

Comment on hess-2021-626

Anonymous Referee #2

Referee comment on "Comparing the runoff decompositions of small testbed catchments: end-member mixing analysis against hydrological modelling" by Andrey Bugaets et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-626-RC2>, 2022

This manuscript details a study that compares runoff decomposition as estimated by end-member mixing analysis on one hand and hydrological models on the other hand. This issue is clearly of great interest for both hydrological processes understanding and model development. At this stage, the paper has several shortcomings in terms of methods and data, which prevents a full understanding of the paper. My suggestion would be to be less ambitious, e.g. in the number of hydrological models used but more exhaustive in the details given throughout the manuscript. Additionally, it should be noted that the English level is pretty poor, I am not a native English speaker but I recommend that the authors proofread their revised manuscript before submission.

Major comments

- Lack of details

Throughout the paper, there is a lack of details. This affects both the data/method section and the results/discussion section at a level that precludes the reader to provide clear guidelines for further improvements. The required additional information is listed in the minor comments hereafter. The two other major comments are related to methodological issues.

- Hydrological model uncertainties and how the methodology of the paper reduces it

As explained in the introduction (l.31-39), hydrograph decomposition may be a powerful tool to reduce equifinality. In this sense, the present study shows relatively similar runoff simulations but with different flow components from hydrological models, but with different flow components. Unfortunately, the authors did not take this opportunity seriously, they used a single optimal parameter set for each model and did not discuss the impact of this choice, nor the way the parameter set is optimized. Consequently, it is pretty hard to conclude the relative weights of structural and parameter uncertainties in explaining the results.

- Short record periods

Only three years are available for model simulation (what about the warm-up year?). It is quite short and consequently, no validation was performed by the authors. Modeling results are presented only for calibration, which is problematic when dealing with parameter/structural uncertainties. Also, as low-flow components are extracted from hydrological models simulations, the authors should verify cautiously model initialization.

Minor (but still important) comments

l.77-78: not clear what is the time step of heavy rainfall and how extreme are these events.

l.79: not clear how averaging is performed, spatial or temporal?

Please add a table with both catchments characteristics (mean annual rainfall, temperature, runoff, land use lithology, topography, etc.). The differences in runoff yields for these two neighbor catchments are huge and I cannot figure out if it is due to lithological differences or specificity of the (short) record periods with extreme events.

Figure 1. Where the WMO station is located?

l.132: It is not clear how the end members are identified, what is the "independent information"?

Please add a table with the characteristics of the three hydrological models (with e.g. basis of the snow components, number of free parameters, spatial and temporal discretization, etc.). The information given for each model is not homogenous. Nothing is said on parameter estimation, which is in my opinion a key issue (see major comment #2).

l.238-249: Are the results are shown in calibration (and by the way, how the calibration is performed)?? Please modify Figure 5 so that the reader can see the whole record period simulation results.

Table 4: it appears that the models present quite different flow decompositions. Could this be due to the fact that the a priori three-component is wrong because too detailed for such small catchments? Since many flow decompositions only concern two flow components ("baseflow" and "surface flow"), did the authors challenge their prior 3-components hypothesis?

l.359-360: These perspectives are quite fuzzy. Please provide a real discussion section in the paper. There is a lot to say on both methodological limitations and further analysis of the results obtained.