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:

The authors well updated the manuscript. The main subject of this study is to show that water vapor transport is an important process shaping the vertical snow density profile in both tundra and forest-dominated areas and to test the performance of the snow model Crocus and explore the adjustments, that is clearly stated in the main text. I understood that the scientific originality is to present the snowpack structure and thermal properties in both tundra and boreal forest which no previous study addressed yet. Following these updates, I tried to check the manuscript focusing on the points strongly related to the main subject. However, some concerns that were pointed out in the previous round of review were not significantly solved.

We did our best to resolve remaining issues.

1.First, the logical flow to conclude “predominance of water vapor fluxes for vertical density/thermal conductivity profile” is still insignificant. According to L422–425, we guess the authors reached the conclusion based on two results: (1) a significant fraction of depth hoar which is a remarkable characteristic indicating active water vapor flux, and (2) the snow density profile increasing towards the snow surface which should be decreasing towards the surface based on the compression process only. However, the strong wind densifies the snow surface, which coincides with high thermal conductivity (wind packing). The density of precipitation particles under the strong wind itself is also high (e.g. ρ_n=418 kg m-2 with T_a=263.15 K and W_s=4 m/s using Eq. 2 above the vegetation height, which is comparable to the observation). These natural processes and the stabilizing effect of shrubs can also cause the vertical density profile which is different from the expected one based on the compaction process only (the result by the default Crocus). The “predominance” should be argued after the order of importance in things is clarified. The authors should address each dependency of the process (compression, water vapor transport, stabilizing effect, and blowing snow effect) on the density profile quantitatively. Without this kind of quantitative discussion, it is difficult for me to understand that the water vapor transport overcomes the effect of the compression process.

First of all, the presence of depth hoar is a non-ambiguous demonstration of the existence of large water vapor fluxes. Numerous studies such as (Trabant and Benson (1972), Strum and Benson (1997), Domine et al. (2016)) have demonstrated that in the Arctic, the water vapor fluxes are large enough to trigger a vertical redistribution of the snow mass. Second, we do not think that wind compaction can be invoked to affect significantly the density profile. First, wind compaction affects all layers, not just surface layers, as seen during our field trips, usually in late March. Second, our climatology monitoring at TUNDRA site and FOREST has shown that wind speed is greater in fall compared to mid-winter, so that it affects basal snow layer more strongly. Top layers are therefore in general less likely to be strongly compacted by wind than lower layers. It sure can be suggested that shrubs would contribute to a lower basal snow density. However, our observations show that birch shrubs are very strongly compacted. Time lapse images show that this happens very early in the season so that their impact is very limited, and is in fact probably visible only in the lowest 10 to 15 cm of our density profiles (Figure 6). In fact, it is well known that birch stems are very supple and rapidly get compressed by snow, which limits their effect on the snow density profile. Sturm et al. (2005) mention (their page 6) “The 1.5- to 3-cm-diameter birch stems (Betula glandulosa) at the tall shrub site were more supple than the 3- to 5-cm-diameter willow (Salix pulchra) stems at the woodland site.” These authors also observed that they were quickly compacted. Finally, our modeling work shows that simulations without water vapor fluxes clearly cannot reproduce observations. This is a strong indication that water vapor fluxes is the missing process in models. Our attempt to improve simulations by adjusting the density parameterization is more an error compensation scheme, a common strategy in modeling, but this cannot be used to negate the crucial role of water vapor fluxes in determining the observed density profiles.

2.Second, the robustness of density/thermal conductivity profiles observed is not enough demonstrated. The problem that the thermal conductivity observation using TP02/TP08 heated needle probes gave a large error was well discussed in the revised manuscript (though a small mistake was found; see a specific comment). I understand that the error would not be a problem in terms of average. However, the profiles were largely variated (Figs. 6 and 7). Whereas it seems that one profile increased towards the surface while another decreased, it is very difficult to see the true variability from a spaghetti plot. The authors should show the confidence interval (or probability density function) or perform a statistic test for the vertical tendency of the profile. Then, the robustness of the vertical tendency of the profile should be discussed. Even though the authors showed comparable profiles between the mean and median in “tc-2022-19- author_response-version2.pdf”, that is only a piece of evidence that the probability density function might be assumed as the normal distribution.

