Dear editor,
We would like to thank you for providing us with the opportunity to revise our paper. Also, we thank both reviewers for providing constructive feedback and helping us improve the overall quality of our manuscript. As detailed below, we have either modified the paper or provided proper justification to address the reviewers' concerns.

Reviewer 1

The authors have carefully considered the comments from myself and the other reviewer, and I am satisfied that they have sufficiently addressed my concerns and suggestions. I do have some minor corrections and suggestions that do not alter the substance of the paper.

Thank you for your kind words.

Suggested minor corrections and changes:

1) Line 65: This is confusing. The cold content could be very negative such that the snow does not melt, as happens for most of the winter. As such, how is a melt delay defined? I can understand reporting that when the cold content was estimated to be zero at 6:00 AM, melt was not detected for another 2.3 - 2.8 hours (perhaps until the radiation balance was no longer negative, or radiation plus sensible heat flux [i.e. the snowpack energy balance] was > 0).

Sorry for this confusion. We have removed this confusing sentence, thanks.

2) Line 66-67: The delaying effect of CC on melt in spring is less because, while air temperature must be at or above 0°C in all cases, in spring the radiation input is greater, which would lessen the delay because of increased energy available for melt.

Thank you for your suggestion, we have modified the sentence, which now reads: "This delaying effect of CC on melt is less marked in spring (Seligman et al., 2014), when excess radiative energy contributes more significantly to melt."

3) Line 68-69: Good point!

Thanks.

4) Line 116: Delete "of".

We deleted it, thank you.

5) Line 180: Change "weighting" to "weighing".

Done, thanks.

6) Line 240: Change "Excpet" to "Except".

We rectified this mistake, thank you.

7) Line 242 & Figure4: I wonder if the largest CC at Sap1 was missed in week 5. Perhaps note this possibility.

Thank you for pointing this out. We have added the following sentence: “Due to logistical constraints, we were unable to sample the Sap1 site on week 5, when all other sites appear to report one of the largest cold content amplitudes of the winter.”

8) Table 3 & Figure 5: The smallest CC at Mat1 appears to be March 20 or on the same date as Sap1 and Mat2 based on Figure 5.

Thank you for pointing out this minor mistake. We have rectified it, please refer to Table 3.

9) Line 275: Do the authors mean Figure 8?

Indeed, thank you.

10) Line 279: Perhaps the density of the top layer is underestimated because of CLASS not accounting for the densification of snow prior to unloading, as well as possible drip from a solar-heated canopy that would freeze. A low density would result in a smaller heat capacity and bias CC towards smaller (less negative) values (although thermal conductivity would also be affected).

Good point! We have now added a sentence to discuss the effect of radiative heating of the canopy resulting in drip from intercepted snow and densification of the top snow layer in the discussion section: “In addition, the intercepted snow could be heated by radiation incident on the canopy. In doing so, local melting could be observed, resulting in dripping and an increase in the density of the top snow layers. It is possible that CLASS does not capture this phenomenon well.”

11) Line 319: Do the authors mean “SM” rather than “SP”?

We have rectified this error, thank you.

12) Line 341: Change “unique” to “different”.

Done, thanks.

13) Line 350: Do the authors mean “Jun1” rather than “Sap2”?

Indeed, we have corrected this error.

14) Line 376: Change “certain” to “a certain”.

Done, thanks.

15) Line 381: Change “generated a reasonable” to “generated reasonable”.

Done, thanks.

16) Line 382: Change “that are more prone to capturing short-lived” to “that are better able to capture short-lived”.

Thank you for your suggestion, we have modified our sentence.

17) Line 403: Drip from a canopy that is heated by solar radiation is also a factor here.

Please see comment 10 above.

18) Line 409-410: Remove one instance of the word “indeed”.

Done.

19) Line 413-14: The effect of high winds during cold air messes is as described. However, often under stable conditions radiative cooling is the dominant mechanism and wind would warm the snowpack during such times.

Indeed, this is why we provided a short discussion about sensible heat fluxes (lines 408 to 411).

20) Line 416-17: I am confused by “The absence of a well-defined at canopy also means greater incoming shortwave radiation.”

We removed “at” from our sentence, thanks.

21) Line 420: Change “This is also the reason why there were 60% occurrence where magnitude of CC”to “This is also the reason why 60% of the time, the magnitude of CC”.

We have changed this sentence accordingly, thanks.

22) Line 422: Change “For site Mat1, there were 32% occurrences where magnitude of CC were higher than at Sap1,” to For Mat1, the magnitude of CC was higher than at Sap1 32% of the time”.

Done.

23) Line 472-3: Since this was not absolute, I would either turn it around and state that “Areas with taller vegetation had the smallest snow accumulation and thus resulted in the smallest peaks in total CC” or state “The two sites with lower vegetation had greater snow accumulation and larger peaks in total CC relative to the sites with taller vegetation”.

