tc-2022-19 - Snow properties at the forest tundra ecotone: predominance of water vapor fluxes even in thick moderately cold snowpacks

Responses to Reviewer #1

General Comments:

This paper addressed the snow properties such as snow height, stratigraphy, thermal conductivity, and density through observations at two sites, located in tundra and forest environment, in northern Canada. The observation results showed some contrasted properties between tundra and forest:

1. the higher snow height in the forest whereas lower height in the tundra.
2. lower density at the top of snowpack in the forest whereas homogeneous density profile vertically in the tundra.
3. the warmer temperature at the bottom of the snowpack in the forest than that in the tundra.

The authors furthermore proposed some modifications on parameterizations for snowfall density, viscus compression, and blowing snow implemented in the Crocus model. Then, the authors demonstrated the Crocus simulation with the proposed modifications reproduced better snowpack properties observed in the two sites than the default Crocus simulation. The content of this work is suitable for the scope of The Cryosphere.

Thank you for your comments on our manuscript.

However, at present, there are many problems for the paper to be published in 'The Cryosphere'. My general concerns are mostly as follows.

1. The title is far away from the subject defined at the end of the introduction. The title or the subject defined in the introduction should be revised. Moreover, the conclusion should be written as an answer responding to the subject.

The subject at the end of the introduction will be revised to align the research question with the title:

‘Here, we present data on the internal physical properties of subarctic snowpacks to show that the transport of water vapor is an important process shaping the vertical snow density profile in both tundra and forest dominated areas. Furthermore, we test the performance of the snow model Crocus and explore adjustments to Crocus that compensate the lack of a water vapor transport mechanism in the model.’

2. The authors concluded that a vertical profile of snow density in the Arctic region is formed with a water vapor transport process rather than a viscus compression process. However, the physical logic to reach this conclusion is not enough given in the main text. In this study, the authors performed a numerical experiment using the Crocus model with parameterizations, developed focusing on snowfall, compaction, and blowing snow. Within this experiment, the water vapor transport is not updated.

Sublimation is also ignored. Nevertheless, the authors concluded that the density profile is dominantly formed through the water vapor transport rather than the viscus compression based on a result of numerical experiment and fact that depth hoar is predominantly observed at the bottom of snowpack in the study sites. However, because the numerical simulation is successful using parameterization modified by decreasing overburden at the bottom snowpack layer (due to vegetation), it is, rather, a more straightforward story that the density profile is a result of vegetation. Or, it is also potentially nice that the density profile is formed by increasing viscosity of snow layer where depth hoar is predominantly formed because the decrease in the overburden is practically the same as the increase in viscosity. Anyway, the authors should revise the logical process carefully to reach the conclusion based on the obtained results.

The conclusion that the vertical density profile in the Arctic is formed due to the transport of water vapor is not only based on the numerical experiment we performed. We discussed the observed density profile, thermal conductivity and the stratigraphy. All of those variables suggested that this process is important for shaping the Arctic snow cover (e.g. the abundance of depth hoar crystals at the bottom of the snowpack which arise only from water vapor transport and a density profile with relatively light snow at the bottom of the snowpack which is not expected by a compaction driven density evolution). In section 4.3, we discuss that observations lead to the conclusion that water vapor transport is the dominant process at TUNDRA and at least deemed important at FOREST. The numerical experiment was performed to back up the conclusion from the observations and show that the current processes implemented in the model are not sufficient to reproduce observations. Thus, we test a simple way to improve the model without taking the transport of water vapor explicitly into account as this is complex to implement. We clearly state that our improvements compensate for the lack of this transport (see L. 14 and 390)

Also, please note that sublimation is included in the simulations. We merely stated that we did not use an option to further increase it (L.108 in the manuscript).

3. Topic sentences are often absent or inappropriate. Also, multiple topics are sometimes included in a paragraph. The topic sentence is generally very important for readers to understand the paragraph smoothly. Moreover, because the multiple topics often confuse readers, the paragraph should be constructed with only one simple topic. The paragraphs that needed to be updated are pointed out in specific comments.

Please see our answers to comments on the specific paragraphs below.

4. The scientific originality is not enough or unclear at present. While the authors conducted many diligent in-situ observations for many years, their conclusions seem very similar to the previous works described in the introduction. For example, your result of 'higher/lower snow height in the forest/tundra' seems very similar to your introduction in L21–37. Moreover, the homogeneous vertical profile of density in the tundra is easily expected because a strong wind often makes wind slabs at the top of the snow cover. In the introduction section, the authors need to more clarify what the previous works addressed/found and what the lacking understandings are. Then, a single concise subject of this study should be defined at the end of the section. Moreover, in the discussion section, the new findings obtained in this study should be more emphasized. By the way, the Crocus simulation was interesting for me. As the authors pointed out in the main text, it is basically difficult to apply the current physical snowpack models to the Arctic region. Even with very simple modifications from previous works

(Barrere et al, 2017; Royer et al. 2021b), the model successfully simulated the observed snowpack properties, which should be more emphasized. However, I was a little bit disappointed that many concepts and evidence that were necessary to understand the modifications were omitted in the current manuscript.

The issue of water vapor transport and particularly the question of how to simulate this process is very timely and has been the subject of active research only recently (Fourteau et al. 2021, Jafari et al. 2020, Jafari et al. 2022, Simson et al. 2021). So we think that observations in a region where very few data exist, here the low Arctic tundra and open forest at the forest-tundra ecotone, are a valuable enrichment for the scientific community.
It is probably an exaggeration to mention that a homogeneous density profile is 'easily expected' on the tundra because of winds. Winds also affect snow at the beginning of the snow season. Why then do not they lead also to dense wind slabs? Clearly, there is no objective reason to "easily expect" our observations. In this specific case, it has been shown (Domine et al. 2016) that the densification of the top of the snow cover arises also from water vapor transported upwards and not just by wind packing.
In the introduction we clearly mention studies that have simulated Arctic snow. However, there are no studies about detailed observation of the density and thermal conductivity in the forest-tundra ecotone.
Concerning the research question, please see comment 1.

