The authors detail the evolution of snowpack cold content at 4 sites with differing vegetation cover properties in Quebec, Canada. They do this using a combination of field observations and model output over one field season at their research site. They detail differences in cold content across the sites and present extensive model validation plots and figures. There is a sufficient amount of in-depth analysis and it's clear the authors have devoted a great deal of time and care to the plots, tables, and manuscript. However, there are two major shortcomings/revisions that I feel need improvement before acceptance should be considered:

Dear Dr. Keith Jennings,

Thank you for the kind words and constructive comments. We are very confident that they will help us to build a new version with a much-improved interpretation of the results and a more straightforward take-home message.

1) Given the large number of multi-layer models available, why use a single-layer model and then attempt to split the layers?

First, it is important to recall why we need to use a snow model. As shown in equation 1 of the paper, the cold content of the snowpack is computed from snow temperature, snow density, and snow depth. During our snow pit surveys, we measured these variables jointly and calculated the associated *CC*. These results are presented in section 3 of the article.

However, several phenomena can occur between the snow pits, taken about every week. Thus, it is interesting to explore *CC* at a finer temporal scale. Thanks to our snow profiling stations, we dispose of 30-min measurements of snow temperature taken every 10-cm in the vertical direction, as well as snow depth. However, we do not have continuous measurements of density, and it is here that we decide to use a model.

Before using a model, it is essential to know its strengths and weaknesses. We chose CLASS is that it was evaluated at our site for its ability to reproduce the observed snow heights (see Alves et al., 2020). The model showed its ability to reproduce the longevity of the snowpack but tended to underestimate the snow height for the reasons discussed in Reviewer 1's comment 1.

There are, of course, several other snow models available, some of which are multi-layered, but before resorting to them, we would have had to do a complete validation at our site, which we felt was beyond the scope of the study.

Since CLASS is a single-layer model, we employ a method to generate multiple density layers, much like Roy et al. did in 2013 to estimate the SSA. Note our process of densifying each layer taken from Shrestha et al. (2010) is similar to what is implemented in the snowpack model.

We, therefore, plan to clarify the reason for our use of CLASS in the new version of the paper by synthesizing the above points.

2) It's particularly unclear to me how the Andreadis method is applied.

In agreement with the other remarks along these lines, we propose to add Figure R4, which will clarify the methodology used to obtain the reconstructed *CC* time series. It shows where the application of Andreadis (2009) method fits into the process.

