

Summary and Recommendation:

This is a very relevant paper for a special issue on ungauged basins in HESS. The objective of this research is to develop a monthly time-step water balance model that can be applied over basins with little monitoring and that captures primary runoff-generation mechanisms in small high-altitude basins, where snow has a significant control on runoff regime. As such, the only 2 calibration parameters relate to the snow module. The authors also introduce a new metric that can be used for assessing model performance, Q1. I recommend that this paper be published in this special issue of HESS with the following general and specific comments and technical corrections addressed.

General Comments:

- In both the abstract and the introduction, the original contribution should be mentioned and emphasized. In doing this, the objective should be immediately placed in context of the ultimate need of that this research is designed to meet. Somewhere in the manuscript, the new water balance model should be compared and contrasted to the mentioned water balance models and a statement as to its originality should be made.
- The authors make an argument in the discussion section that the second parameter is justified based on the improved model performance by including that parameter. However, no justification is made for not including some processes and parameters that may be very important. The most obvious example is the decision on whether or not to include soil moisture storage and associated parameter(s). It is possible that an additional parameter related to soil moisture storage and release may be as important as or more important than the second snowmelt-related parameter that was included. This should be tested. Not including soil moisture storage dynamics is a major weakness in this model that deserves more attention.
- Somewhere in the manuscript, possibly in discussion (and touched on in conclusions and abstract), a description of the types of basins that are appropriate for this water balance model should be included. This should include, basin size, type of climate, topography, etc.. Also, in determining regional-wide calibration parameters, how large of a region can this be approach be used for?
- In using observed streamflow to both calibrate and evaluate the model, was the streamflow observation partitioned in two for each of these? In other words, is there an independent (of calibration) assessment of model performance? I strongly suggest that the authors evaluate the snow dynamics module independently using snow course measurements or any snow depth or SWE measurements that might exist over their study basins. Because the emphasis is on snowpack, this type of evaluation would be an important component of this paper.

Specific Comments and Technical Corrections:

Abstract:

- Line 5: is “transposition” the correct word here? This word has a very specific meaning in mathematics, and I do not think it is appropriate. “Transform” might be better. (Same in all instances of this word.)
- Briefly mention the new performance metric, Q1, in the abstract, as it is a new contribution to the field.
- L14: “obtained results are good” – too subjective. Can this be quantified, enabling the reader to determine whether or not these are good?
- L15: “More in detail” – this is grammatically incorrect.
- L18: “the performance is less”
- L20: “thanks to its parsimony” is too colloquial. Use “Because of its parsimony..”

Introduction:

- P959, L7: “shown in”
- P960, L7: what is a “nuisance parameter”?
- L10: “generally difficult over regional-scale...”
- L16: “seasonal temperature curves”
- L21: “number of calibration parameters to estimate” – there are other parameters in the model, but they are not calibration parameters. In general, be clear throughout the manuscript about parameter with hard-coded value versus calibration parameter.

Model Description:

- P962, L4: “morphometric” here, but I think elsewhere “geomorphic” is used... The second word is more appropriate.
- L9: “partition of monthly precipitation, P_j, into snow or rain depending...”
- Mention assumptions here for snowmelt model: no sublimation, no evap of liquid water content in snow, ...
- Mention upfront that soil moisture storage is neglected. However, see general comment above. This needs further justification and evaluation. Also, on P961, L17, the authors state that SR is “unavailable for recharging soil moisture”. Based on this, it would suggest that soil moisture storage is included in the model, so this is misleading.
- Mention right away that the reason for separating out SR from total liquid P is so that actual ET only occurs from the 0.7P+ portion.
- What is the justification for SR = 30% of liquid P?
- P962, L5-8: The assumption is, therefore, that all runoff exits the basin within one month. This sets limitations as to the size of the basin being simulated. Earlier, the authors suggest that this approach should be applicable to a large range of basin sizes. Assumptions such as this one limit the size of the basin appropriate to be simulated by this model.
- L20: is there an estimate of goodness of fit?
- L26: Is this assumption tested? Also, there is a typo at the end of this line.
- P963, L18: Do the units work out here? mm = mm – mm/month + month*mm/month?

