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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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Final Author Comment (by Skaugen, T. and F. Randen)

Response to reviewer #1 (Anna Nolin)

Major comments:

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.

Response: We have developed a method for modeling the spatial distribution of SWE which can be applied by any hydrologic model (not only the HBV). There are several reasons for why we believe this paper provide significant advances: i) Reduction in tuning parameters. The paper introduces a method that estimates the spatial frequency distribution of SWE which is parameterized from observed precipitation data alone. No loss in precision or detail of the simulations is observed when using the new distribution of SWE. A reduction in the number of parameters to be calibrated reduces the dimensions of the parameter space and thus the parameter uncertainty. A reduction in tuning parameters makes the model less flexible and more dependent on its structure, so that possible structural deficiencies more easily can be identified (Kirchner, 2006). ii) The study reveals two insights relevant for modeling snowmelt. a) The correlation between SWE and melt is negative and b) the "hysteretic" effect (reported by many authors) is given an explanation. iii) The method increases information in that it estimates the spatial variance of SWE, and hence makes it possible to assign a statistical model to SWE. A statistical model for the spatial pdf of SWE facilitates a variety of statistical inferences on SWE (i.e extreme value estimation, trend analysis etc.) and serves as a basis for methods like the "snow depletion curve (SDC)". iv) The method present a new way of estimating changes in SCA, which is totally dependent on having an analytical expression for the spatial pdf of SWE. It is also mentioned in the paper,(p.20, l.16-26) that when an analytical expression for the PDF of SWE is determined, updates of SWE from observed SCA (by satellites) can be carried out. v) The paper presents, for the first time as far as we can tell, observations of the spatial pdf of snow melt.

We will further emphasize these points in the conclusions and include the reference to Kirchner (2006).

Kirchner J.W, 2006. Getting the right answers for the right reasons: Linking measurements, analyses and models to advance the science of hydrology., Water Resour. Res.,

42, W03S04, doi:10.1029/2005WR004362

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..

Response: In our experience it is rather seldom that we, in geosciences, find a physical basis for assigning a theoretical statistical model (an analytical distribution). It is usually a question of how well your data fit a theoretical statistical model. The gamma distribution is found to describe the spatial distribution of precipitation well. Since we operate on spatial scales (the catchment scale) of one to a few kilometers where the variability of precipitation is the main influence on the variability of snowdepth (Liston 2004), the gamma distribution hence seems to be a reasonable choice. It is also not novel to use the gamma distribution to describe the spatial distribution of snow (see p.6, l.3-7 in the paper). We will add a sentence of the reasoning above that includes Liston (2004) as a reference and a sentence that points to the excellent fit between the modeled gamma distribution and observed SWE from Norefjell, Norway reported in Skaugen (2007).

Liston, G. E., 2004. Representing subgrid snow cover heterogeneities in regional and global models, J. Climate, 17, 1381-1397, 2004.

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.

Response: It is, we believe, not straightforward to state how well a sum of one uniform

layer of SWE and a number of log-normally distributed layers of SWE can be approximated by a log-normal distribution and how this layering affects the estimation of the moments. We do not, however, believe this to be a crucial point since the log-normal distribution fitted very well to the quantiles estimated by the LN_model (not shown in the paper). We just treated the problem as one of fitting a theoretical statistical model to observed data points and did not see any point in a lengthy theoretical consideration. We will add a sentence that says we did not encounter any problems fitting a log-normal distribution to the estimated quantiles even though the distribution of total SWE results from a sum of a uniform and log-normal layers of SWE. A gamma distribution would probably also have worked.

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.

Response: This point has also been addressed by reviewers# 2 and #3. We agree that the comparisons between G_mode and LN_ model were a bit qualitative. We have therefore added a table in the MS that shows the root mean square error (RMSE) for both G_model and LN_model. The quantitative results coincide well with the qualitative statements in the paper, and the G_model generally got better RMSE scores (calibrated and estimated from precipitation data) for both Norefjell and Filefjell and we can state with more confidence that the G_model is a better model (this will be emphasized in the paper). We found it necessary to remove the last data point from the Filefjell series since the spatial mean and -standard deviation of SWE for this data point only was estimated from three observations. Regarding the statistical significance of the results, this is hard to assess since we only have one realization of the model. Comparisons and assessments of superiority of a model are thus based on visual inspection of the figures 3-6 (which are new and hopefully in accordance with the reviewers whishes) and now the RMSE scores.