We are open to include the concepts and evidence required to better present the changes we have introduced, but this would require specifying more clearly what you are referring to.

Specific Comments:

5. L26–27: This seems important motivation for this study. The considerable changes described by Payette et al. (2001) and Ju and Masek (2016) should be introduced in detail.

Thank you, that is an excellent point. We propose the following addition:

"In their study, Ju and Masek (2016) found that 29.4% of the land cover in Alaska and Canada showed greening trends. According to their findings, the greening occurred primarily in the tundra, with Quebec and Labrador being the most affected regions. Considering these significant changes, the extent of the forest-tundra ecotone throughout circumpolar regions and its role in the global climate, more research is essential."

6. L42: What does 'this type of snow' indicate? Please clarify it.

We will substitute 'this type of snow' by 'Arctic snow'.

7. L44–46: This statement is inappropriate because the dominant process controlling the density profile is independent of the circumstances in the model development.

In the model development, the dominant process does matter as one might only include processes important for the setting where the model is deployed. Crocus was developed for avalanche research in the Alps. It only describes processes that are prevalent in alpine snow. In alpine settings, the compaction due to the overburden is far more important than water vapor fluxes. In Crocus these fluxes are omitted without detrimental impact on the performance of the model in this setting. The situation is clearly different in settings where water vapor fluxes is important.

8. L40–52: This paragraph probably contains two topics, so I suggest splitting this paragraph into two: the difficulty for modeling the arctic snowpack by the current default snowpack models and the current attempt to overcome the difficulty. The former can be reconstructed from the content of the current paragraph probably. For the latter, the authors need to describe the contributions by Barrere et al. (2017), Gouttevin et al. (2018), and Royer et al. (2021b) appropriately. Then, the unresolved problem should be given.

It is true that the paragraph could be divided into two paragraphs, but our initial idea was to dedicate one paragraph to observations and one to models. We believe that this is the best way to maintain a logical sequence that channels us towards the objectives of our paper.

9. L46: 'this deficiency' is unclear. Please clarify it.

We will replace ‘this deficiency’ by ‘the lack of water vapor transport’.

10. L46–48: I understand modifications by increasing the maximum density are necessary for modeling the Arctic snowpack. However, this statement is a little bit confusing because the authors say the mismatch of density is due to the lack of consideration of water vapor fluxes in the model at the beginning of this paragraph. Please get the difficulty for the modeling arctic snowpack straight more.

This is a good point. In Arctic snow covers, water vapor is transported from the bottom to the top, effectively increasing the density at the top and decreasing it at the bottom (Domine et al. 2019). As this process is not present in the models, another way needs to be found to increase the density at the top, which was done by increasing the maximum density of wind-induced snow compaction. Thus, the lack of water vapor transport and the increase of the maximum density are directly related as suggested in the text. In other words, our strategy is to use an error compensation scheme rather than directly treat water vapor fluxes, as this would be much more complex as it would require a novel model architecture. This is a common strategy in model development/modification.

11. L52: This statement is confusing. Before this statement, the authors say the density in the forest is mainly controlled by the compaction as well as the alpine snowpack (L37–38). If so, because the models have been well validated on the alpine snow, readers intuitively expect the snowpack models can reproduce the density well even in the arctic forest. If the authors would like to problematize it

here, in my opinion, the authors need to describe reasonably how valuable the test to check the ability of models to adequately simulate density is.

This is an important subject of the paper, to find out whether the model is capable of simulating the snowpack in boreal forest environments. Studies on high Arctic tundra have shown that the model was unable to simulate the snow there (Barrere et al. 2017, Gouttevin et al. 2018). It is therefore not surprising that at TUNDRA, a low Arctic site, the model does not work either. However, at FOREST, the important accumulation makes this site more similar to an alpine site, so there is an interest to know how the snow model Crocus, developed for alpine applications, performs in this environment. As mentioned before we, will revise the end of the introduction where we state more clearly the research question and its importance.

12. L64–66: For this, a topographic map is suitable. Could you add such kind of figure?

A topographic map is included in the paper cited in L. 71, Lackner et al. (2021), in which a more detailed description is available. Furthermore, Figure 1b) gives a good overview of the valley and its topography. Moreover, the vegetation transition is clearly visible, in contrast to a topographic map.

13. L66: '70 to 80 % of the upper valley' is unclear because the spatial region of 'upper valley' is undefined. Within a topographic map (my comment #8), you can depict the region where the upper valley is. Or, 'the upper valley' should be replaced with 'the tundra'.

Indeed, it is difficult to define exactly the region of the upper part or the tundra as the regions and biomes merge into each other. We will add a line indicating the approximate limit of the upper part in Figure 1 in the manuscript.

14. L67: Similar to my comment #9, the region of the 'lower valley' is undefined.

See answer to comment 13.

15. L69: Is '2-3 m' a typo for '2–3 m'?

Yes, thanks, this will be fixed.

16. L86: In my opinion, the error of 29% is large, which potentially affects the conclusion. Despite that, this problem is not well discussed later. The authors should demonstrate that the conclusion would be robust even if the measurement included this magnitude of the error.

Yes, an error of 29% is large. We will include a discussion of the impact of this error at the end of section 4.1:

‘Generally, the error of 29% for the thermal conductivity measurements is rather high, however, in this study, the gradient of the profile is our main focus, reducing the impact of the uncertainty on a single measurement. Moreover, by taking the average of all samples, the uncertainty is reduced according to n^{-1} or a factor of ≈4 with *n* = 21 (TUNDRA) or *n* = 15 (FOREST) samples.’

