Author Response for *"Impact of measured and simulated tundra snowpack properties on heat transfer"*, Victoria R. Dutch et al.

We would like to thank the editor and both reviewers for taking the time to read and comment on the original manuscript. We provide a response to these comments below. For ease, comments are in black and our responses in blue. All minor grammatical fixes listed under the edits subheading have been changed.

Reviewer 1:

General comments:

Thermal conductivity of snow is critical and still very challenging to access. Field measurements have been proven to be satisfactory to a certain level but the inclusion of theoretical equation (or assumption) into models is not yet actual. While most of the main parameters have been discussed and tested, and appropriate literature cited, in order to explain the differences between measured and modeled snow KT, the hardness of the snow pack has never been mentioned in the work, neither its permeability. For sure hardness is kind of included if we think of the measurement using micropen, but as demonstrated in the work, such measurements had to be tuned to the Arctic snow pack as originally validated for Alpine snow packs. Vapour kinetics transfer are necessary to form depth hoar, but the original structure of the snow on ground will also affects such transfers.

Thank you for recognizing the importance of properly considering snow physical properties in order to advance process understanding and model parameterizaton of snow thermal conductivity. A novel component to our analysis is the use of SMP measurements to determine high vertical resolution tundra snow density profiles, hence a new approach to the consideration of 'snow hardness' is central to our analysis. While the SMP measurements clearly show the importance of the basal depth hoar layer in influencing the thermal conductivity, this layer is not well captured by land surface models, which is no doubt a key driver of the offset in thermal conductivity values that we illustrate in Figure 6.

The depth proportion of each different layer is indicated on fig 2 but not discussed, as for example to presence of indurated depth hoar in 2019 and not in 2018. Both seasons had some warming events, which seems to not have affected the soil temperature, but what about the snow pack and the formation of internal, even small, ice layers?

A description of the snowpack layering differences is provided in Section 3.2. We have added the following sentence:

"Ice lenses were present in March 2018, but not the 2019 campaigns"

Are there any relation between the wind (speed threshold during storms?), the amount of rain/positive degree days and the errors in term of soil temperature? It is also surprising that no wind data are presented in the climatology...

Figure 7a clearly shows that during the summer period, when temperatures are above zero, and there are PDDs (positive degree days), that the error between modelled and observed soil temperatures is insignificant.
The error in soil temperature during the winter season cannot be correlated with the amount of PDDs during the winter – because there are no PDDs during the period where the soil temperature regime is established (Fig. 1a). It would also not be sensible to attempt to relate summer PDD total

to soil temperature model/observation bias because we'd be doing so with only two datapoints from two snow seasons.
The magnitude of the simulated errors in soil temperature are dictated by the timing, rate, and vertical snowpack structure during the period of snow accumulation. It is possible that rainfall onto the snowpack will alter the properties of the snow itself, but there were no significant rain on snow events during the two winter seasons in this study. Windspeed is important because wind slab properties will influence the thermal conductivity of the snow. However, we lacked reliable wind speed measurements during the winter and so could not consider the role of wind speed on the wind slab properties.

Snow depth has been shown in the work to be the dominant factor for applying the correcting factor, but if one think of snow being deposited as a single homogeneous layer, then only the physical snow parameters are of interest. The amount of snow layer and their internal properties would be then the controlling factor, for example an very hard and dense snow layer or thick ice layer with a low permeability likely to enhance the formation of depth hoar. Such phenomena are likely to be more present in the future as it is expected more extremes events in the Arctic. I do not think it is necessary to change a lot of the work done and am sure all the data are available in the runs done.

A discussion around that topic is to me of interest in order to pave the future work into such topics and go further into the effects of snow heat properties on the soil's ones, as it is unlikely in the coming years we would be able to develop, and run, a full model with a proper physical scheme for snow thermal conductivity, without considering the validation. It is then today only by using such work presented in this paper that we could be able to better understand the soil-snow-atmosphere feedback in the Arctic.

Thank you for your supportive comments! We agree that this is an important area of research.

Minor comments:

Line 117: am not sure a value of 2 for k is what was intended to write

Yes, 2 is the intended value. Now written as 2.0 to make this more obvious.

Figures 8 and 9 appear before 5, 6, and 7

Figure 8 was referenced before figure 7 in the original manuscript, so these have now been renumbered. All other figures are now cross-referenced in order.

Figure 3, November appears before March from left to right

From left to right, months appear in chronological order.

Reviewer 2:

General comments:

the CLM snow module seems from scratch unappropriate to simulate the properties of the Arctic snowpack. This finding is clearly not new ; previous studies, notably Barrere et al., 2017, and others well cited by the authors, have highlighted the deficiences of this kind of models in the context of Arctic snow.
Agreed. This is the motivation for the study. CLM is a widely-used global model (including in the study of Natali et al. (2019) on winter season carbon flux).We address the representation of thermal conductivity in order to improve simulation of soil temperatures, due to the potential implication

erroneous soil temperature simulations may have on estimates of Arctic carbon cycling, rather than solely thinking about how poorly it simulates the structure of Arctic snowpacks.