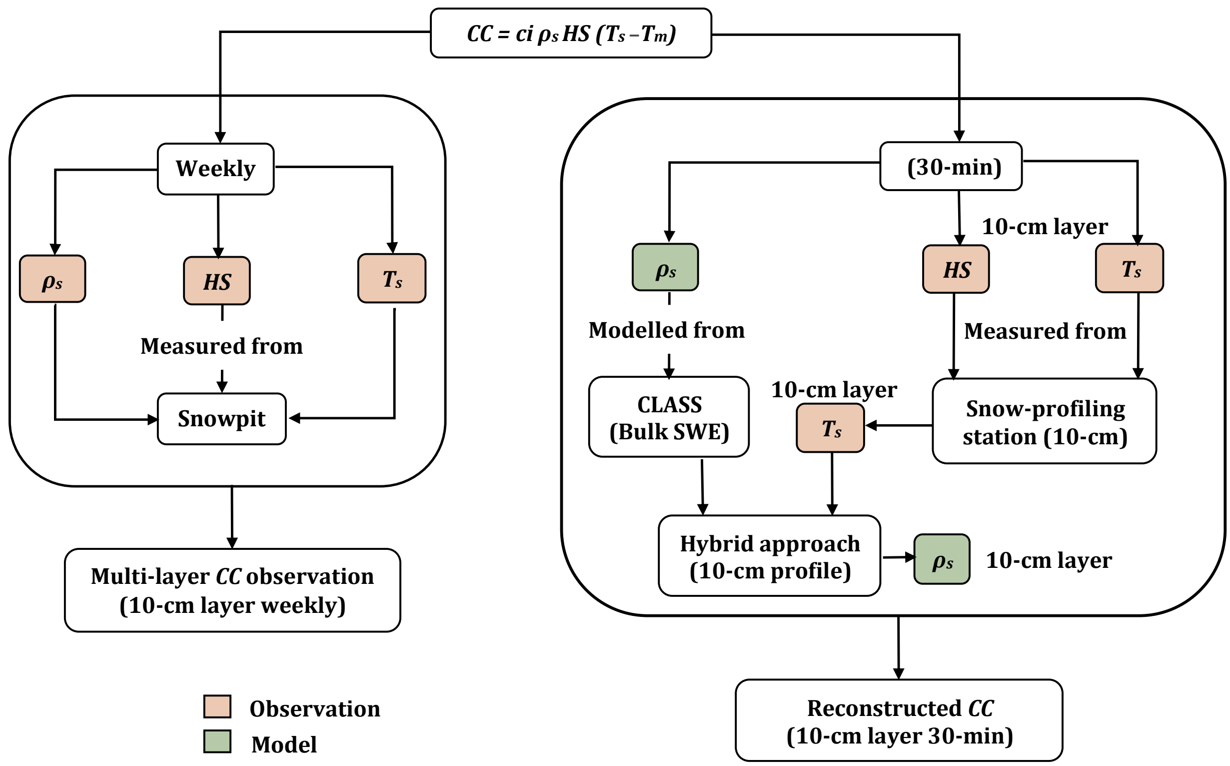

Figure R4: Schematic diagram showcasing the methodology adopted to conduct this study.

3) The results make it seem like the authors only track 3 layers, but then subsequent sections break out layers in 10 cm increments. Do the layers stay consistent or are they combined and divided as would be done in a multi-layer model?

We agree with the reviewer that the methodology section was not sufficiently described in previous version of manuscript. Indeed, we performed the 10-cm vertical layer scheme for our analysis and then divided them into 3 layers only to visualize and understand the difference of snow density across the snowpack.

4) The methods text says simulated SWE from CLASS is used, but then the equations are all for density.

Thank you for your relevant suggestion. In our previous version, *Wns* and *Ws* (derived from CLASS) presented in Equation 6 represented the amount of new and old snow water equivalent, respectively. To make this more explicit, we will change the variable names to *SWEns* and *SWEs*, respectively.

5) Do the final simulated CC series use the thermocouple data or snowpack temperature output from CLASS? If the latter, how is temperature reallocated?

We used thermocouple data, as shown in the new Figure R4.

6) I think a schematic showing the complete workflow for the reconstructed CC time series would benefit the readers, in addition to providing much more detail in the methods text.

This is a great idea, thanks and see previous comments.

7) Scientific contribution. I'm left wondering what the major novel contribution of the paper is, which means it needs further work to be accepted in The Cryosphere.

The main original aspect of the work is as follows:

Research topic

As detailed in Jennings et al. (2018), the lack of direct observation still hinders the understanding of snowpack cold content. To the best of our knowledge, only a few studies have investigated this state variable. In the past, some observational studies (Seligman et al., 2014; Jennings et al., 2018) and snowmelt models have (Jost et al., 2012; Valery et al., 2014) either detailed or inserted snowpack cold content. However, only few of them attempted to understand this state variable at forest (e.g. Jost et al.,2012; Jennings et al., 2018). But none of them compared the variability of *CC* across different stand structures. In this study, we attempted to understand the *CC* variability across four contrasting sites with canopy. We believe, such comparison is first of its kind particularly for cold content studies.

Methodology

Aided by snow profiling station and labor-intensive field measurement, our study used a time series of snow temperature profiles (10-cm apart) and combined with an empirical formulation to produce a multi-layer cold content time series across four different forest sites. The manual weekly observation of multi-layer snow density, snowpack temperature, and snow depth enabled us to understand the behavior of *CC* across our forest sites. The use of 30 minutes 10-cm snowpack temperature profiles combined with snow depth and empirical snow density estimate provided us with a multi-layer time series of *CC*. We believe using an automated snowpack temperature profile at four sites to derive *CC* is the first of its kind as well.