17. L86–87: What was the time-lapse camera used for?

The time lapse cameras were used to qualitatively observe the transport of snow and to check to which extent the snow poles were covered.

We will include this phrase in the manuscript.

18. L88–89: When did you conduct snow-pit observations eventually? Please add a table describing the dates of observations.

Good idea, we will add a table to the supplementary material containing all dates of the snowpits.

19. L91–91: Please describe the vertical intervals in the measurement of density and temperature.

The spacing varied between the snow pits. We mostly used a 3-cm spacing, but sometimes the spacing was increased to 5 cm. We will mention this in the manuscript:

“The spacing was mostly 3 cm between measurements but was increased to 5 cm for some snow pits, mostly the deepest ones.”

20. L92: 'some snow pits' is unclear. Could you specify how many snow-pits is?

We will specify this in the table requested in comment 18.

21. L103–110: The topic of this paragraph is unclear. Probably the most important prior information for readers is that some parameterizations implemented in the Crocus are modified from the default settings. Moreover, this paragraph contains general reviews about the difficulty of modeling the arctic snowpack, which should be described in the introduction (see also comment #4). Please rewrite this paragraph and reconsider the appropriate topic sentence.

As shown by comment 22, the implementations for blowing snow might be a bit counter-intuitive, thus, we think that a quick reminder that the 1D nature of the model causes problems for simulating blowing snow is appropriate.

22. L104–106: Is this really true? The effect of blowing snow on snowpack is generally separated into erosion and accumulation (, and sublimation sometimes).

In reality, it is true that blowing snow is associated with erosion and accumulation, however, as we stated in the precedent sentence it is difficult to simulate those processes due to the 1D nature of the model. So alternatives were implemented to take into account some of the impacts of blowing snow. We will clarify this by explicitly stating the problem in the precedent sentence:

‘Due to the 1D nature of the model, simulating blowing snow is problematic**, and snow erosion and accumulation cannot be simulated explicitly. Therefore**, two separate processes [...]’

23. L107–108: Could you add a quick description of the parameterization of Gordon et al. (2006) here?

The parametrization of Gordon et al. (2006) is not used in this study, so describing it here in more detail does not contribute to the understanding of the model used for the study.

24. (1): Do you mean that the viscus compression is ignored at the top layer?

No, this compaction is additional. We will clarify this:

[...] the upper snow layers are **additionally** compacted according to the following equation [...]

25. L102: Is 350 kg m-3 a value of the default Crocus?

Yes, it is. We will clarify this:
'(set as 350 kg m^{-3} **by default**)

26. L115–122: This content should be described in the introduction. Moreover, I am wondering why the authors do not focus on the water vapor flux which is emphasized as a key process reproducing density profiles at the Arctic region in the introduction.

This is also described in the introduction (see L 41-43), here we repeat the motivation for implementing the modifications.

We did not focus on the water vapor flux as it is complicated to implement in a model. This is because water vapor transport requires the simultaneous transport and mass and energy which makes it difficult to implement in the current structures of snow models. Also, the main process transporting water vapor from the bottom of the snowpack to its upper layers is convection, but it is not possible to explicitly model convection with phase change in a 1D model. So we tried to compensate the lack of this process by using other, simpler processes.

27. L123–124: This topic sentence is inappropriate for the content of this paragraph. The most important information is that the authors basically selected the parameterization of Vionnet et al. (2012)

as well as Royer et al. (2021b). Then, the description of modifications, specialized for your study site, to increase the density at the top of the snowpack should be given.

We will merge the paragraph beginning at L. 123 with the precedent one as there it is stated that our modifications are based on the parametrization in Vionnet et al. (2012) and Royer et al. (2021b).

28. L123–127: This is verbose. Please make the statements shorter.

We will split the sentence starting with “All three depend on...” in two, thank you.

29. (2): T_fus can be replaced with 273.15.

It could be replaced indeed, but we took the formulation from the original paper Vionnet et al. (2012) to avoid possible confusion when comparing both papers.

30. L130–132: According to the introduction, the hard slab at the top snowpack is induced by the strong wind, not the heavy density of fresh snowfall. Why is this modification appropriate for your study site?

The heavy density of the fresh snow is due to strong winds. Thus, the modifications take into account both, the densification of the snow while the crystals are still airborne (fragmentation of the crystals during saltation) and when they are already deposited on the snowpack. We will add a sentence to clearly state that our parametrization of fresh snow also takes into account the densification after deposition:

‘Note that this parametrization includes densification effects of the wind during periods when the snow crystals are still airborne (fragmentation during saltation) as well as when they are deposited onto the snowpack.’

31. L132–133: Please describe this sensitivity analysis. Moreover, related to my comment #26, the density of the upper snow layer is a result of a fresh snowfall, sublimation, and wind-induced compaction. I am concerned that the selected parameters based on this sensitivity analysis are far away from the appropriate settings for the density of fresh snowfall in your study site.

Thank you. We will further describe the sensitivity analysis:

“These values were obtained with a sensitivity analysis where **we varied c_ρ in order to obtain a good agreement between the simulated and observed densities of the top of the snow cover.”**

As mentioned in comment 30, the process takes into account both densification of fresh snow and when it is deposited on the snow cover. We do not give any recommendation to use the parametrization given here as a general equation to calculate fresh snow density. We just say that for our model this equation gives good results in terms of the initial snow density. Other users might want to change the process of snow densification of deposited snow and in this case clearly a different parametrization for fresh snow needs to be used.

