

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 will improve 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.

Agree. This will be done for brevity and clarity.

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).

Agree, this will be revised as suggested.

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 will revise this paragraph to more clearly define the research aim that is currently in the last sentence as the research question only. We will revise to state the broader aim early in the paragraph and re-phrase the sentences to more clearly illustrate how we are approaching the study.

Modeling setup

Several questions remain about the modelling setup.

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

We will add supplementary material that has all of the input information for every parameter used.

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. (line 203)

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?

These will be included in the supplementary material mentioned above.

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 will be 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.

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.

The supplementary material will be made and will include explanations for modeling choices.

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.

This will be added to the interpretation for clarity. We will point out the specific influences of canopy on it's own, but also the feedback loop between north aspects having more water available for plants and generally producing thicker canopies as a result.

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 agree. Clarification on what is directly observed and what is not will be made to ensure clarity in the revised manuscript.

Minor comments.

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

Will do.

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

Will do.

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

Will revise.

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.

Will clarify.

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

Will revise as suggested.

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

It is a new paragraph, but the format of the journal and the previous sentence ending at the margin make it appear to not be.

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.

Good point. Will revise as suggested.

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.

Will revise as suggested.

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.

Will revise as suggested to further clarify why we did not apply the wet snow form as well.

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

Will revise as suggested.

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 ' ϵ ' for permittivity.

Will revise as suggested.

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

Will revise as suggested.

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

Will define as snow depth when it is first introduced.

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

We will add this information.

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

Yes, we will add this information. It is generally 10 – 15 cm on the north aspect, and somewhat deeper at the toe of the slope. This is from memory and I will confirm from notes for the revisions.

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

Will revise as suggested.

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.

Will revise as suggested.

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

Will revise.

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

Agree. Will delete.

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.

It is due to the 3D birds eye view where if North were up some of the transect is actually difficult to see in the image.

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 will double check this and correct if necessary during revisions.

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.

We will consider both options during revisions.

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

We will revise the caption to be more clear.

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

Will do in the revisions.

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

No words seem to be missing, but we will try to revise a bit for clarity.

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.

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.

Will add as suggested.

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.

From my memory hen photos were taken they did not show ice lenses or water ponding very well, unfortunately. We will go back through the photos to check, though.

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

We will consider either removing it, or incorporating the information into Figure 1.

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

Will revise as suggested.

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.

I. 167-175. I get quite confused with ds and ks. Defining ds 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.

Will double check and revise for clarity and formatting.

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

Will revise as suggested.

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

Will revise as suggested.

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

Will revise as suggested.

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.

Will revise as suggested.

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>).

Will add more information on the use of Richards equation in SNOWPACK.

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'.

Will revise as suggested.

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

We did make snow pit observations during the May survey and will add further text concerning observations.

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 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.

We will present the frames in the text in a more logical order, and create a figure with panels side by side. That should not be too difficult to create for comparison and see which one is preferred in the revised version. We agree that the current version is difficult to quantitatively compare values between sites for specific dates.

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).

Will revise as suggested.

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

We can elaborate a little bit more about the depths of the ice lens observations and thicknesses and how there was a larger density across certain depths. Is that what you are looking for?

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.

Will revise axes labels as suggested and look further into the color gradient for density profiles.

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.

Will revise for this order of presentation.

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.

We will add this information.

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

We can add further discussion to this.

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.

We will look at these results to see if they add anything to the presentation of results.

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

Will revise as suggested.

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.

Will add text to specify.

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

Yes, we will add that to be clear.

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

Good point. We will clarify that it is possible for increased wind, but uncertain.

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

We will have to double check on this.

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.

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

Will revise as suggested.

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

We do have some limited temperature measurements that supports this that we can include.

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

Yes, we can elaborate further to include the north aspect slope, including evidence such as the multiple ice lenses observed in the snow pit.

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

Part (b) is based on field evidence and is not changed from Webb et al. (2018). We will clarify that this is how we are interpreting the multiple observations made for part (a) of the figure.

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 certainly related and will be compared in the revisions.

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

Will revise to reflect this.

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.

Will revise to include this, but our writing style is to have guidance for future research in discussion rather than conclusions.

Technical comments.

Will revise as suggested for all technical comments below.

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".