

Dear Editors and Reviewers,

We would like to first thank you for the time in reviewing our manuscript and providing constructive feedback. We have now revised the manuscript based on this feedback and believe the manuscript is greatly improved. Below you may find the responses to each comment that was made, with responses in [blue text](#).

Sincerely,

Ryan Webb

Reviewer 1

In the study 'Aspect Controls on the Spatial Re-Distribution of Snow Water Equivalence in a Subalpine Catchment', the authors investigated how slope aspect influences snow accumulation and melt dynamics using ground penetrating radar (GPR) transects and snow pit and snow core density measurements during the water year 2022-2023 in a small catchment in Colorado, USA. The authors also used the SNOWPACK snow model to support their observations and further develop a conceptual model of water redistribution in the snowpack based on aspect. Overall, this study provides interesting results using different instruments and tools (GPR surveys, snow pits, weather stations and a multi-layer snow model). I acknowledge the authors' efforts in combining these elements to propose a conceptual framework to better understand how aspect may exert a control on the evolution and (re)distribution of snow water equivalent (SWE). However, the proposed conceptual model is based on indirect observations and modelling results rather than on field evidence, which should be clearly stated. I also believe that the manuscript would benefit from a more straightforward storyline and some elements such as the influence of forest canopy on SWE should be detailed. After addressing these issues and some additional minor and technical comments, I am confident that this paper will be a good and relevant scientific contribution to snow research and will provide insights for future studies.

[Thank you for the comments and very detailed review. We appreciate the constructive comments that we agree improved the interpretation of this work. Below are replies to specific comments in blue as well.](#)

Major comments.

Structure of the introduction

The introduction needs to be clarified so that the reader understands the relevance of the study. Here are some suggestions:

The first two paragraphs would benefit from being restructured into one and generally rephrased and shortened.

This has been done.

The third paragraph (l.47 to l.73) is too long and goes in all directions. I would recommend the author to split this paragraph into two, based on the description of methods (l. 47 to 60) and landscape control of snowpack properties (l. 61 to 73).

This was already two paragraphs, but the format of the manuscript does not indent the first line of a new paragraph.

The last paragraph should be completely revised. Lines 85 to 91 should be moved earlier in the Introduction. I would recommend that authors make links to earlier parts of the introduction to emphasise the relevance of their work. I also strongly suggest that a research aim for this work be clearly defined (which is not the case at present).

We have revised this paragraph.

The new paragraph now reads as follows (lines 84 – 89 in revised manuscript):

“Therefore, to assess seasonal variability in the spatio-temporal distribution of SWE as it relates to energy balance dynamics, we employ L-Band GPR technology to survey snow depth and density with respect to north and south aspect slopes and the relative position on each slope. We use these techniques to answer the following research question: How do variations in snowmelt dynamics impact snow density and SWE distribution throughout the snow season based on aspect and relative location on a hillslope? We aim to answer this question in a manner that will provide insights to snowmelt dynamics for mid-latitude forested mountains that develop a seasonally persistent snowpack.”

## Modeling setup

Several questions remain about the modelling setup.

What parameters did the authors use for each simulation (north, south flat)?

We have now added all of the modelling decisions to appendix C (Lines 453 – 469 of the revised manuscript)

Were soil layers defined in these simulations? If not, is there a reason for this, since the authors have a description of the soil (see lines 101 to 104)?

We did not define soil layers as the main purpose of the modeling in this study is to inform us on the timing of snowmelt events, especially snow surface melt events. (line 203 of original manuscript, 205 in revised manuscript).

There are several parameters in SNOWPACK that are site-dependent or have to be chosen arbitrarily by the modellers. What values did the authors use in their simulations for these parameters?

This is now included in appendix C.

I understand that the canopy was taken into account for the flatland simulations. This implies that several other parameters have to be specified. What values did the authors choose?

We apologize for any confusion, canopy was not considered for the flat simulations. This has been clarified in revisions. The flat terrain pit and transect was in a large clearing that had minimal effects from canopy that needed to be simulated.

Line 222: "canopy was not considered in our modelling to represent general conditions for each terrain condition"

I think adding an appendix with the different keys enabled in the model and the parameters used for each simulation would be a clever way to answer these questions. Consider also explaining any arbitrary choices and how site-specific values were obtained.

Appendix C was added as suggested.

### Influence of the canopy

One of my main concerns with this article is that the influence of canopy cover on the spatial distribution of SWE is not adequately addressed. While canopy control is presented in the Introduction (l. 66 to 71) and some results are interpreted based on the canopy in the Discussion (l. 294-298; l. 325-334), the role of vegetation is not presented in the Results section. While I respect the authors' decision not to make this the main focus of their paper, I think the article would benefit from consistent treatment of the influence of canopy on snow redistribution alongside aspect control. Please consider including this in a revised version of your manuscript.

We have added text throughout the manuscript to point out that transects were selected there snow drifts do not occur and any shading is dominated by terrain. We also point out that the location of vegetation is likely a result of more snow accumulating on the north aspect hillslope creating an environment of more plant available water during the growing season, so it is difficult to entirely remove the role of vegetation from the role of aspect.