32. L133–136: This statement is for the effect of wind-induced compaction and blowing snow, not for the density of fresh snowfall. If the authors say that the density of fresh snowfall is generally higher due to fragmented snow with stronger wind, it is an acceptable statement.

To actually consider the processes involved here a 2-D model would be needed where snow can be transported explicitly. However, since this is a 1-D model, it cannot actually describe the physical processes, it can only give a simplified numerical description whose aim is solely to reproduce observed densities. We thus simulate (i) the density of precipitating snow under windy conditions; (ii) the modifications of the density of surface snow due to erosion/deposition processes, under a 1-D simplification scheme.

33. L135: The vegetation height is undefined yet (, but approximate values are given in Section 2.1). Please specify its value. Moreover, is the vegetation height really appropriate, not the canopy height?

Here the exact value is not needed, we present merely the dependence of the parametrization on the vegetation height. However, we will change section 2.4 from just 'Forcing Data' to 'Forcing Data and Model Setup' where we will clearly state the heights used, 0.4 m for TUNDRA and 1.3 m for FOREST

34. L137: What is the stabilizing effect? Please describe it.

It is the effect that vegetation stabilizes the snow cover, effectively reducing compaction. We will add this:

'This takes into account the stabilizing effect **of** vegetation **on the snow cover** and [...]'

35. L139: Is D really snow density? It seems the thickness of the snow layer. By the way, snow density is already defined as ρ (Eq. 2).

Thanks for the remark. *D* is indeed the layer thickness. We will correct this.

36. L142–143: A physical process represented by a factor c is unclear. Is c a fraction of overburden weight undertaken by the shrubs within the snowpack?

Yes, *c* is basically a factor that reduces the overburden. This will be stated in the manuscript:

“Herein, we introduce a parameter c acting **to reduce the effective overburden and as such, the compaction.**”

37. L143–145: Please describe this comparison. Is the stabilizing effect obviously observed in this comparison? How did you recognize the stabilizing effect from the observed density profile?

We will further explain the comparison:

“This value was obtained by comparing observed and simulated density profiles **and varying *c* until a good agreement with observations was obtained.”**

As detailed in comment 2 and 26, we were trying to compensate the lack of water vapor transport. Thus, we were seeking a process that produces the smaller densities at the bottom observed in the snow pits.

38. L147: The height of the canopy is undefined. Please specify its value. Related to my comment #29, is the vegetation height is more suitable?

We will specify the vegetation heights in section 2.4 which are 0.4 m for TUNDRA and 1.3 m for FOREST

39. L148: Please replace 'the lack of blowing snow scheme' with 'the lacking consideration of a blowing snow process' or the other appropriate one. According to the authors’ expression earlier, the blowing process is implemented as sublimation in the Crocus.

Thank you, we will change the sentence as suggested.

40. L152–153: What are offline simulations? Does not the Crocus interact with a parent land surface model?

As described in L. 96, Crocus is part of SURFEX. Offline simulations are simulations where the land surface model is used without an atmospheric model e.g. the forcing has to be provided externally as we have done it here.

We will replace “For offline simulations” by “For offline simulations (no coupling to an atmospheric model)”.

41. L155: According to L160–161 and Eq. (4), the minimum value of $(a+bW_s)$ is 1.0 at $W_s=3$ (m/s), meaning $P_{new}=P_{old}$. So, how did you remove snow without changing sublimation?

With this statement, we refer to the fact that when changing the precipitation with equation (4), one can remove or add snow. In our case, we do not remove any snow, but one could do so by changing the parameters *a* and *b*, for instance at sites where snow erosion is clearly visible.

42. L156: Please remove PR because this abbreviation is not used anywhere else.

Thank you for the comment, PR will be removed.

43. (4): Is the formulation of a one-order linear equation really appropriate in order to account for the blowing snow effect? Could you add appropriate references?

It is true that blowing snow effects are very complex. In L. 382-386, we advocate that it should get increased attention in the Arctic. Simulating blowing snow in a 1D model at the site scale only is inherently very difficult as the eroded snow at one point needs to be accumulated at another point. In L. 151, we say that not including any process of this kind greatly impacts the simulation (see Figure 9 and 10). Thus, in order to test the other processes, we needed to take blowing snow into account.

44. L158: Please add a unit for P_new and P_old.

Units of P_new and P_old will be added, thanks.

45. L158: Is P_old the observed precipitation rate, corrected using a transfer function (L183), at TUNDRA?

Yes, P_old is the initial rate as given to the model through the forcing data which contains the corrected precipitation at TUNDRA.

46. L159–160: This pre-analysis should be given as supplemental information, at least.

As stated in L. 167 the values are not optimized and in L. 382-389 we state that we did not aim to find the best set of coefficients but rather emphasize the importance of some processes. Including an analysis would give the impression that we recommend the used parameters for other sites and/or models, which we do not wish to do. For this reason, we feel it is best not to include this specific analysis.

47. L161–162: If so, there is a large gap in the increase in precipitation around 10 m/s wind speed because (a+bW_s)=3.1 at W_s=10 in Eq. (4). Does not this gap affect the result of the Crocus simulation?

Thanks for pointing out the error. We did not limit it to twice the original precipitation rate but to the 3.1 mentioned here. This error will be corrected.

48. L163–165: This preliminary series of tests should be given as supplemental information. By the way, are the preliminary tests really necessary? In this study, the Crocus simulation is performed on only two sites, and precipitation is observed at the TUNDRA site. Therefore, the necessary preprocessing is to estimate precipitation at the FOREST site including the blowing snow effect.

Preliminary only refers to the first test with Crocus and their analysis, so basically the default version described in the paper, thus, these tests are already included. There we saw that at TUNDRA no blowing snow was necessary but only at FOREST.

49. L165–166: Is this really true? As I pointed out in comment #37, Eq. (4) has only an effect to increase precipitation.

