

## ***Interactive comment on “Complexity and performance of temperature-based snow routines for runoff modelling in mountainous areas in Central Europe” by Marc Girons Lopez et al.***

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Review for "Complexity and performance of temperature-based snow routines for runoff modelling in mountainous areas in central Europe" by Lopez et al

General comments: This paper describes the testing of many alternative conceptual algorithms for snow modelling implemented in the Swedish HBV model. The suitability of the different algorithms has been assessed by split sample procedures for many catchments in Czechia and Switzerland. The paper is well written, well organized and the experimental setup seems, in principle, to be fine. However, the possible improvements of the tested alternative algorithms are extremely subtle and the authors

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recommend exponential snowmelt function and seasonally varying degree-day factor based on tiny improvements which have not, as far as I can see, been tested for their significance. I think the objective of the paper is good, it would be nice if we in objective ways could agree on improved concepts in snowmodelling that when implemented would improve any model, but I am doubtful if the current methods are up for the task. The following issues need to be addressed in order to make paper suitable for publication.

1) The paper misses a major investigation on equifinality issues (see papers of K. Beven and J. Kirchner on this topic). The HBV model itself has a lot of freedom, i.e. parameters to be calibrated, and most of the suggested algorithms for possible improved snowmodelling add calibration parameters and hence to the problem of overparameterization. The point is that many of the suggested snow model modifications may have potential for being better at modelling snow, but the effect is impossible to isolate due to the overparameterization/equifinality. I have personal experience with trying to implement, what I thought was brilliant, ideas of improved snowmodelling to the HBV model. They were all insignificant, and after a while I realized that the compensating powers of all the parameters in HBV made it impossible to isolate and assess the effect of new algorithms (the frustration inspired the development of a new rainfall runoff model). The inclusion of the objective function for SWE is a step in the right direction, it narrows the freedom of the parameters, but probably not enough (you could try to also include Snow Covered Area, SCA). How many calibration parameters are there in the various model configurations? Are the numbers acceptable by any measure? Are their ranges physical at equifinality?

2) I would desire a more stringent terminology. Words like “efficient” and “complex” have really lost their true meaning in the literature of hydrological modelling. Effective parameters really mean parameters that lump many processes or represents areal averages and has little to do with efficiency. A non-linear formulation of a process is not necessarily complex if the parameters are physical and measurable. To me,

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an overparameterized model where, due to the compensating behavior among the parameters, the degree-day factor is suddenly correlated to the parameter controlling the subsurface storage capacity is infinitely complex. Please consider rewriting the paragraph that starts at 525

3) There are several paragraphs subjectively praising the HBV model for its ability to simulate hydrological behavior for various catchment types (p. 24, l.476, p.25, l 520-25, p.26, l563). “Hydrology” is a wide term and comprises more than runoff (and SWE admittedly), what about the subsurface, SCA, evapotranspiration etc. How come we are just presented result for one catchment?

Specific comments:

P1, l.18, “popular” subjective

P1, l.27 “optimal degree of realism”, rephrase

P2, l.143-44 How can “the limitations of data availability–” “pose a challenge to properly monitoring”. rephrase. . . .

P2, l.45-46 “Furthermore. . .” This sentence does not relate to anything above.

P2, l.52 ..available at..

P2, l.53. ..in a distributed way.. Not always, see Skaugen et al., 2018 (Hydrology Research)

P2, l.58 ..relevant for.. Aren’t they relevant everywhere?

P2, l.62 ..distribution function.. What is this?

P3, l.83-84. ..and investigated whether.. See major comment above.

P4, l.98. “well established”, what does this mean? is it good or just old

P5, l.125. . .to it... Refers to HBV or the individual components

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P5, l.125 is precipitation lapse rate missing in the table? Could we have all calibration parameters in the table?

P5, l.131. Heading, “Temperature and precipitation lapse rates”

P7, l.189. ..if somewhat.. How is it more realistic

P12, l.257. ..higher model complexity.. why more complex if you increase the temporal resolution

P12, l.257-58. “Other factors..”, please elaborate

P12, l.266-67. Good, this fights the problem of overparameterization. Could even include SCA

P13, l.292. “efficiency”, efficient how? Faster? gets more done? or just better?

P14, l.322..performance for...

P14, l.323-24. Can we accept improved performance for snow and decreased performance for runoff? I know that other authors have reported this, but is this not a clear indication of model structure flaw? Please elaborate, this is important

P18, l.373.. “catchment dependent..” I do not understand this sentence

P19, Figure. What does the y-axis represent, I struggle with this figure

P20, l.404-407. This paragraph is very complex, can you please explain better

P20, l.408 64 or 63 (see Table above)

P23, l.457 ..are dominant.. meaning strong or better?

P24, l.475. The first sentence is meaningless. Of course it is difficult to improve hydrological models, the processes are complex. The reason why it is difficult in the case of HBV could be due to the overparameterization, not because it has been widely used with acceptable results.

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P24, l.487. ... runoff is modulated.. rephrase

P25, l.533. ... even if model complexity. ...in a sensible way.. The sentence is strange

P26, l.551 different settings.. please be more specific475.

P26, l.563. Unsubstantiated, we have only seen the result for one catchment.475.

P26, l.565. How to proceed with this “better approach”, how to do it in practice?

References Skaugen, T., H. Luijting, T. Saloranta, D. Vikhamar-Schuler and K. Müller, 2018. In search of operational snow model structures for the future - comparing four snowmodels for 17 catchments in Norway. *Hydrology Research*, 49.6, <https://doi.org/10.2166/nh.2018.198>

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