1) In this study, the authors have employed a combination of empirical modelling and measurements to assess the evolution of the cold content of the snowpack in four closely located boreal forests in Québec, Canada. The general methodology of the study is explained sufficiently well and the results are of interest to the forest hydrology and water resource communities, as well as the land surface and snow modelling communities. I feel that the results are worthy of publication, but I would like to see a little more effort towards examining the differences between the snowpack properties and CC estimates between the sites.

We would like to thank the reviewer for investing his/her valuable time and effort in providing constructive feedbacks.

Specific Comments:

2) In Figure 5, can the authors infer from the detailed snow pit data whether the underestimated SWE is caused by excessive sublimation or melt in the model or underestimated precipitation? Are the latent heat flux measurements of any help here at two sites?

Thank you for pointing that out. We will add a new section 4.2 entitled "Performance of CLASS" to our manuscript, where we will discuss some possible reasons explaining the underestimation of SWE at the sites of interest. As mentioned, the latent heat flux overestimation by CLASS (Alves et al., 2020), precipitation bias due to distance between our sites and the precipitation gauge (≈4 km) and deficiency of the single-layer snow layer scheme used in CLASS will be discussed as additional probable reasons of underestimation.

3) In Figure 6, the density of the top layer may be underestimated if the density of fresh falling snow is underestimated, or if densified snow falling from the canopy is not accounted for. If density is underestimated then CC will be too small even if temperature is correct. Site 1 which has a larger fraction of the canopy buried shows a smaller underestimation of density, suggesting that canopy effects are stronger at Sites 2, 3 and 4.

We fully agree with the reviewer. We will include a discussion related to densified snow falling beneath the canopy.

4) Would Figures 7 and 10 be more informative if small x-y plots were employed, with the r^2 or bias value shown? This would also provide the ability to show nonlinear or curvilinear relationships.

Thank you for this remark. To visualize the results presented in Figure 7 as a scatter plot, one can refer to Figure 6. For Figure 10, we went through the requested exercise and prepared Figure R1. The substantial amount of data for the bottom panel (reconstructed CC) makes the plot somewhat unclear, so we find it best not to include it in the paper.

