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

Responses to Dr. Charles Fierz

General Comments:

This contribution addresses the problem of water vapor fluxes modelling in snow-cover models, in particular in regard to Arctic snow and pertinently state on line 52, '... the ability of those models to adequately simulate density profiles has yet to be tested.'. For this purpose the authors use a consolidated multi-year data set collected at two nearby sites within the forest-tundra ecotone and model snow-cover evolution with the detailed physical snow-cover model CROCUS. To do so, three key processes for Arctic snow are adapted to get a much better representation of snow depth evolution as well as measured density and effective thermal conductivity profiles for these two sites.

Thus, even though some results maybe 'site-specific biased', the authors convincingly show that, '..., the integration of water vapor fluxes in snow models, particularly in those coupled to climate models, is a pressing issue.' (see line 429) while blowing snow is a further aspect that needs more attention but difficult to integrate in point simulations.

The paper thus addresses timely a need for improvement in snow-cover modelling and provides a comprehensive data set that can be used in future evaluations. Unfortunately, even so the text is well structured and generally pleasant to read, there are a number of issues I address in detail below.

Thank you for your encouraging words. We highly appreciate your comments, which will clearly help us improve our manuscript. Please find our answers to your comments below.

In summary I recommend accepting the paper after the authors addressed the issues below and the editorial suggestions found in the annotated manuscript.

Thanks for the detailed corrections in the annotated manuscript. We will implement all of them.

Remarks on terminology:

1. 'thick' vs 'thin': I think you need to make this distinction early in the paper and not as late as on Line 404. I would suggest to use deep and shallow. See title too, I stumbled over it.

We will follow your suggestion to use deep and shallow. However, in the introduction we already make the distinction between shallow Arctic snowpacks and deeper snowpacks in the boreal forest.

2. 'snow height' vs 'snow depth', Both terms are used in text and figures. Please use one only throughout the manuscript and I would prefer 'snow depth'. In figures do not capitalize the second word.

Thank you for the remark, we will use only snow height in the manuscript. The reason for that is, that when you make a snowpit, you consider snow from the base and record the height of a layer of interest, not the depth.

3. Line 13: ‘*overburden weight*’ I wonder whether ‘overburden’ would not suffice, or at least switch to ‘overburden load’?

We will change all occurrences of 'overburden weight' to 'overburden'.

4. Line 203: ‘*years*’ I would suggest to use consequently ‘winter’ throughout the manuscript, as done in the caption of Fig. 3 just below.

'Years' will be switched to 'winters' when applicable.

5. Lines 113 & 116: Consider switching from ‘*accuracy*’ to ‘uncertainty’.

We will change 'accuracy' to 'uncertainty' in L.113 and L.116.

Specific Comments:

6. Throughout the text you use the term ‘*layer*’ very loosely. While this term is clearly defined in the International Classification for Seasonal Snow on the Ground (ICSSG) [see also section 3.2.3, line 237] When reading ‘*layer*’ here, it is often difficult to know what is meant. For example, at FOREST, what is the basal layer? On the other hand, you sometime use something like ‘*X % of the snowpack*’ (see for example line 303). I would thus suggest to speak only in terms of such fractions or to visualize them in one profile. One particular example is found on lines: ‘… *with an evident decoupling between air and top layer temperatures beginning in early February.*’ Here I understand you speak of the sensor located at a height of 64 cm, which is in the middle of the snowpack at that time (main reason for the observed decoupling) and not in what I would call ‘*the top layer*’.

Yes, the meaning of layer used in the manuscript does not always correspond to the definition in the ICSSG. We will change the word ‘layer’ when it is not referring to a layer according to the definition of the ICSSG.

7. The very useful concept of depth normalization needs to be discussed in more details. Indeed, that concept can be questioned in view of the potentially marked difference in total mass between ‘*normalised*’ profiles, even taken at the same site. See also my comment on lines 93-94 below.

When looking at the total mass of the snowpack, the use of normalized profiles can certainly be questioned. However, in this study we use the normalization to compare the vertical variations of density and thermal conductivity between different snow pits. We do not look at the total mass of the snow in each pit. For this reason, we think that in our case the use of normalized profiles is appropriate.

