3 Parameter Sensitivity Analysis"

The parenthetical citations seem. . . wrong.

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