

## ***Interactive comment on “Modelled sensitivity of the snow regime to topography, shrub fraction and shrub height” by C. B. Ménard et al.***

### **Anonymous Referee #2**

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#### GENERAL COMMENTS

This paper deals with the effect of shrubs on snowmelt rates and timing. The work is motivated by the ongoing expansion of shrubs in the Arctic and its potential consequences for snowmelt and land surface albedo. The authors have expanded an existing snow melt model (2SOM) to additionally account for fractional snow cover typical for patchy snow in shrubby environments. The enhanced model (3SOM) is used in conjunction with a snow accumulation model that accounts for snow redistribution by wind (DBSM). The authors argue that snow accumulation/redistribution and melt approximately occur as two separate periods, which is why they use DBSM to drive the initial snow distribution prior to melt and switch to 3SOM to predict subsequent snow depletion.

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The paper is generally well written, although the use of acronyms may be a bit overexploited. I appreciate that the authors are not shy of pointing at several shortcomings of their own model. Nevertheless I see a couple of issues that require further attention.

1) Introducing 3SOM is one of two goals of this study. The need for 3SOM is motivated by results showing 2SOM fails in simulating late-lying snow patches. The authors claim to have realized a model improvement by introducing a separate representation of sensible heat over snow and snow-free patches. While this approach to address the shortcoming of 2SOM seems certainly feasible, there is a problem with the involved length scales. Figure 7 shows snow-free patches that are much greater in extent than the grid scale of 8 m. However 3SOM does not include advection of sensible heat between grid cells (P236/L8), just within grid cells. So addressing issues with advection of sensible heat between patches should be addressed explicitly (i.e. between grid cells), not at the subgrid level. Having said this, 3SOM has certainly potential for improving snowmelt predictions of patchy snow when run at coarser grid scale, but not in this case study.

2) The findings related to the second goal of this study are clearly identified as modelled sensitivities. Nevertheless for such a study it should be fundamental to provide enough evidence that the model is well validated and results are transferable to scenarios tested. With the main focus on the spatial heterogeneity of snow, I'm irritated by the fact that the authors do not offer any spatially explicit validation. All data used to demonstrate the model's performance are either point data or spatially averaged data. Figure 7 would allow spatial validation, so would the snow data measured along the transects. In addition, to give one example of problems I see related to the transferability of the model: Given the structure of 3SOM, the calculation of snow cover fraction (Equation 7) must be considered a key component of the model. The function was calibrated using local survey data. However, this calibration is representative of the existing shrub distribution and terrain. How can this parameterization be transferable to scenarios like no-terrain or a vegetation fraction projected to be 6.5 times higher than

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the actual fraction?

3) Figure 6 presents data that allow identification of severe mismatches between modeled and observed CV data. Modeled CV data for south facing slopes seem to be underestimated by a factor of up to three, whereas CV data for north facing slopes seem to be about right. Maybe the model is not yet up for a sensitivity study to challenge previous studies about the feedback of tundra shrub expansion on land surface albedo?

Given the above considerations I suggest to improve the manuscript the following way: a) upgrade 2SOM / 3SOM to include heat transfer between grid cells. If this is impossible the authors should consider to run DBSM at 8 m resolution but then decrease the grid resolution when switching to 3SOM; b) provide explicit evidence that the models can replicate the evolution of spatial snow patterns as observed; c) allow more space for the introduction of 3SOM. What would Figure 3 a and b look like if modeled with 2SOM, or 3SOM at various grid resolutions? Would any improvements relative to 2SOM show up in Figure 6 and 7? d) reduce the weight currently put on the model sensitivity exercise and discuss the findings more cautiously. Given Figure 6 it may not be justifiable to dedicate more than 50 % of the abstract to findings of the modeled scenarios.

## SPECIFIC COMMENTS

P232/L11: If the three surface sources share a single soil column, does this mean that the surface temperature of snow-free patches cannot exceed 0 °C if  $F_s > 0$ ? I'm probably misunderstanding something here, otherwise this approach would severely compromise the benefit of having a separate energy balance equation for bare ground. This needs further context.

P235/L16-26: I suggest to move the content to the previous page (P234).

P237/L2-3: There is no evidence that the models can replicate the evolution of spatial

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snow patterns.

P237/L20: I thought the domain was 1 km<sup>2</sup>, so what's outside of the central domain?

P243/L7: How are  $F_s$  and  $F_g$  factored in?

P254: Why do the transects not extend into zones with variable shrub density?

P255: Somewhere in the paper it should be mentioned that  $F_s + F_g = 1$ , and  $F_v$  is independent of either (if this is the case).

P258: Why is there no measured data above  $\sim 700$  W/m<sup>2</sup>?

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 223, 2014.

## HESSD

11, C329–C332, 2014

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