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Discussions

Interactive comment on "Comparison of performance of tile drainage routines in SWAT 2009 and 2012 in an extensively tile-drained watershed in the Midwest" *by* Tian Guo et al.

Anonymous Referee #1

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This paper aims to evaluate the performance of new physically based tile drainage routines proposed by Hooghoudt and Kirkham. The study is conducted in a small watershed (518 km2) in the Midwest USA. The main objective is to compare simulated flow, tile flow, runoff, nitrate in tile flow and sediment load results for the new tile drainage routines in SWAT2012 and the old one in SWAT2 009 in the LVR watershed and determine which routine provides a better model fit with observed values. Testing of the new routines and identification of parameter sets is given as the primary motivation for this research. In my opinion, the given motivation and objective add very little to the scientific knowledge, thus, do not merit publication in HESS Journal in the current form. The authors claim that the parameter set obtained from this study pro-

vide guidance for field and watershed level applications. In fact, this is not a new and significant finding. Moreover, author do not provide any discussion on physical basis of the selected parameters. Neither differences due to spatial scales are mentioned. Some of the parameter values are also hard to understand, for instance, the range of snow fall and snow melt parameters seems too large (-5 to 5 oC). From physical process point of view, it is hard to explain why these parameters are so different in such a small and mildly sloped watershed? To mention another example, why fitting values of SURLAG differ between sites (how scaling in hydrology may guide explaining this?). Similar can be said for other parameters like curve number, sediment and nitrogen related parameters. Therefore, the currently presented parameter sets adds very little to the available knowledge. A critical discussion on the fitted parameter values, at least explaining physical process related reasons and issues of spatial scales, is recommended. Another major problem is difficulty in following the structure of the paper. Presentation of calibration and validation results for each site demonstrates lot of repetition. This obstruct clarity and the readers could soon start feeling bored as same information comes again without any new insights and deeper discussion. One way of rectifying this issue could be by fully restructuring the paper. For example, results can be separately presented for each indicator (crop yields, flows, sediment, and nitrate) rather than per site. This can also facilitate physical explanation and scale issues when results of all sites for one indicator are combined together. For instance, when it comes to peak flow or runoff simulations, one can see where it was simulated well, at R5 or B or E etc, and then what could be the governing factors (geography, tile drainage density, variation in hydraulic conductivity, effect of CN etc). Although the study mentions previous research on testing the new tile drainage routine, the results of this study are not compared with the previous findings. A detailed comparison with the previous studies would help to understand and position this work much better. While doing so, the authors should at least include topics related to parametrization, characteristics of the studied watersheds, performance evaluation results. Additionally, some very useful comments are made by S. Mylevaganam. In general, I see them valid and constructive (though critical) and could be helpful for improving the manuscript.

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