### Limitations

The fact that not every interpretation is based on field evidence is not critical. However, I would suggest that the authors include a section on the limitations of their study. This would help the reader to better contextualise some of the analyses, especially with regard to SWE redistribution processes through the snowpack.

We have added section 4.2 Limitations of Study to the manuscript. Lines 330-338:

#### “4.2 Limitations of Study

It is important to also discuss the limitations of the present study and potential ways to overcome these in future studies. The SNOWPACK simulations used could be further calibrated to the conditions of each hillslope. In future studies the use of a multi-dimensional models could also be beneficial to further consider the influence of forest canopy and wind transport, factors that have been found to be just as important as aspect at other sites (Mazzotti et al., 2023). However, there is not currently a hydrologic model that incorporates lateral flow through snow, so more snow pits along with quantitative observations of lateral flow processes could further our understanding of these processes (e.g., Thompson et al., 2016). The use of sensors installed within a snowpack could also provide further time-series data (e.g., Díaz et al., 2017) to observe the presence and ponding of liquid water at particular locations of interest. These data could provide more precise observations rather than the bulk estimates using the methods in the present study to further support the interpretations for the perceptual model described below.”

Minor comments.

I. 16. Explicitly mention the use of snow pit and soil moisture monitoring measurements in the abstract.

This has been done in the revised manuscript.

I. 32-33. I do not think this sentence is necessary. Please remove.

The sentence has been removed.

I. 33-36. This sentence is difficult to understand. Please rephrase and break it down into two sentences.

Revised sentences are as follows (lines 36-39):

“Shifting global patterns in moisture delivery contribute to the increased importance in measuring SWE for snow-dominated catchments (Clow, 2010; Nolin et al., 2021). Thus, the expansion of snowpack monitoring is necessary to account for spatial and temporal variability found in mountainous environments (Painter et al., 2016; Fassnacht, 2021).”

I. 42-46. I am not sure if I understand this sentence correctly or if it is necessary for the general understanding of your study. Please rephrase or clarify this idea.

Revision made in new lines 35-36: “these sites offer limited use in streamflow forecasting due to them being point measurements and forecast methods do not account for deviation from climate stationarity (Sturm et al., 2017; Bales et al., 2006).”

I. 57-60. Please consider breaking it down into two sentences.

Through revisions we broke it into three sentences (lines 75-80): “The use of ground-based survey techniques such as GPR allow surveys at intermediate spatial scales (between point-based stations and

airborne platforms). When paired with precise measurements of snow depth ( $ds$ ),  $\epsilon$  can be used to estimate snow density (Sommerfeld and Rocchio, 1993; Kovacs et al., 1995; Webb et al., 2018c; Bonnell et al., 2021; Mcgrath et al., 2022). Because the GPR signal is sensitive to properties such as snow density, GPR surveys enable the interpretation of snowpack properties as they relate to various physiographic controls (Webb, 2017; Mcgrath et al., 2019; Tarricone et al., 2023; Marshall and Koh, 2008; Bonnell et al., 2021; Mcgrath et al., 2022)."

I. 61. I think starting a new paragraph here would improve the readability of the introduction.

It was already a new paragraph, but the format of the journal does not indent the first line of a paragraph.

I. 66-68. You mention the energy balance, but then refer more to the mass balance of the canopy (e.g. accumulation by canopy, interception). Perhaps you should just mention that the canopy changes the energy and mass balance of the snowpack.

Revised text (lines 45-48): "Canopy is another feature that can alter snowpack energy balance (Musselman et al., 2008; Webb, 2017). Canopies can prolong melt by shielding snow from shortwave radiation (Musselman et al., 2012; Varhola et al., 2010; Lundquist et al., 2013). Canopy will also influence the wind redistribution of snow, increasing the variability of snow accumulation and melt..."

I. 74. Consider specifying the 'bulk' snow density here. The distinction is particularly important as you go on to present detailed density profile measurements (Fig. 6). I would also consider adding a few words on how snow density at the layer scale varies with landscape characteristics.

"bulk" now added to snow density here (line 53 in revised manuscript).

I. 77-80. Why is the derivation of snow density from permittivity given for dry snow only? A few words about this method applied to wet snow would be relevant.

We now mention that the method can also be used to determine liquid water content. (new manuscript line 77).

I. 82. Please clarify the meaning of 'spatial relationships'.

This term has been removed from the revised manuscript.

I. 87. I am a bit uncomfortable with  $k_s$  being the symbol for the dielectric permittivity of snow.  $k_s$  often refers to the thermal conductivity of snow. Please consider using the symbol ' $\epsilon$ ' for permittivity.

Revised as suggested.

I. 90. Please delete the following: 'being dragged as fast as a surveyor can traverse the snow'.

Done.

I. 90. What is  $ds$ ? This variable has not yet been defined.

This term is now defined in line 76 of the revised manuscript.

I. 100. Please specify the historical period of the measurements.

Done. Revised manuscript line 97.

I. 103. Do you have the average thickness of the litter? If so, please specify.

Revised manuscript lines 101-102: "A layer of forest litter, or duff layer, also forms on the north aspect hillslope at a depth of approximately 8-15 cm with depths up to 20 cm at the base of the slope."

I. 110-119: Please consider shortening the details of how the DEM and canopy height models were developed.

This paragraph has been revised to state the DEM was used, but focus on the watershed characteristics.

