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Interactive comment on "Hydrologic-Land Surface Modelling of a Complex System under Precipitation Uncertainty: A Case Study of the Saskatchewan River Basin, Canada" *by* Fuad Yassin et al.

Anonymous Referee #1

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General Comments This paper describes the deployment of the MESH model, including assessment and selection of precipitation forcing, model calibration and model evaluation. The procedure is described using the Saskatchewan River Basin in western Canada as a case study. Although the description is thorough and clearly written, the work and methods described lack novelty. The results show that the chosen methodology simply works, not that it is an improvement (in terms of accuracy and/or efficiency) over some benchmark or baseline approach. Hence, the submitted work does not offer anything new to the science (theoretical or applied) of model development, calibra-

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Discussion paper

tion or validation. My overall evaluation is summarized as follows: 1. Does the paper address relevant scientific questions within the scope of HESS? No 2. Does the paper present novel concepts, ideas, tools, or data? No 3. Are substantial conclusions reached? No 4. Are the scientific methods and assumptions valid and clearly outlined? No 5. Are the results sufficient to support the interpretations and conclusions? No 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes 8. Does the title clearly reflect the contents of the paper? No 9. Does the abstract provide a concise and complete summary? Yes 10. Is the overall presentation well structured and clear? Yes 11. Is the language fluent and precise? Yes 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes 14. Are the number and quality of references appropriate? Yes 15. Is the amount and quality of supplementary material appropriate? Yes My recommendation is to reject the submitted work. Given the decision to reject the paper, in the following I will focus on major issues only.

Specific Comments The methodology for choosing the 'best' precipitation product is not defined and is, therefore, not reproducible. Although several evaluation metrics against ground-based observations (precipitation gauge and streamflow) are used, it is unclear how these results were combined and used to rank objectively the various products. As it currently stands, the choice of CaPA as superior to all other products is purely subjective.

The logic of ranking the accuracy of precipitation products by filtering them through and un-calibrated H-LSM to compare to streamflow observations is flawed. For this approach to be entirely valid, one must accept that errors in the precipitation data are propagated identically through the H-LSM for each product, independently of the chosen model parameters. Given that each precipitation product has different error characteristics (bias, magnitude, amplitude variation, seasonality, etc.) it is plausible that the streamflow accuracy obtained via various precipitation product is, to some degree, conditioned upon the choice of 'default' parameter values (i.e. parameters may have default values, but in terms of the model performance, 'neutral' values may not exist). The authors may have chosen a default model parameter set that inadvertently optimised streamflow for the CaPA precipitation product.

The authors state that the rationale for choosing the best precipitation product is to derive the best (most accurate) calibrated H-LSM. However, the authors never end up demonstrating that this assumption is correct; this paper merely reinforces intuition, it does not reveal it as fact. Arguably, any number of precipitation products, once incorporated in the calibration process, could result in several parameterizations of MESH with very similar performance. In addition, by only using one precipitation product, the authors are not actually conducting what I would infer is "Hydrologic-Land Surface Modelling . . . under precipitation uncertainty . . .", as stated in the title.

In the first paragraph of the introduction it reads that the motivation behind the paper is predicated on the fact that the deployment and calibration requirements of Hydrologic-Land Surface Models (H-LSMs) differs markedly from Land Surface Models (LSMs), Hydrology Models (HMs) and Global Hydrology Models (GHMs). Without a clear definition/description of what constitutes/distinguishes these four type of models, treating H-LSMs as unique seems artificial. I am confident that decades of literature on the calibration of HMs does not need to be tossed because H-LSMs are so uniquely different (i.e. there is no need to start from scratch when discussing how to deploy an H-LSM).

Having as an objective the desire to 'improve the H-LSM parametrization using a state-of-the-art computationally efficient calibration approach . . ." seems quite trivial. There has been sufficient research with hydrologic modelling (whether that be an HM, GHM or LSM) to indicate that model calibration improves accuracy and is a necessary step. It is also arguable whether a calibration approach that relies on a single objective function constrained only by streamflow is actually state-of-the-art. In addition, the adopted calibration approach does not actually tests the effectiveness of parameter transferability,as claimed. The use of independent streamflow gauges to evaluate the calibration does not test for transferability, as no parameters have been spatially transferred; they have in fact been calibrated in place (unless I missed something in the text). What is actually being tested using independent gauges is the spatial robustness of the calibrated model parameters.

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