Comment on tc-2021-171

Anonymous Referee #1

Review of: "Understanding wind-driven melt of patchy snow cover" by Luuk D. van der Valk, Adriaan J. Teuling, Luc Girod, Norbert Pirk, Robin Stoffer, and Chiel C. van Heerwaarden

This paper is a fascinating modelling approach to address the very challenging process of wind driven local scale advection contributions to snowmelt. This process has been a persistent challenge on snowmelt modelling but the high complexity of the physical processes has stood in the way of parsimonious/satisfying solutions. This paper provides a unique/novel modelling approach to the problem, especially with the model not requiring a parametrised stability scheme, and overall I think that this is a very important contribution. There are a number of areas that I think need work in this manuscript in order to strengthen its conclusions, and place it more completely into the literature context. With these in mind I would suggest that this paper requires revision prior to publication and would highly encourage the authors to consider what I hope to be constructive comments. I will begin with a number of general comments followed by more specific/technical comments.

Major Comments:

- Literature context: I get the impression that the authors may be newer to the snow field and so some of the statements in the introduction (see comments) need revising. Broadly there is a tremendous amount of research on processed based- energy balance snowmelt modelling that needs a clearer summary/context for this work. In terms of local-scale advection work there is a much more limited amount but can be found back to the 1970's. This needs a more complete treatment to solidify the context of this contribution as well as to distinguish these contributions from previous one. (See comments for examples).

- Validation with SFM: I'm not exactly clear on the process by which the snow depth difference are calculated via SfM on the observed patch in order to compute expected ranges of turbulent flux contributions and model validation. Are we only considering snow depth difference for grid cells that were completely uncovered during melt (so we can have a bare ground reference) or are we also considering cells that were not fully uncovered that would have also decreased in snow elevation due to melt? What areas do these number represent? Do you observed a decrease in melt from leading edge? Many dynamics can be examined with a spatial dataset but I'm not clear on how this is also processed/what it represents and so would welcome a lot of clarification and perhaps a figure to describe this process.

- Latent heat flux decay: There is an extensive treatment of the sensible heat flux decay with patch length while this is not discussed with respect to latent heat flux. Can this also be included or is there are reason it is not included. The heavily cited Harder 2017 paper suggest that latent heat is also an important contributor or at times compensatory (Harder 2018) and so would be very interested in seeing if some of those dynamics could be captured in this modelling scheme.

- Implications: there are some interesting dynamics explained but I'm not exactly clear on how those could be implemented in larger scale snowmelt prediction. There is a scaling relationship articulation for sensible heat over a patch length. Is this considered to be a parameterisation that could be used in basin scale snowmelt prediction models

Specific Comments:

Line 39-40: Snowmelt, especially over continuous snow cover, is governed by the surface energy balance (radiation AND turbulent exchange processes) with radiation being the dominant source (Male and Granger, 1981). Turbulent processes, for which air temperature is a proxy, can clearly be important (especially with advection as seen here). Commonly used empirically based temperature index models erroneously lead to the impression that snowmelt is related to air temperature but if we are to be focused on process interactions this statement is problematic.

Line 43-45: this contradicts the lines 39-43. Distinction between the scales of advection are needed. (Shook and Gray, 1997) Large scale air mass movement can drive turbulent exchange because otherwise the temperature and humidity gradients will tend to

equilibrium as noted here.

Line 53-54: TI models are empirical so another major criticisms is their applicability when applied outside of their calibration periods or domains – especially in prediction of future changes. There are many other physically based snowmelt models out there besides Alpine3D (which is based on SNOWPACK), such as snobal (Marks et al., 1998), CROCUS (Vionnet et al., 2012) or in the multitude of land surface schemes.

Line 61. Few models parametrise lateral snow distribution processes fully/explicitly (CHM (Vionnet et al., 2021) and APLINE3D are the only 2 that come to mind that have actual process level physics involved)– most others are often based on simplified parameterisations.

Line 61-63: (Harder et al., 2018) provides a simple snowmelt advection model to account for subgrid variability in melt. (Marsh et al., 1999, 1997) provide an approach to account for sensible heat advection.

Line 63-64: (Harder et al., 2018) provides a advection modelling framework that makes an argument that in some situations upscaling with and including advection will not make any different to discharge predictions. Ie things can get complicated when the snowmelt is increasing in rate but decreasing in area.