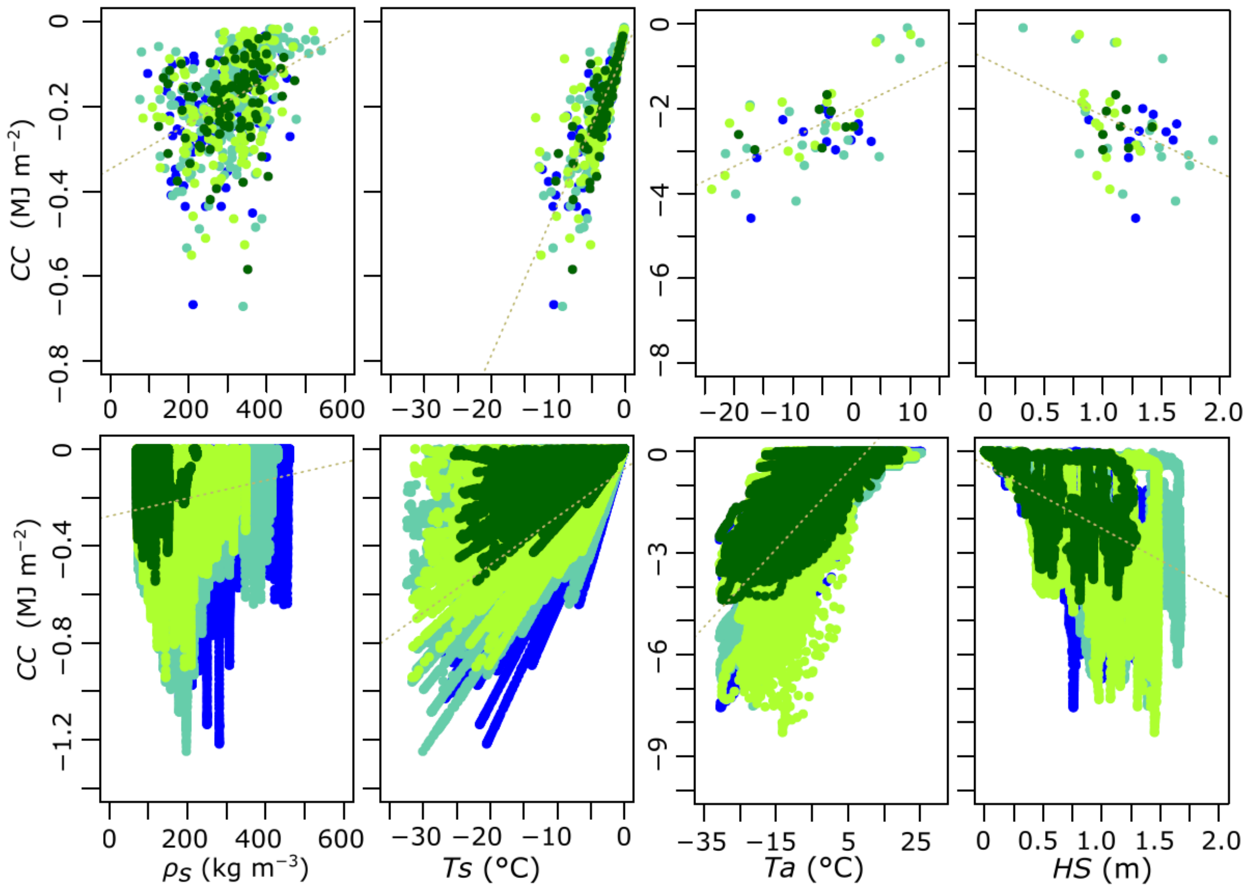

Figure R1: Relationship between *CC* and some pertinent variables, where ρ_s is snow density, *Ts* is snowpack temperature, *Ta* is air temperature and *HS* is snow depth. The top panel applies to snow pit observations and the bottom panel to the reconstructed *CC*.

5) Section 4.2: Site A1 is more exposed to turbulence, solar radiation and longwave losses, and so probably has more rapid temperature changes as weather systems change. I believe the authors are capable of adding more interpretation here. Can they infer snowfall, sublimation, decoupling, and canopy effects? I like the discussion about Site A3 on lines 334-341. I see times in Figure 2 when site A3 has the coldest temperatures. Can the authors relate this to periods with stable conditions and/or low wind speeds to bolster this discussion?

Site A1 is indeed different from the other ones as it lacks a well-defined canopy that acts as a buffer on energy and mass exchanges. Therefore, a more reactive snowpack can be expected. As pointed out by the reviewer, we will attempt to incorporate the effect of snow interception, sublimation, turbulent transport of snow, and snow accumulation and ablation pattern at our sites. In the next version of the manuscript, we include a more detailed interpretation of these processes and their impact on the *CC* at site A1.

Also, regarding cold air pooling at site A3, we will propose a new version of Figure 2 in which periods of low wind speed will be highlighted (see Figure R2 below). Also, we will add some supporting analysis relating the effect of such short-lived events in terms of energy budget perspective.

