

Interactive comment on “Modelling the spatial distribution of snow water equivalent at the catchment scale taking into account changes in snow covered area” by T. Skaugen and F. Randen

M. Zappa (Referee)

massimiliano.zappa@wsl.ch

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General remarks:

The authors elaborate a mathematical procedure in order to improve a previous parametrization in order to account for the temporal evolution of the snow covered area (SCA) and of the average snow water equivalent (SWE) within the snow covered portions of the basin.

The procedure is later implemented in the Norwegian operational realization of the HBV model. They test the obtained results by comparison to observed discharge in 5
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test areas, to estimated SWE in two sites and to MODIS SCA estimates.

The introduced changes allow for investigating issues concerning increasing or decreasing number of calibrated parameters for hydrological modeling experiments. In this respect I like the fact, that visual inspection of the results seems to have also quality when 2 of the three parameters needed to fit the distribution are assigned by analyzing observed precipitation.

The author recognize the minimal deviation of the results of their new procedure as compared to the previous solution and well identify the problem of over-parametrization of (conceptual) hydrological models such as HBV.

All in all there are of course some novel aspects in this manuscript, but is difficult to estimate whether this approach would have an impact on the way how snow hydrology is treated in models other than the HBV version used by the authors.

Major Issues

1) I would generally welcome that the captions are more helpful in understanding the Figures and that all axis have a legend

2) There is little quantitative support to the Figures presented in the manuscripts. Only Table 1 gives some hints about performance of the different approaches. Unfortunately the little differences do not allow for properly assess whether HBV_G is a positive addition to the model parametrization as compared to HBV_LN. In this respect I would welcome if the authors would also indicate the quality of HBV_G when the moments are estimated from precipitation

3) The authors should comment (and quantify with an adequate score) the quality of their approach for the accumulation and ablation phases of the figures 3 to 6.

Minor comments:

1) You use HBV. I assume you use it in its lumped version and that you consider eleva-

tion bands and that you can generate results for each elevation band (so that you can support the statement on perennial snow concerning Figure 9) and for the integral of the basin. Please more detail on the spatial configuration of HBV.

2) Table 1: More information on the catchments is needed (Area, average precipitations, discharge and portion of snow melt to total discharge). Is the indicated elevation the average elevation of the basin or the elevation at its outlet?

3) I would welcome a Table declaring the estimations of the parameters of the gamma distribution as obtained by calibration, and as obtained by analyzing the precipitation data.

4) Figure 1 and 2: could be merged, put in red additional lines in the fields where Figure 1 and 2 are not coincident

5) Figures 3-6: Please declare the unit of the Time axis and of the Y-axis (SCA, SWE). The time axis seems to be a "day with observation", but we ignore how many days are between the observations.

6) Figure 3 and 5: The observed SWE refers to a section at 1000 m.a.s.l.

Final considerations: The manuscript is well prepared and clearly structured. The introduction and the discussion are well done, but the findings presented in the result section are poorly supported by adequate measures of agreement. I hope the authors will be able to provide a revised version of the manuscript covering this flaw and demonstrating how the science of snow hydrology might profit from that.

Best regards

Massimiliano Zappa

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