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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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Final response

We thank the two referees Michael Kuhn and Stephen Dery as well as the editor Michiel Van den Broeke for their valuable and very constructive contributions and comments. In the following we answer all comments. Furthermore we list all relevant changes made in the manuscript:

REFEREE#1

Comment 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?

Response: We added quantitative results to our statements in the abstract.

"The effect, however, appears to be active over rather short distances of about 4 - 6∼m. Measurements suggest that mean wind velocities of about 5 m s-1 are required for advective heat transport to increase snow ablation over a long fetch distance of about 20 m. Neglecting this effect, the model is able to capture the mean ablation rates for early ablation periods but strongly overestimates snow ablation once the fraction of snow coverage is below a critical value of approximately 0.6."

Comment 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.

Response: We only use the mean flow fields calculated with ARPS to run the energy balance model (with respect to the local magnitude of turbulent fluxes) of Alpine3D. The altering of incoming shortwave radiation through terrain is calculated with the fully distributed radiation model (Helbig et al., 2010). We force the radiation model with measured incoming shortwave and longwave radiation from one meteorological station and assume clear-sky conditions. Thus, we do not neglect solar radiation and precipitation within our energy balance simulations. To clarify this point, we have inserted this information in more detail in the methods part:

"M1: The energy balance model of Alpine3D is driven by meteorological input (air temperature, relative humidity, incoming longwave radiation, incoming shortwave radiation, precipitation) from a single weather station located in the study area (WAN 3). In addition, we use wind velocities on a grid with 5 m resolution obtained from mean flow fields calculated with ARPS."

Specific Comments: 1) P. 2160, line 6: Define “ARPS”.

Response: we changed ARPS to Advanced Regional Prediction System;

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

Response: we added Liston (1995) as a 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.

Response: we defined SWE in line 11 and used the acronym elsewhere. We also rephrased the sentence to:

"In recent years, extensive research into the main processes driving the spatial variability of snow water equivalent (SWE) has been carried out."

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

Response: we rephrased “Most of these studies” to : These studies . . .

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

Response: This refers to the vector wind velocity composed of the horizontal and vertical wind velocities.

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

Response: We removed SLF here.

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.

Response: We inserted the measurement domain of P3 and the model domain in Fig. 1. No snow ablation rates were measured and calculated for the flat field Weissfluhjoch. Note that the measurement domain varied with the snow-coverage in the course of the ablation period. We added this information the figure caption.

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

Response: we now try to be more clear in the methods section:

"In this study we only analyse P2 - P6, because snow melt only started around 1 May, 2009 (Egli et al., 2011). The numerical analysis already starts on 9 April 2009, the time of the ALS measurement campaign."

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

Response: We did not calculate latent heat fluxes from the eddy-covariance system because we had fast gas analyzers only for the Versuchsfeld and because we considered the contribution of latent heat flux to melt to be much smaller than the sensible heat flux.

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

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?

Response: we always considered the height of the anemometer above the snow surface and not above the ground.

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

Response: corrected;

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

Response: The albedo of bare ground was set to a default value of 0.3. Please note that this will not affect the energy balance at the snow cover because we do not calculate any feedback to air temperature.

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

Response: corrected;

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?

Response: We assume the vertical profile of temperature to be distributed homogeneously, also independent of varying snow height. We only had a few single point measurements of snowpack temperatures available. At the initial state (9/04/2009), the measured temperature profiles were near isothermal. Due to cold air temperatures in mid and end of April, temperature profiles adjusted to colder temperatures.

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

Response: corrected;

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

Response: corrected;

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

Response: We corrected this;

19) P. 2173, line 9: There is a discussion of the latent heat ïňĆux here but no description of how this is computed in section 2.4 that describes the modeling system.

Response: We use the Monin-Obukhov bulk formulation to calculate turbulent fluxes of latent heat within Alpine3D and describe it in the text.

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.