Yes, you only need to make the parameter b negative and than equation (4) can be used to subtract snow.

50. L171–172: What is the time interval of forcing data? And please remove '(solid and liquid)'.

The time step was one hour. This will be clarified:

'**Hourly** observations of these variables at each of the two sites have been collected since 2012,[...]'

It is important here to mention that we forced the model with solid and liquid precipitation as there are also models which can only take precipitation and then partition it internally using some sort of threshold.

51. L174: Which grid point of ERA5 did you select?

We selected the point closest to the location of the site which is 76.5º W and 56.5º N.

52. L177–181: These statements are not for the forcing data, but for the model settings. Please move to an appropriate position. Related to this, please place an appropriate topic sentence at the top of this paragraph. Moreover, what are the initial temperature and the bottom boundary condition?

Indeed this statement is not concerning the forcing data. We will change the section's name to 'Forcing Data **and Model Setup**'.

The initial temperature does not matter as we used a 5-year spin-up to ensure thermal equilibrium. At the bottom the heat flux is set to 0, which will be stated in the manuscript.

53. L187: However, the authors do not describe the difference of forcing data between TUNDRA and FOREST in this subsection. Please revise this topic sentence appropriately.

Sure, thank you. We will revise the paragraph as follows:

“We used a different forcing data set for each station. However, some of the required variables were not available at FOREST, so we used the precipitation, pressure and specific humidity from TUNDRA. Given the proximity of the two sites, the differences between these variables are presumed to be very small.”

54. L192: Please concatenate this paragraph with the next one.

We will concatenate the two paragraphs.

55. L203: interannual variability?

We will change “fairly variable interannual amplitudes” to “pronounced interannual variability”.

56. Figure 3: A color of 2019-20 is different from that of Figure 4.

We will change the color of 2019-20 in Figure 3 to correspond with the one in Figure 4, thanks.

57. L208–209: Please concatenate this paragraph with the next one.

We will concatenate the two paragraphs.

58. L210: However, a depicted line for 2015/16 at the FOREST site begins at the end of Nov. How did you recognize the onset of snow cover?

Unfortunately, the instrument monitoring snow height at FOREST had several malfunctions. This plot only depicts good quality data, so for the year 2015/16, it was not possible to detect the onset of the snow cover with snow height observations. We were, however, able to estimate it thanks to time series of upwelling shortwave radiation. This detail will be included in the figure caption.

59. L222–223: How many samples did you take?

At this instance we took 172 samples. We will indicate the number in the text:

‘For instance, on 12 April 2018, we made 172 measurements of the snow height within a 100 m radius of TUNDRA [...]’

60. Figure 5: This figure is not suitable for a scientific paper because the figure only shows a result through unclear/subjective post-processing by authors. At least, the source results of the observed stratigraphies should be given as supplemental information.

We will include the observed stratigraphies in the supplementary material.

61. L239: How was the mean calculated? According to Fig. 6, the vertical positions of each measurement were different, so the mean value of the vertical profile would not be simply obtained.

As seen in Figure 6, the height was normalized and subsequently we calculated the mean density in each bin of normalized height.

62. L241: How did you normalize the vertical scale? Simply did you divide it by the height of snow-cover? I suggest normalizing the vertical scale with a logarithm.

Yes, we divided every measurement height by the height of the respective snow cover. We do not see the advantage of a logarithmic scale in this context. Using the snow heights discussed in section 3.2.1, one can very simply get an impression of the intermediate snow heights with a linear scale as opposed to using a logarithmic scale.

63. L242–248: However, there are very large variabilities among the profiles. I suggest depicting confidence intervals in the figure and verifying robustness.

By showing a spaghetti plot as we do here, we believe we can illustrate the range of variability from one profile to another. Overlaying confidence intervals would, in our opinion, make the graph difficult to interpret. In order to assess the robustness of presenting only the mean, we tested the idea of representing the median of the distribution. As shown in the figure below, we can see that the two curves are very similar.

