Dear Editors and Reviewers,

We would like to thank you for the thorough reviews of our manuscript. We appreciate you all taking the time required to provide the feedback. We have addressed all of the comments in the latest revision of the manuscript and provide responses to each of the comments in the text below. We list the comments in the previous review followed by our response in blue text. Again, thank you.

Sincerely,

Ryan Webb

Editor Decision: Reconsider after major revisions (further review by editor and referees) (26 Oct 2020) by Guillaume Chambon

Comments to the Author:

Dear authors,

I thank you for your constructive answers to referees' comments in the interactive discussion. In view of the intended revisions described in your responses, you are invited to submit a suitably revised manuscript, along with a tracked-change version and point-by-point rebuttal letters. Since both referees asked for major revisions, the new version will be sent to them for a further assessment. Final decision regarding publication will be made based on whether their concerns are effectively addressed. In particular, the issue of providing stronger and more quantitative validations against field data, which is raised by the two referees, shall need to be answered carefully.

Best regards,

Guillaume Chambon / TC Topical Editor

Response: We have provided a response to each comment below. In particular, we have added analyses and figures for improved quantitative validations and insights towards our results.

Anonymous Referee #2

Received and published: 14 September 2020

In this paper, simulation of liquid water infiltration in the slope snow was discussed. Water infiltration in the slope snow is an important theme for understanding the water infiltration process. The features of this simulation are two: combining SNOWPACK and iTOUGH2, and application of plot scale. Using different resolution scales for parallel and perpendicular direction for slope, it can support both detailed changes of snow layer for vertical direction and plot scale for horizontal direction. This combination of models is good work. However, this paper lacked the validation of longitudinal flow by comparing field data despite the authors working for field observation about longitudinal flow in previous study. As far as reading this paper, only the number of longitudinal flow paths was validated for three field sites. The comparison of existence of longitudinal flow path is of validation of capillary or hydraulic barrier formation, but not longitudinal flow itself. Since simulated slope flow characteristics (e.g. distance of movement by longitudinal flow) are not authorized by real data, one of the results of this paper, the ratio 250:1 is questionable. Before accepting this paper, validation by quantitative comparison between simulation and field data in terms of longitudinal flow is necessary. Even if the simulation results don't match with the measured result well enough, discussion of the causes of the discrepancies and improvement that will be needed in the future will be useful information for the paper of slope flow with this scale.

Response: Thank you for your comments. We agree that further validation is necessary, though difficult with the field data that were collected. However, we have added further analysis and results to accomplish this. Additionally, we have re-organized the results that include the 250:1 ratio because this ratio is driven by times of low melt and thus low vertical percolation. We now compare averages over the entire simulated period.

New text concerning these analyses are as follows:

Comparison to field data

Lines 193-200:

"Comparisons of SnowTOUGH to field observations varied. The simulated bulk θw showed little temporal variability for both the NT and AT sites while field observations showed greater variability (Fig. 7). Simulated bulk θw remained near 3% for both the NT and AT sites throughout the IOPs. The average of all field observations was generally greater than simulated values, though simulated values were always within one standard deviation of field observations. The mean of all field observations for the NT and AT sites was 4.2% and 3.5%, respectively. The large standard deviations of field observations were largely driven by converging intra-snowpack flow paths creating areas of bulk θw as high as 20% (Webb et al., 2020). Though different, the comparison of θw between SnowTOUGH simulations and field observations are within the estimated error of the field methods (~2%)."

Longitudinal flow calculations/comparisons:

Lines 210-222:

"Comparing simulated longitudinal flowrates to field observations is difficult because no flowrates were directly measured in the field. However, locations of converging intra-snowpack flow paths were used in Webb et al. (2020) to estimate effective upslope contributing areas (EUCAs), defined as the minimum upslope contributing area required to produce observed changes in liquid water content from melt rate estimates if all meltwater was diverted longitudinally and collected in a single observation location. Therefore, these observations are not directly comparable to SnowTOUGH simulations, but insights can still be gained from comparisons. These field observations resulted in peak EUCA of 6 m2 and 17 m2 for the NT and AT sites, respectively. For the NT site, this occurred over a two-hour time period with a total of ~5 mm of melt. At this same time in the NT SnowTOUGH simulations, the location 15 m downslope resulted in an accumulation of longitudinal flow of 28.7 mm of water (Fig. 5bii: 12-Apr 12:00, mean longitudinal flow of ~4 ml s-1 for two hours), or roughly 5.7 times greater than the simulated melt. For the AT site, the simulated melt was ~7.5 mm with a total accumulation of simulated longitudinal flow, at the 15 m downslope location, of 386 mm (Fig. 5cii: 16-May 12:00, mean of 268 ml s-1 for two hours), or roughly 51 times greater than the estimated contributing melt. Therefore, relative to the calculations from Webb et al. (2020), SnowTOUGH simulated longitudinal fluxes are relatively similar to observations at the NT site and greater by a factor of three at the AT site."