In the present version of the manuscript, the description of the snow module of CLM is so light, that it is hard to capture the key features and fonctionning of the model. Typically, are the modifications Van Kampenhout et al., 2017, used in the present study ? It is not clear to the reader. As the CLM snow model is at the core of thisstudy, a more enhanced description of the fonctionning of this model is required in the Methods of the paper : presently the line "Each layer is parameterised using layer thickness, temperature and mass of water and ice, as per Anderson (1976), Dai and Zeng (1997) and Jordan (1991)." (L37 p4) is much too vague as each of these publications contain different variants of constitutive equations. In the present form, it is not possible for the reader to understand which one is used for which process/variable. The constitutive equations for the evolution of layer thickness (hence compaction) and ice/water content or density should be explicited or at least explicitely referenced.

We have substantially enhanced the description of the CLM snow module , which now reads as follows:

"Developments between CLM4.5 and CLM5.0, as outlined in Van Kampenhout et al. (2017) improved the snow scheme in CLM. The version of the model used herein produces a computationally-layered snowpack, with the number of snow layers dependant on the snowpack depth, up to a theoretical maximum of 12 layers (as opposed to the 5 layer maximum in previous versions of CLM). Once the total snow depth exceeds a given threshold, the initial snow layer is subdivided into two layers with equal properties. Snow layer formation continues in this manner as layer thicknesses surpass the prescribed ranges given in Jordan (1991). When a layer divides, the new layer is formed beneath it, rather than new layers being formed at the surface by new snowfall. As this process is not stratigraphically representative, layers are not described by snow type (for example, as per Fierz et al. (2009)), but instead numbered from the snow surface down. Layer thicknesses are also influenced by snow compaction, parameterised followingAnderson (1976). Unsaturated layers may compact due to overburden pressure, the breakdown of new snow crystals or melting, with the thickness of a snow layer a function of the snow thickness at the previous timestep and the rate of compaction. Snow depths below 1 cm are not discretely modelled and are instead combined into the surface soil layer.

Density, thickness and thermal conductivity are output as a daily mean for each layer. CLM calculates snow density as a function of the relative proportions of ice (mass of ice = m_i) and liquid water (mass of liquid water = m_{lw}), weighted by the snow cover fraction (F_{sno}) for each grid cell (Lawrence et al., 2018):

$$\rho = \frac{m_i + m_{lw}}{F_{sno} \times h_{sl}} \qquad (1)$$

In practice, due to the adjusted snow cover fraction and as liquid water in the snowpack is zero until the start of melt out, the computed snow layer density simplifies to the mass of ice (m_i) divided by the height of the snow layer (h_{sl}). Fresh snow density is influenced by both temperature and wind, with the density of the snowpack allowed to evolve as a result of the compaction processes outlined above. CLM does not allow for temperature-gradient metamorphism, and thus does not represent the development of depth hoar layers (Van Kampenhout et al., 2017).

The computed snow layer densities are then used to calculate snow layer effective thermal conductivities (K_{eff}), as per Jordan (1991):

$$K_{eff} = K_{air} + \left(((7.75 \times 10^{-5} \times \rho) + (1.105 \times 10^{-6} \times \rho^2))\,(K_{ice} - K_{air})\right) \qquad (2)$$

Values for K_{ice} and K_{air}, the thermal conductivities of ice and interstadial air, are given in Lawrence et al. (2018). Snow (and soil) temperatures are defined for the midpoint of each layer at an hourly resolution, with the soil column consisting of 25 layers of increasing thickness (down to a depth of 49 m). Despite the simplicity of the snowpack scheme included in CLM, previous evaluation of snow heat transfer in CLM4.0 (Slater et al., 2017) suggests this modelling framework should perform well."

CLM relies on the parametrization of snow thermal conductivity by Jordan, 1991, as recalled in eq 2. However, the authors chose to derive their conductivity profiles from observations using the Calonne and the Sturm parameterization, and not the one by Jordan, which would have allowed a much more direct comparison to CLM results (eg Fig 6). How do you justify this choice, how does the Jordan, 1991 parameterization compare to the others ? Is the effective thermal conductivity mentionned by Jordan et al., 1991 (p18) and accounting for heat transport through conduction and vapor diffusion, effectively used in CLM (and not just the k_snow) ? This should be explicited.

This is an excellent point. As suggested, we have now calculated thermal conductivity from the SMP using the Jordan approximation. The results of the Jordan parameterisation are very similar to those from that of Calonne (2011). Sections 3.2 & 3.3, and Figures 4 and 6a have now been updated to include this.