We have changed this sentence according to your suggestion, thanks.

24) It would be good if the data could be posted somewhere accessible.

Data are available upon request. We also plan to publish them in a data journal soon.

Reviewer 2

The authors present a revised manuscript detailing the seasonal evolution of cold content at four sites in the Boreal forest of Québec, Canada. This version is markedly improved, particularly the introduction and results/discussion. The methods are easier to follow, making the paper more accessible to a wider audience. I recommend publication after a couple minor revisions. These are shown in the list below and detailed more thoroughly in my line-by-line comments.

Thank you for your kind words.

Minor revisions

1. Lines 313–325. Persistently negative net fluxes often translate into cold content magnitudes that are too high. Please check the numbers here and the allocation of energy losses in CLASS.

We agree with reviewer that persistent negative net fluxes often translate into large magnitude of CC. It is also correct that our research site persistently experienced cooler temperature from late December 2017 to the end of January 2018, which might have negatively skewed the melt/refreeze flux. Afterwards, there were rain-on-snow events, cold spells, and episodes with milder air temperature. At our site, the snow stayed for 218 days consisting of 176 days of snow accumulation and 42 days of snow ablation. During the winter, sunshine duration ranges from 7 – 9 hours whereas it ranges from 13 – 15 hours in springtime. Based on your suggestion, we carefully checked CLASS simulation, and the values look ok. Nevertheless, we have to acknowledge the fact that several errors and bias may arise when simulating CLASS. One source of error (latent heat overestimation) is highlighted in our discussion section. Also, Alves *et al.* (2020) already pointed out some deficiencies of CLASS for the present research site.

2. Lines 410–442: Please move novel results to results section and save this space for your discussion text of the energy balance differences.

See our answer to your comment 26.

Line-by-line

3) Line 11: Change “Estimating the cold content” to “Measuring cold content”

We rectified this concern, thank you.

4) Line 16: Add “modeled” before time series

Thank you for your suggestion. It is added.

5) Lines 25–27: Please clarify that CLASS output is compared to bulk values and that the empirical scheme is compared to layered values (unless I misread)

Thank you for your suggestion. We have addressed your concern.

6) Lines 36–37: Not important, but you can typically strengthen sentences by removing words (i.e., “improved decision-making in the area of water resources management” becomes “improved water resources decision-making”)

We modified the sentence, thanks.

7) Line 95: Please modify to note SSA is a property of snow grains

We addressed your concern, thanks.

8) Line 99: Although I appreciate the shout-out, you can remove Jennings et al. (2018) as you've done novel, independent research

We prefer to keep this sentence as it is, thanks.

9) Line 140: Change "it was opted" to "we opted"

We changed the sentence, thanks.

10) Line 145 (Figure 2): The schematic makes the process much easier to understand. The only suggestion I have is that directional arrows in a flowchart typically denote the creation of a new variable from a previous variable. In this context, arrows ps, HS, and Ts in the lefthand box would point to a new variable called cold content (not snowpit). Given these measurements are all from the snowpit, the lefthand box can be annotated to say weekly snowpit. I think the flowchart is probably fine as-is, but can be improved for clarity.

We have modified our schematic accordingly, thanks.

11) Line 247 (Figure 4): There's no light blue shading

We rectified our error, thanks.

12) Line 269: You can also cross-reference your methods subsection here

Based on your suggestion, we have added the cross-reference. Thanks.

13) Line 275: Change "structured" to "summarized"

Done, thanks.

14) Line 290 (Figure 8): We'll agree to disagree here (if you change your mind, please remove the shading and make this a table as the shading provides no additional info past the numbers shown)

We prefer the keep the plot as it is, thanks.

15) Line 306: Should Fig. 8 be 10?

True, we changed the mistake, thanks.

16) Lines 313–325: I am suspicious about the persistent negative flux in the WA period. In general, cold content is a small proportion of the total energy required to warm, ripen, and melt a snowpack. Therefore, persistently negative Qm values over long periods are often improbable because they lead to impossibly low cold content values. For example, if we assume a WA period of 90 days, which is a conservative assumption, the low Qm value of -6.6 W m-2 at Sap1 would produce -51.3 MJ m-2 of cold content (it's not clear whether cold content added by snowfall is included in Qm or if CLASS adds it separately). Perhaps the duration of rain-on-snow events with high positive energy fluxes is enough to counteract this effect, but I'm still skeptical. If we take the December 17th to January 18th period (consistent CC development), CC goes from ~-1 MJ m-2 to ~-4 MJ m-2 at Sap1. This

corresponds to an average flux of just -1.1 W m-2, which may or may not include the CC added by new snowfall. Perhaps the persistent negative values of Qm are "correct" but CLASS is using them to also cool the soil layer and not just the snowpack? No matter the cause, this discrepancy between the CC values and the negative fluxes needs further investigation and clarification. (One note: Subdaily net fluxes can be negative—e.g., nighttime cooling or cold, windy periods of high sublimation—but these would be balanced out over longer time scales so that the average net flux was near zero during the winter accumulation season.)