- P965, L17: “where k is the number of months...”
- P966, L3: There are grammatical problems with this sentence (“Nevertheless, ...”), please correct.
- L6: “head indices are supposed to take on values greater than 5” – is there a reference for this fact?
- L7-8: “the model proves to be able to predict realistic values of ET...”- where are these results and what metrics are used to evaluate this?
- L15: There should be a 0.7 in front of P_j ; Also there is no consideration in Thornthwait ET for aerodynamic and vegetative resistances, correct? Also, there is no consideration for the non-availability of deeply infiltrated water to ET.

Case Study:

- L18: “differ in mean elevation”
- L23: “which is more typical of middle-elevation catchments in this study area.”
- P967, L1: use “reducing” instead of “mitigating”
- L2: The authors must mean that RI is V_i/V_w , or the ratio of volume of artificial lake divided by annual water volume at basin outlet. A value of 25% then would indicate that artificial lake storage is only 25% of annual water volume at basin outlet, which is reasonable. Is this correct?
- L8: “composed of 39 elements”
- P968, L1: “undercatch by wind effects”
- L26: The assumption here is that 100% of the model bias in R is due to underestimation of P. State this assumption here and discuss whether or not this is a reasonable assumption. Is there any reason to believe that ET may be biased high or low? Would not including soil moisture storage cause a systematic bias? This should be fully discussed and evaluated, if possible.
- L26: Another assumption is that the bias is uniform among the months. We know that undercatch is largest when solid precipitation occurs during the winter. Can monthly T and P be used to determine a better division of bias among the months? This would not introduce any further parameters, but it should make the model more accurate.
- P969, L16: Is there a goodness of fit estimate?
- P970, L1: MAE is calculated using mean monthly R. This would produce a lower MAE then if it were calculated for all months for all years. By averaging monthly R for all years, the error is underestimated.
- L8: “in detail, ..”
- L15: “is the r-th smallest value”
- L19-L20: “In fact, being the...” - this sentence is grammatically wrong, please correct.
- L27: Is this altitude determined by looking at where glaciers occur in this region? How is it determined?
- P971, L6-9: If the parameter values with the lowest MAE are in an error of Figure 4 where the snow storage depletion condition is not met, is this an indication that there is something wrong with the model physics? What is going on here?

Results:

- P971, L25 and P972, L1: How are these confidence bands determined? How were these equations derived?
- P972, L6: This underestimation on summer could also be because the snowpack was underestimated, possibly because of uniformly applying the P bias among all months.
- L8: "Being that the evapotranspiration fluxes are only of..."
- L25: "the melting rate results to be..." – grammatically incorrect.
- L25-27: this nuisance is lost to me – why is it interesting to note this?
- P973, L4: "associated with river"
- L17: "contributed to by"
- L25: "model is better able to"
- L29: Why can't Q1 be used to compare different model applications? This is stated both here and in Appendix, but this is not clear to me in either place.

Discussion:

- P974, L2: "Because of its influence on snowmelt, temperature is..."
- L24: "When the partitioning of..."

Conclusions:

- P976, L14: However, I would argue that not all the governing mechanisms are incorporated. Lack of soil moisture dynamics is the main missing mechanism. Therefore, it is not possible to identify the role of soil moisture dynamics without including it in one of the sensitivity test simulations.
- P977, L3: "improved" instead of "ameliorated"? How do the authors suggest the generalized precipitation correction be improved?

Appendix A:

- P977, L26: "to consider also"
- P978, L3: "The calculation of QI requires a scoring of these..."
- P12: "in case of an earlier or late (by greater or equal to 1 month) peak..."
- L27-29: The meaning of this sentence is not clear to me. Please clarify.

Supplementary Material:

- Include RI in this table.

Figures and Captions:

- It would be good to show monthly plots of precipitation and temperature, either on average over the case study region, or by basin, as in figures 5 and 6. This would help readers who are not intimately familiar with these basins in interpreting the results.
- Figure 1: Make the "+" and "-" larger in this figure. Define SR and R in the caption

- Figure 2: Define what is meant by “budget” in this caption.
- Figures 5 and 6: Provide a qualitative definition of QI in the captions. Also, try to emphasize that figure 5 is for local and figure 6 is for regional in the captions, to make it really obvious how they are different.
- Figure 8 caption: Avoid the word “validation” as this is not a validation of the assumptions (that word is much too strong) but just a sensitivity study.