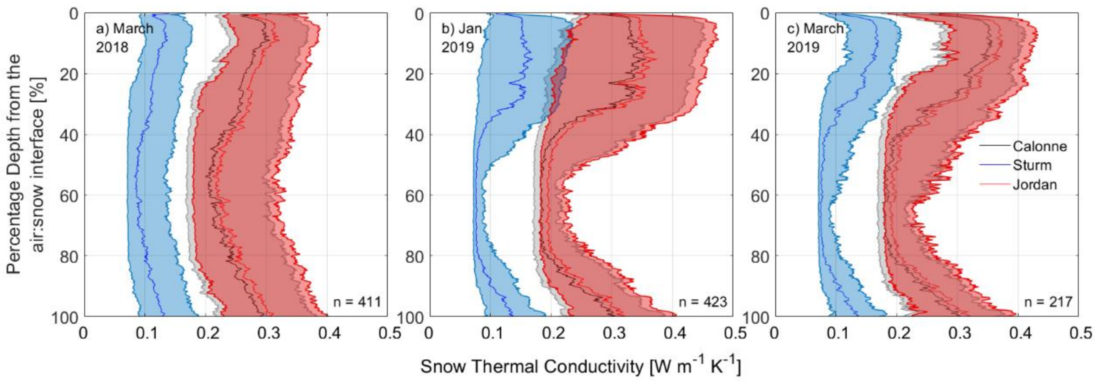


Figure 4: Median thermal conductivity profiles (lines) and interquartile range (shaded areas) approximated from SMP densities, using the parameterisations of Calonne et al. (2011) in black/grey, Jordan (1991) in red, and Sturm et al. (1997) in blue.

The following is more a minor comment : please also beware of the use of the term "effective conductivity". Following Calonne et al., 2011 and in a material science perspective, effective conductivity refers to the conductivity of the ice-airliquid water mixture that constitutes the snow material, while individual components like ice or air, have just a thermal condictivity. In other studies, and often in land-surface modelling as in Jordan et al., 1991, "effective conductivity" refers to the additional inclusion of latent-heat exchange within the conductivity used in the Fourier heat transfer equation. So the use of Keff L149 p4 is appropiate, while its uses L151 p4 are not.

The word "effective" has been removed from what was L151.

As a general question, is there any perspective to generalize the bias correction factor designed here, and have in the future global CLM runs using this usefull correction ? This would strongly enhance the impact of the work carried out in this study.

This is a great question. We are currently working on this to assess near-surface soil temperatures and their implication on soil/microbial respiration fluxes in CLM. Running CLM across a larger number of sites will help in understanding how to generalise correcting snow conductivity. We have also included a paragraph on determining where it is appropriate to make such corrections:

"However, these approaches to improving snow thermal conductivity may not be globally appropriate, especially in climates where temperature gradients through snow are insufficient to create major depth hoar layers and where compaction is the dominant process controlling vertical profiles of snow density. The Sturm et al. (1997) parameterisation demonstrates transferability between tundra sites, having been derived from thermal conductivity measurements on the North Slope of Alaska and successfully applied to both CLM and SMP measurements at TVC. Regardless of approach, the empirical correction factor α or the Sturm et al. (1997) thermal conductivity parameterisation, these changes to the model are most applicable where snowpack structure is considerably influenced by depth hoar, as can be approximated by grid-cell plant functional type or climatology (Royer et al., 2021a; Sturm and Liston, 2021)."

Minor comments:

P3 L110-113 : the way it is formulated gives the impression that the snowpack always entailed the 4 disctinct layers mentionned. I imagined that in practise, more complex layerings were sometimes encountered. Maybe just reformulating saying "Stratigraphic information profiled in each snowpit (n = 115) was used to assign layer types to the measured densities (Fierz et al., 2009), among four different layer types : surface snow, wind slab, indurated hoar and depth hoar. This was made to assess spatial variability in the thickness and properties of different snowpack layers."

Sentence now reads "Stratigraphic information profiled in each snowpit (n = 115) was used to assign one of four different layer types (surface snow, wind slab, indurated hoar and depth hoar) to the measured densities (Fierz et al., 2009) in order to assess spatial variability in the thickness and properties of different snowpack layers."

P3 L112-119: It is hard to understand the nature and impact of these adjustement without being quite familiar with CLM. I would suggest, if this PTCLM version is not published/referenced anywhere else, to just mention "adjustements" here and maybe detail them a bit more explicitely in an Appendix, so that the curious reader may dig full, explicit information from there.

