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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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

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Referee 1

1. The abstract could be made more impactful and more attractive to a reader searching for the paper if you added some specific quantitative results. For example, when you say that the two shrub types differed, state by how much (in terms of days in snowmelt timing, heat flux magnitudes, etc.). When you discuss the model-data comparison, state when (seasonally or in relation to snowmelt timing) and by how much (percentage) they differed.

The valley site held about twice as much snow and melted out 10 days later than the

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plateau site, and the total turbulent heat flux to the atmosphere was 45% greater at the valley site for the two years studied. This information will be added to the abstract. Quantitative model-data comparison is discussed below.

2. Intro. page 1006, lines 15-20: The Latin names of the shrub genera normally should be start with a capital letter (unless journal style differs).

Agreed

3. Sec. 2.1, page 1009: Please add some information about the size of the shrub patches you measured. (And please see related comment below about the eddy covariance measurements.)

This will be greatly clarified by including the pattern of shrub cover in the Figure 1 inset. Information on the size of shrub patches is included in the discussion of eddy covariance measurements below and will be added to the revised manuscript.

4. Sec. 2.2, page 1010: Did you measure only incident radiation? (If yes, please add "incident" here.) If that is true, how did you measure the albedo of the shrub patches? Sec. 3.2 mentions albedo measurements that were used to obtain the parameter values for the model. Please explain more clearly how and when albedo was measured. Finally, it is not clear if you had a net radiation measurement available during the snowmelt time series at the two sites–did you? If yes, this would be useful for evaluating the H and LE fluxes that were measured with eddy covariance system.

The radiation data shown in Figure 3 are incident; this will be stated explicitly. The shrub albedo was determined by measurements over a mass of cut branches on 20 May 2003. Net radiation was measured at both sites and is discussed below.

5. Sec. 2.4, page 1012: I understand that this is not a micrometeorological paper; however, you really need to provide a bit more information that would allow better interpretation of the flux measurements. If the flux measurements are explained in greater detail in another publication, please indicate that reference. Either way, please provide

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a few additional key details here. What was the measuring height at each site? Was the height referenced to the ground or to the top of the snow pack? Was the measuring height adjusted at all during snow melt? What was the approximate size of the shrub patches and what was the length of the shrub-covered fetch in the predominant wind direction? This information is important for interpreting whether the flux measurements are representative of the shrub site and whether the representativeness would have changed significantly during the snowmelt process. Where the snow is deep, the height of the tower above the reference surface (diminishing snow pack and eventually vegetation/ground) would be increasing with time–that would make the flux source area (footprint) grow in size. This effect would presumably be more important at the tall shrub site than the low shrub site, so it could contribute to measured differences between the two. The ideal way to assess this would be using a footprint model, but if the patch sizes are large (in relation to the instrument height), then this would be of less concern. Please provide enough information and explanation to help the reader interpret these issues.

Reba et al. (2009) gives a recent review of the eddy-covariance method for flux measurement over snow; this reference will be added. The measurement heights were 3 m in the valley and 2.3 m on the plateau, relative to the ground. Heights were not adjusted during snow melt, but this would make little difference due to the shallow snow on the plateau and the constant height above the vegetation in the valley. Individual tall shrub patches had an average width of 2.75 m. The predominant wind direction is down-valley, in which direction there is a shrub-covered fetch of several hundred metres. The shortest fetch at the tall shrub site is for flows across the valley; there is shrub cover in excess of 1 m height for 200 m in this direction, so the fetch is large in relation to the measurement height.

Second, did you do any type of assessment of the reasonableness of the flux data? For example, were you able to compare the sum of H + LE against Rnet or do an approximate energy budget closure?

The model imposes an exact closure between turbulent fluxes, snow melt and net radiation, so this provides a check on the reasonableness of the flux data. In the observations, turbulent and snowmelt heat fluxes only amount to 61% of Rnet at the valley site in 2004, but otherwise account for between 80 and 103%.

Finally, if they are not in another publication that can be referenced, please provide at least a brief basic description of key measurement and data processing methods for the flux measurements including issues such as: How/when were the KH2O and LI-7500 calibrated? How was it (and the radiometers and sonic) cleared of snow accumulation (if any) and cleared of frost? What was the separation distance between the sonic and the water vapor sensors, and did you apply a correction (e.g., Moore 1986) for the lateral separation? There is a known problem of sensor self-heating in the LI-7500 that is especially apparent in cold temperatures (see Burba et al. and others)–did you observe these effects and were any corrections applied? (Not necessarily required but it would be good to know if these effects were occurring during the measurements.) What sampling frequency was used and what averaging interval was used for calculating the fluxes? Were (which) coordinate rotations were performed? Were means or running means, etc. removed in calculating the fluxes? Were lag times between the sonic and the humidity sensors computed from the cross-correlations or fixed (or used at all)?

Reba et al. (2009) give a description of measurement methods and data processing for eddy covariance. KH2O and LiCor-7500 hygrometers were used in 2003 and 2004 respectively; these were factory-calibrated prior to installation. Staff were camped on-site throughout the measurement periods to ensure that sensors were clear of snow and frost. Sensor separation was small, on the order of the 10% of height recommended by Moore (1986), and no correction was applied for lateral separation. The self-heating problem identified by Burba et al. (2008) has more influence on CO_2 fluxes than latent heat fluxes and should be less than 3 Wm^{-2} in all conditions (Reba et al. 2009). The sampling frequency was 10 Hz and the averaging period was 30 minutes. Due to

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limitations of the data loggers available, we do not have the full set of high frequency data and covariances required for quality assessment and coordinate rotations, so the flux measurements should be viewed cautiously. Applying the double rotation method recommended by Finnigan (2004) to the valley site sensible heat flux for the period between 22 April and 5 May 2004, however, only gives a mean adjustment of 1 Wm^{-2} and rms 9 Wm^{-2}.