Line 71-76: missing the advection work of Marsh found in the publications in 1997 and 1999.

Line 8—81: "spatially highly variable character of melt rates can complicate the observations" - > "high spatial variability of melt rates complicate the observations"

Line 83-90: there is a tremendous body of work on snow remote sensing at high resolutions that far exceed the Offenbach and rittger references which are not the most appropriate to consider local scale advection dynamics. Perhaps recast this in terms of remote sensing that is suitable for advection (ie high temporal frequency <=daily, and spatial resolution <<10m). Some high resulting satellite products coming online now but really should focus on aerial platforms (ie ASO (Deems et al., 2013; Painter et al., 2016), drone based (you have many of the UAV-Sfm references but lidar applications are coming online now as well (Harder et al., 2020; Jacobs et al., 2021), and terrestrial laser scanning (Grünewald et al., 2010; Hojatimalekshah et al., 2020), and georectification of oblique time-lapse photography (Härer et al., 2013)

Line 129: of -> from

Line 131-133: wind direction was constant you state. Can you provide a wind rose or some sort of metric to quantify this?

Line 138-140: when were these samples taken with respect to the observation interval as snow density is dynamic over melt? Was a snow tube used? Snow pit? How were 100ml samples collected? Did the melt period have a consistently ripe snowpack?

Line 153: desolated - > isolated?

Line 153-159: not exactly clear on this methodology. Base on this and images in Figure A1 we are only looking at the edges and measuring surface change for where the snow melted and a bare ground surface appeared? Related to figure A1 – how do the upwind and downwind edges line up – unclear as they are plotted in separate rows?

Line 160-165: How deep was the snowpack and do you have any information to say that the snowpack was ripe at the start of the melt. Were the cold content requirements satisfied at the start of the period and so all energy could be assumed to be related to melt.

Equation 2: I believe SWE should instead be snow density?

Figure 3: how were the surface temperatures and gradients between snow and non-snow generated? I may be blind but can't seem to see this.

Figure 4: I'm not exactly clear on what area constituted an upwind or downwind edge and how that relates to a specific number/boxplot. There will be a gradient of change. Can this be clarified? This approach does not consider height changes if it that spot does not become snow free by June 15?

Line 334-336: It seems SWE and density are being used interchangeably here which is not correct. Can this be cleared up? These are pretty high densities. Any observations from field notes about water saturation or other structural attributes. What was the overall snow depth variability? Can you report the SWE of the snow patch?

Line 353-354: 60-80% based on computing the overall melt energy needed and radiation melt and the turbulent portion as the residual of this energy balance? If so can that be clarified?

Line 398-403: Granger et al., 2002 and Weisman, 1977 propose similar power law relationship to describe a sensible heat flux. Perhaps worth contrasting this formulation and the meaning of your terms with those papers?

Section 5.3: this section exclusively discusses sensible heat flux only. Your model also considers latent heat flux and observation are available from Harder et al 2017. Can this also be considered or is there are particular reason you did not bring latent heat flux into the results here?

Line 459-461: same order of magnitude regardless of patch size on the upwind edge? Sentence seems not complete.

Line 462: "microclimates" - > micrometeorological? Would suggest that these are very dynamics occurrences unlike what is captured with the "climate" term.

Line 464-467: terrain absolutely plays a role with snow distributions but this is a rather simplistic explanation for very complex physical processes underpinning blowing snow redistribution. Topography, meteorology, surface characteristics all conspire to make any domain very complex in terms of snowpack distribution variability. I'd step this back and say that snow patch size distributions (if available) would improve snowmelt predictions. There are many tools and statistical descriptions of snowpack's available to do so (see snow pack scaling laws in Harder et al., 2018 and the papers cited therein that consider fractal geometry for example).

Line 467-468: "The melt estimates obtained with the SfM photogrammetry are in line with own expectations based on visual estimations, whereas the estimated errors are relatively small." Can you clarify what you mean with "in line with own expectations" - meaning is not apparent to me.

Line 480-490: have you run any simulations with a higher Re in line with Harder 2017 so that you could make some more conclusive predictions of this RE- decay relationship?

Line 664-665: please change to the non-discussion version of this paper

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