8. Section 3.3.1, lines 276-291: I found this section difficult to read and not free of ambiguities. I would suggest to reshape it and concentrate on the salient features due to the implementation of the three key processes. Furthermore, in the caption of Fig. 9 you state that a ‘*blowing snow module is implemented*’ at FOREST. I think this should be better stated in the text, along with recalling what processes (Snowfall, Compaction, Blowing snow) are activated at both sites, referring to section 2.3 for details.

We will clearly state in section 3.3.1 what processes have been activated and we will try to make it clearer:

‘At TUNDRA, the default version shows reasonable agreement with the observations. In winter 2018–19, snow height is underestimated by 15 to 20 cm during the accumulation period and the snow disappearance date is 12 days earlier than the observations. For winter 2019–20, there is better agreement between the default version of Crocus and the observations, with a mean negative bias of 10 cm, leading to a modeled melt-out date that is just two days later than the observed date. Simulations with the adjusted version of Crocus (including the processes *Compaction* and *Snowfall*) for TUNDRA show an increased snow height of 10 to 20 cm compared to the default version. For winter 2019–20, this leads to a delayed melt-out that is 15 days later than the observed date while it is closer to observations in winter 2018-19. One striking difference between the two versions is that the snow height fluctuations are dampened in the adjusted version of Crocus due to the reduced compaction and the heavier fresh snow.

Since the meteorological forcing is nearly the same at both sites, it is not surprising that the snow heights modeled by the default version of Crocus at FOREST are very similar to those at TUNDRA. As a result, the modeled snow heights of the default version are lower than the observed snow heights by a factor of 2. Thus, the simulated melt-out date is early by one month for winter 2018–19 and by 16 days for winter 2019–20. The adjusted version of Crocus (including the processes *Compaction*, *Snowfall* and *Blowing Snow*) simulated the snow heights that are much closer to those observed. However, snow heights are still underestimated by 10 cm to 70 cm. The melt-out date is better simulated, with a difference of 9 days in winter 2018–19 and 0 days in 2019–20.’

9. Line 15: ‘*to some extent*’ to be deleted. Having read the discussion and the conclusions I feel the adjustments are site-specific indeed.

We agree that the adjustments likely are site-specific but to which degree needs to be investigated in future studies in order to determine if they are only applicable at this site or at other sites as well. The similarities between our adjustments and those introduced in Barrere et al. (2017), Gouttevin et al. (2018) and Royer et al. (2021) suggest that at least some of the concepts are indeed applicable to different sites. We will rephrase the sentence and replace ‘but are site-specific to some extent’ with ‘but may not be applicable at other sites’

10. Lines 28-39: ‘*The weather conditions to which Arctic snow is typically exposed differ considerably from conditions in the boreal forest.*’ Does ‘*Arctic snow*’ refer to the tundra? I wonder whether the connection to your site in the forest-tundra ecotone could be better linked to the Umiujaq site where weather conditions are almost alike.

We were referring to the general characteristics of snow in the Arctic (north) vs. the boreal forest (further south), not necessarily at the boundary between the two as is the case in our study. To make it clearer, we rewrote this sentence: “The weather conditions to which Arctic snow is typically exposed differ considerably from conditions **generally found** in the boreal forest **further south**.”

11. Lines 80-81: What was the vertical spacing between the needle probes? Was it identical at both sites? And what about the second pole at TUNDRA? Was the latter used in the analysis?

The heights of the needles (measured from the surface of the lichen) was 4, 14, 29, 44, 64 at TUNDRA and 4, 14, 29 and 64 cm at FOREST. We used the second pole at TUNDRA to compare its results with the other pole. As these data were not used in this study, we will delete the reference to the second pole. Instead, we will include the heights of the needles as follows:

"Snow temperature and thermal conductivity were measured at both sites using vertical poles equipped with TP02/TP08 heated needle probes (Hukseflux, The Netherlands)**. At TUNDRA, five needles were installed at heights of 14, 14, 29, 44, 64 cm above the ground, whereas four needles were installed at FOREST at heights of 4, 14, 29 and 64 cm.** The measurement principle[...]"

12. Lines 93-94: '*A 100 cm3 box cutter … and a field scale were used to measure the density profiles.*' At what spacing? Continuously all 3 cm? This is important to note in particular as I assume many more measurements were performed at FOREST than at TUNDRA.