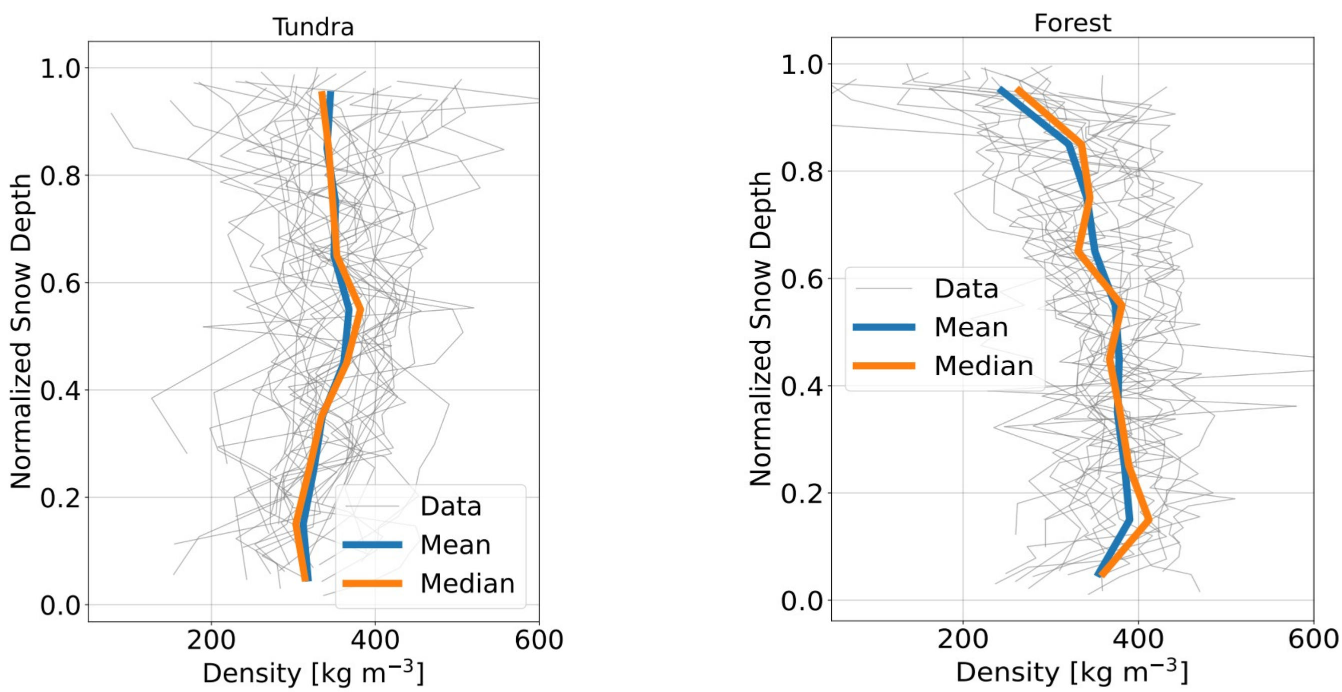

Figure 1. Snow density profiles from 29 snow pits in tundra environments and 18 snow pits in forested environments collected between January and March from the years 2012 to 2019. For better comparability, snow heights were normalized. The means and medians of all profiles are also shown.

64. Figure 7 and L249–257: Same things as comments #57–59.

Please refer to the figure below. Our statement from the previous comment stands.

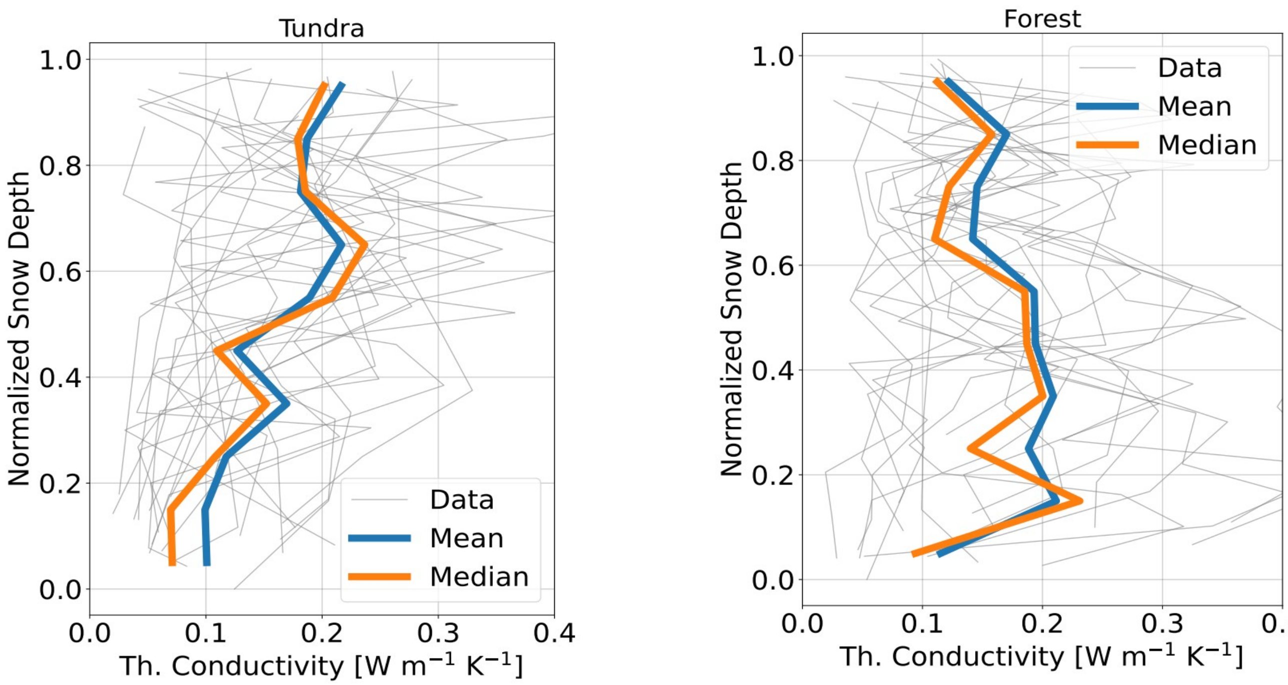

Figure 2. Same as Figure 6 but for snow thermal conductivity. Note that only data from 21 snow pits for TUNDRA and 17 for FOREST were used, as thermal conductivity measurements were not collected for every snow pit.

65. Figure 8: In the caption, there is a statement of 'Heights at which measurements were taken are relative to the surface of the ground'. On the other hand, in the legend, the unit of height is cm, not relative value to the surface of the ground. So, what are the heights depicted in the figure?

As mentioned in the caption, the heights are given relative the ground surface. This means that the ground surface is at 0 cm. Consequently, –9 cm means 9 cm below the surface and 11 cm means that it is 11 cm above the ground surface. Note that in the revised version we will change the height indications at TUNDRA, as we change the point of reference from the soil-lichen interface to the surface of the lichen. This will be clearly stated in the manuscript.

66. L268–269: From mid-Mar., the height of snow-cover is exceeding far away from 53cm (Fig. 4). How did you recognize the temperature of the top snow layer?

We will rephrase the sentence and no longer use top layer:

“At TUNDRA, the difference between air and the temperature at 64 cm varied between 0º and 5ºC”.

67. L269–270: Same as comment #62. From mid-Jan., the height of snow-cover is exceeding far away from 64cm.

We will change the sentence to:

“At FOREST, the difference between air and the temperature at 64 cm reached up to 10ºC, illustrating the impact of the greater snow depth on the snow temperature.”