Additional information has now been added, and the paragraph now reads as follows:

"The Community Land Model v5.0 (CLM; Lawrence et al. (2019)) is the land surface component of the Community Earth System Model v2.0, which can be run at a variety of spatial scales. In this study, 1D "point mode" (a 0.1° x 0.1° grid cell) CLM (PTCLM; Kluzek (2013)) simulations were centred at the location of the TVC station. Minor adjustments were made to the model in order to better emulate snow accumulation and melt at the point scale; the snow accumulation factor was increased (Swenson and Lawrence, 2012) from 0.1 to 2.0 and the standard deviation of elevation set to 0.5 m after Malle et al. (2021; Figure S4). These adjustments limit the period of fractional snow cover, so that PTCLM represents a binary state of snow presence or absence over a flat surface. PTCLM simulations were run from August 2017 to August 2019, with model spin-up from January 2013. Spin-up was necessary in order to allow soil temperatures to equilibrate. Variation between model runs with the same parameterisation after more than 2 full years of spin-up is limited to ~ 1 °C throughout the top 5 m of the soil column. The impact of spin-up on soil temperature is further discussed in Appendix B"

P4 L126 : was a debiaising of ERA5 or adaptation to the local conditions maybe required?(see : https://doi.org/10.5194/tc-2021-255)

The following information has been added;
"Gapfilling was only required for measurements of incoming longwave and shortwave radiation, and comparison of observations and reanalysis data showed an offset of less than 60W m^{-2}. Bias correction of reanalysis data was not undertaken due the small size of this offset."

P5 L 171 : "slower rate of soil freezing" : a delay is visible, but not so clearly a slower rate. Could you justify this more with the observations ?

The words "rate of" have been removed from this sentence.

P5 L176-178 : "anomalously warm mid-winter air temperatures... had little influence on the soil temperature profile" : the March 2019 warming is visible though on the soil temperatures, isn'it ?

Sentence was meant to imply only a limited impact, but has now been rewritten and quantified to make this clearer. Now reads: "Anomalously warm mid-winter air temperatures that approached 0 °C (22 December 2017 and 9 February 2019) or exceeded 0 °C (18 and 31 March 2019, with a rain-on-snow event occurring on the latter of these dates) had a muted influence on the soil temperature profile (Fig. 1d). For example, as air temperature increased by 25 °C between 14 and 31 March 2019, 20 cm soil temperatures increased by 2.5 °C, as air and soil temperatures were decoupled due to snow insulation."

P6 L 211: "Temperature-gradient" metamosphism should be specified here (as wet-snow metamorphism would have different consequences !).

Added. Sentence now reads "The density of this layer became more variable as each winter progressed due to the competing processes of wind compaction (increasing density) and temperature-gradient metamorphism (decreasing density)."

P6, paragraph 3.2 : I think that a line on the increase in density and effective thermal conductivity towards the bottom of the snowpack should be added, to complete the description of the profiles. I noted that possible reasons for this are given in the Discussion, and it could be referred to here.

Description of the density profiles in the results section is now as follows:
"Snowpacks in all three campaigns (Fig. 2b-d) had a very thin surface snow layer (composed of recent snowfall), with low near-surface snow densities (< 300 kg m^{-3}) rapidly increasing in the top 5 % of the snowpack. A higher density (~ 320 kg m^{3}) wind slab layer was evident between 5 - 30 % of normalised depth from the snow surface. The next ~ 10 % of the profile was a transitional section

where density decreased by about 100 kg m^3. The lowest ~ 60 % of the profiles is dominated by a lower density (~ 230 kg m^3) depth hoar layer, the density of which increases slightly towards the base of the snowpack"

P6 L237 : "Whilst the absolute number of simulated snow layers is plausible," : these layers have no physical meaning in the model, this is well explained in the methods. Therefore the remark is in my opinion unappropriate, or it should be specified w/r to what this is plausible.

Good point. Removed. This sentence now reads: "The physical properties of the simulated snow layers do not correspond to observations, with the number and thickness of snow layers only a function of overall snowpack depth."

P7 L250: " with the median thermal conductivity using the Calonne approximation still notably lower". The difference between simulations (0.3 Wm-1 K-1) and the median thermal conductivity using the Calonne approximation (0.25 Wm-1 K-1) is not so high, given the uncertainties attached to thermal conductivity estimations, and their range of variations. I suggest to suppress "notably".
The word "notably" has been removed.

P7 L 264 : what is the "maximum duration of simulated snow cover" ? (the period with continuous snow cover on the model ?)

This has now been edited to read "the maximum annual duration of continuous simulated snow cover (15 Sept – 31 May; RMSE = 5.8 °C)."

P7 L 266-272: wouldn't it be easier to say that density was multiplied by a corrective factor α prior to the calculation of Keff ?
Snow density and thermal conductivity are calculated within different modules within CLM. In order to conserve mass and prevent model instabilities, the multiplier was only included in the module where Keff is calculated – applying the multiplier to the simulated density for the calculation of Keff did not result in a change in snow density at this or any following timesteps.

