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The Cryosphere Discussions

Interactive comment on "Micrometeorological processes driving snow ablation in an Alpine catchment" *by* R. Mott et al.

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Summary: This paper presents an observational and modeling study of micrometeorological processes impacting snow cover ablation across alpine terrain. A combination of an atmospheric model (Advanced Regional Prediction System or ARPS) and a surface energy balance model (Alpine3D) is used to examine small-scale meteorological controls on snow ablation. Model simulations are validated with snow depth data derived from Terrestrial Laser Scanning (TLS) and with energy fluxes measured by an eddy covariance system. The authors find enhanced snow cover ablation rates at the upwind edges of snow patches attributed to the advection of sensible heat from bare ground. The authors conclude that the generation of stable internal boundary layers becomes especially important in controlling the turbulent fluxes later in the ablation

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Interactive Discussion

Discussion Paper

season. The paper is clear and well-written and should be published once the following minor comments are addressed:

General Comments:

1) The abstract could be improved by quantifying some of the results. For instance, how strong must winds be for the advection of sensible heat to become important for the ablation of snow cover over long fetches? What is the threshold snow cover fraction for which the model overestimates ablation?

2) How does the Advanced Regional Prediction System (ARPS; see page 2169) take into consideration cloud cover and precipitation? The presence of clouds will affect the amount of solar radiation reaching the surface and hence ablation rates of snow. Snowfall would increase snow depth and SWE whereas rainfall may contribute to snowpack ablation. It is unclear how these meteorological processes are handled in the current modeling framework. If neglected, then the authors should provide in-situ solar radiation and precipitation data to back up their assumption these can be neglected during the 2009 ablation periods.

Specific Comments:

1) P. 2160, line 6: Define “ARPS”.

2) P. 2161, line 20: Liston (1995) should be added as a relevant reference here.

3) P. 2162, line 11: The authors may define the acronym “SWE” here and use it elsewhere in the paper. The corresponding sentence is difficult to read and should be rephrased.

4) P. 2162, line 16: The sentence starting with “Most of these studies...” is highly repetitive and should be rephrased.

5) P. 2162, line 19: Is this horizontal or vertical wind speed?

6) P. 2164, line 13: “SLF” is not defined.

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7) P. 2165, line 12: Are the spatial domains shown in panels (b) and (c) of Figure 1 the areas over which the modelled and measured changes in snow depth and SWE and reported in Table 1? If not, then the snow survey domains should be shown in these maps.

8) P. 2166, line 5: The authors provide a reasonable explanation for omitting ablation period P4 from the analyses but do not report why ablation period P1 is also excluded from the analyses.

9) P. 2167, line 6: Were direct measurements of latent heat fluxes from the eddy covariance method also available?

10) P. 2168, line 16: Note the spelling mistake in “constant”.

11) P. 2168, line 20: What is the potential implication of neglecting changes in snow depth on the effective height above ground of the meteorological instruments? How deep does the snow reach at this site relevant to the positioning of the ultrasonic anemometers?

12) P. 2169, line 4: Note the spelling error in “friction”.

13) P. 2169, line 11: What is the albedo of bare ground in the simulations?

14) P. 2170, line 11: Note it should read “kg m-3”.

15) P. 2170, line 12: How is the vertical profile of measured snow temperature distributed homogeneously across the model domain when snow depths vary spatially? Is this a bulk snow temperature then?

16) P. 2170, line 13: It should read “nearby”.

17) P. 2171, line 3: Insert “the” before “surface”.

18) P. 2171, line 19: Insert “occurs” after “effect”.

19) P. 2173, line 9: There is a discussion of the latent heat flux here but no description

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of how this is computed in section 2.4 that describes the modeling system.

20) P. 2175, line 20: Section 2.2.2 stated that ablation period P4 was excluded from the analyses yet results are presented here for this ablation period.

21) P. 2176, line 5: Reverse the order of the text to read “also becomes”.

22) P. 2176, line 17: Note that bare ground is also not restricted to temperatures $\leq$0°C as snow is, leading to potentially greater heating of a bare surface. How is the energy balance computed for bare ground?

23) P. 2176, line 19: Replace “huge” with “large”.

24) P. 2178, line 4: Close the bracket in “(Fig. 9a)”.

25) P. 2178, line 18: Rewrite as “a shutdown”.

26) P. 2183, line 29: Morris (1989) is not in the proper alphabetical order.

27) P. 2184, line 11: Insert a hyphen in “spatial-temporal”.

28) P. 2184, line 24: Delete the space in “layer”.

29) P. 2185, line 2: Note the spelling mistake in “prairie”.

30) P. 2185, line 5: Capitalize “Antarctica”.

31) P. 2186, Table 1: Why not provide the simulated changes in snow depth and SWE in addition to the observed data? I thought the advection analysis was not performed for P4 due to low, variable winds.

32) P. 2188, Figure 2: This plot could be improved by adding a horizontal line at 0°C to highlight potential periods of snow ablation. Additionally, why not include other meteorological parameters measured at the site, such as wind speed and wind direction, precipitation, snow depth, etc.? Are the data shown hourly?

33) P. 2190, Figure 4: Do you have any observational data to validate the model simu-

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lations? In the caption, insert “the” before “legend”. The legend for panel d has positive numbers for dSWE while they are negative for the other panels.

34) P. 2192, Figure 6: A difference plot would highlight those regions where the simulations diverge most from the observations. Why are the spatial domains for the simulation results shown in Figures 4-6 all different?

35) P. 2194, Figure 8: Are these local times? Are these fluxes simulated or observed?

36) PP. 2195/2196, Figures 9/10: The snow surface temperature (TSS) appears to go above 0°C during daytime here. Is this correct? Line S2 in the first panel is not visible either in Figure 9 or 10.

Interactive comment on The Cryosphere Discuss., 5, 2159, 2011.