Comment on hess-2023-143

This study by *Tiwari et al* presents a novel utilization of a spatial metric SPAEF for calibrating and investigating the appropriateness of raw SNODAS snow water equivalent information in distributed hydrological modeling. The manuscript compares different calibration approaches including sequential, and multi-objective experiments using SWE and discharge, and a traditional single variable (discharge) based calibration, using different global optimization routines. The study is based in a snow-dominated catchment in Canada. The results indicate a better simulation of the spatial distribution of the SWE when SPAEF is used in calibration, with a more robust hydrological prediction.

I read the paper with interest given the similarity with my recent works. However, I have to say that this manuscript, in its current form, must undergo major revision to enhance the clarity for readers and for publication. The justification for this claim is based on the following review comments from my side.

General comments:

1. The introduction section is not coherent and should be improved. I agree with Reviewer 1 that a major streamlining of the introductory section must be done. For instance, the description of SNODAS data can be brought together and shortened. Similarly with the problems with hydrological models while using single discharge variable for calibration, leading to parameter compensations can be merged. Please avoid repeating statements. A suggestion would be to streamline the introduction section with explanations about the objective functions and optimization routine briefly explained in different sections. TSMM justification can be provided in the introduction section or conclusion, to avoid confusion for the results.
2. Consider putting a short novelty of the research in the abstract as well and shorten it with specific information from your research.
3. The novelty of the study should be explicitly highlighted in the introductory section along with major findings. Multi-objective calibration is not a novel approach and has been identified to provide more reliable snow estimations with better or similar hydrological performance in different studies. However, the use of SPAEF for calibrating raw SNODAS data, as a novelty, should be highlighted comparing with past research employing similar approach with SNODAS or other data. The SPAEF metric used in other modeling approaches (Line 527) can be pulled to the introduction section.
4. The usage of ‘parameters’ and ‘variables’ should be uniform as it can impart different meanings to the readers.
5. Please make a clear distinction between the spatial unit used for calibration. Is it RHHU or pixel based? If pixels, how is the spatial information (esp. SNODAS data) aggregated at RHHUs?
6. How is the discharge extrapolated to the basin outlet?
7. The reasons behind calibrating the model solely against SWE distribution for March (peak accumulation) should be elaborated. Why not take the whole snow-season? How sensitive are the results when the calibration is done during the onset or the melt phases of the snow season? Since the study focuses on how raw SWE data can be used to constrain a hydrological model calibration, these sensitivity tests would further add value for other researchers and practitioners, for operational use.
8. I agree with Reviewer 1 on the explanation of RMSE and NSE in the objective function section. This can be reduced. Also consider explaining why KGE specifically was used for validation.

9. Given the coarser resolutions of the input drivers, how is the elevation dependent temperature and precipitation trends accounted for by Thiessen polygon method? This can have a detrimental effect on the simulation of snow accumulation and melt processes. Also, regarding the base model parameters used for comparison of model performance, did the cited research work with similar global model drivers? Consider giving a short explanation to formulate a sound basis for comparison.
10. Avoid reexplaining the calibration strategies, which makes the whole section longer.
11. Streamline the discussion section focusing on the results and corresponding relevant literature. Please avoid repetitions within the section and with the introduction. A short discussion on the uncertainties related to input data and SNODAS as compared to station observations would be better. Additional information on snow improving the hydrological understanding is not novel. Please focus on how your approach better represents the snow processes as well as parameter identifiability.
12. Please specify the base conditions used for calibration (i.e choice of input data, model and the calibration variable) for strong conclusions like Concluding remarks #2, considering the study was done in only one catchment.
13. How does this research differ from those who use bias corrected SNODAS information, particularly in terms of capturing the spatial distribution of snow cover and discharge simulation?

Specific comments:

Line 2: Please make a clear and uniform distinction between snow ‘parameters’ and ‘variables’.

Line 4: Full form of SWE at the first instance.

Line 8: Remove ‘approach’. ‘calibration’ should suffice.

Line 9: Full form of SNODAS at the first instance.

Line 14: Which “model performance”? Consider adding SWE and discharge simulation.

Line 5: ‘hydrological events’

Line 56: Remove ‘which’.

Line 66: Add references here.

Lines 101 - 102: Consider rephrasing the sentence ‘Ensuring….. streamflow forecasts’.

Line 136: ‘spatialized’?

Line 150: Consider ‘higher elevations’ instead of ‘hilly places’.

Line 175: What does ‘other modules’ mean? Consider rephrasing.

Line 178: ‘discharge is simulated’ not ‘model’

Line 202: Please check ‘2015-2020’.

Lines 214-215: Not necessarily. Add references or please rephrase.

Line 231: flow simulated at corresponding RHHU or at the outlet?

Lines 268 – 269: Please refer to general comments 5 on making a distinction between grid or RHHU. Better to make it clear beforehand in model description section.

Line 280: 'run with some random parameter values' instead of 'ran with some random value'.

Line 306: It would be interesting to look at the SPAEF values for the base model as well.

Line 318: Describe figure 4 in a better way than this. The current statement is confusing. Also please explain the significance of adding the 'rain-on-snow' description in this section.

Table 4: Please add base model performance as well.

Lines 327-328: Consider rephrasing this sentence about Figure 4.

Lines 336-338: Consider rephrasing the sentence 'SPAEF….. year to year'. It will be interesting to see the SPAEF values for the onset and depletion phases of the snow season as well.

Figures 3, 4, 5 and 7: Bigger font size please and also check the uniformity of the titles of these figures as well (esp. on top left). Consider plotting an average SWE line (simulated and observed) in these figures.

Lines 356 – 361: "relationship" or "comparison" ? Consider moving this section earlier or please put the experiment information in the caption in Figure 6.

Line 395: Please check if the calibration is done following 'NSE' based calibration.

Line 415: "under the considered model setting".

Line 432: Check $SPAEF_{SWE}$

Lines 431-432: Explain more on this or remove this sentence about computational efficiency altogether.

Line 516: This sentence is confusing. Please rephrase.

Line 518: 'simulation of complex processes' rather than 'complex processes'

Line 519: What is the high resolution defined here?

Line 530: 'Snow variables' instead of 'snow parameters'

Overall, this research has some interesting findings on how raw SNODAS data can complement the existing hydrological modeling techniques, especially with a novel use of a SPAEF metric in a multi-objective calibration setting. However, the structure of the manuscript must be substantially revised for further acceptance. I would thus recommend further consideration of the manuscript after major revision.