Anonymous Referee #1 Received and published: 6 April 2018

General comments: This work presents a simple model for the advection of sensible and latent heat, which is very welcome in hydro-meteorological studies. A certain strength of this study is the availability of experimental data presented by Harder et al., (2017). Generally, the manuscript is well written and presents interesting results on the effect of heat advection, especially the relative contribution of latent heat versus sensible heat considering different upwind surfaces. I encourage the authors, however, to improve the structure of the paper, which is confusing at some parts – especially in the results section. In its current form the manuscript provides information dropwise and some is missing (mainly in the methodology part). Also, the authors miss to introduce the process of heat advection and the complex nature of resulting heat exchange over snow. Although the model is a simplified approach not accounting for some of the processes, the interaction between heat advection and boundary layer development over patchy snow covers should be shortly explained in the introduction part. The presentation of the model results is a bit vague, especially when the authors explain the non-existing difference in the energy balance when using heat advection and without using it. The explanation is not very convincing to me. This part certainly needs improvement. Furthermore, the effect of heat advection is based on one certain model input. A kind of sensitivity analysis with at least varying relative humidity, air temperature and wind speed would provide a better estimate of the range of relative contribution of heat advection to total melt energy.

Thank you for the detailed review of this work. Comments and suggestions are addressed in the following specific comments below in red text.

Detailed comments

1. Introduction: The references are very limited and only refer to model approach of heat advection. The process itself and how it affects the heat exchange over snow is very complex and should be introduced here. Already published experimental studies on the influence of heat advection on the boundary layer and heat exchange over patchy snow covers are not referenced at all (Mott et al., 2016 and Mott et al., 2017) or are not discussed in the introduction (Harder et al., 2017). The number of recent scientific studies on local heat advection are very limited. To highlight these efforts in the last few years these results should be discussed and referenced here to motivate the study presented here and the need for a new/extended model approach! There is also one new approach, a temperature footprint approach, presented by Schlögl et al., (under review, but close to acceptance). If the work is accepted earlier, it would be interesting for this study to give a comparison of model estimations of the effect of heat advection to total snow melt. Please add Sauter and Galos, 2016 to the references as they also applied LES to simulate local heat advection, but over glacierized area.

For simplicity this work is focused on modeling advection and therefore the introduction was limited to advection modelling references. The introduction is therefore lacking observational contributions and these are now used to expand and strengthen the description of this complex energy exchange process in the revised manuscript. Without seeing this new temperature footprint approach of Schlogl et al. it will not be possible to conduct such a comparison.

2. Methodology P3: In addition to the reference to Harder et al. (2017) I would like to see a very brief description of the SSAM model, especially in comparison with the EBSM model. This will be important for later comparisons and interpretations of model results. Although references are given, the paper should stand on its own and should provide all information necessary to understand the methodology.

   The description of the SSAM model is now expanded in the revised manuscript.

   EBSM: here it would be worth to already mention the indirect consideration of the patchy snow cover in the model by the mixed albedo approach and how this is implemented in the model (briefly).

   The EBSM description of its indirect approach to advection has been moved to the methodology section in the revised manuscript

   2.1: an information on the development of SCA in the model area would be very interesting as in many areas the patchy snow cover duration is very short, compared to the continuous snow cover situation. This means that the effect to total snow melt can be rather small and strongly depends on the spatial snow cover distribution. Snow covers with a high spatial variability will show a longer period of patchiness, thus stronger influence of heat advection to total snow melt. Also, this should be discussed in the results part.

   In the Canadian Prairie domain of level topography with shallow snowcover, SCA can go from complete to patchy to no-snow very rapidly (but dynamics every year are different) and the influence of advection is very dynamic and brief. Notwithstanding, the objective of this manuscript is to introduce a simple model that is not limited to this domain and therefore it would be tangential to focus on SCA dynamics for the Canadian Prairies. The SCA depletion model used here accounts for the relationship between SCA and spatial variability of snow depth. This model dynamics are re-emphasized in the revised manuscript.

   P4: how do you determine the atmospheric stability, you use for coefficient b? Does this refer to the upwind stability only or also to stability over snow? Even if this information is provided in Granger et al., 2002, such information is critical for understanding the methodology. You are using fixed atmospheric conditions to test the effect of heat advection: Of course, chosen relative humidity, air temperature and wind velocity have a large effect on the results and a sensitivity analysis would be very important at this point. At least cases with low and high humidity should be added to this analysis – the same for wind speed and temperature. This is especially important when showing the differential behavior between dry and wet upwind surfaces, as the atmospheric stability and the boundary conditions of air temperature are very important for the results.