P7 L 274 : " between the interquartile range of observed values shown in Table 2" : for a given snow type of for all ? If for all, the range should be specified as it is not in Table 2 if I am not mistaken.
The range of values used spanned the interquartile ranges of all snow types. Changed to "between the interquartile range of observed values for all snow types (73 – 365 kg m^3)."

P7 L 284-286 : It is really not clear to me how this linear regression was performed, could you give more details ? (also regarding the temporal sampling used)
More detail about the temporal sampling has been added. Sentence now reads:
"A multiple linear regression was undertaken to quantify the influence of snow depth and snow depth error on the value of the best fit correction factor, for the period from snow onset to the start of simulated snow melt-out (when the simulated snow cover fraction was equal to one)."

P9 L337 and 361 : "vital" seems a bit strong and out of place in this context
The word vital has been removed.

P9 L 346-347: maybe also mention here that the insulating effect of snow somehow saturates after a certain snow height has been reached, impliying a stronger sensitivity of soil thermal regime to snow depth in the early winter close to snow onset, when the snow cover is very thin.
The following sentence has been added to the end of the section:

“The soil thermal regime is also more sensitive to snow depth at the start of the season as snow depths are lower and have not yet reached a point where their insulative capacity has become saturated (Zhang, 2005; Lawrence and Slater, 2009; Slater et al., 2017).”

P9 L356: Comparing this point simulation with adjusted Keff, to CMIP5 models evaluated globally (if I am not mistaken..) is actually very unfair to the CMIP5 models ! The string differences in setup should be mentionned to balance this statement.

The following edit has been made to balance the comparison to the CMIP5 models:
“Ignoring interannual variability and calculating one SHTM value (SHTM = 0.733) for the entire model run appearing to show better model performance than 8 of the 13 CMIP models compared in Slater et al. (2017) and suggesting a more accurate simulation of soil temperatures from the baseline model run than seen in Fig. 7a. However, we note the role of different site conditions and model configurations on intercomparisons, a important caveat when comparing our single point simulation to the global CMIP5 simulations.”

P9 L354-358: in relation to one of my major comments : did you tests other parameterizations than Jordan, 1991, for Keff in CLM, for instance the Sturm ? (I do not know how it positions w/r to the Jordan, but this should be mentionned if likely to also yield improvements).
We have now re-run the model with the Sturm parameterisation of thermal conductivity, as well as the default (Jordan) parameterisation. We have added the following paragraph to Section 3.4;

“Additionally, simulations were also undertaken where the default parameterisation for snow thermal conductivity (Eq. 1; Jordan (1991)) was substituted for that of Sturm et al. (1997):

$$\begin{aligned} \rho \leq 156 \quad & K_{eff} = 0.023 + 0.234\rho \\ \rho > 156 \quad & K_{eff} = 0.138 - 1.01\rho + 3.233\rho^2 \end{aligned} \tag{5}$$

The Sturm parameterisation resulted in lower simulated thermal conductivities (Fig. 6b) and closer temperatures to observations (Fig. 7b; RMSE = 2.5°C). Soil temperatures in 2017-18 were still too cold regardless of parameterisation used, likely due to model underestimation of snow depth (Fig 7c).”

We have also added an additional paragraph to Section 4.3 discussing the implications of these results:

“Changing the parameterisation of snow thermal conductivity in CLM from that of Jordan (1991) to that of Sturm et al. (1997) improves the simulation of both snow thermal conductivity values and underlying soil temperatures. Similar issues in snowpack and soil temperature simulations have been found for simulations of Arctic snowpacks using other models, i.e., Crocus, SNOWPACK, ISBA-ES (Barrere et al., 2017; Domine et al., 2016; 2019; Royer et al., 2021b). Use of the Sturm et al. (1997) thermal conductivity parameterisation also improved soil temperature simulation in Crocus (Royer et al., 2021b), with a RMSE of 2.5 °C for soil temperatures from Crocus and CLM. However, these approaches to improving snow thermal conductivity may not be globally appropriate, especially in climates where temperature gradients within the snowpack are insufficient to create major depth hoar layers and where compaction is the dominant process controlling vertical profiles of snow density. The Sturm et al. (1997) parameterisation demonstrates transferability between tundra sites, having been derived from thermal conductivity measurements on the North Slope of Alaska and successfully applied to both CLM and SMP measurements at TVC. Regardless of approach, the empirical correction factor α or the Sturm et al. (1997) thermal conductivity parameterisation, these changes to the model are most applicable where snowpack structure is considerably influenced by depth hoar, as can be approximated by grid-cell plant functional type or climatology (Royer et al., 2021a; Sturm and Liston, 2021)."

This sentence has been added to the final paragraph of the conclusion (Section 5):

Alternatively, different parameterisations of snow thermal conductivity also improve simulation of soil temperatures, with that of Sturm et al. (1997) more appropriate for Arctic snowpacks than that of Jordan (1991) used by default in CLM.