The reviewer invokes the spatial and temporal variability of our observations to minimize the strength of our arguments. We feel this is not reasonable. Any field snow scientist knows that snowpack vertical profiles are highly variables in both time and space and that apprehending the properties of snowpacks at a given location can only be done by averaging, as we have done. These averages do show significant trends and with reason based on those.
Several options to illustrate the variability of the profiles exist. We prefer to keep the use of spaghetti plots because it allows to show the range of the observed values, while the single profiles are still visible. As suggested by the reviewer, we included the standard deviation of each normalized height increment in the figures containing the profiles.

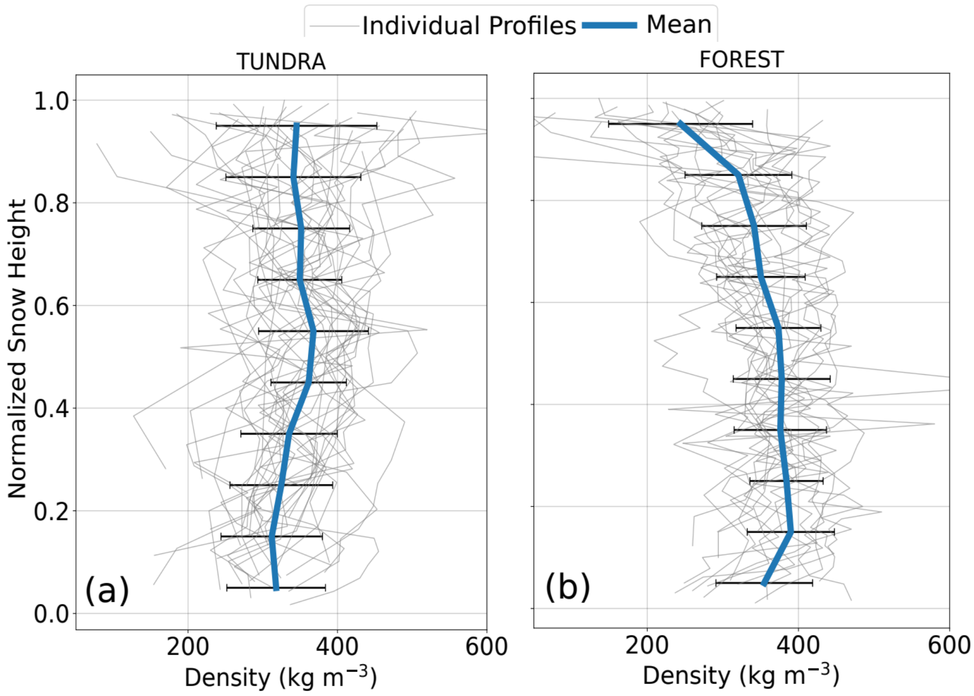

Figure 6: Snow density profiles from 29 snow pits near TUNDRA and 18 snow pits near FOREST collected between January and March from the years 2012 to 2019. For better comparability, snow heights were normalized. The means of all profiles are also shown, together with the standard deviation.

However, as mentioned before the median is a statistically robust measure and therefore guarantees the robustness of the vertical profiles.

3.Third, much evidence for coefficients of the adjustments (Eq. 1–4) is not still disclosed even though exploring adjustments is one of the study subjects. This is an important point in terms of scientific reproducibility, too. The authors should appropriately show the methods and results of preliminary simulations (L137–138, L152–153, L167–168) and discuss the improvements from the original adjustments by Barrere et al. (2017), Gouttevin et al. (2018), and Royer et al. (2021b). Although the authors were concerned as “Including an analysis would give the impression that we recommend the used parameters for other sites and/or models”, that is not a problem since the authors appropriately made remarks about it at L413–420. Moreover, I guess that the coefficients a_ρ, b_ρ, c_ρ, a, b, and c are intrinsically obtained based on the multivariable model with a very large degree of freedom: the density and snow height are outcome variables and a_ρ, b_ρ, c_ρ, a, b, and c are explanatory variables. Eq. (1) also affects the selection of coefficients. Since this seems very complex model to obtain the coefficients, the authors need to describe the procedure carefully.

As stated in the manuscript, we did not conduct any multivariable model analysis to obtain the parameters. Instead, we prescribed them ourselves by visually comparing simulated and observed profiles. This is one reason why we specifically emphasize that we do not recommend the use of these specific parameters and equations in other studies. As for scientific reproducibility, it is ensured by presenting all the parameters used, in addition to the fact that the default and adjusted versions of the model are available online.

