Hydrology and Earth System Sciences Discussions

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Review of hess-2021-602

Anonymous Referee #1

Referee comment on "Modelling evaporation with local, regional and global BROOK90 frameworks: importance of parameterization and forcing" by Ivan Vorobevskii et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-602-RC1, 2022

Summary

In this study, the authors attempt to qualitatively analyze the uncertainty in modeling evaporation arising from different parameterization and model forcings at multiple spatial scales using the BROOK90 model. Although the objective of the study is interesting and relevant, I have serious concerns about the experiment design of the study and the utility of a qualitative assessment of model uncertainty presented in this manuscript.

Major Comments

- The authors use just 15 combinations of model parameterization and forcing data to arrive at different conclusions regarding the importance of the two for modeling evaporation. In my opinion, this is severely inadequate for a robust assessment of uncertainty, let alone making any absolute conclusions about the importance of either model parameterization or forcing data, especially for a model which has greater than 20 parameters for modeling evaporation. A systematic uncertainty quantification would involve Monte Carlo simulations with a robust sampling scheme such as the Latin hypercube (by varying model parameters and meteorological inputs). As it stands, the results do not offer any conclusive quantitative evidence and as such is very superficial, and frankly not very useful.
- I have doubts about what the authors term as uncertainty in “model parameterization”. From what I can gather, the only difference among the two models (BROOK90 and EXTRUSO) is land cover type and some input datasets. I do not think this is enough to quantify the uncertainty in model parameterization. The difference in the different models would then mainly arise from the difference parameter values of the calibrated

and non-calibrated models. I do not understand how this difference can be construed as parameterization uncertainty. Either the authors should choose models which have completely different evaporation models (Penman vs Priestley-Taylor vs Hargreaves etc) or present a more robust quantification of the model parameter uncertainty (Monte Carlo simulations described above).

- In the same vein, the lack of uncertainty seen due to model forcings is just a function of the 3 datasets (in-situ, RaKliDa, and ERA5). The present analysis does not provide sufficient evidence that forcing uncertainty is not as important parameterization uncertainty (Vrugt et al. 2008).
- The attempt to study the differences in the spatial scale of evaporation modeling is commendable. But the authors do not discuss the differences among the different models from the perspective of spatial scales sufficiently. It is quite obvious that a model calibrated with local data would perform better. However, the interesting thing is to understand the differences in the regional and global model. There is no discussion pertaining to this. I would think this is because of the inadequate sample space in which the study operates. I recommend that the authors perform a systematic quantitative assessment of uncertainty.
- Many of the design choices are not explained and seem adhoc,
  - The authors do not explain why a multi-objective optimizer was used here. Why attempt to create a Pareto-optimal solution for calibrating evaporation (growing period vs winter)?
  - Why compare ERA5 hourly and ERA5 daily? Why only 3 input datasets? I can imagine that for Europe there are many observed forcing datasets (such as E-CAD).
  - Why was the BROOK90 and EXTRUSO model chosen for this study?
  - Why were only 20 parameters chosen? Was a sensitivity analysis conducted? Which are the most important parameters which contribute to the uncertainty?
- In summary, the study as it stands is very superficial and the authors have to make a strong case for why a qualitative assessment is sufficient to understand the uncertainty in model parameterization and forcings. In my opinion, the evidence provided in the manuscript points to the contrary: uncertainty assessments need far more robust experiment design to weed out spurious conclusions.

Minor Comments

- The abstract is very vague. What is the main conclusion of the study? What is the main implication of the conclusion?
- The manuscript needs to be edited to remove some idiosyncratic language use. For example Line 9: "Evaporation occurs on each surface…", Line 26: "...evaporation exposes larger variability…". Line 28: "...deepening knowledge…". Line 41: "The project allocates standardized …". Line 65: "the parameter set or meteorological input" should be "the parameter set and meteorological input".
- Line 40: I am not sure FLUXNET is an operational measurement network. I would term it as a database which collates measurements from different flux tower sites.
- Line 215. Do you mean that the goodnes of fit should increase (rather than decrease) from global to local scales?
- Why did the ERA5 daily outperform ERA5 hourly?
- Line 355: This is a very absolutist claim. The partitioning of evaporation is a topic of

major debate and the 60% estimate from Wei et al. 2017 is just one estimate. There is some uncertainty here varying from 55-85% depending on which study one considers.

- Figure 7: It does not show which model result is shown in which pie chart.
- The results section uses very subjective terms to describe model performance (example, 'fairly good' in Line 404).
- Line 449: I do not understand "...underestimation of the real site footprint or by permanent".
- Line 487: "...parameterization gave us higher spread". Where is this higher spread quantified? I recommend the authors attach some numbers to such claims, just a visual inspection is not enough.

References

Vrugt, J. A., ter Braak, C. J. F., Clark, M. P., Hyman, J. M., and Robinson, B. A. (2008), Treatment of input uncertainty in hydrologic modeling: Doing hydrology backward with Markov chain Monte Carlo simulation, *Water Resour. Res.*, 44, W00B09, doi:10.1029/2007WR006720.