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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A pre-calibration approach to select optimum inputs for hydrological models in data-scarce regions" *by* E. Tarawneh et al.

Anonymous Referee #2

Received and published: 7 July 2016

A: GENERAL COMMENTS

The manuscript investigates sensitivity of the SWAT model in Wala catchment in Jordan. The main idea of the article is to develop a framework to test the effects of various data sets in hydrological models to support water management and planning in data scarce regions. The results also support to identify gaps that need to be filled by e.g. improved monitoring. For this purpose the authors tested extends of errors in predictions due to the use of different types of input data to SWAT. They developed eighteen hydrological models (using combination of three land use maps, two soil maps, and three climate time series from the local and global sources), and they evaluated the models using measured monthly discharge and constructed sediment yield data at the

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outlet of the watershed. The authors showed that significant performance gain can be obtained with the proper combination of inputs. They conclude that selection of quality data will reduce uncertainty of hydrological model outputs.

The subject falls within the general scope of the journal. The aims of the study are interesting for the readers of the HESS. The obtained results appear encouraging. It is however strongly advisable to extend the following points in the paper:

1. This study tests relative quality of the existing datasets from local/global sources to support the statement in “Page 2, L22-29” on reducing uncertainty in data poor regions when transferring parameters/knowledge from neighboring or geographically similar catchments. While the general idea is very interesting, but I felt the main hypothesis never tested. I think the authors can improve novelty of the work by quantifying how the traditional knowledge (parameter) transfer from the neighboring watersheds (e.g., using calibrated parameters of poor model-scenario in this study) versus parameter transfer from a better model (using quality data model-scenario in the study) help reduce uncertainty in model predictions of the data poor regions. One would expect that this could be done by classical sample test in small portion of the watershed.

B. SPECIFIC COMMENTS

1. Authors may provide more background about previous SWAT applications in the study region (if published in any peer reviewed journals or reports that are available for public); and also strength and limitation of the model in behavior simulation of the major hydrological events in such arid environment with intense, highly intermittent, and often localized storms. Authors may add this in the “model selection” and “discussion” parts. Authors emphasized on the importance of input data uncertainty but never discussed other sources of errors in hydrological modeling: e.g., model structure (process simplifications, which might be case in this work), and parameter estimations.

2. Page 6, L23-24: How the two weather stations (Qatraneh and Errabbah) represent climate conditions in the study area? If the stations are not representing actual condi-

tions, the generated data (to fill the gaps in the recorded time series) will be subjective, and as a result poor hydrological performance will be obtained (it is seen also from the results).

3. Page 6, Section 3.5: How the HRUs were defined? Please indicate if you used dominant or multiple (/threshold?).

4. Page 7, Section 3.7: In this study the sediment yields are not measured but estimated using the streamflow data. This cause a subjective comparison results when testing different scenarios in this study: (i) It is obvious from the results that model-scenarios that perform better in the simulation of streamflow, present higher performance in modeling sediment too. Therefore, any judgment on the performance of the input data in model simulation will be subjective. (ii) The inherent errors in the estimated sediment yields may be compromised/offset by the model prediction errors due to less quality input data, resulting in a wrong conclusion in scenario selection. The authors may provide a background on how the sediment data were estimated and how the above mentioned points may be justified when evaluating model-scenarios in this work.

5. Page 7, L 29-31: The strong relationship between Q and T should be explained. The provided reference is not enough and not available for readers.

6. There are many studies suggesting multi gauge evaluation (rather than single outlet comparison) in spatially explicit hydrological modeling, to prevent the spatial errors in the upstream catchments that may offset and not observed in the outlet. If the gauge data are not available in other tributaries of this study to conduct multi-gauge evaluation, at least the importance of this effort should be highlighted to also support the statement on Page 3, L3-5, on the investment efforts to improve the limited data.

7. Authors may add three other layers of data into Fig. 5: (i) river network; (ii) streamflow station (Wadi Wala) where the models were evaluated; (iii) Wala dam. With the geographic coordinates only, it is hard to understand the system.

8. Page 7, L27-28: The authors evaluated their model-scenarios using the discharge data of Wadi Wala station that is located downstream of the dam location. How the operation of dam and the effects on hydrological regime in downstream station was considered? Authors may explain if the operation of dam was simulated in the model.

9. Page 7, L32: The "parameterised" SWAT model is not clear. Does it mean that authors make change in model parameters (for any calibration purposes?). If so, it contradicts with several statements in the text that that the evaluations were performed prior to calibration.

10. Page 7, L32: SWAT model operates on "daily" not monthly or yearly or seasonal. Please rephrase the sentence. Authors may aggregated data from daily to monthly etc.

11. Page 9, L7-8: "NSE drops ... and all cases". These are interesting results. Authors may explain why CFSR data performed better than local observations. Are the findings consistent with other studies around the world that applied CFSR in hydrological models?

12. Page 9, L11-13: usually a better performance is expected when using locally produced high resolution maps (e.g., the one from Al Bakri). Authors may explain the possible reasons why the course resolution map of global source performed better in this study.

13. Page 10, section Weather data: Please refer to my previous comment in this section (comment #2).

14. Page 10, L12-16: (i) When looking at the top left graph in Fig. 9, the high performing statistics are usually representing low flows, while high flows are almost completely underestimated (or not predicted at all). How the authors will consider both low flows (important for drought) and high flows (major events and important for flooding and water saving) explain in their model evaluation and scenario selection? (ii) As indicated in several places in the text, the Wala basin is located in semi-arid area that is prone

to intense (sub-daily) rainfall events. My concern is that how model evaluation at the monthly scale will ensure representation of locally important short-term events? (iii) As mentioned previously, the data are compared at downstream station that might be affected by operation of a dam.

15. Page 11, L13-22: Another reason is the use of statistics generated from inappropriate weather stations: please refer to my comment in this section (comment#2)

16. Page 11, section 4.3: It would be interesting to apply the calibrated parameters of the model-scenarios to support the main idea of the study on how appropriate model setup and quality data help managers of data scarce watersheds when transferring knowledge (e.g., parameters) from neighboring watersheds. Please refer to my general comment.

C. TECHNICAL CORRECTIONS

1. Authors may keep the acronyms consistent: either provide full definition for all of the NSE, RSR, and PBIANS; with the acronyms in the bracket; or provide acronyms only. 2. Page 3, L3: "..quality and/or quality" : "..quality and/or quantity". 3. Page 2, L6: "..with extremely ... and" is repetition of previous statement. Either remove this sentence or rephrase. 4. Page 4, L16: "basin characteristics" are NOT represented by the USDA CN method, but the "surface runoff' is simulated. 5. Page 24, Fig. 5: for the ease of visual comparison, I suggest using similar coloring pattern for the same LU classes in the three maps. 6. Page 6, section 3.4.3: the authors may move this short paragraph to section 3.3, where you first discuss the DEM in the study area. 7. Page 7, L23: "Running SWAT ...for full description": statement does not fit in this section. I suggest to move it to the section 3.2, where the authors introduce SWAT model, or to remove it. 8. Page 30, Fig. 9: Titles and legends are too small. Only scenario 16, and scenario 2 are presented in the figure, while the caption indicates scenarios 13, 18, 5, and 3 as well as 16, and 2. Please carefully check the figures, captions, and discussion in the text to match these three parts. 9. Page 30, Fig. 9: the explanation of

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the sediment yield graphs are missing from the caption and text. 10. Page 31, Fig. 10: please provide high quality graphs with consistent size and formatting, and consistent scale in the vertical axis to help comparison of the graphs. With the current format it is hard to read and compare the graphs.

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