Lines 106-115: "LiDAR data were used to develop terrain and canopy height datasets to quantify the spatial variability of the site (Co, 2016). Using a 1-meter digital elevation model (DEM), the flat terrain shares low angle north to west facing surfaces and contains the tallest canopy height resulting in moderate solar radiation (Fig. 1b). The north aspect consists of a mixture of north to west facing surfaces and the south aspect consists of primarily south to southeast facing surfaces. Solar radiation was calculated using the solar radiation tool in ArcGIS Pro for 1 Mar (Fig. 1c). The north aspect has medium to low solar radiation from terrain shading and the highest solar radiation is seen on the south aspect hillslope. Also from the DEM, the north aspect is slightly steeper than the south aspect. A canopy height was also calculated (Fig. 1d) showing denser canopy at the base of the hillslopes, with a shorter sparse canopy at the middle of the north aspect, and open canopy near the top of the north aspect. There is less canopy influence during winter months on the south aspect due to fewer trees and those trees being deciduous species."

I. 114. I understand the meaning of the word 'canopied', but as this term is quite uncommon, it distracts the reader from the text. Consider using another term.

This sentence has been revised. Revised manuscript lines 112-113: "A canopy height was also calculated (Fig. 1d) showing denser canopy at the base of the hillslopes, with a shorter sparse canopy at the middle of the north aspect, and open canopy near the top of the north aspect."

I. 117. Please check and correct the end of this sentence.

The last two sentences (including this one) of the paragraph have been removed during revisions.

I. 118-119. I do not think this sentence is necessary.

Sentence removed as stated in previous comment.

Figure 1.

Why is north pointing to the left? I think it would be better to rotate your map 90 degrees and make it pointing upward.

Figure 1 has been revised as suggested.

I am not sure that the orientation of 1a is the same as 1(b to e), please check. Consider adding the river to figure 1a.

We have made all panels have the same orientation. We do not have precise enough location information to include the stream in figure 1a.

1 b and Fig. 1d could be combined into one figure using elevation lines.

We also looked at this and believe this is a style choice where we prefer the current version.

1c could be removed. If the authors decide to keep it, please indicate how shortwave radiation was calculated.

This panel is still included. We indicate how shortwave radiation is calculated in lines 109-110: "Solar radiation was calculated using the solar radiation tool in ArcGIS Pro for 1 Mar (Fig. 1c)"

I. 121-123. Please rephrase. It took me a few reads to understand the sentence.

New Figure 1 caption revised for clarity: "Figure 1: a) The location and imagery of the Dry Lake watershed including general location within the western USA. (Imagery gathered via Google Earth Pro v. 7; Google Earth, 2024; © Google). b) Aspect map, c) solar radiation model for 1 Mar, d) percent slope of terrain, and e) canopy height. Survey transect locations are indicated by black circles."

I. 126. Please indicate the exact start and end dates of the data collection.

Done. Line 121 "In the winter and spring of 2023 (12 Jan through 1 May)..."

I. 131-133: Please revise these two sentences. It seems that some words are missing...

Revised lines (129-131): "All transects included GPR data collected with surface-coupled, common offset GPR units pulled over the snow. The first three surveys used a plastic sled hold the GPR, whereas the GPR was pulled freely without a sled during the final two surveys. Both methods of towing were manually towed behind an individual on skis."

I. 135. Why did you use two different systems? And how might this affect your results?

One of the systems needed to go to AK for a different campaign to have the same system as other teams. The physics should remain the same for this change in frequency so the only impact would be a change in precision and uncertainty in two-way travel times. It is actually higher precision and reduced uncertainty with the Mala system, but it is much more difficult to pull on hillslopes with the sled setup we have so was only used for the single survey.

I. 137-142: Please consider adding a table of snow pit measurement dates, indicating which density measurement method (wedge cutter or tube) was used on which date.

This is now included as Appendix B, lines 449 - 452

I. 142. Please indicate how water ponding and ice lenses were identified. Perhaps a photo of a snow pit experiment (if you have one) would be relevant here.

Water ponding in layers is fairly easy to see with the naked eye, but difficult to photograph. Thus, photos were taken but did not show ice lenses or water ponding very well, unfortunately.

I. 145. I would remove Figure 2 from the manuscript.

The figure has been removed.

I. 147. Please add a few words about ReflexW.

New sentence reads (line 147-148) “Radar data for each transect were processed using ReflexW, a software developed for near-surface geophysical data processing and interpretation.”

I. 147-163. I really appreciate this paragraph, which is fluent and easy to read. I think a conceptual figure of the multi-step data processing method would be nice. Please consider replacing Figure 3 with this conceptual figure.

We believe that a flow chart or conceptual figure of the steps may be a bit redundant given the details in the paragraph. This is also pretty standard GPR data processing, but may vary for other retrievals depending on what the data look like.

I. 167-175. I get quite confused with  $d_s$  and  $k_s$ . Defining  $d_s$  first would definitely help, but still. This part with the equations is a bit messy. Please check that the correct variables are used and described. Please also include the number of each equation.

