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This is an interesting study combining some classical modelling approaches with modern measurements of advection over patchy snow and highlighting the role of latent heat fluxes. I just think that there are some errors that need to be corrected before publication. As they differ from previously published results, it would also be interesting to see an example of the snow patch images and power laws fitted to patch number and size data.

Thank you for your detailed review of this manuscript! All responses will be in red text following each comment. Some examples of snow patch images and fitted power laws are included in the revised manuscript.

page 1, line 18 “advection of dry air” would be a more physically appealing description than “negative latent heat advection fluxes”.

This has been revised

page 2, line 21 It is not correct to say that advection of LEA from ponded meltwater is not represented in any model; the Liston (1995) model advects moisture and assumes that the snow-free patches are saturated.

Agreed and this has been clarified in the revised manuscript

page 3, line 15 It seems unlikely that “Initial melt is dominated by energy advecting from emerging snow-free patches”, which initially only provide a small source area.

“Initial melt” should read “Initial advection contributions to melt”. This has been revised

page 3, line 17 “energy entrained by air movement across isolated snow-free patches” is not completely advected to surrounding snow if the snow surface is aerodynamically decoupled from the warmed air as observed by Mott et al. (doi:10.1175/JHM-D-17- 0074.1).

As clarified with respect to RC1 this model ignores stability influences to propose a simple model framework. This limitation is acknowledged and clarified in the revised manuscript.

page 4, line 20 Coefficient a is not dimensionless.

Coefficient a is represented by a best fit parametric expression/scaling relationship proposed by Granger 2002 which gives it dimensions of W/m3 and this is updated in the manuscript.

page 4, line 27 If heat and moisture are advected by the same mechanisms (presumably the justification for assuming the same parametrizations of a and b), what is the justification for using different stability parameters?

The stability parameters all come from Weisman 1977 and the only difference is that they represent differences due to consideration of the units of temperature or water vapour scalar gradients.

page 4, line 30 A pedantic point, but humidity is a property of air; “surface humidity” is not a meaningful quantity, and what is intended here is humidity in the microlayer where exchange between the surface and the air occurs.

This has been revised.

page 5, line 1 “surface water vapor pressure”

This has been revised.

page 5, line 5 “_soil” should be subscript

This has been revised.

page 5, line 25 The derivation of Equation (25) is opaque. Trying to reproduce it, I arrived at the equivalent but more compact expression Sret = 1 π sin(πF) − F cos(πF) (1)

Applying trigonometric identities the same expression is resolved. We now use your more elegant expression. Thank you!

page 6, line 3 More informatively, Equation (10) is a closed form fit to the parametric SCA curve produced by homogeneous melt of a log-normal SWE distribution.

This has been revised.

page 6, line 13 A more intuitive way to write Equation (11) would be F(Ap) = Ap Amin −Dk/2 (2)

Agreed. This has been revised.

page 6, line 17 Hack’s law relates stream length to basin area. Granger et al. (2002) attribute the use of Equation (12) relating linear dimension and area to Rignon et al. (1996).

This has been revised.

page 6, line 25 The integrand in Equation (13) should be written as either F(Ap)dAp or F(x)dx, but the equation is incorrect anyway. Probability is given by an integral of a probability density function, which F(Ap) is not; 1 − F(Ap) is a cumulative distribution function, the derivative of which would be a probability density function. I think that the intended equation is p(Api) = F(Api−1) − F(Api) (3)

Agreed, this relationship was inappropriately presented in the equation in the manuscript whilst the code used reflects this more appropriately. This equation has been revised.

page 8, line 24 Table 2

This has been revised.

page 10, line 3 It would be useful to state that HA and LEA are estimated by Harder at al. (2017) from vertical temperature and humidity profiles.

This has been clarified

page 11, line 21 No justification is given here for the statement “It is evident that SLHAM can quantify the key advection behaviours”.

This has been clarified to be with respect to first order controls on the advection process.

page 12, line 4 Because three figures with normalized time axes have already been presented, the normalization needs to be explained before this.

This section has been reworked in the revised manuscript so this so comment is no longer applicable.

Figure 4 Dk, as defined by Equation (11), should be positive.

This has been revised.

Table 1 A in the parameterizations for b should be W

This has been revised.