Take-home message

Our study aimed to understand the *CC* behavior across four different forest stands. Based on our findings, we were able to relate the role of vegetation on the accumulation and ablation of snow and its role on *CC*. Furthermore, we have presented the effect of short-lived events, such as the effect of cold air pooling on *CC*. Here site A3 exhibited a unique thermal regime which ultimately resulted in the difference of *CC* at this site compared to others. We also would like to thank the reviewer for raising pertinent issues related to the inclusion of the rain-on-snow event to bolster our discussion. Indeed, the rain-on-snow event resulted in *CC* removal and led to almost uniform *CC* distribution across our sites (regardless of differences in vegetation and vegetation altered energy exchanges).

As presented in Jennings et al. (2018), the authors detailed the relationship between cumulative temperature and precipitation with cold content development. However, due to snow interception, snow accumulation and melt are more variable in forest than any other locations (Parajuli et al.,2020). In a forest, the intercepted snow may immediately unload or stay there (densify) for several days to months and unload (Dewalle and Rango, 2008). Thus, this study attempted to relate pertinent variables such as snow depth, air and snowpack temperature, and snow density. We believe this is also a novel aspect of our study.

8) Overall, my feeling is the authors can devote less results text to model validation and output (particularly the analysis of the 3-layer scheme as I note in my line-by-line comments below) and spend more time on process-based analysis. For example, what happens during the rain-on-snow events from an energy balance perspective? Why are there differences in cold content development across the sites? Is it the effect of canopy cover on snow accumulation and/or the snowpack energy balance? Given the field observations and modeling, I feel there is a lot more the authors could unpack that give us, the readers, deeper information on the processes governing cold content development and removal at these particular sites.

We agree with the reviewer and add several sentences to describe different energy budget components and other process-based analyses in our discussion. This includes the role of the canopy on *CC* variability, the role of latent flux difference on snow accumulation and ablation and ultimately *CC*, the effect of rain-on-snow events on *CC* distribution, the importance of solar radiation and its role on *CC* variability, and the importance of snow interception on snow densification in forest and its role on *CC*. We will also compare different short-lived events from an energy balance perspective.

Line-by-line comments:

9) Line 9: Surface melt can begin before snowpack cold content = 0.

We will remove the sentence, thanks.

10) Line 37: The snowpack energy balance predates USACE (1956). For example, Angström's work from the 1910s on the radiation budget and Western Snow Conference papers from the 1940s published in Transactions of the American Geophysical Union. I'm sure there's plenty more going back in time.

Thanks for bringing this up, we will update the reference.

11) Line 37: DHSVM was developed with a single-layer snowpack, but now uses a two-layer formulation (Wigmosta et al., 2002): Wigmosta, M. S., Nijssen, B., Storck, P., & Lettenmaier, D. P. (2002). The distributed hydrology soil vegetation model. Mathematical models of small watershed hydrology and applications, 7-42.

We agree with the reviewer. Thanks for your concern we will update it accordingly.

12) Line 46: ρ_w is not in the equation

We will rectify the error, thanks.

13) Line 55: Change snow surveys to snow pits.

We will change this, thanks.

14) Line 55: Reconsider “tedious and demanding.” I'd probably say time-consuming (I find digging snow pits to be quite enjoyable, not tedious).

We will rephrase the sentence, thanks.

15) Line 56: You can note example datasets here. E.g. the Williams and Morse Niwot LTER data: Williams, M., J. Morse, and Niwot Ridge LTER. 2020. Snow cover profile data for Niwot Ridge and Green Lakes Valley, 1993 - ongoing. ver 15. Environmental Data Initiative. https://doi.org/10.6073/pasta/a5fca9d02a4a6a0744cc0d0ffccacd09 (Accessed 2021-05-06).

We will cite the reference you have provided, thanks.

16) Lines 56–59: This reads very similarly to the opening sentence of paragraph 2 in Jennings et al. (2018). Please note as such.