Discussion of comparisons:

Lines 244-256:

"In general, it is likely that the natural heterogeneity of both permeability and capillary barriers will decrease the amount of longitudinal flow simulated in this study. Thus, SnowTOUGH simulations are likely overestimating the amount of longitudinal flow for specific flow paths.

Relative to estimates of EUCA, the AT site simulations overestimated longitudinal flow. However, it is important to note that the field methods used to estimate the EUCA likely underestimate the value because it assumes all diverted liquid water remains in the snowpack at the point of calculation. Additionally, the low melt rates as a result of the incoming storm add uncertainty to the appropriateness of these calculations using snowmelt rates. Therefore, it is likely that the true value of EUCA is between 17 m2 and 51 m2 for the AT site. Conversely, the NT site simulations resulted in a similar amount of longitudinal flow within the plot-scale simulations as field observed EUCA suggests. Considering the underestimation of the number of flow paths simulated at this site and the underestimation of EUCA from field methods as previously mentioned, the true EUCA is likely larger than the SnowTOUGH-simulated longitudinal flow. Additionally, the longitudinal flux for the single flow path is likely overestimated. We recommend the use of snow lysimeters similar to those implemented in Eiriksson et al. (2013) in future studies to further quantify intra-snowpack longitudinal flow for comparison to the SnowTOUGH model."

Minor comments

P3 L75-82 Please clear what scheme did the author use for the water transport in the SNOWPACK (bucket, Richards equation or dual domain approach). According to Fig. 2a, I guess the bulk scheme was used. In this study, the water infiltration may be estimated by iTOUGH2 part and received little effect by water infiltration scheme of SNOWPACK. But even if so, the type of scheme should be written.

We have now clarified, added justification for choices, and added another figure.

Lines 83-89:

"Liquid water transport was simulated using the default bucket scheme for full water year simulations and the Richards equation option (Wever et al., 2014) for simulating the intensive observation period (IOP) at each study plot. Full water year simulations were used to offer context to the timing of each IOP relative to peak snow water equivalent (SWE) and snowmelt processes (Fig. 2). For the IOP simulations, initial conditions were provided through manual snow pit observations (Webb et al., 2020; Webb et al., 2018c) so that we could focus our analysis on the intra-snowpack flow of liquid water and comparisons to field observations rather than the accuracy of the SNOWPACK simulated stratigraphy and the potential implications on our results."

P4 L91-92 The resolution of elements differs 50 times between parallel and perpendicular to the slope. Does it lead any problem for correct simulation due to this large resolution difference?

Response: The heterogeneity occurs in the vertical direction and we do not believe that element dimensions have an effect on the results at the scale of our simulations. We did run some initial tests at multiple resolutions with synthetic data and found no effect, but did end up using 25 cm long elements for the purpose of visualizations of results. In the original manuscript, 50 cm long elements were mentioned, we have corrected this as we actually used 25 cm long elements.