$d_s$  is defined in the introduction. The revised lines (169 – 173) are now as follows:

“The median TWT (ns) for each GPR transect and associated average measured  $d_s$  (m) were used for the following calculations to estimate bulk snowpack density:

$$v = \frac{d_s}{\frac{TWT}{2}} \quad (1)$$

where  $v$  is the radar wave velocity in  $\text{m ns}^{-1}$ , and  $\epsilon$  is calculated with the speed of light ( $c$ ) in a vacuum:

$$\epsilon = \left(\frac{c}{v}\right)^2 \quad (2)$$

and bulk density ( $\rho_s$ ,  $\text{kg m}^{-3}$ ) is estimated using Kovacs *et al.* (1995):

$$\rho_s = \frac{\sqrt{\epsilon} - 1}{0.845} * 1000 \quad (3)$$

SWE was also calculated by multiplying the estimate of  $\rho_s$  by the observed  $d_s$ . “

I. 180-181. These two sentences should be merged into one.

Done. The revised lines 180-181 now read “Hourly data from SNOTEL and RAWS stations in the Dry Lake study site were utilized for the 2023 water year to contextualize field measurements taken during the observation period and as inputs into a physical snowpack model.”

I. 182-183. This sentence should follow the description of the data provided by the SNOTEL and RAWS stations.

Done. The longwave radiation sentence is now lines 195-197.

I. 191. Please mention that redistribution (e.g. by wind or canopy unloading) is neglected.

Done. Lines 189-191: “Note that this processing method assumes that wind redistribution and canopy unloading is negligible, which is a reasonable assumption for this SNOTEL station based on observations and distance from any canopy.”

I. 198. 2023 water year? Consider adding a label on the x-axis of the plot instead.

Will revise as suggested.

I. 200. Could you add a sentence explaining why SNOWPACK was used instead of another model?

For our purposes, any snow energy balance model would have worked. It was somewhat due to convenience as this model was taught for different purposes (profile liquid water and temperature imagery) in a class during this master’s project coursework. But, SNOWPACK is an option that is often used for studies looking at liquid water content.

I. 204-205. This is not exact. Please be more specific about how SNOWPACK creates, removes or merges snow layers.

Lines 202 – 204: “This study uses SNOWPACK due to past studies validating the liquid water representation in the model structure (e.g., Wever et al., 2014)”

I. 206. A clearer explanation of the liquid transport processes could be given here. See Wever et al. (2014 - <https://doi.org/10.5194/tc-8-257-2014>).

This reference is now included. However, the liquid transport process in the model is not a focus of the study.

I. 207. In fact, SNOWPACK relies on fundamental physical principles to simulate snow metamorphism. Please remove the statement that it has ‘a unique empirical scheme’.

Revised as suggested.

I. 235-237. Can this be confirmed by any snow pit observations?

No snow pit observations were made at this location. We have clarified that this is an interpretation, lines 236 – 240: “This base of the north aspect also resulted in an unrealistic value during the May survey (Fig. 4d) that we interpret as the result of excessive liquid water content due to a very low radar velocity and high relative dielectric permittivity (e.g., Bradford et al., 2009). This location has also been previously observed to result in excessive liquid water during spring snowmelt (Webb et al., 2018a), though no snow pit was dug at this location in May for the 2023 water year.”

Figure 5. While I appreciate the effort put into this figure, I think it could be simplified. The way the figure is presented makes it difficult to compare results from different sites. Also, in section 3.1 of the text, the frames (a, b, c ...) are not presented in any order, which makes it confusing. I would suggest a typical side-by-side plot where we can more easily compare the north-facing slope, the south-facing slope and the flat terrain.

Revised as suggested. We also rearranged to present the data in the order of flat, north, then south aspects to be consistent with the text throughout the manuscript.

I. 251. As snow pit observations were not systematically performed during your field surveys, I would recommend listing each snow pit date in a table (perhaps in the method section).

This is now included in appendix B that also shows the methods used for each pit.

I. 257-259. That is an interesting observation. Could you elaborate?

Revised manuscript lines 263 – 270: “The flat terrain pit did not have any ice lenses/layers in January, but one ice lens was observed in April that was approximately 3 cm thick and ~230 cm above the ground (Fig. 7a). The north aspect only had a single snow pit observation during the April survey, but ten ice lenses/layers were observed throughout the snowpack from 30 cm to 210 cm above the ground, all were approximately 1-2 cm thick (Fig. 7b). Seven of the ten observed ice lenses/layers were observed within a 70 cm section of the pit, from 110 – 180 cm above the ground (240 cm total pit depth). Pits dug at the base of the south aspect showed a single ice layer during the 28 February and 1 April surveys. This ice layer was approximately 4 cm thick at ~150 cm above ground in February and approximately 11 cm thick ~70 cm above ground in April (Fig. 7c).”

Figure 6: Please increase the size of the axis labels. Consider also using a colour gradient to display density profiles (see Fig. 3c-d from Bouchard et al. (2022 - <https://doi.org/10.1002/hyp.14681>) as an example). This would allow each profile to be shown on the same frame and would make them easier to compare.

The axes labels have been revised. Using the colour gradient leaves a lot of white space for the locations that only have a single date of pit observations. We did revise the line colors to make it easier to compare.

