Editor Comments:

Overall, the manuscript was largely improved and I am convinced of the importance of this study.

In a next round, please consider the comments of both reviewers, and in particular the comments of reviewer 2 regarding (discussing) potential constraints and strengthening the statistics and uncertainty analysis.

I am looking forward to the revised manuscript addressing the comments in detail.

Best regards,

Franziska Koch (Associate Editor, The Cryosphere)

Reviewer 1: Benjamin Bouchard

The authors have substantially improved their manuscript from the first version submitted to The Cryosphere. They have addressed the vast majority of comments raised by the reviewers. In this sense, I recommend this version of the manuscript for publication, subject to one clarification. In Appendix C, the authors report a multiplication factor applied to RAWS data for shortwave radiation and refer to the main text for explanations (see captions of Tables C2, C3 and C4). However, the justification for the multiplication is missing from the manuscript. Before publication, I would like this point to be addressed and the multiplication factor clearly explained in the manuscript.

Thank you for pointing this out. We have added the following text to lines 225-226:

"Incoming shortwave radiation for each simulation used the RAWS station data and a location specific multiplication factor determined by the 1 Mar solar radiation model from the 1-meter DEM (Fig. 1c)."

Reviewer 2

I appreciate the authors revisions and find the topic of this study highly relevant. However, I maintain my concern regarding the reliance on qualitative reasoning in drawing conclusions, which often lacks robust support from the field data.

Specifically, the following points require further attention:

The study scope (and title), encompassing five campaigns over a single melting season (five months), presents a limited dataset. This constraint significantly affects the robustness of your conclusions, particularly regarding the complex interactions being investigated in a partially forested area (the observations are of SWE and density but the main driving process seems to be the isolated by the authors in the lateral flow of liquid water). Indeed, the influence of the forest canopy on snow processes is a critical factor, especially considering the presence of at least three transects within forested areas. The manuscript would benefit from a more rigorous approach to disentangling the effects of canopy cover from other variables. Currently, it is unclear how the observed increases in snow density and SWE at the North and South base sites (inside the forest) during April and May were isolated from canopy influences.

We agree that it is not entirely isolated from canopy influences, though there will generally be denser canopy where there is more water available for vegetation to grow so it is difficult to fully disentangle these processes. We have modified the text (lines 415 – 424) to better reflect this:

"Canopy and wind drifting influences are not interpreted as major contributions to the redistribution of SWE at Dry Lake due to the wind direction being parallel to the study slope contours and observations of drifting not occurring at our transect locations. However, some canopy shading will influence the observations at the base of the north aspect hillslope though terrain shading dominates the energy balance as previously mentioned. Further, the deciduous canopy at the base of the south aspect has been observed to have more SWE than on the hillslope (Webb, 2017), but under canopy conditions here generally have lower snow density whereas we observed an increase in density at the base of the south aspect slope. Thus, we interpret the lateral flow of liquid water to be an important factor in the redistribution of SWE as presented in the perceptual model for the Dry Lake site (Fig. 8), though we are unable to fully disentangle the influence of canopy and lateral flow from one another."

The derivation of uncertainty requires a more rigorous formulation. As it stands, the lack of a clear statistical framework hinders the comparison of density measurements. I strongly recommend a more robust approach, such as: calculating the standard deviation of the snow depth measurements along the transect. Propagating this uncertainty to estimate the uncertainty in the averaged density and SWE. This will provide a more transparent and statistically sound basis for comparing measurements and assessing the significance of observed differences.

Currently, the manuscript implies differences in density and SWE between locations. However, without a proper uncertainty analysis (also in the plot expressed as uncertainty bars), it is difficult to determine if these differences are statistically significant.

To enhance the scientific rigor of the paper and provide a more comprehensive understanding of the interactions between canopy, topography, and snow processes, I suggest the authors address these points.