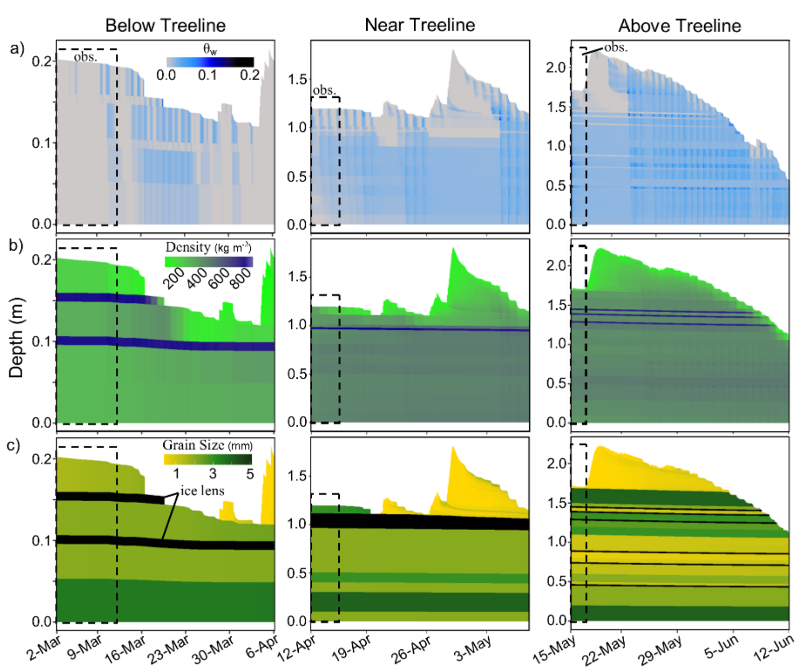
If lateral heterogeneity were to be included in future studies then this would have a much larger impact.

P4 106-116 This paragraph describes that authors performed the snow pit observation of tracer experiment, measuring water content distribution. These data should be used for quantitative validation especially the distance of the water movement for longitudinal direction. Comparison with these observations enhances the value of simulation result.

Response: We have added this comparison. Note that we only measured the spatial distribution of bulk water content in the snowpack, not the liquid water profile in each snow pit. The snow pits at the end of the experiments were used to find where the dye tracer had traveled. There were many snow pits dug to do this and so there was not enough time to get full detailed profile information at each location.

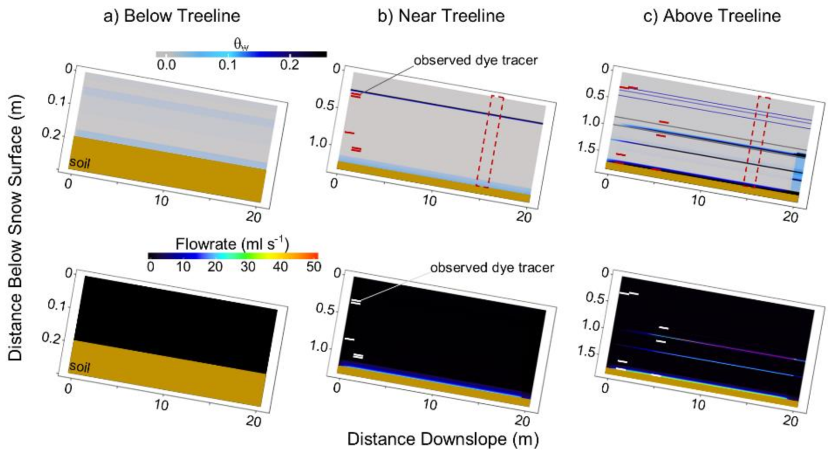
P5 L131-135 Snow profile data should be shown in Fig. 3 especially grain size and snow density. These parameters relate the formation of capillary or hydraulic barriers which lead to longitudinal flow.

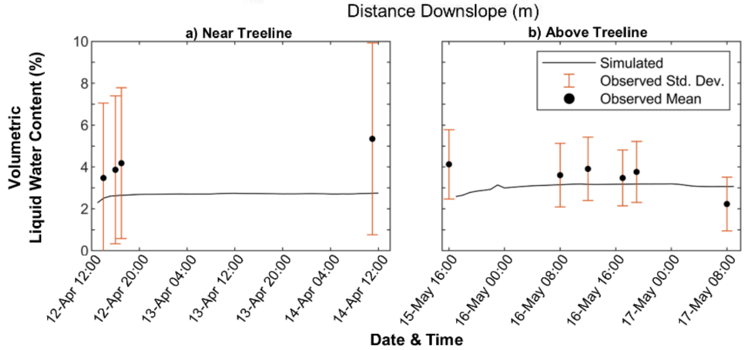
Response: A new figure has been added. New figure 3 (shown below) shows these data and the following SNOWPACK simulations of the profile.



P5 148-155 The number of longitudinal flows were used for the validation of this model. Although, accuracy of longitudinal flow path means that capillary or hydraulic barrier was reproduced correctly, it did not mean the slope flow characteristics was reproduced well. Also, authors should show figures of result of field experiments, not only reference.