Please see the response to comment 1, thanks.

17) Line 327: Remove extra period

We removed it, thanks.

18) Line 328: Add clarification after "positive correlation" (i.e., "meaning cold content magnitude increased as air temperature and snowpack temperature decreased")

We have added this clarification, thanks.

19) Line 341: The air temperature values are nearly identical. Please change

This paragraph has been removed as you suggested in comment 21.

20) Line 343: Figure 13 does not show cold content

This figure has been removed as you suggested in comment 21.

21) Lines 341–347: On closer reading, this paragraph and figure can be removed. The text is difficult to follow and it's unclear how the figure contributes to the discussion past the figures already displayed.

We agree and have removed Figure 13 and the associated discussion, thanks.

22) Lines 356–361: An alternative hypothesis is that a cold snap led to snowpack cooling. The more exposed site (Sap1) experienced greater cooling than the less exposed sites.

We have added the following sentence at the end of this paragraph: "Finally, as explained in section 4.3, the Sap1 site is more exposed to atmospheric conditions without the 'protective' effect of the canopy, making it more likely to respond to cold snaps for example."

23) Lines 365–366: Change "Based on our findings, CLASS successfully estimated snow density and CC. Although CLASS 365 reasonably predicted SWE, one cannot deny the fact that it underestimated observations" to "Based on our findings, CLASS successfully estimated snow density and CC, while underpredicting SWE."

We have modified the sentence accordingly, thanks.

24) Lines 367–371: Are these sentences suggesting class overestimates sublimation, leading to an underestimation of SWE? If so, please clarify.

The sentence is suggesting that CLASS is overestimating latent heat flux. This now reads as: "Based

on these recent studies, it seems fair to conclude that the overestimation of latent heat fluxes by CLASS could lead to SWE underestimation."

25) Lines 380–407: This section should be shortened significantly. I think it is sufficient to say the new snow density formulations led to persistent underprediction errors for the top layers, even when compaction is considered. This is likely a shortcoming of both the fresh density and compaction due to metamorphosis equations.

We believe this is an important section of this manuscript. We prefer to keep it as it is.

26) Lines 410–442: I really like the inclusion of the energy balance analysis, but these sentences contain novel results. Please keep the discussion parts here while moving the quantifications to an additional results subsection. (Also, please limit the use of "indeed" to start sentences.)

First, we would like to point out that the presence of the energy balance analysis is in response to a past request from the reviewers and that without them, this very relevant addition to the paper would have been impossible.

After careful rereading of this section, it appears that the vast majority of it contains analyses based on many figures from the results section and therefore, in our opinion, it is more appropriate to leave it in the discussion. There are certainly some small new results, but these are only intended to provide a very focused addition to the discussion. We therefore prefer to keep the section as it is now, hoping that the reviewer now shares our opinion.

Finally, you are right, there was a slightly exaggerated use of the word "indeed". We have either removed or replaced 3 of its 4 occurrences.

27) Line 417: Remove "at" from in front of "canopy"

Done, thanks.

28) Line 419: My take on this is that the 0.1 MJ m-2 difference is negligible given the observation precisions and model biases.

True, we removed the cross-reference here, thank.

29) Line 434: Please expand on this sentence. The canopy blocks incoming solar radiation (more CC) but also increases incoming longwave radiation (less CC) compared to an open site.

We have expanded this sentence, thanks.

30) Line 437: Please cite peer-reviewed literature on energy fluxes during rain-on-snow (e.g., Marks et al., 1998): https://doi.org/10.1002/(SICI)1099-1085(199808/09)12:10/11%3C1569::AID-HYP682%3E3.0.CO;2-L

We have cited this reference, thanks.

Reference

Alves, M., Nadeau, D. F., Music, B., Anctil, F. and Parajuli, A.: On the performance of the Canadian Land Surface Scheme driven by the ERA5 reanalysis over the Canadian boreal forest, J. Hydrometeorol., 21(6), 1383–1404, doi:10.1175/jhm-d-19-0172.1, 2020.

Seligman, Z. M., Harper, J. T. and Maneta, M. P.: Changes to snowpack energy state from spring storm events, Columbia River headwaters, Montana, J. Hydrometeorol., 15(1), 159–170, doi:10.1175/JHM-D-12-078.1, 2014.