The spacing varied between the snow pits, mostly it was measured continuously at a 3 cm spacing (also at FOREST) 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, essentially those with thicker snow**.'

13. Lines 144 & 146: '*we selected a fixed value of 0.05*' Read that way, it sounds it was always applied. It would be very helpful to move up here the remark on lines 146-147.

We will move up L. 146-147 so that it directly follows the statement mentioned. Thanks.

14. Table 1: Why was the long wave component not included?

We did not include the longwave component as no significant differences were observed. In general, radiation should be rather similar between the two sites due to the short distance between them. We further investigated the shortwave part (and showed it in the manuscript), because we assumed some influence of the steep ridge close by in combination with the lower elevation of FOREST resulting in a higher topographic shading. For the longwave counterpart, this should not play a significant role and as the tree density is very low at FOREST, no significant difference in longwave radiation was expected.

We will add a short explanation of why we did not show longwave radiation:

'Due to the low density of trees, no significant differences in longwave radiation were observed'

15. '*difference*': Was it TUNDRA - FOREST or vice versa? It matters wrt the mean and you need to add a 'Δ' in the table header.

Thanks for the remark, it was TUNDRA-FOREST. We will include this in the caption of the table as well as the 'Δ' in the header.

16. Line 184: '*fixed threshold of 0.5 °C*' Based on observations?

As we detailed in L. 184-186, the threshold was obtained by comparing manual observations of precipitation type at the nearby airport with the measured air temperature at the site.

17. Line 188: '*specific humidity from TUNDRA*' Is this not questionable in that case. The value of specific humidity influences turbulent fluxes, which may be quite different at both sites.

We agree that the specific humidity has a significant influence on the turbulent fluxes. However, at FOREST there are also measurements of this variable available. Unfortunately, there is only a short period when the instrument worked properly. Thus, we decided not to use the measurements as forcing but instead used them to compare the relative humidity at both sites. We concluded that it was sufficiently similar in order to use the same relative humidity for both sites. See comment 19. in the response to reviewer 2.

18. Line 233: '*melt-freeze forms were often present within these basal layers*' What did trigger these melt-freeze events? Turbulent fluxes? Long wave radiation from the nearby trees? I think this could be of importance when discussing the different processes at work at both sites.

We will include our hypothesis in the paper:

'We hypothesize that the melt-freeze crystals are formed at both TUNDRA and FOREST due to short warm spells at the beginning of the winter. However, at TUNDRA, the high values of the temperature gradient in December and January (compare Figure 11) trigger a more intense recrystallization compared to the FOREST site which and the melt-freeze form disappear at TUNDRA while they remain at FOREST.

19. Line 240: '*similar environments*' Can you clarify how far from the TUNDRA site this may be? This could be included in the methodology section though.

Usually, the snow pits were dug within 500 m of the TUNDRA station. As we were asked by reviewer 1 (comment 18) to include a table containing all the snow pits, we will add the geographic location.

20. Line 241: 'In *order to make the profiles comparable, the snow heights were normalized.*' While I agree this is neat, I question whether it is straightforward wrt to the variability of HS, from winter to winter at TUNDRA, within a winter at FOREST (see Fig. 4.)?

It certainly covers differences in snow height at the different pits and times. However, the idea of the normalized heights is that regions of the snowpack which are subject to the same influences are shown at the same normalized height. For example, the upper parts of the snowpacks are affected by strong winds, incoming radiation and other external influences which are all independent of the snow height. So effectively, even when there are differences in snow height they do not impact the conclusions drawn from the normalized profiles.

21. for Forest, it is more questionable for Tundra as ΔHS may be up to 200%

We do not compare snow pits from the beginning of the winter with others from the end. Most of the snow pits were dug in March, so differences are not as high as 200%. Furthermore, the interannual comparison of the snow heights at FOREST shows that there is a strong resemblance of the evolution of the snow height from one winter to another.

22. Line 247: '*The scatter* …'Could you also say something about how many profiles did or did not follow the trend of the mean?

It is hard to come up with a definition or criterion that defines when a certain profile follows the trend of the mean or when it does not. We will publish all data, including the snow profiles, so the reader can compare the mean with every single profile directly.