The results of the field experiments are now shown in two figures. One showing the location of the dye tracers (Fig. 4) and one showing the liquid water content comparisons (Fig. 7). We have also added text to compare field data to simulations for longitudinal flow as previously quoted.





P6 173-175, P7 L193-195 In my opinion, neglecting heterogeneity affects the ratio of water flow direction (parallel or perpendicular to slope) rather than the number of longitudinal flows. Heterogeneity sometimes leads the movement to a difficult direction, which leads to decreased the ratio of water flow direction. So the ratio (250:1) has the possibility to be overestimated of the ratio due to neglecting heterogeneity. Furthermore, it was not endorsed by field observation.

Response: We apologize for not being clear, but we did mean heterogeneity has been previously shown to have minimal influence on the distance of lateral flow from capillary barriers. But you are correct that for permeability barriers it will likely decrease the flow. We have now added text to clarify that these simulations likely overestimate longitudinal flow. We additionally discuss what we believe the correct flows likely are and make recommendations for future studies.

Lines 241-256:

"However, previous studies of capillary barriers at the interface between soil layers have shown that homogenous layer assumptions, as those made in the present SnowTOUGH simulations, capture the average of randomized heterogeneous simulations (Ho and Webb, 1998). The validity of this assumption for snow should be further studied. In general, it is likely that the natural heterogeneity of both permeability and capillary barriers will decrease the amount of longitudinal flow simulated in this study. Thus, SnowTOUGH simulations are likely overestimating the amount of longitudinal flow for specific flow paths.

Relative to estimates of EUCA, the AT site simulations overestimated longitudinal flow. However, it is important to note that the field methods used to estimate the EUCA likely underestimate the value because it assumes all diverted liquid water remains in the snowpack at the point of calculation. Additionally, the low melt rates as a result of the incoming storm add uncertainty to the appropriateness of these calculations using snowmelt rates. Therefore, it is likely that the true value of EUCA is between 17 m2 and 51 m2 for the AT site. Conversely, the NT site simulations resulted in a similar amount of longitudinal flow within the plot-scale simulations as field observed EUCA suggests. Considering the underestimation of the number of flow paths simulated at this site and the underestimation of EUCA from field methods as previously mentioned, the true EUCA is likely larger than the SnowTOUGH-simulated longitudinal flow. Additionally, the longitudinal flux for the single flow path is likely overestimated. We recommend the use of snow lysimeters similar to those implemented in Eiriksson et al. (2013) in future studies to further quantify intra-snowpack longitudinal flow for comparison to the SnowTOUGH model."

P7 L197-199, 205-206 Authors have several field data. But they were only used for mention for consistency of the trend. Can the author make quantitative comparison between field data and simulation?

Response: Yes, as discussed in previous comments and shown in quoted revisions. Thank you for your insights provided in your review. These comments certainly improved the quality of the manuscript.

Nander Wever (Referee)

nander.wever@colorado.edu

Received and published: 24 August 2020

The authors report on model simulations of lateral water movement in a sloping snowpack. The topic is highly relevant, as it is an open research question to what extend lateral flow in snow modifies the hydrological cycle in snow covered catchments. The coupling of a dedicated snow model with a dedicated water flow model is a unique and very interesting approach. The authors find a much larger lateral flow than vertical flow, suggesting that lateral flow is highly important, even in relatively flat terrain (10 degrees slope angle). Even though the concept of the study is very robust, the manuscript itself falls short at several aspects, and many issues are not, or only weakly discussed. It is strongly encouraged that the authors thoroughly improve the manuscript. The writing style is of very high quality, with clear figures. However, I think the authors should avoid using the jet-type color scale used in some figures.

Response: We have expanded on our descriptions and analysis as described below, and changed the color scheme for a number of the figures.

Main concerns:

The main drawback of the way the study is presented is the lack of validation with the available field data (as for example published by the authors as Webb et al, 2020), except for the observed dye tracer layers showing the lateral flow layers. There must be more validation data available to validate the results of, for example, the SNOWPACK simulations. This should be worked into the results and discussion sections. For example, does the field data confirm the degree of wetting of the SNOWPACK at the initiation of the field campaigns?