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?

Response: This is merely an observation made from the figures 3-6 that the observed spatial standard deviation continues to increase some time into the melting period. This phenomenon is also observed by other authors (the hysteretic effect, Egli et al. 2011), and the observation serves as a starting point for a brief discussion on the relationship between spatial mean and standard deviation. The proposed model captures this phenomenon. We will add a brief discussion that links these observations, the proposed model and observations made by other authors.

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?

Response: To determine whether one Nash value is significantly different from another would demand an uncertainty analysis of the HBV model which is beyond the scope of this paper. This is, however, an important point and a full uncertainty analysis is probably how the introduction of new process algorithms should be validated. We would, on the other hand, have preferred a rainfall- runoff model with less tuning parameters than the HBV in order to have more confidence in the uncertainty analysis. Our compromise was to test the new method for several (five) catchments in order to get an impression if there is a significant difference. In addition, due to the above, we felt the need to evaluate our model using the snow course data from Norefjell and Filefjell. Based on quantified information in Table 1 and visually by the figures 8 and 9, our rather modest claim is that, when implemented in a rainfall runoff model, the new method performs similarly as the standard method although there is one parameter less to calibrate.

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.

Response: Also Reviewer #2 (R#2) addresses this point, and a table (Table 2) in which landcover characteristics, topographic- and hydroclimatic information (mean precip., temperature and runoff) is inserted. Some of the parameters R#1 and R#2 ask for are unfortunately not at hand, but we believe the readers will be quite informed by those provided.

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.:

Response: We do not use the MODIS binary product. There is obviously too little information in the paper on how the MODIS SCA values were obtained. The following will be inserted: "Each pixel in the MODIS image is assigned a SCA value between 0-100% coverage using a method based on the Norwegian linear reflectance to snow cover algorithm (NLR) described in Solberg et al. (2006). The input to the NLR algorithm is the normalized difference snow index signal (NDSI- signal) described by Salomonsen and Apple (2004)".

Salomonson, V.V. and Apple, I., 2004. Estimating fractional snow cover from MODIS using the normalized difference snow index. Remote Sensing of Environment, Vol. 89, pp. 351-361.

Solberg, R., H. Koren, and J. Amlien, 2006. A review of optical snow cover algorithms.

SAMBA/40/06, Norwegian Computing Centre, Norway, 15 December.

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

Response: We will insert the sentences: Average snow density was measured at Norefjell from two snow pits at sites with average snow depth. At Filefjell the density was measured using a snow tube (see Dingman, 2002, p 174) at every tenth stake.

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.

Response: You are quite correct and the sentence is rephrased.

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

Response: The sentence should read: This can be carried out for satellite derived SCA both higher and less than modelled SCA.

Section 4.2. "MODIS" This acronym is not defined.

Response: Moderate Resolution Imaging Spectroradiometer (MODIS) will be inserted

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

Response: We found the figures useful when developing the theory but two almost identical figures is clearly unnecessary. Reviewer #2 thought they could be merged into one figure. We keep one figure and refer the other case to this. Hopefully some readers might find them illuminating.

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.

Response: Done!

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.

Response: Done!

Response to reviewer #2 (Massimiliano Zappa)

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

Response: We have reviewed the figure captions and hopefully made them more clear. Axes now have legends.

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 parameterization 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.

Response: The Nash values in Table 1 are indeed from when the parameters of the HBV_G are estimated from precipitation (p.18, l. 11). A table of RMSE is inserted that quantifies the performance of the G_model and LN_model, and the G_model comes out as the better model. Comparing the results of HBV_G and HBV_LN is not conclusive because of the problem of overparameterization of the HBV model, but we merely state that their performance is similar. (see also response to R#1 Point 4)

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.

Response: This is partly done. (See above), and a comment will be added on the differences in the accumulation and ablation period. Data points are too few to calculate scores for the separate periods.

Minor comments: 1) You use HBV. I assume you use it in its lumped version and that you consider elevation 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.

