Dear Editors and Reviewers,

We would like to thank you for reviewing the revised version of our manuscript. We appreciate you taking the time required to provide the feedback a second time. 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 with quoted revised text and line numbers, where appropriate. We list the comments followed by our response in blue text. Again, thank you.

Sincerely,

Ryan Webb

Editor Decision: Publish subject to minor revisions (review by editor) (20 Jan 2021)
by Guillaume Chambon
Comments to the Author:
Dear authors,

I have now received the feedback of two referees on your revised manuscript. They are both happy with the revisions and the responses brought to their comments, and recommend publication of the paper. They do however suggest a small number of additional corrections, mainly of technical nature. Please consider addressing these last issues before submitting the final version of your paper.

Best regards,
Guillaume Chambon / TC Topical Editor

Response:
Thank you for your work on this manuscript. We have now addressed all of the comments as detailed below and made appropriate revisions.

Report #1
Nander Wever, nander.wever@colorado.edu

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)
The authors provided a thoroughly revised manuscript, which can be accepted for publication after taking the comments below into consideration, which are mostly of technical nature.

1) L88/89: "For the IOP simulations, initial conditions were provided through manual snow pit observations (Webb et al., 2020; Webb et al., 2018c)" It's not a trivial task to start SNOWPACK simulations from snow pit observations, since you need parameters such as bond size, sphericity and dendricity, which is typically not available from manual snow pit observations. Please provide more details here.

Response:
We tried to be as consistent as possible with the way SNOWPACK represents these parameters with the measurements we had taken (provided in text as new lines 89–93).

- We assigned liquid water fraction values based on field observations (very wet/slush = 0.07, wet = 0.05, little water = 0.03, sticky = 0.01) that were also confirmed by GPR bulk estimates.

- Because the snow was old at time of observation, we set dendricity to 0 and sphericity to 1
- Bond radius was set to be 0.3*rg where rg is the measured grain radius
    - If multiple grain measurements were given in a layer, rg was the average of all values
- Grain types were coded based on Fig. 8 in Lehning et al. (2002)

We've since added all the model data to a Mendeley Data repository with the link added in the acknowledgements: http://dx.doi.org/10.17632/9wjjvsy82p.1 (will update when paper accepted, repo can be previewed here: https://data.mendeley.com/datasets/9wjjvsy82p/draft?a=8596c963-250a-4f1f-b24f-7e6c88b8d8d8)

New text added to manuscript:
Lines 89-94: "We revised input files for these simulations to be as consistent as possible with SNOWPACK's representation of measured and non-measured parameters. For example, we set liquid water fraction values based on snow pit observations (very wet/slush = 0.07, wet = 0.05, little water = 0.03, sticky = 0.01; Bradford et al., 2009; Techel & Pielmeier, 2011; Webb et al.,2018c). We also defined sphericity to be 1 and dendricity to 0 as most of the snow had metamorphosed by the time of observation, and we set grain types based on Lehning et al. (2002). We estimated bond radius as 30% of the measured grain radius."

2) L113: this porosity is very high for typical soil. Is it maybe (1-porosity), i.e., soil volumetric content, that is reported here?

Response:
The porosity is on the higher end. However, this will allow for more soil storage to avoid any soil saturation and exfiltration from impacting our results. We maintained similar hydraulic properties elsewhere based on field collected hydraulic conductivity data.

New text:
Line 117: "The slightly high porosity was chosen to allow for additional soil moisture storage, if necessary"

3) L118: To prevent confusion, maybe write: "Similar to the SNOWPACK simulations used for the IOP, ", since there are also the seasonal simulations using SNOWPACK that are not initialized using field observations.

Response:
Agreed and revised.

4) L125: "Flowrate calculations for each simulation was calculated for a 1 m x 1 m footprint of hillslope." Awkward sentence, please reformulate.

Response:
Revised. New text:
Lines 128-130: "Flowrates for each simulation were calculated for a 1 m x 1 m footprint of hillslope. Thus, 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 m^2 area on the ground surface summed over the entire depth of the snowpack."

5) Section 4.1 should offer a more detailed explanation of what can be seen in the figures 2 and 3 (see my comment in my earlier review that figure panels are poorly discussed, which I think is still the case). Examples are to mention dates explicitly when the first wetting can be found in for example the BT site, the simulated capillary/hydraulic barriers for the NT and AT site that impact water flow, etc. Section 4.1 is really short, so there is room to really set the stage and make sure that readers understand how these sites differ in simulated snow cover.