6. Sec. 3.1. The resistance terms are not clearly defined. Please define the resistance terms (verbally) when they are first used in Eqs. 2-5. The order of Eq. 6-8 is not the same as the resistances are presented in Eqs. 2-5...it would be clearer if you listed them in the same order. The r_as with the prime is not explained in Eq. 10.

The resistances defined in Equations 6-8 are for turbulent transport between the canopy air space and the atmosphere, the vegetation and the canopy air space and the snow surface and the canopy air space, respectively. Equations 6-8 will be reorganized to match the order of Equations 2-5. Equation 10 is the definition of r'_{as}; this will be explicitly stated.

Referee 2

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.

H_v is multiplied by the vegetation fraction because it emanates from the vegetated fraction of the surface only; the fluxes H_s from the surface and H to the atmosphere are for the entire surface, so are not multiplied by f_v. The surface exchanges heat with air at the canopy height without the mediation of T_v, as can be seen in Equation 2.

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

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profile, which is not physically correct to assume it valid in the canopy.

The model used here is an adaptation of that presented by Blyth et al. (1999), which is based on the assumptions of logarithmic profiles and no stability corrections in the canopy layer for simplicity. While acknowledging that these assumptions are not physically correct, we expect that they will be of less consequence for the sparse canopies considered here. A reference to CLM (Oleson et al. 2004) will be added here as an example for the benefit of readers interested in the structure of more sophisticated models.

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.

We find small turbulent heat fluxes and snowmelt dominated by radiation at the tall shrub site, so the model is quite insensitive to sub-canopy stability correction. Lee and Mahrt (2004), however, found snowmelt predictions to be sensitive to sub-canopy mixing parameterizations at a more southerly site with shorter shrub vegetation. This is an important reference to add to the paper.

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?

LAI would typically be used in model parameterisations of sub-canopy sky view and

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transmissivity, but these quantities were determined directly from hemispherical photography and radiometer measurements. We are developing a mechanistic model and parameterisation of shrub burial by snow, but we feel that the limited data available for evaluation of the parameterisation do not yet justify its inclusion.

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.

The melting snow cover at the plateau site was observed to be patchy during the periods (days 113-115 in 2003 and 120-122 in 2004) when the model and observations give fluxes of opposite sign. This is a situation that the model cannot represent; while there is snow on the ground and air temperatures are above 0°C, the model will not predict upwards heat fluxes. We are now developing the model to include a representation of snow-free ground and dynamic exposed vegetation fractions to see if that addresses the problem.

The following two tables will be added to give a quantitative evaluation of the model. Some of the errors are large, but the model captures many of the qualitative features of the observed fluxes, and the results were obtained without any model calibration.

	mean	rms	r^2
Plateau 2003	-23.5	52.9	0.78
Valley 2003	-18.8	41.5	0.85
Plateau 2004	-13.6	50.0	0.57
Valley 2004	-25.6	44.4	0.77

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Table 1. Mean and rms errors in sensible heat flux (Wm^{-2}) and r^2 for all simulations.

	mean	rms	r^2
Plateau 2003	2.3	16.0	0.56
Valley 2003	2.3	13.1	0.62
Plateau 2004	-6.0	15.2	0.66
Valley 2004	-2.7	17.0	0.30

Table 2. As Table 1, but for latent heat 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

Observational studies such as Chapin et al. (2005) and Sturm et al. (2005) suggest that changes in shrub cover will change to surface energy balance of tundra regions. Models of shrub tundra and snow surface energy balance, and evaluations of models against observations such as this paper, are required for predictions of these impacts. Chapin et al. (2003) estimate the relative importance of components in the surface energy balance; this reference will be added.

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.

For the situation considered here of aged snow melting without fresh snowfall, the change in surface albedo is dominated by the emergence of snow-free ground rather than changes in snow albedo.

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 covariance measured turbulent fluxes over snow in mountain environments, WRR, 2008

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Turbulent flux measurements are discussed further in the response to comments from Reviewer 1 above.

References

Blyth, E.M., Harding, R.J., and Essery, R.: A coupled dual source GCM SVAT. Hydrology and Earth System Sciences, 3, 71-84, 1999.

Burba, G.G., McDermitt, D.K., Grelle, A., Anderson, D.J., and Xu, L.: Addressing the influence of instrument surface heat exchange on the measurements of CO_2 flux from open-path gas analyzers. Global Change Biology, 14, 1854-1876, 2008.

Chapin III, F.S., and 20 others: Role of land-surface changes in Arctic summer warming. Science, 310, 657-660, 2005.

Finnigan, J.J.: A re-evaluation of long-term flux measurement techniques. Part II: coordinate systems. Boundary-Layer Meteorology, 113, 1-41, 2004.

Lee, Y.-H., and Mahrt, L.: An evaluation of snowmelt and sublimation over short vegetation in land surface modelling. Hydrological Processes, 18, 3543-3557, 2004.

Moore, C.J.: Frequency response corrections for eddy correlation systems. Boundary-Layer Meteorology, 37, 17-35. Oleson, K.W. and 13 others: Technical description of the Community Land Model (CLM), NCAR Technical Note NCAR/TN-461+STR, Boulder, Colorado, 2004.

Reba, M.L., Link, T.E., Marks, D., and Pomeroy, J.W.: An assessment of corrections for eddy covariance measured turbulent fluxes over snow in mountain environments. Water Resources Research, 45, doi:10.1029/2008WR007045, 2009.

Sturm, M, and 7 others: Winter biological processes could help convert Arctic tundra to shrubland. BioScience, 55, 19-26, 2005.

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