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Hydrology and Earth System Sciences Discussions

Interactive comment on "The spatial variability of snow water equivalent" *by* T. Skaugen

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1. Instead of the fact that the ergodicity assumption is not an intrinsic part of the paper, I would like to clarify my remarks relating to this assumption.

Stochastic process y(t) is 'ergodic' if the statistics of numbers sampled from any sample function, yi(t1), yi(t2),..., yi(tm) are identical to the statistics of the numbers collected across the ensemble at any instant, y1(t), y2(t)..., ym(t). As the author has pointed in the reply to Ref.#1, y is interpreted in the paper as a realization of a stochastic field. In this case, under the accepting an ergodicity assumption, the parameters alfa0 and nyu0 should be estimated by the analysis of the only realization of a stochastic field. In the paper, however, these parameters are estimated from the only point of a field. Additionally, the covariance function of the ergodic process should be rapidly decreasing but it has a constant value for all time-lags, as it has been assumed in the

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paper. That is why, the ergodicity assumption is, in my opinion, misinterpreted in the current version of the paper and I suggest excluding the assumption from the revised text.

2. I am still inclined to believe that the assumption about identity of the spatial distributions of a unit melt event and unit snowfall event is questionable and should be tested. In the mentioned paper (Essery and Pomeroy, 2004), the identity is assumed between spatial distribution of melt and SWE. On my understanding, this is not the same as it has been intended in the reviewed paper.

Finally, I agree with the author that the demonstrated results of the model application are very favourable for both Norefjell and Aursunden data sets. However, as far as I concluded from the text, these results were obtained due to the model calibration (by the way, is the covariance c the only calibrated parameter? also, it is not clear for me, how the u-parameter is estimated?). If this is the case, then I suggest to show results of the model validatation by using the data sets which were not used for the model calibration (e.g. for some other years at the same sites)? These results would be a confirmation of the validity of the proposed methods and assumptions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 1465, 2007.