Dear Referee,

We would like to thank you for taking the time to review our paper and for all your constructive suggestions, which will help to improve the quality of the paper. For now, we would like to answer to your major comments (and some larger specific comments). Our response to the comments appears in *italic*. We will take the remaining detailed comments into account when preparing a revised version.

1. The authors describe a process they term advection of turbulent heat flux and reference studies discussing local advection of sensible heat as described in Mott et al. (2018) and also Harder et al., (2017). It is not clear to me to which term the authors are really relating to as it seems to me that they mix up advection of sensible heat with the vertical turbulent sensible heat flux. The ambiguity becomes particularly clear when the authors compare modelled sensible heat fluxes with estimated advected sensible heat as presented in Harder et al (2017). I recommend to include equations where they clearly state at which terms they are looking at and how these are calculated. Equations for advection of sensible heat are presented in Harder et al. (2017) and Mott et al. (2020).

   In a revised version, we will describe more clearly which process we are considering. Also, we will include equations stating which set of equations is used by the model (e.g. van Heerwaarden and Mellado., 2016; equation 12) and which terms we are considering.

   Regarding the comparison with the results of Harder et al. (2017), we do think that the performed dimensional analysis for setting up the DNS is consistent with the system presented by Harder et al. (2017). However, we do realize that Figure 7 needs some revision. We will adapt Figure 7, such that we can distinguish the advected energy from the currently presented turbulent sensible heat fluxes at the surface. For this comparison, we will use equation 2 from Harder et al. (2017), which will also be referred to in our revised version.

   van Heerwaarden, C. C., & Mellado, J. P. (2016). Growth and Decay of a Convective Boundary Layer over a Surface with a Constant Temperature, Journal of the Atmospheric Sciences, 73(5), 2165-2177, https://journals.ametsoc.org/view/journals/atsc/73/5/jas-d-15-0315.1.xml

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2. The Introduction of the process and its relevance could be extended to allow the readers an easier access to the very complex interplay of near-surface boundary layer processes that become important over patchy snow covers. I think that the manuscript would particularly benefit from a more detailed background (also including “older” studies) on wind-driven heat exchange processes, the development of internal boundary layers (.e.g. Granger et al., 2002; Essery et al., 2006) and the local advection of sensible heat (e.g. Marsh et al., 1999).

   We will include a more elaborate discussion of the amount of research done regarding the topic. Thank you for your suggestions.

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3. The connection between the experimental and the numerical part of the manuscript is not totally clear to me. For the experimental part, the study would particularly benefit from a more detailed

analysis on the spatial aspect of the process, i.e. analysis of fetch distance related snow melt and advection estimates. What is the added values of the experimental part?

We use the field observations to illustrate that the processes can be important and even can be observed with relatively simple and cheap methods on relatively short timescales. Of course, especially the importance has also been shown by previous studies.

Also, we try to discuss which processes play a role for the melt we observed in the field with the help of the simulations. Additionally, these simulations show the potential of DNS to be used for studying this kind of system. As disadvantage, these simulations are in an idealized environment and do not include any complex interactions, for example between topography and atmosphere, which probably are playing a role in the field. Therefore, in the discussion we try to uncover which processes are missing in the simulations and how these could affect our understanding of what is going on at the observed snow patch.

We do realize that this has not been formulated elaborately enough and will add this to a revised version.

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4. Why are such extreme boundary conditions used for the DNS leading to unrealistically high calculated turbulent heat fluxes? In my view, more representative meteorological boundary conditions (i.e. matching up with the conditions at the observed snow path) would provide more meaningful conclusions. Also, Schlögl et al. (2018a) did a similar modeling study using ARPS. Please set your results more in context of this recent study. What are the benefits of using DNS? How do the results compare? What do we learn? How can we represent the process in larger-scale models?

   We used the conditions reported by Harder et al. (2017) for 30 March 2015. Indeed these conditions are relatively extreme, whereas the usage of the meteorological conditions at our observed snow patch would allow for a better comparison. However, due to the absence of accurate local meteorological measurements at the snow patch, we decided to use the data reported by Harder et al. (2017), with the advantage that their system is relatively more similar to an ideal system. In a revised version of the manuscript, we will treat these choices and consequences more elaborately.

   Additionally, we will include a discussion on how our study relates to Schlögl et al. (2018), also treating the benefits and drawbacks of DNS. Among these are the advantage that DNS does not use the Monin-Obukhov similarity theory, of which the horizontal homogeneity assumption is violated for a patchy snow cover, but also the potential influence of the applied boundary conditions and relatively low Reynolds number on surface fluxes.