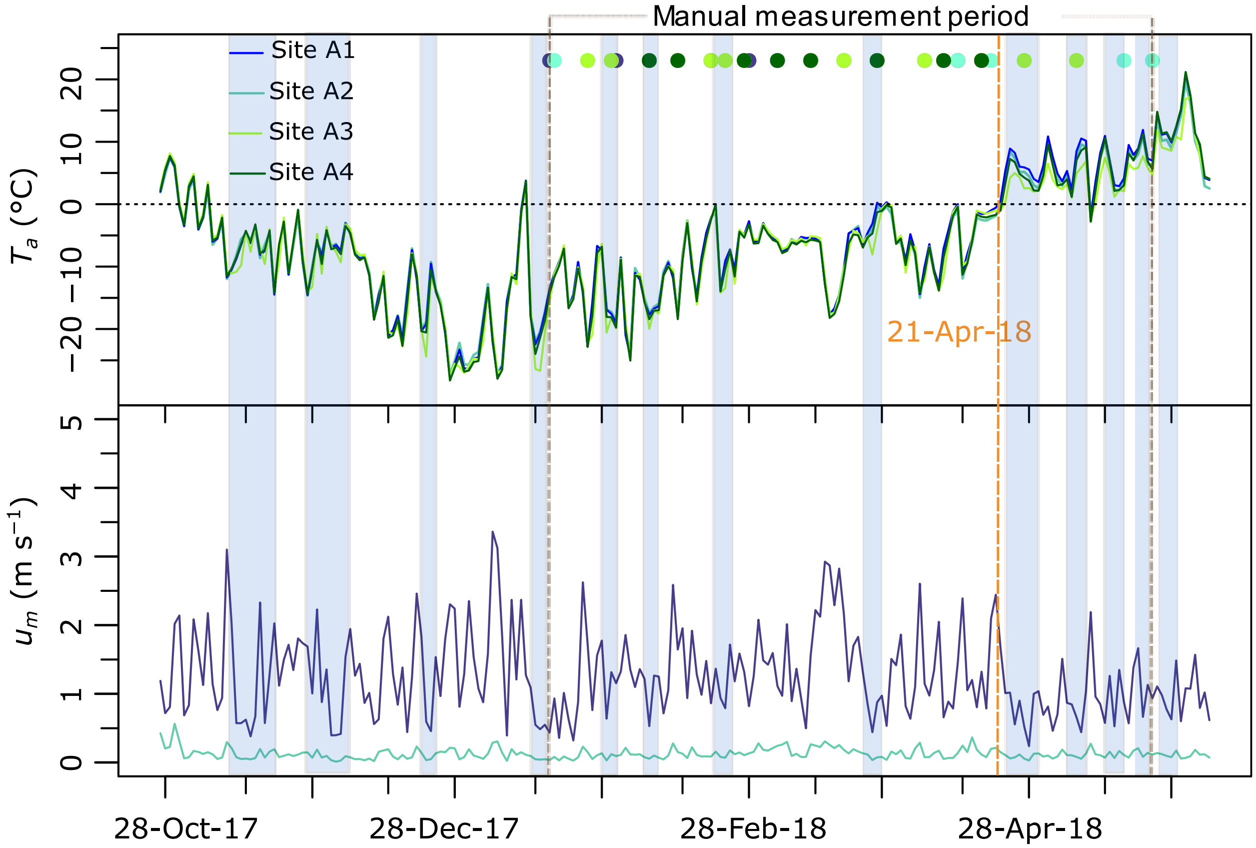

Figure R2. (a) Daily 2-m air temperature (*Ta*) observations for all the study sites. (b) Daily 2-m wind speed (u_m) for sites A1 (sapling) and A2 (juvenile). Shaded regions denote periods of low wind speeds (< 0.8 m s^{-1}). Coloured points illustrate site specific snowpit surveys while period between two grey dots indicates the extensive snowpit measurement period. Spring melt started on 21 April, 2018.

6) Figure 12 and related discussion: Are the authors comparing the formulae for the density of fresh falling snow against the density found in the top 10 cm of the snowpack? I would only consider this a valid comparison if the snow survey were conducted immediately after a snowfall event and before unloading or density changes had taken place.

Sorry for the misunderstanding; we are not comparing the fresh density but deriving the superficial snow density (top 10-cm) resorting to different methods. For the sake of clarity, we have now prepared a new figure that highlights both fresh density and top snow density after metamorphism (Fig. R3). The method implemented here is:

- If there is snowfall, we apply different empirical equations, namely Diamond-Lowry (Russel et al.,2020), Hedstrom-Pomeroy (Hedstrom and Pomeroy 1998), Brun (Brun et al., 1989) and a constant 100 kg m^{-3}, to estimate fresh snow density.
- For every snowfall event, the top snow density is reset to fresh snow density estimate determined by one of the above empirical equations.

- If there is no snowfall, then snow undergoes metamorphism. We utilize Equation 4 to derive snow density after metamorphism.
- This is the top or superficial snow density which includes both fresh (< 24 h since last snowfall) and densified snow.
- As the top (10-cm) layer has no overlying weight, which causes densification, so we do not use Equations 5 and 6.

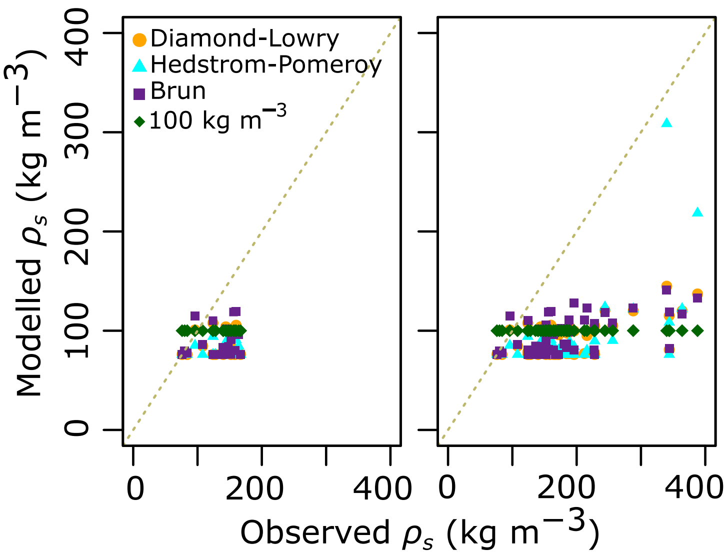

Figure R3: Top 10-cm snow density derived from different empirical methods. a) Observed versus Modelled fresh snow density (< 24 h since last snowfall) a) Observed versus modelled snow density including both fresh and snow density after metamorphism utilizing Equation 4.

