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Hydrology and Earth System Sciences Discussions

Interactive comment on "Measurements and modelling of snowmelt and turbulent heat fluxes over shrub tundra" *by* D. Bewley et al.

Anonymous Referee #2

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The present work presents a modeling study on the turbulent fluxes over shrub tundra in the melting season. It is a very interesting topic, which is very well motivated in the introduction section. This work is one of the very few modeling studies that account for the effects of shrubs on the snow surface energy balance in arctic and subarctic environments. However, in the model presented and used apparently there are several inconsistencies that need clarification:

1) it is not clear why Hv (eq. 3) is multiplied by the vegetation fraction, while Hs and H (eq. 2 and eq. 4) are not, even if they depend on the vegetation temperature (Tv). Tv should be defined only in the vegetated fraction, and the unvegetated fraction should exchange heat directly with the atmosphere without the mediation of Tv.

2) It is not clear on which theoretical assumptions the formulae 6 to 8 for the resistance

are based. Some more explanations are needed. They should come from the logarithm profile, which is not physically correct to assume it valid in the canopy.

3) The C coefficient, after Zeng for dense canopies (eq. 10), may be insufficient to account for possible decoupling of canopy air from air above canopy (see Niu & Yang, 2004, JGR, Effects of vegetation canopy processes on snow surface energy and mass balances; Lee & Mahrt, 2004, HP, An evaluation of snow melt and sublimation over short vegetation in land surface modelling), and here a stability correction should be included. The authors should discuss more the assumptions they are doing in a more theoretical context, rather than stating that the physical processes are not well understood, and then it is worth only using simple formulae.

4) Why vegetation is not described through a LAI (Leaf Area Index) or PAI (Plant Area Index) or SAI (Stem area index)? Concerning this matter, I understand that the snow shrubs burial is an important process, therefore why have the vegetation properties (shortwave radiation transmissivity, sky view factor and vegetation fraction), not been let vary in time, as a physical based representation may suggest?

Other comments:

5) In my opinion, the paragraph 3.3 is the kernel of the paper and should be expanded. No discussion concerning the implications of the theoretical assumptions in the model is done. For example, the fact that in some days fluxes of opposite sign are reproduced could be also related to an underestimation of the vegetation fraction (related to the shrub emergence process), or to an uncorrected representation of the resistance between canopy air and snow surface. In addition, the comparison should be done more quantitatively, rather than saying that the model is able to capture the transition from small to high turbulent fluxes.

6) Apart from being able to reproduce the fluxes, how does the model add something to our understanding of the effect on the shrubs of snow surface energy balance? Can some considerations be done regarding the relative importance of the surface energy

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7) Snow albedo is normally described with a decay function with respect to fresh snow albedo. Here it appears that only constant values are used. This is not fully justified.

8) The measured turbulent fluxes are crucial for the current paper and I think that the flux measurements should be described in more detail, with particular reference to data treatment, fetch condition, and correction applied. For corrections to apply to turbulent fluxes see Reba et al., An assessment of corrections for eddy covari- ance measured turbulent fluxes over snow in mountain environments, WRR, 2008

In conclusion, I think that, if the paper addresses the above-mentioned issues, it will be a very valuable contribution.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1005, 2010.

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Interactive Comment