We will reword the text to avoid too much similarity to Jennings et al. (2018) and add a reference to this paper.

17) Line 59: Please replace all uses of resort/resorted with a more appropriate word (e.g., used, leveraged, utilized, etc.). Resort typically has a negative connotation (i.e., there was nothing else we could use so we had to use this.).

We will replace the word throughout the manuscript, thanks.

18) Lines 62–64: This was noted in Jennings et al. (2018) (please add text as such). Lines 65–66: This is again very similar to the language in the Jennings et al. (2018) manuscript. Please be careful with paraphrasing and providing citations for paraphrased work.

We will improve our paraphrasing and add the reference.

19) Line 68: Jennings et al. (2018) includes a forested site (the subalpine) and compares it to a higher, open site (the alpine).

We agree with the author and will add this precision in the paper.

20) Line 69: Remove “obviously”

We will remove it, thanks.

21) Lines 78–79: “model articulated around the energy balance” > rephrase for clarity.

We will rephrase the sentence, thanks.

22) Line 84: “even models such as this are not free of biases” > rephrase because all models have some form of bias. You could highlight the previously cited snow model comparison literature.

We will add a supporting sentence for our statement.

“For instance, Raleigh et al. (2016) tested three physically based snow models (Utah Energy Balance (UEB), Distributed Hydrology Soil Vegetation Model (DHSVM) and Snow Thermal Model SNTHERM) and reported bias in longwave radiation estimation ranging from −12 to +18 W m^{-2}.”

23) Line 87: What is “acceptable” in this context? Most readers, myself included, will not have a good intuition of what large and small SSA values are.

Thank you for your concern. For the sake of clarity in the range of SSA value we will add the following sentences.

“For instance, Roy et al. (2013) disaggregated CLASS-derived snow water equivalents into multilayer values at each time step, for the purpose of estimating the specific surface area (SSA) of a snowpack. In their study, the authors reported a specific surface area ranging from 33.1 to 155.8 m2 kg^{-1}, for an acceptable root mean square error (RMSE) of 8.0 m2 kg^{-1} in CLASS-derived SSA for individual layers.”

24) Line 89: Remove “obvious” and rephrase sentence. I think you're referring to cold content modeling specifically, but there are many forest snow model studies.

Based on the reviewer suggestion, we will rephrase the sentence.

25) Line 105 (figure 5 caption and throughout): Consider changing the names A1 through A4 to more meaningful terms or abbreviations. There are three forest cover classes (sapling, juvenile and mature). You could use some variation thereof and specify which mature forest site is denser.

We will change the name to “Sap1”, “Juv1”, “Mat1” and “Mat2” instead of A1 through A4.

26) Lines 117–124: This could use greater explanation. What instruments were used, what was the vertical frequency of density and temperature sampling, etc? How were the pit values used to impute the missing data?

The snow pit data (snowpack density, temperature, and depth) are exclusively used for the observational study (sections 3.1, 3.2, and 4.1). For model validation, we used the snow density from snow pit surveys. However, the snow profiling station provided the 30-minute 10-cm snowpack temperatures.

27) Lines 128–130: “A simple approach would be to interpolate the density values extracted from snowpits, but this would be incomplete and error-prone given their limited number and absence early in the season.” This seems overly subjective, especially considering the low r^2 values for the hybrid approach.

Our site has the particularity of being very snowy, and it is not uncommon for episodes delivering >10 cm of fresh snow to occur (see Figure 8 from the paper). In this sense, a simple linear interpolation of density values appears to be a hazardous exercise. Also, as stated in the cited sentence, if we were to do this, we would have no value during the early season and very few towards the end of the melting period. This will be emphasized in the next version of the paper.

28) Lines 136–137: Further explanation needed. For example, what are the prognostic variables?

We thank reviewer for raising this issue. We will add the prognostic variables.

“In this analysis, CLASS version 3.6 was used in offline mode and with a 30-min time step, ensuring an uninterrupted time series of the prognostic variables (Roy et al., 2013) thereby allowing the inclusion of multiple soil layers and accounting for snow interception, snow thermal conductivity, and snow albedo, as described in Bartlett et al. (2006).”