As for the other studies, directly comparing them is not possible as all used different sites and a different model. For instance, SNOWPACK was used in Gouttevin et al. (2018) in Siberia. Barrere (2017) modeled a site in the High Arctic almost 2000 km further north, while Royer et al. (2021) made Arctic-wide simulations. All these studies obtained site-specific fitting parameters that were all error compensation tricks to make up for the lack of description of water vapor fluxes. We agree that a detailed analysis comparing all the parametrizations at all the sites could be useful to investigate the capabilities of such simple schemes to improve simulations of the snowpack in the Arctic.

Specific comments:

4. L46–48: This sentence should be revised as "These models have been developed for alpine applications, where the dominant process that controls the density profile in the models is the compaction that results from overburden."

We thank the reviewer for this suggestion. However, the sentence rephrased in this way suggests that the dominant process controlling the density profile is overburden only in the models, whereas this process is dominant in both nature and the models. We therefore prefer to keep the current wording.

5. L48–50: This parameterization is ad-hoc to fit the calculation result with the observation as the authors say in "tc-2022-19-author_response-version2.pdf". The water vapor transport has not been directly implemented. Please emphasize this point here and revise this sentence appropriately.

We inserted a statement to emphasize that no water vapor transport was simulated:

To overcome the lack of water vapor transport, Barrere et al. (2017), Gouttevin et al. (2018) and 'Royer et al. (2021b) all introduced modifications, **without explicitly simulating water vapor transport**, by increasing the maximum density of wind-induced snow compaction and adapting compaction to vegetation characteristics.'

6. Fig. 1: Please describe what a dotted line of the figure indicates in the caption.

A description of the dotted line was integrated into the caption of Figure 1, thank you. We mentioned that the dotted line is the boundary between the forested area and the shrub and lichen tundra.

7. L108–116: First, you indicate the blowing snow effects consist of snow erosion and accumulation (L109). However, after that, you say sublimation and wind packing effect are implemented in Crocus to simulate blowing snow (L110, L115). This is very confusing because the sublimation and wind packing effects are strictly different from erosion and accumulation, respectively. Please revise the wordings appropriately.

We revised L.109 to mention that additional effects come along with snow erosion and and accumulation.

‘However, the 1D nature of the model does not allow a direct simulation of snow erosion and **accumulation and the associated effects (additional compaction and increased sublimation rates)**.’

8. L128–143: The terminology of Snowfall is very confusing. According to L141–143, Snowfall includes densification effects of the wind, which associate with Blowing snow. Please replace it with Snowfall density or something appropriate word. Moreover, the densification effects of the wind have been already considered in Eq. (1) as a wind packing effect. Is this an appropriate procedure? Please clarify it in the main text.

The terminology *Snowfall* sums up all effects related to the deposition of snow and the snowpack surface. We prefer the use of a single word for the sake of brevity. The density effects described in Eq. 1 were not sufficient to account for the observed densification. For this reason, we included the process Snowfall.

9. L137–138: It is very hard to understand how the authors obtain a_ρ by varying c_ρ to be comparable density between the simulation and observation. Please describe the procedure more!

Sorry for the confusion. In fact, we vary both to obtain a comparable density profile in simulations as observed on site, as detailed in the manuscript. It should be noted that we had omitted a_ρ in L. 137-138, which probably contributed to the confusion. We rephrased as follows:

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

10. L156: The terminology of Blowing snow is confusing because this includes snow erosion, accumulation, and wind packing until this sentence. Please replace it with Snow redistribution or something appropriate words.

The process *Blowing Snow* was included to account for the snow accumulation at FOREST. We fear that using *Snow redistribution* would give the impression that we actually redistribute the snow from one point to another (e.g. TUNDRA to FOREST), which is not the case.

11. L163: Please delete “or remove” from this sentence. This is confusing because this study does not remove snow using Eq. (4).

We removed the ‘or remove’ from the manuscript.

12. Eq. (4): You can replace “W_s” with “min(W_s, 10)”.

Thank you for your suggestion, we presented the equation using min(U,10) as suggested (U replaced Ws as requested by reviewer Dr. Charles Fierz).

13. L173–174: Please delete this sentence. This is confusing because this study does not remove snow using Eq. (4).

The sentence was removed.