As a general comment, be sure to follow a same order of presentation of the results (e.g. 1. flat, 2. south, 3. north) in the different sections where you refer to them.

This has been revised throughout the manuscript for the order of flat, north, then south.

I. 263. Although this is not the objective of the study, I think it would be interesting to compare the simulation results for snow density with your snow pit observations. This would give a better idea of how the model performs at your site. Consider adding this analysis.

This has been added. Lines 280 -292:

“SNOWPACK simulated bulk snow density showed a root mean squared error of 48 kg m<sup>-3</sup> when compared to pit observed  $\rho_s$ . During the first two surveys (12 Jan and 6 Feb) SNOWPACK overestimated  $\rho_s$  whereas it underestimated  $\rho_s$  during the late February and May surveys (28 Feb and 1 May). SNOWPACK simulated  $\rho_s$  was within 10 kg m<sup>-3</sup> of pit observed  $\rho_s$  for all three pits on 1 Apr, representing a bias of less than 2% near peak SWE. All model simulations indicate a spike in density prior to completely melting out, but with different amplitudes and timing. The simulated SWE shows similar patterns relative to SNOTEL data. SWE peaks in both the flat and south aspect simulations on April 5 (~810 mm and ~285 mm, respectively), the date of a snowstorm prior to a period of warmer weather, whereas peak SWE in the north aspect model occurred on April 24 at ~920 mm. In comparing the SNOWPACK simulated SWE to GPR estimated SWE near simulated peak SWE dates, we see the flat terrain had an estimated 744 mm from GPR (833 mm from SNOTEL pillow) compared to simulated 786 mm for 1 Apr (~5% difference), the north aspect slope had an estimated 603 mm compared to a

simulated 813 mm (~35% difference), and the south aspect slope had an estimated 392 mm compared to the simulated 265 mm simulated for 1 Apr (~33% difference). The SNOWPACK simulation captures the increase and decrease in SWE relative to north and south aspect, respectively, with similar magnitude differences compared to transect estimates of SWE near peak SWE.”

I. 272. The difference in peak SWE is huge! I think this needs to be highlighted and explained.

Revised lines 289-292: “the north aspect slope had an estimated 603 mm compared to a simulated 813 mm (~35% difference), and the south aspect slope had an estimated 392 mm compared to the simulated 265 mm simulated for 1 Apr (~33% difference). The SNOWPACK simulation captures the increase and decrease in SWE relative to north and south aspect, respectively, with similar magnitude differences compared to transect estimates of SWE near peak SWE.”

We discuss that the model could have been further calibrated in the new “limitations” section as well.

I. 274-275. Is this based on volumetric water content (Figs. 6c-d-e)? I think the surface runoff simulation would be interesting here. Consider adding them to Figure 7.

Yes, this is volumetric liquid water content. We have added symbols that show when runoff is happening.

Figure 7. Units and date formats should be consistent with other figures (Figs. 4 to 6).

Done.

I. 283-284. In fact, ponding of liquid water at the base of the snowpack was not demonstrated by your results, but rather suggested by simulations and SWE observations. However, evidence of ponding could be provided by snow pit observations. If you have such observations of ponding at the base of the snowpack, consider adding them. Otherwise, please revise the wording of this sentence.

Revised as suggested.

I. 288. Just to be sure, by observational data, do you mean the SNOTEL station measurements?

New sentence now reads: “The flat terrain SNOWPACK simulations results also matched well with observational data from the SNOTEL station as well as survey transect data (Fig. 7).”

I. 298-300. The comparison with the northern aspect remains speculative as there were no wind speed measurements taken there.

These lines were written with reference to the south aspect slope with reference to the RAWS station data of observations.

I. 302-304: Have you applied any wind undercatch corrections to the forcing precipitation?

We did not in the presented data. When we estimated undercatch using the SNOTEL station or use the SNOTEL station precipitation data, the model simulations are greatly biased in the other direction (showing little difference compared to flat terrain simulations). We further discuss that more focus on calibration could have improved the modelling component of this study, but it was not the focus. Regardless of the SWE error, the timing of snow surface melt remained similar and provided the information that we were targeting for our study.

I. 309-310. This response may be enhanced by lateral flow over ice layers in the snowpack. See Eiriksson et al. (2013 - <https://doi.org/10.1002/hyp.9666>).

Only if the ice layers are thick enough to be continuous. The Eiriksson et al. (2013) paper actually found that ice lenses did not divert liquid water laterally very well. We expand these statements as follows:

Lines 382 – 386: “These mid-winter melt events on the south aspect coincide with increased density at the base of slope (Fig. 4) and the formation and thickening of an observed ice lense (Fig. 6), that we are interpreting as an indication of likely downhill migration of SWE through intra-snowpack flowpaths (Webb et al., 2020a; Webb et al., 2022; Eiriksson et al., 2013). The ice layer observed at the base of the south aspect is indicative of lateral flow in sloping terrain (Webb et al., 2018b; Schlumpf et al., 2024) as it is likely thick enough at 7 cm to create an hydraulic barrier and promote lateral flow”

I. 313-314 and Figure 8. This should be moved to the Results section.

Done.

I. 316. Do you have any temperature observations from your snow pit observations (even once) to support this?

Yes. These data have been added as supplementary material.