   The upwind atmospheric stability used for coefficient b is a function of the snow-free surface temperature or humidity and the blended atmosphere temperature/humidity and uses the parametrizations as proposed by Weisman (1977). It quantifies how much energy is entrained in the air mass over a snow-free patch. The assumption made is that this is the same amount of energy that will subsequently be removed by exchange with the snow surface downwind of a snow transition to reestablish a steady state equilibrium. This approach was implemented as attempts to explicitly account for stability are very sensitive to the underlying stability similarity function assumptions and to the nature of the boundary layer schemes that are implemented. These may not be appropriate for all situations. The approach used here provides an alternative to excessive boundary layer model complexity and so can be more readily used in snow predictive models. More detail has been added to the revised manuscript. Figures 6-8 use fixed conditions to express the model behaviour and sensitivity to inputs which obviously departs from any actual snowmelt situation and this is why the analyses in Figure 9 and 10 are presented to show implication of application with real meteorological data. A sensitivity analysis of Tsoil, RH, Ta and u (in addition to Twat) has been added to the revised manuscript.

3. Results: Section: 3.1 Especially the neutral stratification approach is very problematic as very high stabilities and instabilities can develop due to advection processes. Strong atmospheric stability, for example, will lead to a decoupling effect (see Fujita et al., 2010; Mott et al., 2016; Mott et al., 2017), preventing heat advection to be transported towards the snow cover. Of course, such processes cannot be accounted for by such a simple model, but these limitations need to be discussed somewhere in the results section.
Also note that this approach is highly sensitive to an accurate estimation of atmospheric conditions (stability). This should be clearly stated in the text.

We appreciate the role of stability but have not found it to limit the role of advection in our three decades of field experiments in the prairies, Arctic and mild mountain topography. As stated this entire model is meant to be simple and avoid the dynamics of stability, which can make modelling this phenomenon non-trivial. The stability dynamics raised in these papers are now discussed in the revised limitations section. We note that the goal of the manuscript is to propose a framework to estimate areal average advection contributions- not propose a final model. It identifies the key processes that need to be parameterized and provides an initial approach for each. Future work by the authors or other contributors will be required to refine each process representation.

P10, L7: please write boundary layer depth instead of simply saying boundary layer.

This has been corrected in the revised manuscript.

P11, L8-10: this sentence should be reformulated – I do not really understand the meaning of this because it is still an average and not a total rate. Advection is only active over a certain fetch distance over snow. This means that a decreasing snow cover fraction not necessarily means that the areal average melt rate/energy decreases. I would even say that the opposite is the case because the percentage of snow pixels affected by heat advection increases resulting in an increase of the mean average melt.

This sentence has been rephrased. Advection over a specific patch increases melt rates per unit area of snowcover as more energy becomes available, on the other hand as the snow-covered area (SCA) decreases then the areal melt flux decreases. To represent the same control volume as one-dimensional models the energy is represented as an "areal average" term that accounts for SCA. Ultimately the areal average melt rate will be a function of the snow surface melt rate (advection and non-advection contributions) and SCA (and depending on the specific rates of change) and will differ from that estimated by assuming a fixed SCA and ignoring advection. This has been clarified.

Figure 7: I really like this figure as it nicely shows the fluxes depending on SCA and for the different setups. This figure is, however, not really discussed in the text. Interestingly, not only the net advection flux changes when considering wet or dry upwind source areas, but also the peak of the flux is shifted to later stages in the melting period. Please also discuss this point in this section here, because this has a very strong implication for the effective duration of the melting period and thus snow hydrology.

More discussion of this figure has been included in the revised paper.

P12: section 3.3.: This section on the implication of process representation is not clear to me. Please explain more clearly why an implementation of advection processes to the energy balance term does not really change the SWE depletion curve. Is this explained by low frequency of clear days favoring energy advection? How do you explain lower areal averages of snow melt for the earlier year when considering the advection process?

When implementing advection one is also constraining the exchange surface to SCA. Therefore advection will only increase melt if its contributions are greater than the corresponding decrease in areal melt energy with declining SCA.

The 2015 year melt period was characterized by low wind speeds meaning that the advection contributions were relatively more limited than the windier 2016 period.

P12: L 12-15: SSAM and SLHAM-SSAM simulations do not only show very small differences in SWE depletion but also in the calculated fluxes – which is not explained here.

Total energy may not be different but the sources of the energy area and this has been expanded upon.

P12/L20: what do you mean with vertical snow-atmosphere fluxes – turbulent fluxes of sensible and latent heat? Also, this explanation is very vague.

Yes, we meant turbulent terms. This explanation has been clarified in the revised manuscript.