29) Line 147: Change Table 2 to Table 1

Thank you for your suggestion. We will change this.

30) Table 2: It doesn’t seem like all sites should be marked as having PPT measurements when the text says it was measured at one location 4km away?

We agree with the reviewer and will modify the table accordingly, thanks.

31) Line 165 (Section 2.2.3): Please see my major revision comment #1. This section needs significant improvement in terms of clarity and specificity.

Thank you for raising this issue. We have prepared a schematic diagram (Fig. R4) and will take great caution at explaining every steps of the approach.

32) Lines 169–171: Text says minimum, but equation uses maximum.

Indeed, the text says minimum as we took the minimum value of snow density derived by snow pit surveys which is 76 kg m^{-3}. However, the use of Equation 2 sometimes resulted in snow density estimates below 76 kg m^{-3}. Thus, we used a maximum value as in Brun et al. (1989).

33) Lines 206–207: Are these differences computed within the snowpack at each site per time or computed across all sites?

Here, we presented the minimum and maximum cold content from all the sites for individual layer. We will clarify this in the next version.

34) Line 213 (and throughout text): Please change amplitude to magnitude.

We will change them throughout the manuscript, thanks.

35) Figure 3: I like the amount of info conveyed by this figure, but find the color scale to be confusing. Because it's representing a single, non-divergent variable, you would be better suited by using the shade of single color. Also, please add "Layer cold content" to the scale bar.

The reviewer raised a good point. We have prepared a new version of the plot with shades of a single colour (Fig. R5). We find it more difficult to make a quantitative interpretation of the figure and would prefer to keep the original version.

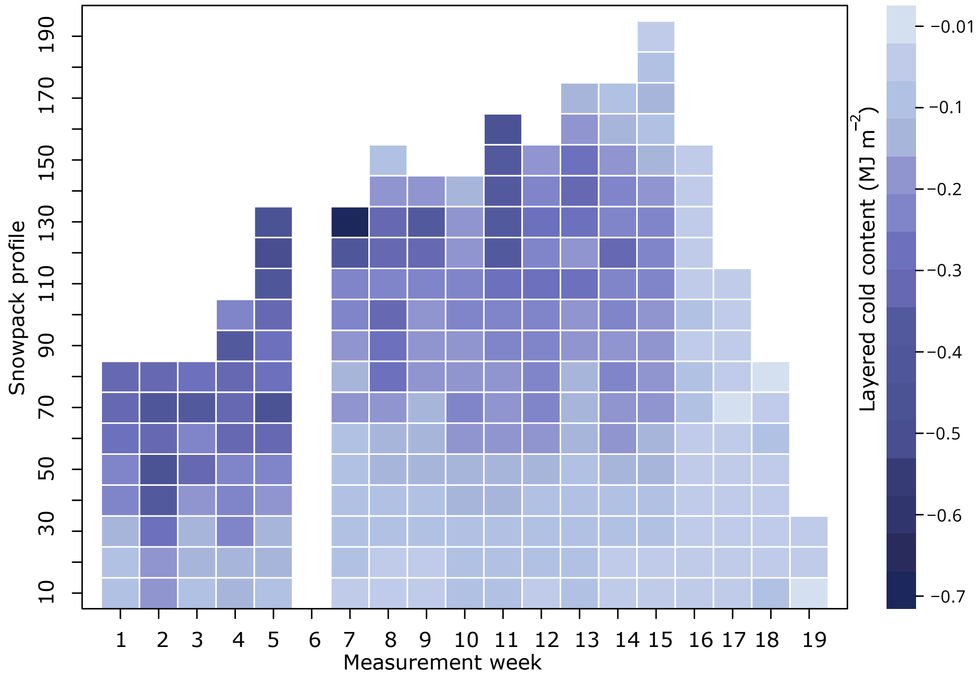

Figure R5: Example of multi-layer cold content observation using the shades of the purple colour.

36) Line 222: Is this the maximum difference or difference in maximum CC? Table 3 shows a larger difference between A1 and A4.