I. 342. Can you elaborate on the prevalence of hydraulic barriers in the northern aspect snowpack rather than in the southern aspect snowpack?

Revised lines 365-370: “This excess of water on the north aspect slope, paired with fine-grained soils with low infiltration capability, could explain ponding of liquid water at the base occurring with the onset of the melt phase. Snow pits dug on 1 April at the base of slope further support this interpretation, as several ice lenses/layers distributed throughout the snowpack were observed indicating the presence of multiple hydraulic barriers with the potential to divert liquid water laterally in the snowpack the entire length of the hill slope (Eiriksson et al., 2013; Webb et al., 2018b).”

Figure 9. This conceptual figure is interesting, but it is not based on field evidence. This should be clearly stated in the text.

Revised throughout the manuscript to reflect this.

I recommend that the authors compare their results with those of Mazzotti et al. (2023 - <https://doi.org/10.5194/hess-27-2099-2023>)

This paper is relevant and has now been cited in multiple locations in the manuscript.

I. 380-381. This has not been directly observed and remains a hypothesis. I would refrain from drawing conclusions from this.

We have added text to be clear that these are interpretations.

I. 384. Please add a few words on how these results would differ in different locations/climates. Please also add some concluding remarks on how the results of this work can improve our global understanding of snow in complex terrain and provide guidance for future research.

We have now added some guidance for future research in the “limitations” section of the manuscript.

Technical comments.

All of the below comments have been revised as suggested.

I. 12, 20, 24 and so on... Please consider using the term “ponding” instead of “pooling” throughout your manuscript.

I. 12. This study measures --> In this study, we measured.

I. 15. input --> inputs

I. 16. models --> simulations

I. 16. missing word (that?)

I. 21. (snow) pit.

I. 31. ‘Regional distributions in SWE also impact ecosystem services through surface albedo, effectively cooling earth surfaces and regulating climate’. It took me a few reads to understand this sentence. I recommend the following change: ‘Regional distributions in SWE also impact ecosystem services through surface albedo, which effectively cools Earth’s surfaces and regulates climate.’

I. 41. measure --> estimate

I. 47. I do not get what you mean by “snow cover” being a snowpack properties.

I. 87. Please include the year of that reference

I. 97. Please include the year of that reference

I. 98. Please indicate that masl means meters above sea level

I. 100. Please verify the format of the date.

I. 127. were --> was

I. 153, un-necessary --> unnecessary (?)

I. 225-226. Please revise the syntax of this sentence

I. 265-266. Please, revise this sentence.

I. 266-267. Please indicating Fig. 7a-b only once.

I. 294. doesn’t --> does not

I. 318. Please remove “and requires further research in the future”.

## Reviewer 2

This study examines the influence of aspect and slope position on snowpack parameters i.e., depth, density, and liquid water content (LWC), within a subalpine watershed in Colorado, USA. The variations of these parameters are evaluated using GPR, in situ stations, snow pits and SNOWPACK modeling. The study found that mid-winter melt events predominantly affect south-facing slopes, triggering later flow of LWC downslope and the redistribution of SWE. Additionally, ice layers develop on south-facing slopes during mid-winter periods. Flat terrain exhibits a steady increase in soil moisture throughout the winter. In contrast, as spring progresses, north-facing slopes witness the pooling of liquid water at their base.

The findings underscore the importance of considering aspect and slope position when estimating snow water resources. However, many conclusions are based on qualitative reasoning and are not always supported by the collected field evidence. While the snow modeling community is undoubtedly moving towards better representation of complex snow redistribution and melting processes, this paper does not provide sufficient quantitative evidence to significantly advance our current understanding of snow dynamics. If the authors intend to maintain a qualitative and conceptual approach, the manuscript should be retitled to reflect this focus. Additionally, a dedicated section should be included to address the study limitations. For instance, the paper could discuss why factors such as wind, canopy, terrain roughness, and eventually gravitational transport were not explicitly considered in this analysis.

Thank you for the comments. We appreciate the constructive suggestions and agree that further clarification on what is being interpreted versus directly observed better represents this work. Additionally, more details on uncertainty and limitations were expanded on during the revisions. Below are replies to specific comments in blue as well.

### Major comments.

- While I appreciate the complexity of organizing extensive snow campaigns and the integration of various tools like GPR, snow pits, and SNOWPACK, I'm uncertain about the optimal utilization of GPR in this study. While GPR can efficiently survey transects, its application here seems to be limited to average this information to a single-point observations (derived from averaged TWT and snow depth along the transect). The potential uncertainty associated with this approach is not explicitly addressed, and it appears to be significant. Additionally, GPR limitations in wet snow conditions and its inability to provide detailed snow layering information, particularly regarding ice lens formation or wind redistribution, makes the use of GPR difficult to justify in this work. Furthermore, the absence of radargrams as supplementary materials, which is an interesting data per se, hinders reproducibility and future works.

A section has now been added for uncertainty. We are averaging along a transect to represent a location on a hillslope because of the known variability of snow and snow processes and include citations to support the survey design for number of measurements, etc.. We have also included minimally

processed radargrams in the supplementary material. More detailed responses with line numbers and quoted revised text are included in response to specific comments below.