14. L182: Please replace “ERA5” with “the closest grid point data of ERA5 to the study site”.

ERA5 was replaced with the suggestion given.

15. L185: Please replace “rain and snow” with “liquid and solid”.

Thank you, we followed your suggestion.

16. L188: Typo.

Thanks for pointing out the typo, we corrected it.

17. L236: Why do you need to simplify stratigraphy? Fig. 5 and Supplementary figure 2 are very similar.

We simplified it to emphasize the important characteristics.

18. L244: However, melt-freeze forms are depicted at the bottom in Supplementary figure 2. Please make sure if there is no mistake.

Indeed, sometimes melt-freeze forms are present at TUNDRA, such as in this snow pit. As mentioned in the comment above, we simplified the profiles to emphasize the important and dominant characteristics of each site. Obviously, there is no real snow pit that shows exclusively these characteristics. In this specific snow pit, the melt-freeze forms were less abundant and less striking compared to FOREST. As such, we have chosen to omit them in the simplified profiles to present the typical profiles found at the site.

19. L246: However, no precipitation particles are depicted in Supplementary figure 2. Please make sure if there is no mistake.

The sign ‘/’ in supplementary figure 2 stands for ‘Partly decomposed precipitation particles’. Thus, these are slightly broken precipitated particles. As for the comment before, we made the choice to alter the profiles slightly to highlight the important characteristics.

20. L252: Please revise as “..., every measurement height was divided by the height of the respective snow cover.”

The sentence was revised as suggested.

21. L254: However, the variability among the measurements is very large. Is the profile really different between TUNDRA and FOREST significantly?

Please see answer to comment 2.

22. L327: I feel like there is something a little bit off with this sentence because the adjustments were ad-hoc procedures to fit the calculation result with the observation. Is it appropriate to say "the adjusted version does reproduce the density profile ..."? Please reconsider the terminology.

We did not adjust the model to the mean of the profiles but to single profiles. It is thus not self-evident that the mean of the model reproduces the mean of the observations. Particularly given that the mean in the model contains many profiles from January through March over several years.

23. L351: Snow amount at TUNDRA would be expected to be reduced by a strong wind. However, Crocus simulation, which ignores the snow erosion, relatively simulates snow height (Fig. 9) and snow water equivalent (Supplementary figure 1). How do you interpret this result? Please describe it in the main text.

As already stated in the text, the snow height further up the valley was found to be very small. Thus, we concluded that snow at TUNDRA does get eroded but there is also deposition of snow coming from further up in the valley. We added this to the manuscript:

'**As snow erosion and deposition at TUNDRA likely balance each other,** the snow height at TUNDRA is more closely correlated with the precipitation rate, which is typically low from January to March (see Figure 3).'

24. L354–357: Do you have some cases when the depth hoar was not or weakly developed with much snow amount? In such cases, if this hypothesis was true, the difference between the default Crocus simulation and the observation should be smaller.

Depth hoar or indurated depth hoar was found in all snow pits.

25. L380: Does "the uncertainty" indicate the standard error? If so, "n-1" should be correctly "n-1/2". Please make sure if there is no mistake.

Yes, this is a typo. It should be $n^{-1/2}$. Thank you for spotting this.

26. L403–404: So, did your implementation, not taking the whole vegetation height as a zone where compaction is reduced, effectively improve the simulation score? This can easily demonstrate quantitively through the preliminary sensitivity analysis (L153). Even though you responded to my comment as "we did not focus on a quantitative assessment..." in "tc-2022-19-author_response-version2.pdf", the quantitative assessment is necessary to demonstrate the predominance of water vapor transport.

The choice to include only part of the vegetation height slightly worsened the results. However, this choice was made as we observed bent shrubs in the snow pits and as we demonstrate in the manuscript, the bending of shrubs has already been studied (Ménard et al. (2014) and Belke-Brea et al. (2020). Thus, we feel it is a scientifically sound approach to take only part of the vegetation height to stabilize the snow.

27. Supplementary figure1: This should move to the main text.

We agree that this figure might be more interesting for people interested in snow hydrology, but in this study, we focus on the density profiles and not the SWE. Thus, our primary choice would be to leave it in the supplementary material.

28. Supplementary figure2: No legend for “/”.

Thank you for the remark. This legend was added.