It is well-understood that forest canopy will be denser where plant available water is more abundant. This will happen, for example, on north facing slopes where more snow accumulates. We mention this in lines 356 – 358: "This difference in canopy cover is likely attributed to aspect as the deeper snow and increased soil moisture on the northern aspect increases the amount of plant available water for vegetation growth". Thus, it is not possible to fully disentangle the role of canopy from the processes. We have modified the text as quoted in the previous comment to better reflect this.

For the uncertainty, we have revised the analysis as suggested to use standard deviations with revisions as follows.

Lines 179 – 186:

“2.4 Uncertainty Estimation of Survey Data

The above-described methods in estimating ρ_s and SWE require additional estimates of uncertainty. We used the standard deviation (σ) of the GPR TWT and manually measured d_s data to estimate the uncertainty associated with the derived values using equations (1), (2), and (3). Due to d_s and GPR TWT being correlated to one another and not independent, we estimate the range of v through:

$$v_{+\sigma} = \frac{d_s + \sigma_{ds}}{\frac{TWT + \sigma_{TWT}}{2}} \tag{4}$$

$$v_{-\sigma} = \frac{d_s - \sigma_{ds}}{\frac{TWT - \sigma_{TWT}}{2}} \tag{5}$$

where $v_{+\sigma}$ and $v_{-\sigma}$ are the v calculated with variables plus or minus their associated σ, respectively; and σ_{ds} and σ_{TWT} are the σ associated with d_s and GPR TWT, respectively. These values of $v_{+\sigma}$ and $v_{-\sigma}$ were then used to propagate this variability through equations (2) and (3).“

New Figure 4:

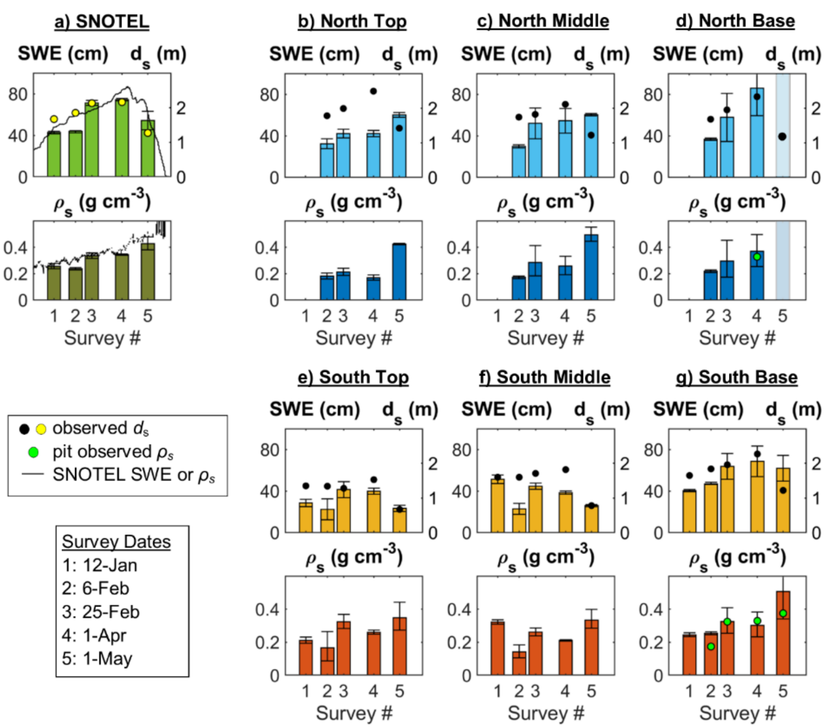

Figure 4: Results from the transect observations including calculated SWE, observed d_s, and GPR derived ρ_s for: a) Flat Terrain around the SNOTEL station, b) North Top, c) North Middle, d) North Base, e) South Top, f) South Middle, and g) South Base locations. Pit measured average densities are shown when collected, and SNOTEL station data are displayed for additional

comparisons of SWE and ρ_s. Uncertainty bars are shown for SWE and GPR derived ρ_s using σ of collected data. Note that the GPR results that gave unrealistic values due to the presence of liquid water is slightly greyed in panel d.