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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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The paper is devoted to the problem of modeling spatial variability of snow water equivalent (SWE) for the scales from hundreds meters to a few kilometers. Spatio-temporal variability of SWE during both accumulation and, especially, melt season strongly influences on hydrological processes over a watershed, thus the proper description of the spatial snow dynamics in a hydrological model can lead to an essential improvement of the model capability. Modelling the spatio-temporal variability of SWE is a very complex problem because of the interrelated, multiscale nature of the processes involved and poor, as a rule, spatial resolution of the available measurements. There are two approaches to modelling spatial variability of snow: (1) the deterministic, process oriented approach which is based on simulation of the physical processes (snow blowing, effects of elevation, aspect, vegetation, etc.) which govern dynamics of spatial variability of snow-related quantities (this approach is reviewed, for example, by Tarboton

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et al, 2000), and (2) the statistical approaches. Leaving aside statistical approaches to scaling of snow fields (a perfect review on the subject is presented by G. Blöschl (1999)), the statistical methods are mainly used in the lumped hydrological models for simulation of the snow depletion curve reflecting dynamics of snow-free area during the melting season.

To my knowledge, the author is the first who has suggested a pure statistical model for simulation of spatio-temporal variability of SWE from the beginning of snow accumulation to the end of melt season. The essence of the suggested approach is to simulate spatial SWE distribution as a gamma distribution with temporary changing parameters which are derived from the statistical moments of the snowfall and melt events.

Principle Comments

1. The values of the parameters of spatial SWE distribution were estimated from the mean and the variance of time series of daily precipitation amounts measured at a single meteorological station. It was assumed, that the spatial marginal distribution and the spatial correlation structure of precipitation are identical to the corresponding probability properties of precipitation time-series. To me, the assumption looks questionable. I do not know any study where such an assumption is verified and confirmed for whatever spatio-temporal scales. The assumption looks even more dubious if one takes into account that the spatial variability was desired for the scales up to 2 km; the standard deviation estimated from the precipitation time series is too large for the considered relatively small spatial scales. In order to ratify validity of the suggested approach, the author assumed the ergodicity property of the concerned processes but the ergodicity is not correctly interpreted. First, the author assumed that the precipitation process has a constant covariance. In this case it is not an ergodic process. Second, SWE is calculated in the paper by summation of the underlying precipitation process. Even if precipitation process is stationary, then integrated process is not stationary and, naturally, non-ergodic.

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2. In the paper, spatial distribution of SWE is modeled as a gamma distribution. At that, the author made an attempt to show that gamma distribution of SWE can be obtained analytically as a consequence of the fact that daily precipitation amounts are gamma distributed as well. This is correct only for independent gamma-distributed variables with the same scale parameter; however, the precipitation amounts were assumed as dependent variables in the paper. Thus, one of the objectives of the paper ("to demonstrate that the spatial distribution of SWE can be adequately modeled as the summation of correlated in time daily precipitation") looks unachieved.

3. The assumption that the spatial distribution of a unit melt event is identical to that of unit snowfall event is adopted in the paper without any testing. However this assumption looks very dubious, at least because these events are caused by quite different meteorological processes.

The aforementioned remarks are, in my opinion, a matter of principle and relate to the basis of the model presented in the paper. In spite of the fact that the paper addresses relevant scientific problem within the scope of HESS, however scientific methods and assumptions, which are suggested by the author for solving the problem, don't look valid and I can not recommend the current version of the paper for publication. I suggest the author to give more attention for basing and testing the adopted assumptions. If the revised paper will be re-submitted in HESS, it needs to be reconsidered and re-reviewed.

Blöschl, G. (1999) Scaling issue in snow hydrology. Hydrological Processes. 13, 2149-2175 Tarboton, D., Blöschl, G., Cooley, K., Kirnbauer, R., Luce, C. (2000) Spatial snow cover processes at Kuhtai and Reynolds Creek. In: In: Grayson R., Blöschl G. (Eds.) Spatial patterns in catchment hydrology. Cambridge University Press, Cambridge, UK, pp. 158-186.

1. Does the paper address relevant scientific questions within the scope of HESS? YES 2. Does the paper present novel concepts, ideas, tools or data? YES 3. Are

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substantial conclusions reached? NO 4. Are the scientific methods and assumptions valid and clearly outlined? NO 5. Are the results sufficient to support the interpretations and conclusions? NO 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientific (traceability of results)? NO COMMENTS 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES 8. Does the title clearly reflect the contents of the paper? NOT COMPLETELY; SPATIAL SCALES SHOULD BE POINTED IN THE TITLE (E.G. “MESOSCALE SPATIAL VARIABILITYĚ” 9. Does the abstract provide a concise and complete summary? YES 10. Is the overall presentation well structured and clear? YES 11. Is the language fluent and precise? NOT COMPLETELY (see comments of Referee #2) 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES, BUT THE ASSUMPTIONS USED FOR MATHEMATICAL MANIPULATIONS ARE NOT TESTED AND LOOK QUESTIONABLE 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? FIGS 1,2 LOOK UNNECESSARY 14. Are the number and quality of references appropriate? YES 15. Is the amount and quality of supplementary material appropriate? YES

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