Response:
We agree that more could be added, though are trying to keep the text brief. The timing of melt is currently included in section 4.1 in lines 156-159. New text added is as follows:

Lines 165-172: “All three site simulations expressed diurnal melt cycles, including the retention of liquid water overnight. The BT simulations resulted in diurnal melt cycles that ranged from 0-1.1 mm hr-1. The NT simulations resulted in melt rates ranging from 0-3.9 mm hr-1, and the AT simulations resulted in melt rates ranging from 0-2.7 mm hr-1. Additionally, the NT and AT SNOWPACK simulations display the occurrence of hydraulic barriers that persist throughout the entire melt season (Fig. 3). These results also highlight the increased stratigraphy and formation of ice lenses that occur at higher elevations and impact liquid water flow processes (e.g. Webb et al., 2018b). Thus, the IOP SNOWPACK simulations resulted three different snowpack conditions that varied in melt-freeze cycles, metamorphism, and snow accumulation/disappearance throughout the profiles.”

6) L164: grain size or density? I see mostly density changing during melt, not so much grain size. Or do the authors mean that grain size at the surface changes because deeper layers (the one with an ice layer) come at the surface? Or is maybe the color bar not really clear here?

Response:
Grain size is correct. When initialized with snow pit data, SNOWPACK-simulated grain sizes were highly persistent (I.e., little observable change). We mean that grain sizes changed as the grains melted away and disappeared.

7) L165: "All three sites resulted in simulated diurnal melt cycles" Awkward sentence, please reformulate.

Response:
Changed to “All three site simulations expressed diurnal melt cycles"

8) L174-175: Something doesn't fit here. After discussing the BT site (which is the lowest of all), the text continues by talking about "These simulations display...", followed by a sentence discussing the high elevation sites. Please rephrase.

Response:
Agreed. Edited for clarity. Now, after discussing the BT site we added text to transition the discussion to the higher elevation sites. The new text reads:

Lines 179-180: “This dominance of vertical flow contrasts with the simulations of the two higher elevation snowpacks, which display the higher occurrence of hydraulic barriers as a result of the more complex stratigraphy”

9) L174: Figure 5 should be introduced when mentioning the 1.5 ml/s statistic.

Response:
Added

10) Fig. 5: Why is Fig. 5ai,bi and ci not depicting the middle of the profile, or at 5m from the top, to make a more meaningful comparison with the plots shown in 5aii,bii and cii?

Response:
The top row is the average for the middle of the hillslope (from 5 m to 15 m downslope) as mentioned in the caption. This shows an average proportion of lateral diversion per profile that accumulates at the 15 m downslope location. We have modified the label to indicate this and hopefully reduce any confusion.

11) L189: Please mention the specific date for the onset of the storm here to make it easier to compare to the figures.

Response:
Revised to specify “the afternoon of May 15”

12) L205: "Comparisons of SnowTOUGH to field observations varied." I assume you mean that the comparisons showed varying results, not that the comparisons varied?

Response:
Yes, revised to read: “Comparisons of SnowTOUGH to field observations indicated varying results based on site and parameter of interest”

13) Section 4.1/4.2: simulated melt rates are mentioned in Section 4.3, but I think the interpretation of Section 4.2 is helped when melt rates are reported there, to put them in context with the simulated flow rates.

Response:
We have now added melt rates from the SNOWPACK simulations to section 4.1 as quoted in a previous response.

Report #2
Hiroyuki Hirashima, hirasima@bosai.go.jp

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

This paper has been improved by corrections that I requested in the first review. In addition, a comparison with actual measurements of longitudinal flow has been included, making the paper more informative than the first manuscript. Since the consistency of the model was confirmed by comparison with observations, I think it is suitable to publish the paper in TC.

For the comparison with actual observations which is newly added in 4.3, I have comments on it. This is not a requirement for acceptance, so please refer to it when preparing your final draft.

Line 193-200

In the comparison, the measured water content was larger than the simulation which means underestimation of simulation. Considerable causes of this are underestimation of the retained moisture content, percentage of wet snow, or size of water saturated layer due to capillary or hydraulic barrier in the simulation. Do you have any opinion on which one affected this underestimation? If the water saturated layer was underestimated in the simulation, it may lead smaller longitudinal flow. Since this is a comparison with the field observation, it may be difficult for detailed analysis. But If you can describe possible views, it will help to raise future research themes.

Response:
I think this is part of the text that mentions the high mean values are partly driven by the locations with liquid water contents as high as 20%. Also, this is within 2% of observations which is within the estimated error of the techniques, so it could be a possible error in the observations or simulations. I think it is likely a combination of uncertainty in both the modeling and the field techniques. That is why I try to end the paragraph with a statement about the error of the field methods.

Line 210-222

This paragraph was most required in my review. I think the detailed comparison process had better been written because it took a while to understand that EUCA and simulated longitudinal flow per melt rate was compared from this paragraph.
This comparison is desirable to perform in multiple environments and data in the future. In that sense, this paragraph is useful information as a method and first comparison example.

Response:
Thank you for your previous request for this information. We agree that this comparison has improved the manuscript.