Hydrol. Earth Syst. Sci. Discuss., 11, C469–C471, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C469/2014/

Hydrology and Earth System Sciences
Open Access
Discussions

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

Anonymous Referee #3

Received and published: 15 March 2014

HESS doi:10.5194/hessd-11-223-2014

Modelled sensitivity of the snow regime to topography, shrub fraction and shrub Height

C. B. Ménard, R. Essery, and J. Pomeroy

In this manuscript the authors present a significant model development for simulations of energy fluxes between the land surface and atmosphere in shrub dominated, seasonally snow-covered environments. This data–rich, process-based approach to improved understanding of the impact of shrubs and topography in sub-arctic tundra offers an improvement to current capabilities in high spatial resolution modeling. The manuscript is well written and the sensitivity experiment illustrates the importance of better describing spatial variability in snow cover in land surface models. The devel-

opments illustrated by Menard and colleagues are important, but I would like them to consider two important points which I feel would improve the manuscript.

1. The addition of a bare ground component to simulation of spatially variable energy fluxes (especially during melt out) is an important, but incremental addition to our modeling capability of sub-arctic tundra environments to that presented by Bewley et al. (2010). I would like to see some quantitative analyses of how big this improvement is; 3SOM sounds a conceptually exciting proposition when in a traditional 2-component situation, but this needs to be explicitly demonstrated.

2. The authors repeatedly accept that turbulent fluxes between gridbox will have an important impact on melt, but these cannot be accounted for. While I agree that a full consideration of horizontal movement of fluxes between grid boxes is a separate study, some treatment of the effect of gridbox size needs to be included. For example, a short additional experiment testing the impact of gridbox size to evaluate the influence of boundary line location on fluxes would be welcomed.

Minor comments:

P 288, ln 7 – please cite the key relevant studies (of the 100 available) rather than rely on the pers comm.

P 232, ln 8 – can the difference to JULES albedo be stated briefly to explain why this has been changed?

P 232, ln 20 – quantify how much closer modelled SWE and depth are to measurements in 2004 than 2003.

P 233, ln 27 – 'perform well enough'. What is well enough? Can a quantitative threshold be provided for this assertion?

P 234, ln 17 – why did you choose a 8 m grid – please justify briefly?

P 234, ln 18 – what is the resolution of LiDAR data?

P 235, ln 11 – was the WIA – plateau wind speed difference higher or lower?

P 236, ln 3 – rewrite to say 'there are some large errors'.

P 236, ln 24 – are these 'errors' just enhanced uncertainty during melt as a result of increased spatial variability?

P 237, ln 2 – you have not currently shown that the models have been able to capture evolution of braod spatial patterns. Need to demonstrate this or re-write.

P 241, ln 28 – in relation to my first main point above, I would suggest that the known limitation (and improvement resulting from this study) need to be explicitly demonstrated here through comparison between 3SOM and the two-source model.

Table 1 – although relatively intuitive, please state the units in the table.

Table 2 - please state units (presume meters?).

Fig 5 – 'a' and 'b' are not visible on the plots. Are the peaks in the measured data missing prior to April 30? Why?

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