And we have also added the following to the abstract:

"The use of an alternative parameterisation of snow thermal conductivity also improved simulations of wintertime soil temperatures (RMSE = 2.5 °C)."

P9 L363-365: "Larger values of the correction factor are needed to replicate observed soil temperatures later in the winter season, as errors in simulating earlier season snow depth are additive, leading to larger discrepancies for both snow depth and soil temperatures" : Could you specify to which specific Figure or result statement this assessment relate ?

A reference to Figure 7 has been added to this sentence.

P9 L367 : "This bias compensation between underestimates of snow depth and overestimates of snow thermal conductivity": I had rather say bias compensation between underestimates of snow depth and UNDERestimates of snow thermal conductivity (?) please correct me if I am wrong.

Corrected.

P10 L 380 : " Reducing simulated thermal conductivity by 80 % (α = 0.3) produces changes in soil temperatures approximately equivalent to the impact of changing depth hoar fraction from 0 to 60 % (Zhang et al., 1996), suggesting the inclusion of vapour transport in the snowpack is at least equally important as values of snow thermal conductivity in accurately simulating wintertime soil temperatures. ". The sentence is ambiguous and the message hard to understand (not sureI understood properly). The inclusion of vapour transport in the snowpack will change the thermal conductivity of the snowpack by two ways : i) because this will form depth hoar with lower Keff ; ii) because of vapour transprt induced heat transport. Anycase the inclusion of vapour transport in the snowpack, means different values for snow thermal conductivity. I think the sentence should be rephrased.

This sentence has been removed.

P9 L381-384: Honestly, the explicit inclusion of vapour transport within the snowpack in the snow modules of land surface models is a (very) long way from now. I am unaware of very concrete plans in that direction for CLM, but please correct me if they exist. I am much more confident that physically representative approaches, (like Royer et al., 2021), though not physically explicit, will be first used, and this would be the short to mid term perspective.

To my knowledge, no such plans for CLM are currently in the works. We have edited this section to refer to such approaches.
"Further improvements in SHTM in future iterations of CLM will require a physically representative approach to snow density and thermal conductivity through explicit inclusion of vapour transport within the snowpack, currently under development in stand-alone snow microphysical models (Fourteau et al., 2021; Jafari et al., 2020; Schürholt et al., 2021). However, this presents computational and mathematical challenges, as outlined in Jafari et al. (2020). The inclusion of physically representative parameterisations of snow properties in land surface models, such as that of Royer et al. (2021b) where the densities of lower snow layers are not allowed to exceed a maximum observation-based threshold, are more likely in the near future than the explicit representation of snowpack vapour transport. Meanwhile, both the empirically derived scaling factor or the substitution of the Sturm et al. (1997) thermal conductivity parameterisation provide a computationally efficient compromise, reducing both the value of K_{eff} and the cold bias of simulated wintertime soil temperatures considerably (RMSE reduction of 3.7 °C for α = 0.45, RMSE reduction of 3.3 °C for the Sturm parameterisation)."

P11 L448: Figure A2c does not explicitely show that the relationship between FÌ and L is not heteroscedastic; it is rather the combination of all figures from this panel (?)
This sentence now refers to Figure A2, rather than just the last panel specifically.

Fig 1 is hard to read, maybe consider having a grid (making date correspondances easier to follow), use backgroun color instead of the color of very small histograms for precipitation phase ; maybe also enhance y-axes sizes and line width. Plus, the caption contains some errors, please check.

Figure 1 has been updated as below, white space between plots has been removed and the line widths increased. Caption has also been revised slightly.

