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Hydrology and Earth System Sciences Discussions

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

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The goal of this paper is to "present a method for estimating the spatial PDF of SWE at the catchment scale through estimating the temporally varying spatial moments of SWE while taking changes in SCA into account."

Working with the HBV degree-day model, the authors have developed a variant of the current HBV snow spatial distribution routine. Here the authors model the spatial frequency of snow water equivalent (SWE) as sums of temporally correlated stochastic fields of gamma a 2-parameter gamma distribution. In this paper, the authors are

using the catchment scale spatial distribution of SWE including areas that are snow-covered and snow-free. Temporal correlations between snowfall, accumulated SWE and snowmelt are all considered in their spatial estimates of catchment scale SWE. The spatial distribution of melt is also modeled as a gamma distribution.

Their results show that the spatial moments compare reasonably well with spatial moments from observations at two sites. They also compare HBV modeled snow covered area (SCA) with measured SCA and modeled runoff with observed runoff.

I continue to be surprised that snow scientists are still spending time trying to improve an archaic model like HBV. While it has been in use for many years in Norway, modeling has progressed well beyond this approach and one would hope that it will soon be replaced by a more mechanistic modeling approach such as NOAA's National Snow Analysis. That said, HBV remains the main operational tool for snowmelt-runoff modeling in Norway and physically-based models only improve results when the forcing data are available and of sufficiently good quality.

My main comments are as follows: 1. It is not clear from this manuscript that this relatively minor modification in snow distribution for a model used in Norway is of interest to the broad readership of HESS. Moreover, the authors did not provide statistical evidence that this modification really makes a difference. In this case, it seems that the paper does not make any significant advance in the field of snowmelt-runoff modeling.

2. What is the physical basis for using a gamma distribution? While previous work has used such a distribution to describe the spatial distribution of precipitation, this may not be applicable for snow since snow transport and canopy interception significantly affects the spatial distribution of snow accumulation. The authors need to provide more substantial support and a physical basis for their choice of PDF.

3. The authors state that because HBV doesn't record the spatial moments of accumulated SWE, they approximate them by fitting a log-normal distribution to the SWE quantiles from the model. However, the LN_model uses a uniform spatial distribution of

SWE up to a threshold after which it implements a log-normal distribution. The authors need to explain how this difference affects the approximation of the moments and how that influences their comparison of the G_model results vs. the LN_model results.

4. Section 3.1. “The LN_model has a better prediction of the conditional mean. . .” How much better? Is it statistically significant? Same comment for the other comparisons. Using comparative terms such as “better” and “good” are not meaningful. There needs to be quantitative explanation with statistical significance testing to back up the comparisons. 5. Section 4.1: “we observe an increase in observed spatial standard deviation at the onset of the melting period”. How much of an increase? Is it significant?

6. Section 4.2. “The validation results are slightly better with the G model for the catchments Atnasjø and Narsjø, and slightly inferior for the other catchments.” (and other similar statements in this section). Are these differences significant?

7. Looking at five watersheds, the authors test their gamma distributed snowmelt model (G_model) with the previous version with uses a uniform + log-normal combination of spatially distributed snow water equivalent (LN_model). However, there is no description of the watersheds. The authors need to describe them in terms of area, elevation range, proportion of the watershed in the seasonal snow zone (e.g. Jefferson 2011), land cover characteristics, and fraction of groundwater contribution to discharge.

8. It is not surprising that SCA from the MODIS snowcover product exceeds that of HBV when SCA is high and is lower than that of HBV when SCA is low. The MODIS binary product significantly underestimates SCA when snow is patchy and it overestimates snow cover for high snow cover. This is because for snowcover less than about 50% the MODIS product will record zero snow and for snow cover greater than 50% the product records 100% snow. You are comparing your modeled SCA with a product that has known flaws.

Additional comments:

Section 3.1 How was snow density measured at the two snow survey sites?

Section 3.1 “measurements of the spatial moments of SWE at the catchment scale is not known in Norway.” This sentence is ungrammatical and unclear. I think you mean that because of a paucity of measurements of SWE at the catchment scale, the spatial moments of SWE at that scale are not known. Please clarify.

Section 4.2 “This can carried out both for satellite derived SCA higher and less than modelled SCA.”

Section 4.2. “MODIS” This acronym is not defined.

Figs. 1&2. These figures are not particularly informative and should be omitted.

Figs, 3-6 Axis labels need to indicate units. The figure legend should be placed within the white space of one of the four plots, not in the caption.

Figs, 8-9. Axis labels need to indicate units. The figure legend should be placed within the white space of plot (a), not as a figure title. The time axes on these should show the years of the validation period, not 0-500.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 11485, 2011.