Response: The following sentence will be inserted: The model is semi-distributed in that the moisture-accounting (rainfall and accumulating and melting snow) is performed for ten elevation bands. The catchment averages of precipitation and temperature are distributed to the elevation bands using lapse rates.

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?

Response, see response to R#1 point 7

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.

Response: A table will be added and commented that shows the parameters for the G_model estimated from precipitation and from calibration.

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

Response: Only one figure will be kept and referred to.

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.

Response: Done!

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

Response: We are not quite sure what the comment is here, but figures 3-6 refer to data from Norefjell and Filefjell which both are sampled at 1000 masl (see sect 3.1).

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.

Response: We hope that the improvements in the MS and the quantification of the scores of the G_model and the LN_model have made it easier to see the paper as an advance in snow hydrology.

Response to reviewer #3 (Anonymous)

Skaugen and Randen propose a new method using a gamma distribution to represent the spatial distribution of SWE in catchments. They then apply the new method and compare it to one standard method. While this is a good approach, the authors could do a better job in demonstrating the added value of this approach. As it is now, one can say that there is basically no improvement in runoff simulations (Tab 1), and the internal snow simulations are not evaluated in a quantitative way including an evaluation of the significance of differences. Looking only on the figures it is not that easy to assess how much different the two methods actually were.

Response: See response to R#1, point 4 and R#2, point2.

The new approach looks mathematically very sophisticated, but the authors did not convince me that it is necessarily more realistic in a physical sense. One crucial point is the assumption of a gamma distribution, which should be motivated better.

Response: See response to R#1, point 2.

Also, as far as I understand, snow redistribution is ignored in this approach. While this is true for many approaches, I think considering redistribution in many cases might be more important than using different distributions for the variation of snowfall.

Response: At the catchments scale it is believed that spatial variability of precipitation is the dominant process explaining the spatial variability of snowdepth (Liston 2004). Redistribution due to wind is identified as being important from tens to some hundred of meters (Liston 2004). (See also R#1, point 4).

I term unit (section 2.1. ff) needs to be clarified. What is actually meant by unit? How large is one unit? How can these abstract 'units' be related to reality?

Response: A unit is chosen to be 0.1 mm of SWE(see p. 8, l. 7, this sentence will also be improved) and is the "building block" which when summed gives us a snow fall event and the snow reservoir found on the ground. The distribution of the sum of such unit gamma distributed variables, gives us the distribution of the snowfall events and the snow reservoir on the ground. The reality check is the comparison between the observed and modeled spatial distribution of SWE expressed by its moments. We found that the observed moments corresponded very well to the moments obtained if you summed correlated gamma distributed units (see also R#1, point 2).

It remains rather unclear how topography and vegetation are considered. Only in the results I found an indication, that elevation zones might have been used. This needs to be clarified!

Response: Vegetation is not considered specifically, which is again a reflection of the spatial scale considered. Snow canopy interactions is believed to have an effect on the scale of one to hundreds of metres (see Liston 2004). The spatial configuration of the HBV model is somewhat elaborated upon (see R#2, minor comments 1)

In the conclusions the authors emphasize that the new formulation produces similar

good results with one parameter less to be calibrated. However, if the parameters alpha and v are not estimated from observed precipitation, which probably is the case in most applications, the number of free parameters actually is increased.

Response: We do not agree on this point. The method for estimating alpha and v has been carried out for all of Norway also at locations with very few precipitation stations. You only need a measure of the spatial variability of precipitation associated with different measures of the spatial mean of precipitation. (see Skaugen and Andersen (2010) for more detail). Three or more precipitation stations will suffice (the more the better of course).

The manuscript generally reads well, but the structure could be improved. As it is now some methods are first mentioned in the results and the discussion section includes some parts which better would fit in the results section.

Response: We are not quite certain what the reviewer aims at here, but we can make a guess: The methods presented in this paper are, of course, the derivation of the spatial moments of SWE and the method for estimating the changes in SCA. We do not consider the implementation of the method into the HBV model to be another method, merely an application. It should thus remain in the results section. However, the presentation of figs. 8 and 9 could perhaps be moved to the results section in order to improve the structure. This will be done.

Please provide units in all figures. Remove titles in Figs 8&9.

Response: Done!

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