- The paper introduces the canopy influence as a key factor affecting the energy balance (L66 on), yet the specific role of canopy within the study domain remains unclear. While LiDAR data is mentioned and depicted in Figure 1e, its utilization in the analysis is not explicitly detailed. The discussion on canopy effects often lacks specificity, relying on generic considerations rather than relate to the specific test site. Similarly, the approach to estimating snow density from GPR data is confusing. The introduction suggests that density is generally considered uniform and that GPR can provide spatialized accurate measurements (L74 on). However, the subsequent averaging of density along transects contradicts this assumption. It would be beneficial to see a comparison of the radargrams, also at a qualitative level, before averaging them (this may further support the conceptual model of Fig 9). Additionally, the absence of uncertainty quantification in the results section hinders the interpretation of comparisons and the reliability of conclusions. I suggest addressing these points, such that the paper can strengthen its scientific rigor and provide a more comprehensive understanding of the complex interactions between canopy, topography, and snow processes.

We now include uncertainty estimates in the discussion. We also further discuss the influence of canopy and that transect selections were made to avoid direct influence of canopy so that terrain shading dominates. The previous reviewer suggested that we reduce specifics of LiDAR data processed, which we followed because it is not vital to analysis. LiDAR data were used to characterize the site rather than for any formal analysis.

Detail comments

L14 From Sec 2.3. it is not clear how the calibration of GPR snow density is done using snowpits and SNOTEL stations.

Revised manuscript lines 176-178: “GPR transects were also conducted next to snow pits and the SNOTEL station to calibrate GPR-derived  $\rho_s$  estimates for each survey date based on average bias when comparing the transects with an adjacent snow pit or SNOTEL station data.”

L23 This assertion seems to be limited to the particular characteristics of the study area and may not generalize to other conditions.

We have added “for the Dry Lake watershed” to this statement and clarify what processes may apply more broadly in the discussion section of the manuscript.

L75 Typically, bulk snow density is measured using a federal tube or within snow pits by summing the density derived by smaller volume tubes (or triangular prisms), as described by Kinar and Pomeroy, 2015.

Revised to reflect this and cited Kinar and Pomeroy paper (revised manuscript line 56).

L91 Snow depth can vary significantly, even over short distances, due to the rugged and heterogeneous nature of alpine terrain. This variability, combined with the small area sampled by a probe, highlights the importance of quantifying uncertainties in snow density estimates. Generally an average of N measurements should be done.

Revised manuscript lines 136-137: “the number of depth measurements ranged from 8 to 30 measurements with an average of 13 manually probed ds measurements to average for each transect area (López-Moreno, 2011).”

L92 If the primary focus of the research is to investigate the impact of aspect and slope position on snowpack dynamics, a thorough justification is required to explain why factors such as wind, canopy, terrain roughness, and gravitational transport were not explicitly considered in the study, especially given their potential influence on snow distribution and melt.

Revised text lines 84-89: “Therefore, to assess seasonal variability in the spatio-temporal distribution of SWE as it relates to energy balance dynamics, we employ L-Band GPR technology to survey snow depth and density with respect to north and south aspect slopes and the relative position on each slope. We use these techniques to answer the following research question: How do variations in snowmelt dynamics impact snow density and SWE distribution throughout the snow season based on aspect and relative location on a hillslope? We aim to answer this question in a manner that will provide insights to snowmelt dynamics for mid-latitude forested mountains that develop a seasonally persistent snowpack.”

Fig 1a please rotate it consistently with the other figure (i.e., North up)

Done.

L162 Please explicitly state that, as reported in Webb & Mooney 2024c, TWT is calculated as an average value.

The text states median TWT is used. Line 160: “The median TWT (ns) for each GPR transect and associated...”

L170 the equations must be numbered.

Done.

L175 Please provide a method for calculating the uncertainty associated with the TWT measurements. Given the potential for significant error propagation due to small denominator values, a rigorous uncertainty analysis is essential.

Done. Section 4.1 in the discussion is now dedicated to uncertainty estimates.

Section 2.4 how the SNOWPACK free parameter has been calibrated?

No calibrations were conducted. Appendix C has been added to provide further details of the modeling component of this study.

Figure 5 is difficult to interpret. A simpler, more traditional visualization would improve the comparison of differences between the data.

Done.

Figure 6 please report the uncertainty for all the measurements.

Done. Section 4.1 in the discussion is now dedicated to uncertainty estimates.

L287 “unusual results” respect what?

This phrase was removed during revisions.

L305 “model weakness”? Can you better elaborate the sentence?

Model biases are now further discussed in the revised manuscript. L378-380: “Despite model differences in melt out dates, the simulated LWC parameter shows when surface melt occurred due to its root in physical processes and qualitative comparison to snow pit observations.”

L308 Can you better justify this sentence showing the evidence of this mechanism?