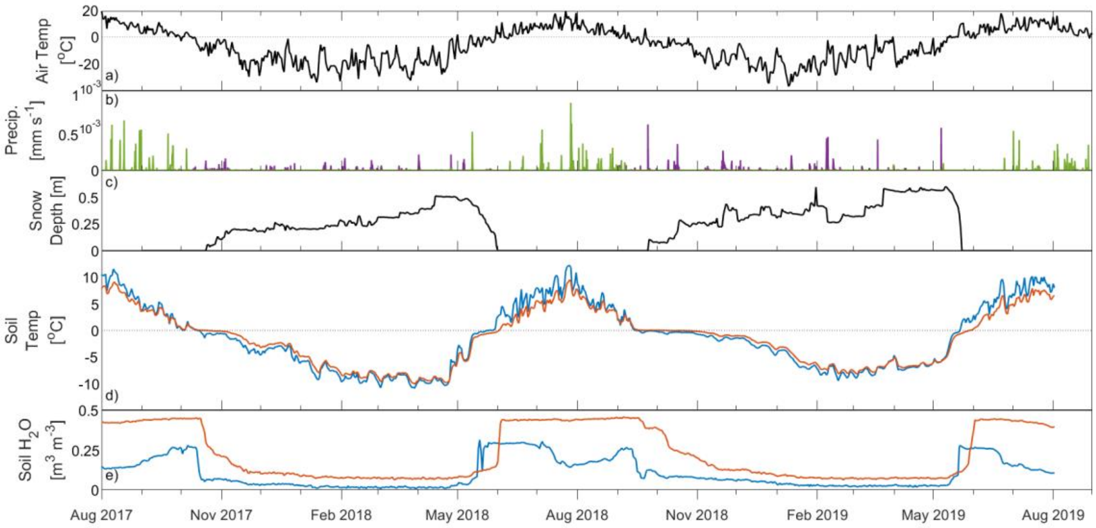
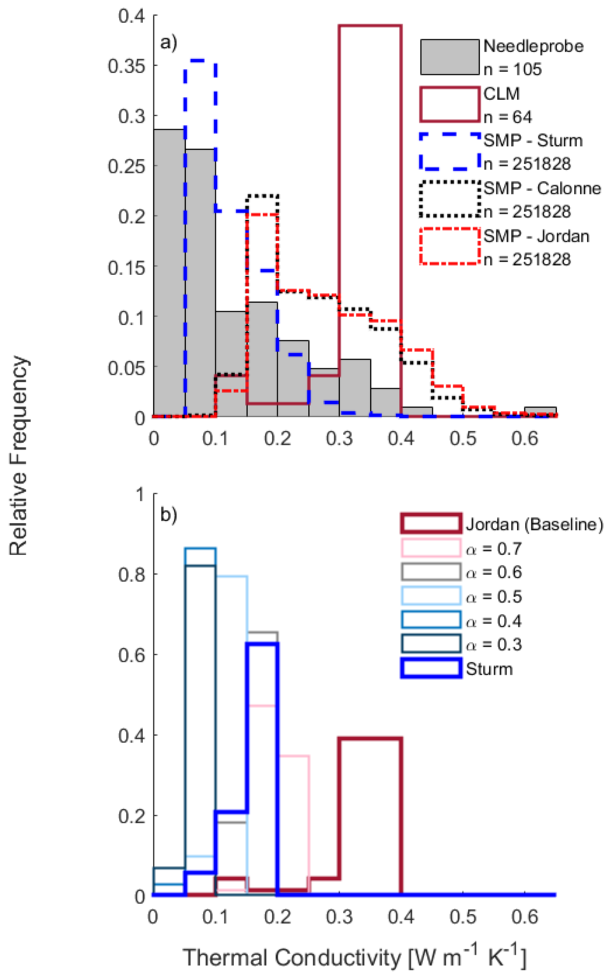


Figure 1: Meteorological and soil conditions at Trail Valley Creek from 1 August 2017 to 31 August 2019; a) 2 m air temperature, b) precipitation: snow (purple) and rain (green), c) snow depth, d) soil temperatures at depth of 5 cm (blue) and 20 cm (orange) and e) volumetric soil water content at 5 cm (blue) and 20 cm (orange) depths.

Fig 6 : nice figure !

Thanks! The figure has been updated to include SMP-derived Keff from the Jordan parameterisation (Red dashed line, top plot) and the use of the Sturm parameterisation in CLM (Thick blue line, bottom plot):



The caption now reads:

"Figure 6: a) Histograms of measured and simulated thermal conductivity from March 2018 and 2019 sampling campaigns, b) Sensitivity testing of simulated thermal conductivities for the same time period using both the default CLM snow thermal conductivity parameterisation and the Sturm (1997) parameterisation. Note the different scales on the y-axes."

Fig 8 is mentionned in the text before Fig 7, they should be inverted. Could you thicken the lines a little bit (should be feasible when reducing the y range) – the figure is quite hard to read.

Numbering of figures 7 and 8 has been reversed as detailed in response to Reviewer #1 above. An additional panel has been added to Figure 7, as shown below:

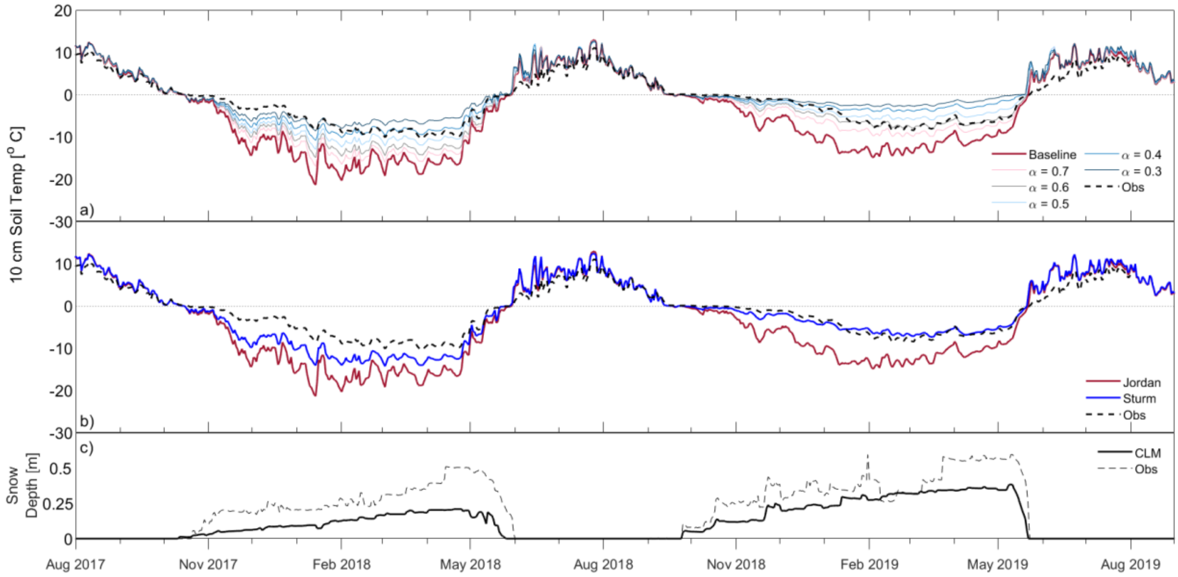


Figure 7: a) Timeseries of 10cm soil temperatures timeseries for K_{eff} sensitivity tests compared to field measurements, b) Timeseries of simulated 10cm soil temperature timeseries using both Jordan (1991) and Sturm et al (1997) parameterisations of snow thermal conductivity,
c) Observed and simulated snow depths for the same time period.

The line weight on the new Figure 8 has been increased, as shown below:

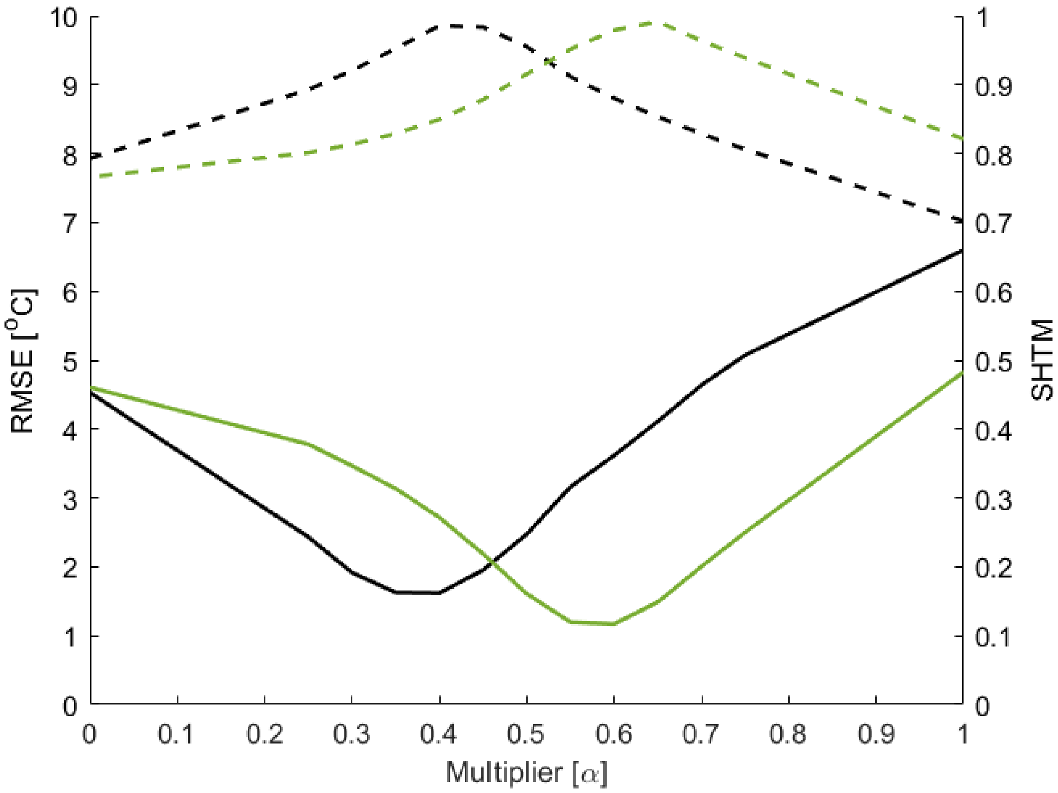


Figure 8: Changes to model performance when adjusting snow thermal conductivity, measured using RMSE of 10cm soil temperatures (solid lines) and the Snow Heat Transfer Metric (SHTM – dashed lines) for the 2017-18 (black) and 2018-19 (green) snow seasons.