We have added more validation as well as clarification. We discuss the model set up clarifications in the next comment, but now quote the added validation analyses.

<u>Comparison to field data</u>

Lines 193-200:

"Comparisons of SnowTOUGH to field observations varied. The simulated bulk θw showed little temporal variability for both the NT and AT sites while field observations showed greater variability (Fig. 7). Simulated bulk θw remained near 3% for both the NT and AT sites throughout the IOPs. The average of all field observations was generally greater than simulated values, though simulated values were always within one standard deviation of field observations. The mean of all field observations for the NT and AT sites was 4.2% and 3.5%, respectively. The large standard deviations of field observations were largely driven by converging intra-snowpack flow paths creating areas of bulk θw as high as 20% (Webb et al., 2020). Though different, the comparison of θw between SnowTOUGH simulations and field observations are within the estimated error of the field methods (~2%).

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Also, it is reported that: "Initial conditions were provided through manual snow pit observations" (L92), while at the same time, it is reported that: "Within this domain, the iTOUGH2 numerical model simulated the flow of liquid water with time-varying snow layer properties provided by the SNOWPACK model." (L59). That seems contradictory. I have some difficulty reviewing the remainder of the manuscript, since this issue is so unclear. When only material properties are taken from SNOWPACK, without any consideration for the wetting of the snowpack, or the exact layer transitions that are simulated by SNOWPACK (which are a reflection of the driving data to run the simulations), while at the same time observations are used to initiate the iTOUGH2 simulations, then I think that the coupling introduces large errors, simply resulting from the mismatch between simulated and observed stratigraphy. I'm highly confused at this point what exactly has been coupled. In fact, material properties don't vary that much for the couple of days simulated with iTOUGH2, such that when iTOUGH2 can be initialized with observations, the SNOWPACK simulations may not add any information regarding the change in microstructural properties. Maybe it would be a sufficient approach then to have SNOWPACK inform iTOUGH2 about melt rates only, to prevent the problems with potential mismatches between modeled and observed stratigraphy?

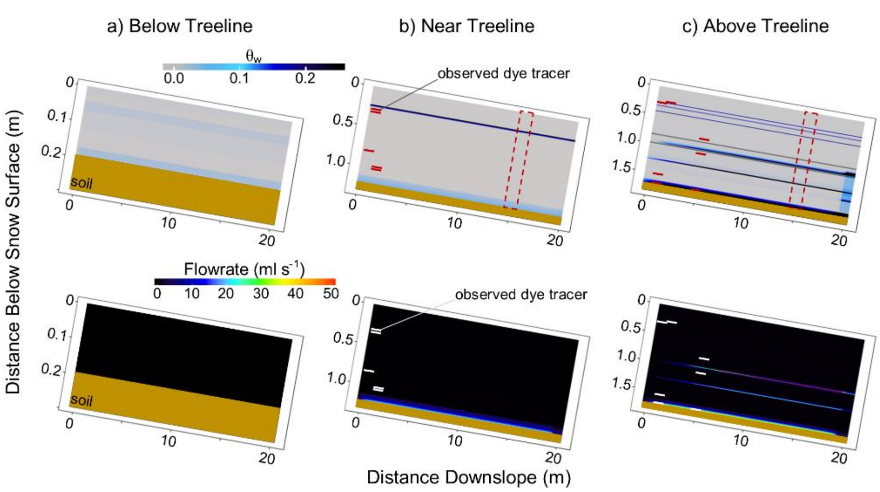
All this remains very unclear now.

Response: We have clarified all of this in the revisions; in fact, we were essentially doing what you were suggesting. We were also initializing SNOWPACK simulations by pit observations, for our observation period. This was to focus on the longitudinal movement of water rather than how well SNOWPACK captures the stratigraphy.

Lines 79-89:

"Simulations were run at hourly timesteps with quality-controlled meteorological observations. Air temperature, relative humidity, wind speed, incoming shortwave radiation, incoming longwave radiation, and snow depth data were used as forcing data for the SNOWPACK simulations. The SNOWPACK canopy module was activated for the below treeline study plot (described below) using physically representative values of leaf area index (4.0 m2 m-2), canopy height (7.0 m), and direct throughfall fraction (0.2, dimensionless). Liquid water transport was simulated using the default bucket scheme for full water year simulations and the Richards equation option (Wever et al., 2014) for simulating the intensive observation period (IOP) at each study plot. Full water year simulations were used to offer context to the timing of each IOP relative to peak snow water equivalent (SWE) and snowmelt processes (Fig. 2). For the IOP simulations, initial conditions were provided through manual snow pit observations (Webb et al., 2020; Webb et al., 2018c) so that we could focus our analysis on the intra-snowpack flow of liquid water and comparisons to field observations rather than the accuracy of the SNOWPACK simulated stratigraphy and the potential implications on our results."