68. L292–295: This result and L159–160, where the authors say a reasonable agreement between the simulated and observed snow height, contradict each other. Is the obtained parameters a and b in Eq. (4) really appropriate?

The statement in L159–160 refers to the general snow height, which is reasonably simulated, as can be seen in Figure 9. L292–295 are about specific events where inconsistencies arise due to a lack of a proper blowing snow scheme. This, however, does not strongly affect the overall snow height and equation (4) is not used. Also, here we talk about TUNDRA and earlier we state that equation (4) is only used at FOREST.

69. L295–296: Probably this is true. However, in order to say this, ideally, the snow water equivalent should be checked because the snow height is a result of snowfall, compaction, blowing snow, and sublimation. Could you add such kind of figure?

Thank you for this excellent suggestion. We will add a figure depicting the SWE in the supplementary material. See answer to comment 2 of reviewer 2.

70. L298: Please concatenate this paragraph with the next one.

We will concatenate the two paragraphs.

71. L305: The 'residual' is unclear here. What is the residual from?

We will change ‘residual’ to ‘difference’.

72. L311: Same as comment #67. The residual is unclear.

We will change ‘residual’ to ‘difference’.

73. L326–334: Does sublimation, ignored in this study, affect the contrasted snow height between TUNDRA and FOREST?

We do not ignore sublimation in this study.

To clarify this, we will add the following phrase:

'In other words, sublimation is included in the study, but it is not increased by the blowing snow parameterization.'

We just chose not to further increase it, as measurements from Lackner et al (2022) suggest that simulated and observed sublimation are already comparable. Although sublimation is likely to differ between the two sites, judging by the generally low rates, we do not believe it plays a significant role in the snow height discrepancies.

74. L332–333: I miss this observation result in the result section. Is this the snow height at TUNDRA?

No, this is not at TUNDRA. However, 'very top of the valley' might be confusion here. We will clearly state that the observations were taken further up the valley from TUNDRA:

"Snow surveys that were conducted at the very top of the valley **(≈ 500 m from TUNDRA)** revealed a very thin snowpack (<40 cm)."

75. L333–334: This sentence seems not to be related to the blowing snow effect. Is this really necessary to emphasize the effect of blowing snow?

The fact that the snow height at TUNDRA is more closely correlated to precipitation than at FOREST is an indication that blowing snow does play a significant role for the snow height at FOREST, whereas its role is less pronounced at TUNDRA.

76. L348: This is an inappropriate topic sentence. Probably a key point of this paragraph is that the mismatch between density and thermal conductivity profiles is not simply explained by the traditional relationship. Please revise it.

We will revise the topic sentence:

"A key factor that is not included relationships between snow density and thermal conductivity (e.g. Sturm et al. (1997), Calonne et al. (2011), and Fourteau et al. (2021)) is the snow type."

77. L350–351: Evidence is obviously lacking for this hypothesis. At first, you need to demonstrate how much the traditional relationship between the snow density and thermal conductivity explains the result

of this study. Then, a potential reason for the mismatch should be given. Appropriate references, that show a correlation between the thermal conductivity and snow grain shape, are also necessary. Otherwise, this paragraph should be deleted.

Thank you for raising this important point. For instance, Lehning et al. (2002) discuss the impact of microstructure (i.e. snow type) on thermal conductivity. We will add this reference in the text.

78. L357–359: This is an inappropriate topic sentence. Probably the content of L362–363 is a suitable topic for this paragraph.

This sentence states that simulating Arctic snow is generally a challenge as most sophisticated snow models have been developed for alpine snow. We believe it properly introduces the content of the paragraph, and as such wish to keep it as is.

79. L357–371: This paragraph is redundant. Please make the paragraph shorter. Moreover, please demonstrate how your modifications from Barrere et al. (2017), Gouttevin et al. (2018), and Royer et al. (2021b) improved the simulation skill.

The setting is different, we use these modifications in the forest-tundra ecotone, whereas the other studies were done in the Arctic. Then, we detail that we change the impact of vegetation.

80. L375: non-linear equations?

The study proposes allometric equations.

81. L372–378: So, did your implementation, not taking the whole vegetation height as a zone where compaction is reduced, effectively improve the simulation score? Please demonstrate it quantitively.

As stated earlier, we did not focus on a quantitative assessment, but rather by exploring possible means to improve snow simulations in the Arctic.

Furthermore, the choice to take a reduced vegetation height was based on the fact that shrubs bend (Ménard et al. (2014)). As such, we believe that taking the full height is physically less sound.

82. L390–392: The parameterizations implemented in this study are developed focusing on fresh snowfall, compaction, and blowing snow, not focusing on upward water vapor fluxes. Therefore, it is very hard to understand this sentence. Please update your statements.
Section 4.3: This section can concatenate with section 4.2.

Exactly, we argue that it would be necessary to include the water vapor transport in order to avoid site-dependent parameters. So it is the absence of this process in the model together with the fact that the parameter are site-dependant that leads us to the conclusion that water vapor flux is important for Arctic and boreal snow.

As requested by the other reviewers, we will extend section 4.3. Thus, we won't merge this section with section 4.2.

83. L397–398: This is hard to understand. No modification accounting for water vapor flux was implemented in this study. Sublimation is also neglected. Nevertheless, why can the authors conclude that the water vapor transport is dominant over compaction? Moreover, it is unclear what kind of physical quantity is dominantly controlled by water vapor transport rather than compaction.

