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The Cryosphere Discussions

Interactive comment on "Simulating snow maps for Norway: description and statistical evaluation of the seNorge snow model" *by* T. M. Saloranta

Anonymous Referee #1

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General comments

The paper presents a new modeling system to estimate the snow distribution within Norway. This kind of system would be really needed, especially when taking into account a terrain with difficult topography which means also problems in in situ (and also remote sensing) observations. The paper addresses a relevant scientific question whether it is possible to use a rather simple modeling system reliably, and under which circumstances the model works well and not so well, and why so.

Paper presents a novel modeling tool, but in my opinion the conclusion reached is that the system does not estimate the snow depth or SWE distribution that correctly, that it would be useful in applications needing more exact values of the snow cover. Nevertheless, comparing locations and years in relative terms seems to be more promising

way to work with the modeling system. Paper also reaches the conclusion, that the snow model needs development and/or recalibration. It was clear according the paper that elevation and time of the winter affected the model performance. I would still like to see more thorough discussion about possible reasons for the not-so-good performance of the model, and discussion on which processes in the model will need to be developed.

The quality of the paper is good – it is well structured and written, language is fluent. In any case, check the tense throughout the paper. Methods are clearly described, although some assumptions behind the parameterization are not explained. Observations and calculations are sufficiently described. Title and abstract are descriptive enough. Mathematical formulae, symbols, abbreviations, and units are correctly defined and used. Some more references to other models of snow distribution and structure could be added. Discussion would benefit also of references on some classic works on snow distribution and snow depth/SWE variability (listed in e.g. in Handbook of snow etc).

Specific comments

In discussion on overestimation of SWE and density please make it clear which of these quantities are calculated first, and which only second, using the other quantities as input.

Try to clarify which processes may contribute to overestimation/underestimation in which conditions – forest cover, weather type, homogenus or not homogenous grid cell. . .

Did you compare the two data sets with each other – are there possible comparisons that could give new insights?

Your input data lacks daily variation in meteorological parameters. Could you comment how this may affect the simulation quality.

More discussion on possible problems related to observations could be added.

Overall comment on snow model – are there references to validation studies on snow density / densification etc. – I mean VIC and SNTHERM process model validations?

Mapping of the snow cover could perhaps be validated also against some remote sensing products?

Technical corrections (line numbering refers to the printable version)

p 1338, line 10 – "distribution of model fit" sounds strange p 1340, line 14 – can you say it is a rather good agreement? P 1341, line 9 "thoroughly spatiotemporally evaluate" is complicated wording Line 15 what does "lack of accurate absolute values of snow conditions" mean, use another wording Line 27 – word missing somewhere

P 1342, line 13. Threshold temperature Ts seems quite low – 1.2 has been used in some other applications. Line 21-22. Do the parameters fs, fr change from one grid cell to other? Are they experimental? How about Cm, is it experimental – it takes into account forest cover and latitude. Should other parameters be considered also?

P 1346 line 7. Please consider linguistically more correct naming for the Norvegian meteorological institute data series than "met.no-data". Line 19. Density is calculated from SWE and snow depth? So overestimation of modeled snow density is just a consequence of overestimation of SWE or underestimation of SD? Wordings "somewhat better suited" is complicated.

P 1347 line 7, method description complicated P 1348 line 10, why in February? P 1349 line 21, you did not use original density observations? P 1350, line 13, why so? Line 19 "almost equal dates" is a bit strange Lines 21-23, complicated sentence, please re-write. Line 30, kg/l is not SI-unit. P 1352, line 2, to explain – are included? Line 7, spatial natural variability – natural spatial variability P 1353 line 8-13. Sublimation of blowing snow and snow redistribution certainly have an effect on observed snow amount! P 1354 line 17, this model is perhaps more process based than a statistical

model, but it still lacks several processes. . . P 1355 line 17, sentence has a complicated structure. P 1356 lines 14-20. This discussion is somehow out of place. P 1357, line 1. Perhaps best, but also includes great sources of error. . . Beginning from lines 23 and 27. Sentences have complicated structure. Line 25. seem to be – are? P 1358, line 1, in order to

Table 2 "only positive values" – is this needed? In the footnote and in the table different elevation ranges are given. What can you say about locating the stations in respect to forest cover and slope angle – not only to elevation?

Interactive comment on The Cryosphere Discuss., 6, 1337, 2012.