L134: Why did you not measure the spatial distribution of snow ablation over the entire snow patch? How did you determine the local wind direction? Also, was the wind fetch always constant through the measurement time period?

Indeed, having a photogrammetry product covering the entire snow patch would be ideal for this study and allow for a more detailed analysis of the snowmelt. However, this would require other equipment than what was available. Still, with the equipment at hand, we try to illustrate that with relative simple *and cheap methods, it is possible to come up with relatively decent snowmelt estimates. In a revision, we will explain this more elaborately.*

The reported values for the wind direction (Table 3) are obtained from the meteorological flux tower. Through experiencing the local wind direction at the field site, we determined that this local wind direction resembled the wind direction at the flux tower. We are aware that these numbers include uncertainty, but still are illustrative for the wind direction at the snow patch. When revising, we will emphasize that the reported wind direction numbers are only an indication, and not necessarily the exact numbers at the field site.

L139: why did you not measure SWE for the entire snowpack? Doing so at different sites with different snow depths would allow a more precise information on SWE of the snowpack at the snow patch. Was the snow pack already isothermal at the start of the measurement campaign?

We are aware that taking these samples only on a single location and only on one day does not reflect the potentially complex spatial (and temporal) dynamics of the snow density and SWE. However, we assume the variations occurring on these spatial and temporal scales to be relatively small compared to other uncertainties introduced to our method for computing contribution estimates of the turbulent heat fluxes to the snowmelt. Moreover, we do think that these densities are realistic estimates and represent a continuously ripe snowpack, given the fact that largest discharge peak had taken place already 1.5 month before the fieldwork (Figure 1) and the air temperature never decreased to freezing point during the campaign (Table 4). Additionally, during the campaign it was noted that the snow pack was relatively wet. In a revised version of the manuscript, we will articulate these considerations.

L 161: what do you mean by assuming a snow albedo between 0.6 and 0.8? changed the value in time? Can you provide a reference for choosing those numbers? The albedo value has an extreme effect on your energy balance calculation and your estimated contribution from turbulent heat fluxes.

We agree that it is not clear how we used these albedos. We will express this more clearly in a revised version. These albedos are both used in the computations, because we don't know the exact albedo of the snow patch, let alone spatial and temporal variations. Moreover, with this range we try to account for other uncertainties we have in the shortwave radiation component. This also is the main cause for the ranges in our eventual estimates.

The values are based on Harding (1986), who did measurements in the same region, in approximately the same time of year and reports albedos varying around 0.8 in May. We will add this reference in a revised version.

L 318: how do you define up-wind and downwind edge? is it the first grid cell? How do you deal with grid cells which become snow-free during the observation day? The daily-melt rate will be underestimated if you also consider pixels which become snow-free during a measurement day. Would be interesting to see a snow ablation rate curve depending on fetch distance.

We agree that the text describing the SfM, and especially the post-processing of the DEM and orthoimages can be better formulated. We will include a more elaborate explanation in the revised version of the manuscript.

To answer your comments, we have grids for two locations, i.e. the upwind and downwind edge of the same snow patch. So, when referring to either the upwind or downwind edge, we mean the location of the grid (Figure A1).

Through the filtering process (which we will state more clearly in the revision), we only consider grid cells that are continuously covered by snow and have a recorded height change on each day, to reduce the chance of cells being random scatter. Indeed, as additional advantage this method does not include cells with relatively shallow snow depths of which the recorded melt could be affected by the presence of the bare ground. Our choice for these filters is supported by the fact that when loosening these filters, the size of the boxplots increases drastically, also to unrealistic values and variations in snow surface height, such as large increases over the course of these 5 days.

The resulting height differences over time correspond to 6.7 m^2 and 30.7 m^2 for respectively the upwind and downwind edge. We are aware that these areas are limited by our filtering choices, especially on the upwind edge due to the varying locations of snow covered grid cells or the retreating snow line (Figure A1). For the downwind edge, the approximately constant location of the snow covered grid cells combined with the little retreat at this edge, causes this area to be significantly larger. Even though these resulting areas are relatively small, we are convinced that the obtained height changes obtained are decent estimates, also based on our error estimates.

Unfortunately, as a disadvantage of the size of the upwind area consisting of multiple separate smaller areas, we decided to treat the edge as "point" and not look further into the spatial distribution of the recorded melt (e.g. how is the melt related to fetch distance?). The smaller areas are too far apart to do so.