Response: we only excluded P4 for the local advection analysis (reasons are given in section 2.2.2), but not for the analysis of the spatial distribution of ablation rates and the corresponding processes.

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

Response: we reversed the two words here;

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

Response: we adjusted the text to read: "The heating of bare ground is invoked by a significantly lower albedo for bare ground than for snow-covered areas. Also, the surface temperature of bare ground is not restricted to 0°C."

The energy balance is calculated for the bare ground in the same way as for snow, but with different surface characteristics (e.g. albedo, conductivity). It is not important for the calculation of snow ablation, however, because no advection of sensible heat is implemented in the model (as stated in the text), which means that the surface temperature of bare ground will not affect snow ablation rates. Nevertheless, we indirectly account for the heating of bare ground, because we use the measured air temperature from the weather stations to drive the energy balance model. The measured air temperature will be influenced by the heating of bare ground in the surroundings.

"The overestimation of heat fluxes above snow is believed to be caused through the initialization of the energy balance model with the measured air temperatures at 3 m above ground. This temperature is not in surface layer equilibrium with the snow surface. Based on the constant flux layer assumption we simply integrate between the surface temperature values of snow and the atmospheric values (see Eq. (2)), neglecting the atmospheric stability directly above the snow surface, which forms locally within a new internal boundary layer and effectively decouples the warmer temperatures from above."

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

Response: we replaced huge with large.

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

Response: we closed the bracket;

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

Response: we now write shutdown;

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

Response: we changed the order;

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

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

Response: we deleted the space;

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

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

Response: we capitalized 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.

Response: Yes, P4 was excluded from the advection analysis; we changed this in the table and added the modelled changes in snow depth obtained from model setup M1;

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?

Response: we added a line at 0°C in Figure 2 and also included snow depth as a second figure panel;

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

Response: As stated in the methods section, we measured ablation rates for each of the ablation periods with TLS. We decided to show a comparison of the measured and modelled spatial distribution of ablation rates only for P3. In addition, we compare measured and modelled mean daily ablation rates for all periods in Fig. 7. We changed the legend in Fig. 4.

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?

Response: We added a difference plot in Fig 6. The figure caption is changed accordingly: "Modeled (a) and measured (b) daily ablation rates in snow depth (HS) averaged for the time period P3 (12 May – 15 May, 2009). Differences between modelled and measured snow ablation (c). Note that negative values indicate an underestimation of snow ablation by the model. The x- and y axes give Swiss coordinates in meters. (base map: Pixelkarte PK25 © swisstopo (dv033492))." The measurement domain is due to measurement shadows which of course have not been reproduced for the model domain. In Fig. 6, we compare modeled and measured ablation rates. To facilitate the readability and comparability we therefore decided to show the same domain for modeled results as for measured results. In Fig. 4 and 5, where results are not directly compared to measurements we show the whole model domain (also those pixels which could not be measured with TLS). Otherwise we would lose too much information.

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

Response: These are local times. Fluxes were modeled. Note, that we do not have any measured fluxes at the Wannengrat area for spring 2009. The figure caption is changed accordingly: "Modeled mean energy fluxes for ablation periods P2 (a), P3 (b), P4 (c), P5 (d) and P6 (e): sensible heat flux (Qs), latent heat flux (Ql), net radiation (Q*), melt energy (Qm) during daytime (7:00AM - 5:00PM Local time) for snow covered cells at the end of the respective ablation period."

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.

Response: We changed the color of Line S2 in Figures 9 and 10. Positive snow surface temperatures are due to problems with the accuracy of the sensor. We add this information in the text: "Note that positive snow surface temperatures are due to limitations in the accuracy of the infrared sensor."

REFEREE # 2: Comments: Comment: 1. how variable is the initial snow depth distribution from year to year?

Response: "At the Wannengrat test-site, the snow depth distribution was proven to be highly affected by the local wind field (Mott et al., 2010) leading to end of winter

snow depth distribution with a high inter- and intra-annual consistency (Schirmer et al., 2011). They found correlations ranging between r=0.97 and r=0.93 for the end of winter snow depth distribution of two consecutive years."