As also explained in the response to comment 2, we do not base our conclusion solely on the numerical simulations. In fact, a large part of this conclusion is based on the observations e.g. the stratigraphy containing depth hoar, the density profile and the strong temperature gradients which favour water vapor transport. For this conclusion, the numerical simulations serve as additional evidence. The high temperature gradients create water vapor transport and as this process is absent in the model this backs up the conclusion that water vapor transport is dominant in shaping the density profile.

To make it clear what kind of physical quantity is controlled, we will clearly state that density profiles are most affected and thereby also the thermal conductivity.

As mentioned before, sublimation is simulated by Crocus and included in the simulations.

References

Barrere, M., Domine, F., Decharme, B., Morin, S., Vionnet, V., and Lafaysse, M.: Evaluating the performance of coupled snow–soil models in SURFEXv8 to simulate the permafrost thermal regime at a high Arctic site, Geoscientific Model Development, 10, 3461–3479, https://doi.org/10.5194/gmd-10-3461-2017, 2017.

Calonne, N., Flin, F., Morin, S., Lesaffre, B., du Roscoat, S. R., and Geindreau, C.: Numerical and experimental investigations of the effective thermal conductivity of snow, Geophysical Research Letters, 38, https://doi.org/10.1029/2011GL049234, 2011.

Domine, F., Barrere, M., and Sarrazin, D. Seasonal evolution of the effective thermal conductivity of the snow and the soil in high arctic herb tundra at Bylot island, Canada. *The Cryosphere,* https://doi.org/10.5194/tc-10-2573-2016, 2016.

Domine, F., Picard, G., Morin, S., Barrere, M., Madore, J.-B., & Langlois, A. Major issues in simulating some Arctic snowpack properties using current detailed snow physics models: Consequences for the thermal regime and water budget of permafrost. *Journal of Advances in Modeling Earth Systems*, 11, 34– 44. https://doi.org/10.1029/2018MS001445, 2019.

Fourteau, K., Domine, F., and Hagenmuller, P.: Impact of water vapor diffusion and latent heat on the effective thermal conductivity of snow, *The Cryosphere*, 15, 2739–2755, https://doi.org/10.5194/tc-15-2739-2021, 2021.

Gordon, M., Simon, K., and Taylor, P. A.: On snow depth predictions with the Canadian land surface scheme including a parametrization of blowing snow sublimation, Atmosphere-Ocean, 44, 239–255, https://doi.org/10.3137/ao.440303, 2006.

Gouttevin, I., Langer, M., Löwe, H., Boike, J., Proksch, M., and Schneebeli, M.: Observation and modelling of snow at a polygonal tundra permafrost site: spatial variability and thermal implications, The Cryosphere, 12, 3693–3717, https://doi.org/10.5194/tc-12-3693-2018, 495, 2018.

Jafari, M., Gouttevin, I., Couttet, M., Wever, N., Michel, A., Sharma, V., Rossmann, L., Maass, N., Nicolaus, M., and Lehning, M.: The Impact of Diffusive Water Vapor Transport on Snow Profiles in Deep and Shallow Snow Covers and on Sea Ice, *Front. Earth Sci.*, 8, 25 pp., https://doi.org/10.3389/feart.2020.00249, 2020.

Jafari, M., Sharma, V., & Lehning, M. Convection of water vapour in snowpacks. *Journal of Fluid Mechanics*, *934*, A38. doi:10.1017/jfm.2021.1146, 2022.

Lackner, G., Nadeau D.F., Domine F., Parent A.-C., Leonardini G., Boone A., Anctil F., and Fortin V. " The Effect of Soil on the Summertime Surface Energy Budget of a Humid Subarctic Tundra in Northern Quebec, Canada", *Journal of Hydrometeorology*, 22, 10, 2547-2564, https://doi.org/10.1175/JHM-D-20-0243.1, 2021.

Lackner, G., Domine, F., Nadeau, D. F., Parent, A.-C., Anctil, F., Lafaysse, M., and Dumont, M.: On the energy budget of a low-Arctic snowpack, *The Cryospher*e, 16, 127–142, https://doi.org/10.5194/tc-16-127-2022, 2022.

Lehning, M., Bartelt, P., Brown, B., Fierz, C., and Satyawali, P.: A physical SNOWPACK model for the Swiss avalanche warning: Part II. Snow microstructure, Cold Regions Science and Technology, 35, 147–167, https://doi.org/10.1016/S0165-232X(02)00073-3, 2002b.

Ménard, C. B., Essery, R., Pomeroy, J., Marsh, P., and Clark, D. B.: A shrub bending model to calculate the albedo of shrub-tundra, Hydrological Processes, 28, 341–351, https://doi.org/10.1002/hyp.9582, 2014.

Royer, A., Picard, G., Vargel, C., Langlois, A., Gouttevin, I., and Dumont, M.: Improved Simulation of Arctic Circumpolar Land Area Snow Properties and Soil Temperatures, Frontiers in Earth Science, 9, 515, https://doi.org/10.3389/feart.2021.685140, 2021b.

Simson, A., Löwe, H., and Kowalski, J.: Elements of future snowpack modeling – Part 2: A modular and extendable Eulerian–Lagrangian numerical scheme for coupled transport, phase changes and settling processes, *The Cryosphere*, 15, 5423–5445, doi: 10.5194/tc-15-5423-2021, 2021.

Sturm, M. and Benson, C. S.: Vapor transport, grain growth and depth-hoar development in the subarctic snow, Journal of Glaciology, 43, 42–59, https://doi.org/10.3189/S0022143000002793, 1997.

Vionnet, V., Brun, E., Morin, S., Boone, A., Faroux, S., Le Moigne, P., Martin, E., and Willemet, J.-M.: The detailed snowpack scheme Crocus and its implementation in SURFEX v7.2, Geoscientific Model Development, 5, 773–791, https://doi.org/10.5194/gmd-5-773-2012, 2012.