Furthermore, I struggle to relate earlier reported dye tracer levels to the ones shown here. For example, I assume that the "above treeline" site in Webb et al. (2020) corresponds to the alpine site in this study (it may be a good idea to use the same naming for the different sites as the previous study). Fig. 9(aii) in Webb et al. (2020) shows a dyed layer at the snow/soil interface and around 1.50m inside the snowpack (about 35 cm below the snow surface), both which I can find back in Fig. 3c. However, Fig. 9(aii) in Webb et al. (2020) also shows a layer around 1m above the soil, which seems to be indicated much lower in the snowpack in Fig. 3c. Also, for the treeline site the layers don't seem to correspond. Fig. 9(bii) shows a first dyed layer around 60cm below the snow surface, and another one 60cm above the soil/snow interface, with a third one in between. Fig. 3b shows the highest dyed layer much higher in the snowpack (about 30 cm below the surface) and the lowest about 20cm above the snow/soil interface.



In Webb et al. (2020), it is reported that the SNOWPACK simulations were run using the Richards equation water transport scheme. However, the liquid water content distributions shown in Fig. 2 in Webb et al. (2020) and Fig. 2a in this manuscript, are remarkably homogeneous, even though I found in multiple studies that using Richards equation in SNOWPACK leads to inhomogeneous water distributions. To me, it looks like results from the bucket water transport scheme. The SNOWPACK model also should indicate the hydraulic barriers, since those impede vertical flow, even though the SNOWPACK model does not consider lateral flow.

Response: You are correct and we apologize for this oversight. We have clarified this in the revised manuscript as quoted above in our previous response to one of your comments.

Many figure panels aren't discussed in the manuscript.

At several instances, it is important to repeat information from earlier published work. For example, the setup and driving data of SNOWPACK simulations should be explained in more detail. Some information is necessary to interpret the results, and only referring to earlier published work is then inadequate in my opinion. For example, it's important to know what the source of precipitation is for SNOWPACK, to understand if the stratigraphy would match local conditions or not. Typically when SNOWPACK is driven with in-situ measured snow depth, it shows better agreement with local stratigraphy, compared to when it is run with rain gauge data. Since there is a large uncertainty in rain gauge data for solid precipitation, individual snow fall events can be severely over- or underestimated. Based on citations, I assume SNOWPACK was run with insitu snow depth data, but I think it's important to repeat that kind of crucial information here. Similarly the use of the canopy module, or a better description of the field sites are important aspects to repeat.

Response: We have now added more detailed descriptions of the modeling work to explain these concerns. The SNOWPACK text is quoted in an above response.

Minor comments: - I would recommend to use the same terminology as in Webb et al. (2020) to denote the field sites (i.e., above treeline, near treeline, below treeline).

Response: Agreed, this has been changed in the manuscript.

- A discussion of boundary conditions upslope and downslope in iTOUGH2 needs to be added. It seems to be a zero flux boundary to the left and right, such that water accumulates at the downslope boundary. This may also explain the role of the drain mentioned in L91, to prevent water accumulating in the model because of zero flux boundary conditions.

Response: We have now added the following language to clarify this.

Line 107-109: "The boundary conditions of the upslope and downslope ends of the domain were simulated as no-flow conditions and a drain was modelled at the downslope end to remove excess liquid water that may build up on the no-flow boundary"

- L77: "was calculated by subtracting theta_w from rho_s". Please reformulate, because this doesn't seem to be a calculation that makes sense if theta_w is not multiplied by the density of water. Furthermore, why not simply multiply theta_ice with the ice density of 917 kg/m^3 to get dry density?

Correct, we did multiply by 1000. We actually conducted both calculations as a simple check on some of our calculations. We have edited the text to the theta_ice calculation for ease of readability.