Table R1: Efficiency metric for snow density derived from different empirical methods. Here R^2 represents the coefficient of determination and the Pbias (%) denotes the percent bias between modelled and observed snow density.

Models	Fresh snow density		Snow density after metamorphism	
	R^2	Pbias (%)	R^2	Pbias (%)
Diamond-Lowry	0.018	−35.2	0.23	−53.1
Hedstrom-Pomeroy	0.012	−39.6	0.25	−51.6
Brun	0.028	−32.1	0.18	−50.2
100 (kg m^{-3})	5e^{-5}	−24.0	0.11	−48.8

7) Line 364-6: If the snow density estimates were disastrous when employing the Brun model, why did the authors not change to a different model or to fixed values of 100 kg/m^3 for fresh snow?

This is indeed a reasonable estimate for the density of fresh snow density (Fig. R3a), but not in the presence of metamorphism (Fig. R3b).

Minor points and Corrections:

8) Line 50: I would phrase this as "plays a central role in the timing of snowmelt". "Delaying" makes it seem like the snow is not melting at the correct time.

We will rephrase it, thank you.

9) Line 59: I would replace “resorted to” with “employed”. “Resorted to” implies that snow pits are not a good method. They are labour intensive, but can provide much information.

We agree with the reviewer and will correct it.

10) Line 62-64: Is “Of note, a slight contrast was observed by Seligman et al. (2014), who reported that the contribution of spring snow storms to CC had a smaller impact on delaying snowmelt than the porous space from dry fresh snow” intended to mean, that in spring storms the snow is near 0°C and so adds the minimum possible heat content based on its mass, whereas the pore space in cold dry low density snow results in a low thermal conductivity which delays snowmelt more than the cold content of the warm spring snow? If so, a few extra words would make that more clear.

The reviewer understood the meaning of the sentence, but we will indeed add a sentence to clarify your concern.

11) Line 64-65: “However, Jennings et al. (2018) reported shifts in the onset of snowmelt by 5.7 h and 6.7 h at alpine and subalpine sites, respectively, when CC at 6:00AM was less than 0 MJ m−2.” Shifts from when to when, caused by what? By definition, CC must be ≤ 0 MJ m−2 but surely the amount would affect the timing of snowmelt.

The reviewer has a good point, and we will rephrase this sentence as:

“ Nonetheless, Jennings et al. (2018) tested the role of *CC* on delaying the snowmelt and reported shifts in the onset of snowmelt by 2.3 h and 2.8 h at alpine and subalpine sites, respectively, after the removal of CC at 6:00AM . However, when the *CC* at 6:00 AM was less than 0 MJ m^{-2}, there was a delay of snowmelt onset by 5.7 h at the alpine and 6.7 h at the sub-alpine site, respectively.”

12) Line 81: I would delete “our” unless the field observations were provided to a different group.

We will delete it, thanks.

13) Line 83: I would change “resort to” to “employ” or “use”.

Thanks, we will rectify it.

14) Line 91: I would change “forested” to “forest”.

We will modify it, thanks.

15) Line 93: I would change “was” to “were”.

We will change “was” to “were”.

16) Line 136: I would change “enabling the stability of the prognostic modelled variables” to “ensuring an uninterrupted time series of the prognostic variables”.

We will rephrase the sentence relying on reviewer’s suggestion.

17) Line 152: Change “weighting” to “weighing”.

Thanks, we will rectify this.

18) Line 152-3: I would acknowledge that there can be significant differences in snowfall for some snowfall events over a 4 km distance. It is probably not a consistent spatial bias, however, so the methodology is acceptable as long as there is such an acknowledgement.

We agree with the reviewer and will add some sentences to acknowledge the bias present in the precipitation input.

19) Figure 2: Could site A1 be a brighter blue to aid in seeing it as distinct from site A4? The dots are otherwise fine as long as none are completely hidden. If any are completely hidden, perhaps some can have no fill or have the sizes made smaller.

Thank you for relevant suggestion, we will change the color of the plot throughout the manuscript.

20) Figure 11: I am not sure what the light blue shading in the caption is referring to.

Thanks, we will remove the light blue shading.

Reference

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