Responses to Dr. Charles Fierz

General comments

I carefully went through all replies of the authors and the changes to the original submission. The authors adequately responded to the issues of the reviewers and in particular those I addressed. So I find the manuscript to have substantially improved. It is know also clearer that there is no direct evidence of the importance of water vapor transport from simulations as there is no such process implemented in the model. A few of my suggestions below could help further disentangle this aspect that was strongly pinpointed by reviewer #1. Of course, with major revisions, a few new questions arise that are reflected in my comments below.
In summary I recommend accepting the paper after the authors addressed these minor issues.

Thanks again for your comments. We also believe that they have helped us to improve the quality of the manuscript.

Comments to your replies

1. Point 2: Your choice, no problems. But the reason for it is not overwhelming. Snow depth – or here snow height – is defined as "… the total height of the snowpack, i.e., the vertical distance in centimetres from base to snow surface." Or even more inclusive the "vertical distance from the snow surface to a stated reference level." It is thus not associated with the direction of the vertical axis.

We agree that the difference is minor between snow depth and snow height.

2. Point 20: I eventually agree with you, in particular because all your snow pits were recorded around the same time in the year.

Indeed, comparing snow profiles from the very beginning of the winter with ones from the end of the winter could be more problematic and misleading.

3. Point 29: 'The sign of the gradient depends on the definition' I'd rather say it depends on the sign of the vertical axis that you obviously define as positive upward. Accordingly, what you call 'positive' is mathematically negative. Please correct.

Yes, we meant that the sign of the gradient depends on the definition of the direction of the vertical axis. This was corrected.

4. Point 30: '… while convective transport is believed to be the main process.' Thus the question is not settled for now! While I agree convection may be difficult to include in 1D models, Jafari et al. (2022) presented a numerical 2D study that may help setting the conditions for convection to occur in snowpacks. See Jafari, M., Sharma, V., and Lehning, M.: Convection of water vapour in snowpacks, 934, https://doi.org/10.1017/jfm.2021.1146, 2022.

The question might not be completely settled, however, Jafari et al. (2022) state themselves that ‘It has been concluded that significant convection must occur in snowpacks to explain the observations [...] and diffusion of water vapor is too slow to explain observations (Domine et al. (2016), Fourteau et al. (2021)). Thus, it appears highly unlikely that diffusion is the main process responsible for the vertical water vapor transport. We fully agree that studying 2D convection can help to understand conditions when it occurs and that 1D models might benefit from those findings.

Specific comments to the revised version

5. Lines 12-13: I suggest to move the sentence starting, "_We demonstrate …_" to Line 10 after the sentence ending, "… typical of the Arctic." such that it becomes clearer that no water vapor transport was implemented in Crocus.

Thanks for the suggestion, we swapped these two sentences.

6. Line 145: I am afraid your notation for wind speed W s is still inadequate. Variables are usually noted with one letter only, thus my proposal to use U instead. If you still want to use two letters, use WS (in roman typeface and not italic). Apply change in Figure 2 too.

We changed Ws to U.

7. Lines 252-253: ‘…, the snow heights were normalized’ In fact, the snow depth - or snow height here is the normalization factor. Consider changing to "…, the heights were normalized by the snow height.". The same applies to the captions of Figures 6 & 7.

Good remark, we changed this.

8. Lines 308-312: I am asking myself whether the ‘discrepancy’ is not rather due to mismatch in depth of snowfall? At least your comment on the situation at FOREST points towards it. The wording may have to be adjusted accordingly.

We do not have enough measurements of SWE to track its evolution over the course of a winter. Thus, there is some uncertainty when it comes to a comparison between the observed and modeled SWE. However, the large overestimation of the SWE at FOREST suggests that there is a problem with the total mass of the snow, rather than just in the depth of the snowfall.

9. Lines 454-456: Like in the abstract, I think the sentence starting, "However, …" is misplaced here. It should be moved after the sentence ending on Line 451 and may need some adjustment in wording.

We moved the sentence to the indicated position.