Revised lines 380-392: “The south aspect simulation reveals several mid-season surface melt events that are not present in the flat or north aspect models, which is likely a response to increased solar radiation exposure that were also qualitatively observed during surveys. These mid-winter melt events on the south aspect coincide with increased density at the base of slope (Fig. 4) and the formation and thickening of an observed ice lense (Fig. 6), that we are interpreting as an indication of likely downhill migration of SWE through intra-snowpack flowpaths (Webb et al., 2020a; Webb et al., 2022; Eiriksson et al., 2013). The ice layer observed at the base of the south aspect is indicative of lateral flow in sloping terrain (Webb et al., 2018b; Schlumpf et al., 2024) as it is likely thick enough at 7 cm to create an hydraulic barrier and promote lateral flow. Observations of surface melt occurring on the south aspect also included small runnels forming late in the afternoon during the April survey, which is further supporting this interpretation. Additionally, soil moisture sensors at the SNOTEL station indicate a steady rise in soil moisture that align with snowpack accumulation, indicating a steady source of moisture throughout the winter (Fig. 5). With lateral groundwater fluxes from outside this watershed assumed to be negligible, the source of soil moisture rise is likely from melting snow on the south aspect as snow elsewhere in the watershed remains cold enough to not provide moisture inputs (Supplementary Tables S1-S7).”

L 333 Why “unrealistic”? Can you better elaborate it?

L 360 – 361: “unrealistic increase in  $\rho_s$  during the May survey ( $\rho_s > 1000 \text{ kg m}^{-3}$ ).”

L 354 The answer to the main research question of the paper is answer considering only the melting. So, the melting was the focus of the research?

We did further clarify that the intermittent melting was the focus of the study. The manuscript title has also been revised to better reflect this.

Figure 9. This conceptual figure is interesting, but it is not based on field evidence. This should be clearly stated in the text.

Revisions throughout the manuscript include improved representation of what is interpreted vs. what is observed.

L 367 I suspect that Dingman simplified his modeling to a homogeneous snowpack. While the four-phase model remains valid for individual homogeneous layers, additional complexity is necessary to accurately represent real-world snowpacks (which however is made up of different homogeneous layer, possibly at different phase).

This is true. Our main point here is that a snow pit can show a cold snowpack that is not yet isothermal and may require another week or two to warm up before it really produces runoff, while a snowpack on

a south facing slope may have been producing runoff for some time. Revised L 425 – 427: “The traditional 4-phase snowpack model of a homogenous snowpack going through accumulation, warming, ripening, and melt (Dingman, 2015) may not be representative for all snowpacks everywhere in a single watershed at a given time, especially when considering hillslope processes.”

L371 Given the significant spatial variability in snow depth, particularly in complex terrain, it is challenging to believe that traditional probing methods can accurately capture these variations without averaging N measurements and without a rigorous uncertainty analysis.

As mentioned above, an uncertainty analysis is now included.

L313-328: “4.1 Uncertainty Estimation of Survey Data

The above-described methods in estimating  $\rho_s$  through manual depth probes and GPR will include uncertainty from both data sources. The manually measured  $d_s$  data are estimated to have an uncertainty of 3% for the transect area for an average of 13 measurements (Lopez-Moreno et al., 2011), while the GPR TWT will have an uncertainty that is approximately equal to 0.25 of the wavelength, or 0.25 ns for 1000 MHz (Burger et al., 2006). It is important to note that both of these uncertainty estimates are conservative and the true uncertainty is likely lower, though these estimates provide a quantitative measure of maximum uncertainty for our  $\rho_s$  estimates. These estimates result in relative uncertainty for radar  $v$  of 3.1% that propagates to  $\rho_s$  estimates that averaged approximately 17% (~45 kg m<sup>-3</sup>) that is driven predominantly by  $d_s$  uncertainty. These are similar results to other studies pointing towards snow depth being the greatest source of uncertainty when using equation (3) (McGrath et al., 2022). Further details of these uncertainty calculations are provided in Appendix D.

The estimated uncertainty of ~45 kg m<sup>-3</sup> is similar in magnitude to the mean bias of 48 kg m<sup>-3</sup> (mean absolute deviation of 60 kg m<sup>-3</sup>) of the transect  $\rho_s$  estimates relative to snow pit and SNOTEL data. This direct comparison was used for calibration due to the observation of liquid water being present in snow pit observations during surveys, indicating that the relative uncertainty of using equations (1), (2), and (3) is likely lower than the above estimate for dry snow conditions along the transects of this study. After calibrating the transect  $\rho_s$  estimates to snow pit and SNOTEL data, the mean error was less than 5 kg m<sup>-3</sup> (mean absolute deviation of 45 kg m<sup>-3</sup>).”

L374 and conclusion: So this is only a study on the energy balance and not on snow redistribution processes?

The manuscript title and text throughout the manuscript has been revised to clarify this.

As a final note, while there are no explicit publisher guidelines against self-citation, it is generally advisable to minimize excessive self-referencing. For instance, the accurate prediction of LWC by SNOWPACK could be supported by citing previous studies (as done in the current self-cited works) that provide also detailed information about the model details, which is not developed by the authors.

Thank you for pointing this out. This has been done.

The References section is difficult to read due to the lack of spacing between entries. Additionally, some references appear to be formatted incorrectly e.g., L87 Clark et al. should be Clark et al., 2015.

Formatting of references sections has been corrected.

Kinar, N. J. and Pomeroy, J. W.: Measurement of the physical properties of the snowpack, *Rev. Geophys.*, 53, 481–544, <https://doi.org/10.1002/2015RG000481>, 2015.

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