23. Figure 8: It looks like part of the time you show temperatures at either 53 cm (T) or 64 cm (F) the sensors where not covered by at least 10 cm of snow. Please clarify or adjust the figure.

First, please note that the snow height is not necessarily the same at the snow gauge and where the snow temperatures are measured. Nonetheless, at the beginning of the period where we show the temperatures at 64 cm, the sensors might not be covered with 10 cm of snow. At FOREST, there is no time-lapse camera and at TUNDRA it is a several meters away from the post, so visual inspections of the snow height at the posts are either impossible (FOREST) or rather difficult (TUNDRA). Therefore, we mostly relied on temperature measurements themselves and decided to show the temperatures when a significant decoupling from air temperature took place. As measurements were taken every other day at 5 AM, heating through solar radiation is not an issue.

24. Lines 295-296: '*This mismatch between observations and simulation is due to the transport of snow by wind.*' It is not clear to me whether you refer to a modeled or an observed (how?) event. Please clarify.

Here we refer to observed transported snow and those events were determined using the time-lapse cameras. We will clarify this in the manuscript:

"This mismatch between observations and simulation is due **to observed events of snow transport** by wind."

25. Line 298: '*The mean observed density profiles* …' You need to explain how modeled profiles are averaged here. You do so in the caption only … and how is not at all clear to me. Please clarify.

For the mean modeled profiles, we considered the months of January to March in the years 2017 to 2020 and took one profile each week during these periods. The mean is then obtained by normalizing each profile and taking the mean. We will add a description of this procedure to the text:

'[...]' to the default and adjusted model runs in Figure 10. **For calculating the mean of the simulated profiles, one profile per week during the months January through March from 2017 to 2020 was taken, normalized and then the average profile was calculated.**

26. Line 318: '*The impact of air temperature differences on snow cover was modest,* …' What about the impact of turbulent fluxes? And what about incoming long wave?

There may arise differences in turbulent fluxes due to differences in wind speed, but as the turbulent fluxes are rather small, possible differences are even smaller and most likely negligible.

The same applies to the incoming longwave radiation, there are only very small differences between the sites. And given the close proximity of the FOREST site to the TUNDRA site, no large difference in long wave radiation are to be expected.

The only forcing variable that significantly differs is the wind speed and two distinct datasets were used for this variable for the two sites.

27. Line 330: '…, *snow is continuously transported from the upper parts* …' Confirmed by measurements of wind direction?

The prevailing wind direction is west, from the Hudson Bay. However, wind direction is still often such that it allows snow to be transported from the upper parts of the valley to its lower parts. Additionally, photos from the time-lapse cameras confirmed that snow is being transported in the direction towards the lower parts of the valley.

28. Line 341: '…, soil freezes earlier in the winter.' I may miss a point here, but from Fig. 4 the snow depth does not seem to be significantly different in early winter. Later on I agree. Could there be other reasons for this early and deeper freezing?

It is true, the difference is smaller but still it does make a difference for the freezing of the soil. However, there are also other reasons such as the higher soil water content at FOREST (see Figure 1 and 2)

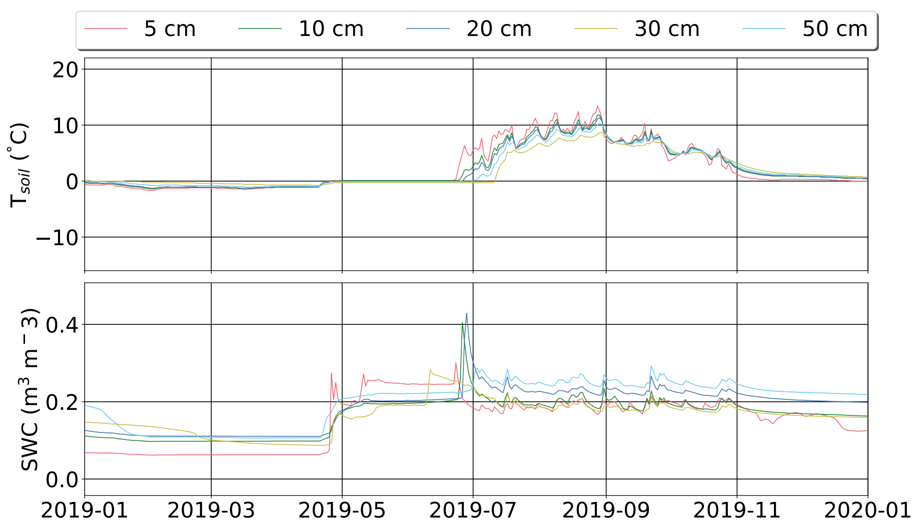

Figure 1: Time series of the soil temperature and the soil water content at FOREST at depths 9 cm, 15 cm, 27 cm, 39 cm, and 50 cm for the year 2019.