10. Figure S1: A pity you do only have one profile at FOREST in 2019! An additional point would have been interesting, particularly to show the spread among measured profiles at FOREST (see comment to Lines 308-312). However, you may consider comparing the measured SWE to accumulated precipitation (see Figure 3) as they should somehow match, in particular during precipitation events. Of course, blowing snow, sublimation, and imperfect correction for undercatch add to the uncertainty may make it a cumbersome task …

Having more profiles is certainty always beneficial, however, considering the large snow height at FOREST, it was usually not feasible to dig several snow pits there. We agree that comparing the SWE to cumulative precipitation a priori might be interesting, however, considering the important snow transport by the wind, the cumulative precipitation and the SWE do not necessarily match (SWE in 2019 at FOREST: 535 mm; cumulative precipitation: 233 mm) and thus, no conclusions can be drawn from the comparison. To study the impact of wind and the topography, specialized measurements at the scale of the valley would be needed.

Minor comments to the revised version

All the minor comments were implemented in the manuscript except for comments 18 and 36 (see below for details).

11. Line 14: Replace ‘The adjustments that were made to Crocus …’ to “These adjustments …”
12. Line 156: Replace ‘the lacking consideration of a’ to “not considering any”
13. Line 166: If Pnew and Pold are rates, the unit should be kg m-2 s-1 and not just kg m-2.
14. Lines 186 &188: Replace ‘0.5° C’ and ‘0.8° CC’ to “0.5 °C” and “0.8 °C”, respectively. Check throughout the text that there is a space between the value and the unit, that is x °C.
15. Lines 195-196: Replace ‘of 0.4 m height (TUNDRA) and 1.3 m height (FOREST)’ to “of 0.4 and 1.3 m height at TUNDRA and FOREST”
16. Line 199: Add ‘However, as some …’
17. Fig. 2 and Table 1: ‘SWdown’ As for wind speed, use a one letter variable or use roman typefaces if you want to stick to SW. Also, note that descriptive subscripts are always in roman typeface . This is valid for all variables throughout the text (see also https://physics.nist.gov/cuu/pdf/typefaces.pdf)
18. Figure 3: Consider using “kg m-2” here too instead of ‘mm’.

We prefer to stick to mm to make it comparable to indications of precipitation.

19. Figures 4, 9: Change y-axis label to “Snow height (m)”
20. Figures 5 & S2: The dates of the profiles have to match those in Tables S1 & S2 and given consistently in both figures. For example, I do not see any profile taken on 24 Feb at FOREST, but on 26th or 28th, the latter with Hs = 148 cm ?

Consider adding the snow depth to the dates, for example “(25 February, snow height is 69 cm)”.

Indeed, the data was wrong and was corrected. Also in the drawing the snow height appeared to high to represent a height of 148 cm. This was corrected as well.

21. Figures 5 & S2: Change y-axis label to “Height (m)”
22. Figures 6, 7, 10: Change y-axis label to “Normalized height (m)”

The normalized height is no longer in the unit m. So we changed the label to ‘Normalized height’.

23. Line 256: Replace 'snow layer' by "part"
24. Line 257: Replace 'this general' by "that decreasing"
25. Figure 8: Add missing y-axis label "Temperature"
26. Line 289: Replace 'disappearance' by "melt-out"
27. Line 308: Replace ' e.g. the total snow mass.' by ", i.e. the total snow mass per unit area."
28. Lines 356-357: Reword 'creating a greater vertical gradient' as "a large vertical gradient"
29. Figure 11: Add missing y-axis label "Temperature gradient"
30. Line 371: Reword 'not included relationships' as "often not included in relationships"
31. Line 378: Replace 'Generally' by "Admittedly"
32. Line 350: Add the geographical direction, I guess "~500 m north from TUNDRA"
33. Line 380: Replace 'the all' by "all"
34. Line 444: Replace 'wind-induced' by "wind-driven"
35. Figure S2: What do the thick lines in the FOREST profile indicate?

These are ice layers. We clarified this in the caption.

36. Tables S1 & S2: It may be interesting and valuable to indicate the measured bulk snow density (or/and SWE) for all measured profiles in these tables, whenever possible.

Oftentimes, the resolution of the measurements does not allow the calculation of the bulk density. For this reason, and also because our focus lies purely on the density profile, we did not include a column with the bulk density. For those interested, the dataset containing all density values will be available together with a paper describing the dataset. These data were submitted to PANGAEA together with a full dataset of soil and meteorological observations at Umiujaq TUNDRA and FOREST sites. Hopefully, the data set will be available if and when the paper is accepted and this will be indicated in the paper, so all the data will be available to readers.

37. Tables S1 & S2: Use the ISO-format yyyy-mm-dd for the dates.

References

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