P12/13: section 3.4.: yes, the energy fluxes will compensate each other in case of dry upwind surfaces, but the sensible heat fluxes are therefore larger leading to larger net fluxes. Reading the text at is presented now, it appears as the compensation leads to lower net fluxes for dry surfaces than for wet surfaces. This is also shown in Figure 7. Table 6 shows that including advection does not really change the turbulent fluxes above snow? Can you explain that more in detail?

If wet surfaces are the same temperature as dry surfaces then this would be the case but field observations show this is not the case as the latent heat (evaporation) lead to much cooler wet surfaces. Despite such compensation, dry surfaces will still have a larger net advection term than wet surfaces.
The advection and turbulent transfer terms are uncoupled in this framework so this interaction in not explicitly included in the model and therefore cannot be discussed/investigated.

Section 3.6.: The authors already provide a limitations section. Within this section I would like to see a short discussion on processes that are not covered by the presented approach but are shown to be important for situation with strong heat advection. Such processes are mainly induced by the increase of local stratification close to the ground leading to a suppression of the advection effect or even decoupling effects (these results are discussed in Fujita et al., 2010; Mott et al., 2016 and Mott et al., 2017). As mentioned earlier, I strongly miss the connection to experimental findings (apart from Harder et al., 2017) achieved in the last years. This also means a discussion on the complex nature of boundary layer development during advection situations, which of course is difficult to include in a simple advection model. The reader should, however, be aware of this.

This has been addressed in response to previous comments and the introduction/limitations section has been revised in the updated manuscript.

Also, heat advection is strongly reduced in the downwind distance over the snow patch. This strong dependency of heat advection on fetch distance has strong implications of the spatial snow melt dynamics and the duration of the melt season. I would like to see a discussion on limitations that are connected to areal average advection

The relationship between heat advection and downwind distance is implicitly accounted, and already discussed/addressed, by the model through application of snow patch length scaling laws.

Conclusions: Model results indicate that advection constitutes an important portion of melt energy: 11% of the melt observed in the 2016 snowmelt season. I am bit confused because Table 6 (also Figure 9) shows almost no difference in turbulent heat fluxes when using the advection model???? The authors try to explain this in section 3.3.2, but this explanation is still not very convincing.

Differences in turbulent fluxes are largely due to the differences associated with the SCA depletion and to a lesser extent any feedbacks through the quantification of the surface temperature (which is constrained to be a maximum of 0C during snowmelt). The terms presented in Table 6 are the net sensible and latent heat terms and account for the compensation between SCA (exchange surface) and differences in advection and non-advection exchange intensity. This has been clarified in the revised manuscript.

Additional information on the mean snow patch size and duration of patchy snow cover is important for your model estimation of 11%. Furthermore, an upper limit of the contribution of heat advection to the total melt energy, depending on snow patch size distribution and duration of patchy snow cover would be highly interesting. Although not published yet (but very close to acceptance) the paper of Schlögl et al., 2018, presents estimates on the effect of heat advection of total melt rates of a catchment (increase of melt rates of approximately 3- 5%). As these are the first studies really estimating a contribution of advection to melt energy for the whole melting season, these results should be compared.

Mean snow patch size is not a meaningful metric as distributions of patch sizes are highly skewed and there is no consistent decrease in size during melt. Snow patches receive differing net advective energy and also break up as they ablate, making snow patch geometry very complex during ablation. The upper limit of advection contributions is highly sensitive to the input variables which will vary greatly between regions and therefore we hesitate to provide such a constraint. Without seeing this new temperature footprint approach of Schlogl et al. it will not be possible to conduct such a comparison.

P 15, L11: a "to" is missing here ntroduction:

Will be corrected

Table 6: what is the unit here?

MegaJoules/square metre

These references need to be added:

Sauter and Galos, 2016: Effects of heat advection on the spatial sensible heat flux variation on a mountain glacier, The Cryosphere, 10, 2887-2905,2016.

Fujita et al. (2010): Fujita,ÂaK.,Â ˘ aK. Hiyama,Â ˘ aH. Iida, andÂ ˘ aY. Ageta ˘ (2010),ÂaSelfâ ˘ A˘ Rregulated fluctuations in the ablation of a snow patch over four ˘ decades,ÂaWater Resour. Res.,Â ˘ a46, W11541, doi:10.1029/2009WR008383. ˘

Mott et al., 2016: Mott, R., Paterna, E., Horender S., Crivelli, P., and Lehning, M.: Wind tunnel experiments: Cold-air pooling and atmospheric decoupling above a melting snow patch, Cryopshere,10, 445-458, 10.5194/tc-10-445-2016

Mott et al., 2017: Impact of Extreme Land Surface Heterogeneity on Micrometeorology over Spring Snow Cover. J. of Hydromet. , DOI:Âa10.1175/JHM-D-17-0074.1.

References have been added as appropriate.