Indeed, here we are referring to the mean total *CC*. As such, we believe this statement is correct.

37) Table 3: The minimum appears to occur in late March (early April?) for site A2 as seen in Fig 4.

True. We will correct this mistake, thanks.

38) Line 232: Please use less subjective terms (for example, "CLASS produces low mean biases in…").

Thanks, we will modify the sentence.

39) Line 233: Somewhat interestingly, the negative SWE bias would indicate the model has a cold bias in snowpack temperature in order to get CC so accurately. Please include snowpack temperature in the validation.

See our answer to comment 1 from Reviewer 1.

40) Line 239: It appears you're producing a 3-layer scheme, but this is not specified in the methods section. Please add that along with information on how the layers are defined/combined/separated (see major revision comment #1).

See our answer to your comment 3.

41) Lines 240–242: You need to clarify when you're using snow temperature from the thermocouple arrays versus from the snow pit or CLASS. It's not clear here or in the methods section. I think it would be worth adding some material to the methods and again to the results so the readers are certain when observed versus simulated values are being used.

The new figure (Fig. R4) will avoid any confusion in this regard.

42) Line 249: This statement seems incorrect. If you're using the snow temperature from the thermocouple, then most of the error is coming from the density estimates and layering scheme.

True, most of the error is coming from density estimate. Thus, we have portrayed Figure 11, to identify the probable reason for uncertainty in snow density estimate.

43) Figure 6 and preceding text: I'm leaning towards getting rid of the arbitrary 3-layer scheme and only validating/describing the 10 cm layer results shown below. It's unclear what information/utility the 3-layer scheme provides given that most of the findings are based on the observations and the 10 cm layer discretization.

It is a way to visualize a large amount of data. To reproduce the same figure but for 10-cm slices would be very difficult to analyze in our opinion.

44) Figure 7: Please change to a table. The color scale provides the same information as the numbers, but the numbers on their own are easier to interpret.

We prefer to keep the plot as it is. A similar figure is presented in Parajuli et al. (2020a) and we believe there are several advantages to it. For instance, the colour box immediately notifies the reader about the existing difference in our error metric. Also, the text will provide a minor detail present in this plot. We believe, both text and color scale are complimenting each other.

45) Lines 264–265: Were the rain-on-snow events missing because no snow pits were dug at those times or the snow pits suggested different changes to cold content than the simulations? Either way, this needs to be clarified.

Indeed, no snow pits were dug during rain-on-snow events. Please refer to Line 119 for more details.

46) Figure 8: Same comment as figure 3. Consider changing the color ramp and adding "Layer cold content" to the scale bar.

Please refer to comment 35, thanks.

47) Lines 273–274: This assertion is presented without supporting evidence. In line with my major revision comment #2, this would be an ideal place to test some process-based hypotheses.

We disagree with the reviewer. Parajuli et al. (2020b) reported the effect of stand structure on snow accumulation and ablation in the present research site, which is acknowledged in the discussion section. Here, as presented in Table 1, the stand structure is not uniform. Also, differences in snow accumulation and ablation are shown in Figure 9b, influenced mainly by vegetation. I believe this assertion is not presented without supporting evidence. However, we will add some more details to justify our statement.

48) Figure 9: Why is simulated cold content plotted with observed snow depth? It seems like observed snow depth should be plotted with earlier observational figures. Also, keep the rain-on-snow shading consistent with previous figure.

Here, our simulation is largely built from observations. We believe the new figure (Fig. R4) helps clarify this. We agree with the reviewer for the rain-on-snow shadings and will modify this accordingly.

49) Lines 281–285: These results need more unpacking and their associated methods need to be moved to the methods section. You should also be careful with "positive" and "negative" correlations here. Most cold content values have been discussed in terms of their magnitude, which may lead readers to think cold content declines (i.e. approaches 0) when air temperature decreases. Additionally, simulated and observed values need to be noted explicitly in the text along with the period of comparison (is CC correlated with 30-min air temperature, daily air temperature, or average air temperature to date?).