L336 and table 4: Please provide more precise explanation on your estimate ranges. Please also state whether any spatial interpolation is done to the meteorological variables or not.

L343: I assume that you are taking the difference of snow melt due to radiation (equation2) and the actual snow melt to estimate the contribution of the turbulent heat flux. Please add more information how you exactly calculate the turbulent heat flux (latent and sensible turbulent heat flux?)

Indeed, our explanation on the computations used to come up with our estimate ranges can be clarified. In a revised version, we will include a more precise explanation on these computations.

For the meteorological variables, we have not applied any spatial interpolation. We are aware that these number do not exactly represent the local circumstances at the observed snow patch. However, the shortwave radiation is treated with the potential uncertainties and the longwave radiation is assumed to be an appropriate estimate for the larger region. For both, we agree that we have not dealt with all potential uncertainties, which we also try to discuss in Section 4.1. Yet, we will more clearly define these uncertainties in a revised version.

L353/354: and how does this compare to the contribution at the downwind edge? As mentioned earlier it would be extremely interesting to have a fetch distance related estimate of the contribution of turbulent heat fluxes (sensible and latent). Also, if you provide a number of 60-80% contribution at the upwind edge – what does this exactly mean? Over which area? As known from other studies, the contribution strongly changes with fetch distance. These high numbers of 60-80% might be very misleading looking at the relevance for the catchment scale snow melt. It would be very interesting to see an analysis on the contribution of heat advection to total snow melt for varying snow patch sizes and snow cover fractions. Furthermore, the relative contrition of heat advection to total snow melt strongly depends on the spatial variability of snow depths as snowpacks with a high spatial variability of end of season snow depths are typically characterized by a longer time period of the patchy snow cover stage and therefore a higher importance of the heat advection process. A more detailed discussion would allow a better comparison to the study of Schlögl et al., 2018a. Please relate to results of Schlögl et al. (2018), who tried to put the local scale estimations into the catchment scale context to draw conclusions for its relevance.

As we explained in a previous comment, we treat the observed height change at the both edges as "point" data, due to the small coverage area. Indeed, if we had better coverage of the areas, an analysis of the spatial distribution of the melt would be very interesting and provide insight into the role of the turbulent heat fluxes.

Regarding the estimated contribution of the turbulent heat fluxes to the snowmelt at the upwind edge, we will articulate more clearly how this melt does relate to the downwind edge in the revision and also state how these numbers relate to snowmelt on catchment scales. For this perspective, we will also relate to the results of Schlögl et al. (2018).

Section 4.1: These estimations include many uncertainties (snow density differences depending on snow height, differences in shortwave radiation between snow patch and actualmeasurement location due to terrain shading, albedo). The high number of turbulent heat fluxes at the surface do not tell us how much of this turbulent heat flux originates from the higher air temperatures at the upwind edge caused by the local advection of sensible heat. Regarding the uncertainty in the net shortwave radiation the authors should consider doing radiation modelling for the area for the respective time period including high-resolution terrain information.

We agree that there many uncertainties in computing these estimates. We therefore specifically chose a relatively large range in albedo to cover the uncertainties in shortwave radiation, and we include these uncertainties in our subsequently computed melt estimates. So we can still be confident that the numbers hold and support our conclusions.

We also agree that performing radiation modelling combined with high-resolution terrain information is relevant for snowmelt runoff simulations. There are planned studies looking specifically into this issue for our study region (cf. e.g. Silantyeva et al., 2020), but it would be out of scope to consider this in the study we present here.

Both of these points, we will discuss this more elaborately in a revised version.

Regarding the contribution of the higher air temperatures and moisture content at the upwind edge caused by the local advection of turbulent heat, we do assume that the atmosphere has adapted itself to the patchy snow cover and is approaching equilibrium. In all directions and great distances from the observed snow patch, there was a patchy snow cover present. Based on this we do assume that our estimate of the total turbulent heat flux is dominated by the local advection of sensible and latent heat to come up with our estimates. Yet, we are aware that these estimates can be affected by the large scale atmospheric conditions. We will add these considerations to a revised version and also relate this to our revision of Figure 7.

Silantyeva, O., Burkhart, J. F., Bhattarai, B. C., Skavhaug, O., and Helset, S.: Operational hydrology in highly steep areas: evaluation of tin-based toolchain, EGU General Assembly 2020, Online, 4–8 May 2020, EGU2020-8172, https://doi.org/10.5194/egusphere-egu2020-8172, 2020