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Hydrology and Earth System Sciences Discussions

Interactive comment on "Distributed hydrological modeling in a large-scale watershed of Northern China: multi-site model calibration, validation, and sensitivity analysis" *by* S. Wang et al.

Anonymous Referee #1

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Anonymous reviewer recommendation – "Distributed hydrological modeling in a large-scale watershed of Northern China: multi-site model calibration, validation, and sensitivity analysis" By S. Wang, Z. Zhang, G. Sun, P. Strauss, J. Guo, and Y. Tang

The authors demonstrate that using the multi-site calibration can have better simulations of streamflow compared to the single-site calibration in a distributed hydrological model, MIKESHE. They use three different kinds of model performance criteria to evaluate the model results; they calibrate at the period of 1991-1995, and validate at the period of 1996-1999. Generally, when more information are used to constrain the

model, the model should have better performance and the authors try to demonstrate this fact. However, the results in this manuscript seems plausible to me. More works need to be done before it can be published. Below are some comments.

Major comments:

1. In the multi-site calibration (section 3.2), the authors change the value of Ks from 2e-6 to 4e-6, but this parameter is fixed in the single-site calibration. Maybe the authors want to add this calibration parameter in the single-site calibration as well. By doing so, the comparison will be more consistent.

2. Both single- and multi-site calibration have the problem when simulating the low flow conditions. I suggest the authors use some criteria having the "LOG" formula that can better constrain the low-flow conditions. Both EF and RMSE focus on the "peak flow" instead of low-flow and they basically tell the same ranking so the authors may consider that just use one of them, and the other criteria use some cost function with "log" formula, such as "log(obs-model)".

3. Is there any figure showing the spatial distribution of soil depth? In p5709 line 9 to 16, the authors say "Since the northern of the Chaohe watershed is adjacent to the Inner Mongolia Plateau, the northern part of the watershed is commonly characterized by high soil water storage capacity due to the deep soil profiles, which caused much of water stored in the unsaturated zone available for recharging the groundwater and discharge the river flow subsequently. However, in the middle and downstream area of the watershed, the thin soil profiles resulted in small soil water storage in the unsaturated zone and less recharge to the saturated zone,"

I think if there is one figure showing the spatial distribution of the soil depth, it will be great for the readers. Because the authors try to increase the Ks to increase the soil water storage, so it's necessary to let the readers know what is the spatial difference of the soil depth.

4. In page 5710, line 15-19:" Generally, the model was insensitive to the parameters of ET and overland flow, but was sensitive to the parameters of unsaturated zone and saturated zone modules (Table 3). This indicated that process of ET and overland flow were less important in affecting streamflow generation of the watershed, whilst unsaturated flow and saturated flow played an important role."

This statement has some problems (especially I highlight in red). The reason could be due to the chosen of ET parameters are not sensitive to ET, but it doesn't mean that ET is less important in affecting steramflow generation. On the other hand, it doesn't mean the unsaturated flow can play an important role.

The authors need to future work on this part. Maybe some time series of ET, streamflow, and so on.

5. Finally, the most important one, from table 2, I really can't see the benefit of using the multi-site model calibration when compared to the single-site calibration. Especially for Dage station, the RMSE doesn't increase for the validation period, and the R even decreases for the calibration and validation period.

Hence, the last sentence of the conclusion is not convincing at all that "Multi-site model calibration protocol would greatly further reduce the modeling errors resulting from the inherent great spatial variability", which is the main conclusion of this paper. Therefore, I suggest the authors should focus on this topic in the revision.

Again, the key issue is on the low flow simulations in the Dage station, so I suggest that the author should use other statistical criteria (as the above comment #2) to make this statement more convincing.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5697, 2012.