We will be more transparent in this section. We will move all material associated with the methodology to the appropriate section. Also, the negative correlation is due to negative values of *CC* and positive values of snow depth. We will modify the text accordingly.

50) Lines 286–291: Similar to my above comment, this needs much further explanation. This section could become an important part of the paper (and its novel contribution to the field) if you can further evaluate forest cover differences and their quantitative effect on cold content evolution. For example, you could include an assessment of snowpack energy balance differences and/or changes in snow accumulation as caused by forest cover in both observations and the model.

We disagree with the reviewer and believe that we have evaluated the forest cover difference and their quantitative role on *CC* evolution. Figure 3 to Figure 10 detail the spatiotemporal variability of cold content with respect to the vegetation. As illustrated in Figure 3 and Figure 4, based on observations, the detailed multi-layer cold content time series is presented for respective sites with diverse stand structures. In Figure 9, we sought the relationship between the snow depth (accumulation and ablation of snow) and its role on *CC*. Figure 10 attempted to establish the relationship between different pertinent vegetation inputs with *CC*. In this plot, we performed such an assessment detailing differences in the vegetation. In the result section, we have described the mean, peak, and even the variability of *CC* across sites. Nonetheless, we will improve and add more relevant information as suggested by both reviewers. We agree with the reviewer, and we will now present the *CC* variability and short-lived events with a snowpack energy budget perspective. Please refer to comment 8.

51) Figure 10: Please change to table (same comment as figure 7).

Please refer to comment 44.

52) Line 298–299: Figure 11 does not show cold content.

Indeed, the cold content time-series for site Sap1 was plotted before (Please refer to Figure 3)

53) Figure 11: Need to clarify if these are simulated or observed values. There is no light-blue shading in the plots. Also, the color ramp should not be divergent as the values they represent are not. Consider using gradation of single color.

We agree with the reviewer and will modify accordingly, thanks. For colour ramp please see the comment above.

54) Lines 304–309: Figure 4 does not show mass, only cold content. It might be worth further evaluating SWE, depth, and cold content over time in the results sections. Also note that the average winter temperatures in Jennings et al. (2018) were ~4°C cooler at the alpine site (colder frozen mass and more of it led to greater CC in the alpine).

True, Figure 4 is based on weekly measurements, and we don't have complete series of SWE and depth. However, as presented in Figure 9, we have plotted *CC* with the snow depth as the snow-profiling stations provided continuous time series of snow depth.

Thank you for providing the detail description of Jennings et al. (2018), we will add this information.

55) Lines 318–350:) Lines 318–350: I like this section, but I feel like the paper would have a greater impact if there was a greater reliance on results versus discussion when comparing the sites (please see my comment on lines 286–291). For example, you discuss cold air pooling here, but don't provide data. Why not add this to the results section with data from the air temperature sensors at the different sites? If these data don't support the hypothesis, then it can be removed. Data from Jennings et al. indicated that the energy balance was typically positive when snow was not actively accumulating. However, there were exceptions at night as a result of radiative cooling from the snowpack. You could provide a comparison from your sites here by providing energy balance output from CLASS in the results.

We agree with the reviewer and will modify the cold air pooling discussion. As pointed out by reviewer 1, we will add the discussion relating to the low wind speed (stable atmospheric condition) and cold air pooling mechanism, referring to Figure R2. Also, we will compare the energy balance output for the period with low wind speeds and rain-on-snow events.

56) Figure 12: This is an unfair comparison. The density of freshly fallen snow is not comparable to the density of the top 10 cm of a snowpack. There's no fresh snow I know of that falls at 400 kg m^{-3}.

Sorry for misunderstanding. However, the plot displays the superficial snow density not the fresh one. Please refer to Figure R2

57Lines 362–366: Please see comment above. Consider removing plot and text unless the analysis is changed to provide a more important validation of new snow.

Please refer to above comment 56.

58) Line 397: Please add link in revised manuscript.

At this point, we are working on a data manuscript and will share our data once our paper undergoes discussion. Also, the dataset will be available from authors on a reasonable request immediately. Thanks.

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