Comment 2: 2163, 11: what is meant by “an atmospheric model”?

Response: we changed atmospheric model to atmospheric prediction model

Comment 3: 2165, 22: omit “simply”

Response: we omit simply here

Comment 4: 2167, equ.1: add cp

Response: we add density and heat capacity of air in the formula.

Comment 5: 2168, equ.3 and line 22: give an example of how Ch changes if zo is changed by a factor of 2 and add your opinion of the uncertainty of zo.

Response: The values for z0 for snow typically vary between 0.001 and 0.05, depending on the influence of terrain and the footprint. For our investigations we found the value of 0.01 leads to an overestimation of the turbulent flux. Z0=0.005 → Ch=0.0236 → Qs= 284 W/m2; Z0=0.01 → Ch=0.0302 → Qs= 364 W/m2; Z0=0.001 → Ch=0.0201 → Qs= 254 W/m2;

Comment 6: 2169, 4: “friction” instead of fiction

Response: done

Comment 7: 2177, 9 and Fig.9: is L determined from measured covariance or from modeled fluxes? Why is there unstable layering (L<0) in the morning hours when TA is always > TSS? Make a comment on positive values of TSS.

Response: Note that the sensors show fluxes and stability for discrete points above the snow cover. Although TA measured at the weather station is above the snow surface temperature, a local temperature maximum below 4 m (which may be caused by the advection of a sheet of warm air) will lead to an unstable atmosphere at the second level (S2), while stable conditions are still observed at the first level (S2). This is a good example of air temperature measured at a discrete height at a weather station giving a wrong estimate of the atmospheric stability at this point. For some of our data this means that the maximum temperature is below that measured by sensors 1 and 2 (both sensors show an unstable atmosphere). If stable conditions are only shown by the S2 sensors, the temperature maximum is between two and four meters. This shows that the height of SIBL is varying! We added a discussion to the text and included an additional figure, in which we schematically sketch the temperature profiles and sensible heat fluxes. This was sketched for various situations in the morning following the constant flux layer assumption (model) and the non-constant flux layer assumption:

Results and discussion: "Air temperatures were continuously higher than snow surface temperatures (Fig. 9c, Fig. 10c). Consequently, continuously stable conditions for the layer between the snow surface and the measurement height could be assumed. The additional flux measurements, however, indicate unstable conditions at both sensors (S1 and S2) for most situations in the morning (Fig. 9b and Fig. 10b). These results suggest that there must be a local temperature maximum below 2 m for these situations (Fig. 11, situation 1). For some hours measurements even showed a flux directed downwards at S1 (stable), but upwards at S2 (unstable) (Fig. 9a,b and Fig 11). Thus, the local air temperature maximum was between 2 and 4 m (Fig 11, situation 2). This strong temporal flux divergence in the first four meters above ground is a clear sign of the existence of a stable internal boundary layer. We believe that the advection of a sheet of warm air in the morning caused this temporal change of the depth of the stable internal boundary layer. " . . .. "For most situations in the morning, the constant flux layer assumption of the model led to a wrong estimation of the atmospheric stability at higher levels (2 - 4 m). For these situations the model assumed the whole layer to be stably stratified up to 4 m, although measurements showed that the height of stable internal boundary layer was below 2 m. Consequently, the model strongly overestimated the sensible heat flux directed towards the snow

cover (Figs. 9a, 11)."

Positive snow surface temperatures are due to limitations in the accuracy of the sensor. We add this information in the text: "Note that positive snow surface temperatures are due to limitations in the accuracy of the infrared sensor."

Comment 8: 2177, 23: “has been cooled by longwave radiation” I would prefer “by a negative energy balance”

Response: we changed *cooled by longwave radiation* to "cooled by a negative energy balance"

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