Line 91-92: “The dry density of each snow layer (ρds) was calculated by multiplying θi by the density of ice (917 kg m-3).”

- Eq. 1 and 2: These equations come from Yamaguchi et al. (2012), not Yamaguchi et al. (2010).

Yes, they do. We used the more recent parameterization. Thank you for catching this, the reference has been fixed.

- L90: "10-30 cm deep soil". Please denote what kind of soil was prescribed and how this relates to the soil at the study plots. Related: in L138: the lateral flow at the snow-soil interface may be caused by the prescribed soil type. Thus, it is imperative to discuss this.

We have added these data as follows:

Lines 105-114:

“The layered snowpack was modelled above a 10–30 cm deep soil, increasing in depth under deeper snow. Deeper soil was modelled under deeper snow to increase pore storage volume available for any infiltrating water released from the snowpack. The boundary conditions of the upslope and downslope ends of the domain were simulated as no-flow conditions and a drain was modelled at the downslope end to remove excess liquid water that may build up on the no-flow boundary (Fig. 1). Soil types for each site are known as silty loam and retention parameters common to this soil type were used. These parameters were a van Genuchten m value of 0.29, a porosity of 0.67, and van Genuchten α value of 0.02 cm-1. Saturated hydraulic conductivity estimates of the soils were taken as the mean of more than 15 mini-disc infiltrometer observations distributed evenly across a 10 m ⍰ 20 m plot at each site. These saturated hydraulic conductivity values for the below treeline (BT), near treeline (NT), and above treeline (AT) sites were 1.36 ⍰ 10-3 cm s-1, 6.93 ⍰ 10-4 cm s-1, and 8.46 ⍰ 10-4 cm s-1, respectively.”

- L91: Please explain why deeper soil under deeper snow.

Text has been added as quoted in the previous comment response.

- L91: Please explain the role of the drain in the simulations and in the analysis.

Text has been added as quoted in the previous comment response.

- L102: Please specify if the north or south facing subalpine site was taken.

Text has been added to indicate the aspect of all sites in the study. The new text is:

L132-133: “ranged in elevation from a north facing BT site at 2700 masl in a lodgepole pine forest, a south facing NT site at 3350 masl in a large forest clearing, and a southeast facing AT site at 3500 masl.”

- L112-116: It’s not clear where/how this data is used in the manuscript.

Response: Further analysis has now been added that compares these data to simulations.

- In L139, it is not clear how the vertical water transport is calculated. It is obviously not homogeneous in the vertical direction, so does this concern the average vertical flow over all layers in the snowpack? I actually think that it is also interesting to relate this number to meltwater input, or water arriving at the snow/soil interface, or water infiltrating into the soil, to put it in broader perspective with respect to hydrological processes.

Response: We have added the following text to describe this as well as the infiltration information as suggested.

Lines 122-128:

"Flowrate calculations for each simulation was calculated for a 1 m ? 1 m footprint of hillslope such that longitudinal flow is for a 1 m wide section of hillslope summed over the entire depth of the snowpack and vertical flow is for a 1 m2 area on the ground surface summed over the entire depth of the snowpack. These calculations were conducted at a location 15 m downslope and as an average per snow profile for 10 m upslope from this location. Bulk θw values were also calculated for all snow profiles for this same area between 5 m and 15 m downslope for SnowTOUGH simulations to compare to field observations. SnowTOUGH results were analysed for this 10 m length of hillslope to eliminate boundary effects of the upslope and downslope boundary conditions on analyses."

- L143: "liquid water continued to flow" Please specify if this is true for both SNOWPACK and iTHOUGH2, or that it only concerns the lateral flow in iTHOUGH2.

Response: We have now added text stating that the flow is predominantly in the longitudinal direction to make it clear we are discussing the SnowTOUGH simulations. However, you can also see in the new figure of SNOWPACK simulations that liquid water remains and slowly moves vertically as well.

- L190: liquid water in the snow matrix is not necessarily evidence of flowing water, since the capillary forces will retain some liquid water (the irreducible liquid water content, or residual water content).

Response: Agreed. We have now added text discussing this.

L261-262: ". Furthermore, multiple layers retained liquid water that will be more readily available for transport during later melt events"

- Fig. 3: right panel is labeled "b) Alpine" which should be "c) Alpine"

Thank you for pointing this out, it has been corrected.