Also, there is more organic litter of low thermal conductivity under taller vegetation (see Gagnon et al. 2019). This further slows down soil cooling.

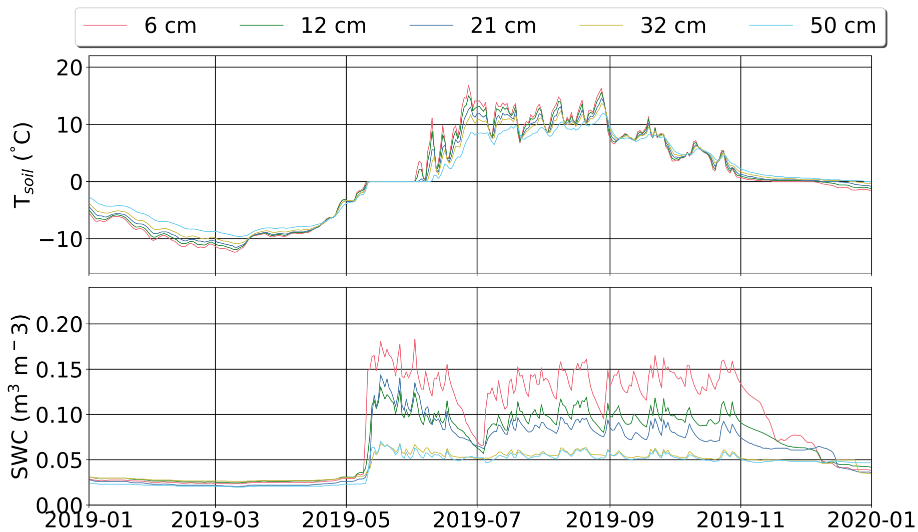

Figure 2: Time series of the soil temperature and the soil water content at TUNDRA at depths 5 cm, 10 cm, 20 cm, 30 cm and 50 cm.for the year 2019.

As the different snow cover is not the only reason for the later freezing, we will change the wording in L. 341:

'In part due to the reduced snow height and fairly similar snow thermal conductivity, soil freezes earlier in the winter.'

29. Figure 11: Is the sign of the temperature gradient correct? I would expect the contrary assuming the vertical axis is taken positive upwards.

The sign of the gradient depends on the definition i.e. whether the temperature at the upper measurement height is subtracted from the temperature of the lower one or vice versa. Here, we subtract the temperature at the upper height from the temperature at the lower height. We will clarify this in the caption of the figure:

"Positive gradients indicate higher temperatures close to the ground surface than in higher snow layers."

30. Line 392: '… *the solution may be to actually include them in models.*' … which has been actually done here:

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.

I am not saying it works in Arctic snow though.

Thank you for the remark, however, Jafari et al. (2020) only included diffusive water vapor transport while convective transport is believed to be the main process (Trabant and Benson, 1972). We will include this reference together with another one (Simson et al. 2021) and state that first attempts were already made to address this issue.

References

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Gouttevin, I., Langer, M., Löwe, H., Boike, J., Proksch, M., & 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, 2018.

Gagnon, M., Domine, F., and Boudreau, S.: The carbon sink due to shrub growth on Arctic tundra: a case study in a carbon-poor soil in eastern Canada, Environ. Res. Commun., 1, 091 001, https://doi.org/10.1088/2515-7620/ab3cdd, 2019.

Royer, A., Picard, G., Vargel, C., Langlois, A., Gouttevin, I., & Dumont, M. (2021) Improved simulation of Arctic circumpolar land area snow properties and soil temperatures. *Front. Earth Sci.* 9:685140. doi: 10.3389/feart.2021.685140, 2021.

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.

Trabant, D. and Benson, C. S.: Field experiments on the development of depth hoar, Geol. Soc. Am. Mem., 135, 309-322, 1972.