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

Responses to Reviewer #2

General Comments:

In "Snow properties at the forest-tundra ecotone: predominance of water vapor fluxes even in thick moderately cold snowpacks", the authors present an observation dataset and modelling with CROCUS of snowcovers in a forest-trundra eco zone in north eastern Canada.
Overall the manuscript is well written. The ability of modern snow models to accurately resolve snow depth and microstructure including accurate density estimates remains a key challenge. This paper presents some interesting observational and modelling results that, after tightening should make it a contribution.

Thank you for this positive appreciation.

1. My major criticism is that the treatment of the canopy and canopy impacts, e.g., canopy interception and canopy sublimation, are not clearly articulated. This is especially the case in the modelling description. I would like to see this more readily described. Without, it makes the forest results difficult to interpret. I understood the forest site to be similar to the Ménard, et al site where the shrubs are buried by snow (~1m tall), however the black spruce (~5m) are almost certainly not buried and thus have canopy affects. Given the canopy dynamics can impact shortwave transmittance, longwave (in/out), albedo, etc I feel the manuscript is missing this critical section.

As detailed in the manuscript (L. 68), trees only cover approximately 20% of the surface. Thus, there is no closed canopy, but rather sparsely distributed trees. Snow observations were made at some distance from the trees. As such, any impact of interception would be negligible. The impact of trees manifests itself on the reduction of surface turbulence, and hence of increased deposition of drifting snow and reduced snow erosion. We will clarify this by adding the following phrase after L. 94:

'At FOREST, snow pits were dug at some distance from trees, so interception of snow is negligible.'

The impact on shortwave transmittance is likewise neglected, given the precise location of our observations (no obstructions by surrounding trees) and the limited tree density. Observations were made where shrubs were present. Shrubs were treated as in the Tundra case, i.e. bending, densification. This will be clarified in the manuscript:

'At the exact location of the instruments at FOREST no trees were present that obstructed measurements.'

2. More of a "this surprised me" rather than a criticism, I was very surprised at the total lack of SWE observation and comparison with the model. I realize the focus of this manuscript is on the depth and density estimates. However, as snowdepth includes the uncertainty in both snow mass and density, it is

a bit difficult to attribute differences in SD as entirely due to density errors versus more systematic snow mass errors. For example, biases in snow loss due to surface sublimation cannot be diagnosed with snowdepth results alone. I would strongly suggest a small comparison of model v. obs SWE so-as to allow the reader to confirm snow mass is being correctly estimated.

Indeed, we chose to focus our analysis on snow depth and density, as well as on the vertical structure of the snowpack and its evolution in time. Obviously, as both the density profile and the snow height are computed, conclusions on the SWE can be drawn as well. As the number of measurements from which we can calculate the SWE is small and limited to one short period in each winter, it is impossible to follow the evolution of the SWE, for instance before and after major snow blowing events. For this reason, we decided to focus on the snow height to show the evolution of snow accumulation. However, we acknowledge the importance of SWE for hydrological applications. Therefore, we will add a figure depicting some SWE values calculated from the density profiles to show that in fact the SWE simulations are rather comparable to observations. We will include the figure in the supplementary material and we will mention it at the end of section 3.3.1 together with a short discussion of the results:

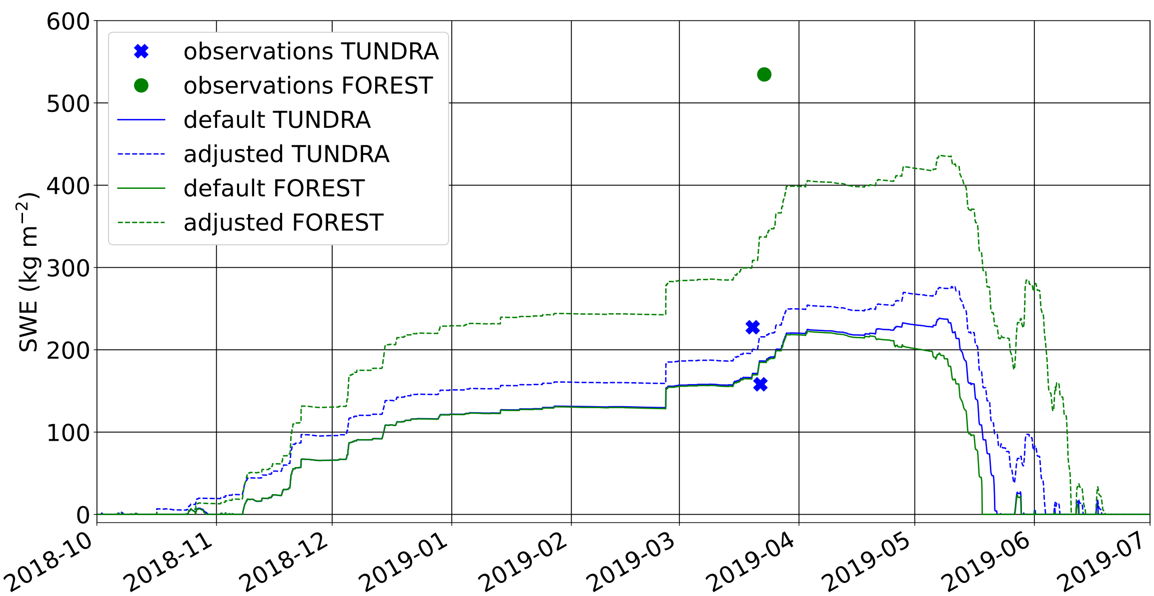

Figure 1: Observations and simulations of the SWE at TUNDRA and FOREST during winter 2018-19.

“Discrepancies in the simulation of snow height can also arise due to uncertainties of the SWE e.g. the total snow mass. To verify whether this is the case, we compared simulations of the SWE with observations that were obtained using the density profiles (see Supplementary Figure 1). At TUNDRA, there is a good agreement between the observed and simulated total snow mass. At FOREST, the model underestimates the SWE, similar to snow depth in Figure 9. Thus, the amount of blowing snow was higher than the one simulated by the model.”

3. Lastly I think there needs to be a better treatment of the uncertainty of the parameters in the snowmodel and in the observations (specifically the conductivity). I realize the authors are not interested in calibrating the model. However, for example, how impactful are the decisions on line 160? Although I get the sense these are chosen by evaluating the physical processes in play, they are still somewhat arbitrary and may dramatically impact the interpretation of the results.

This is relevant remark. We will discuss the impact of the uncertainty of the thermal conductivity 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 vertical gradient is our main focus, reducing the impact of the uncertainty of a single measurement. Moreover, by taking the average of the all samples, the uncertainty is reduced according to n^{-1} or a factor of ≈4 with *n* = 21 (TUNDRA) or *n* = 15 (FOREST) samples.’

and the uncertainty of the parameters at the end of section 4.2:

‘The parameters in the processes *Compaction* and *Snowfall* were robust against small variations, meaning that the output of the model did not heavily depend on the exact choice of the parameters.’

However, we don’t think it is useful to discuss the impact of the uncertainty of the parameters used in the process *Blowing Snow.* We clearly state in section 2.3 that this process was only implemented to increase the snow height at FOREST as otherwise simulations there would make no sense. We do not recommend the general implementation of the parametrization used in the process *Blowing Snow* in snow models and discussing this in detail could create the contrary impression.

Specific Comments:

5. L5 The TUNDRA and FOREST sites being all-caps surprised me. I'm fine with this, but I am wondering what the motivation is versus proper names such as "Tundra" and "Forest"?

We decided to capitalize TUNDRA and FOREST to make a clear distinction between the case when we refer to those sites in contrast to the tundra and forest biomes.

6. L16 "models leads to an inadequate representation" of snowdepth or SWE? The distinction matters w.r.t policy. E.g., if the mass is still right, then at these scales that is often sufficient: "we will still have X m^3 water input to reservoirs under policy Y".

Here, we refer to the density profile. This will be clarified:

[...] leads to an inadequate representation **of the density profiles** of even deep and moderately cold snowpacks, [...]

7. L34 "precipitation are typically higher" Given this study focuses on the transition, certainly they remain somewhat similar 1km apart. The boreal forest is a large region. Where, exactly, is this transition point?

We agree that in this case the precipitation is very similar. L.34 is a general statement about the distribution of precipitation in the Arctic versus the boreal forest. Groisman and Easterling (1994) showed that typically boreal forest environments receive higher annual precipitation than the Arctic tundra.

8. L38 "similar to alpine snow" This is below-tree-line alpine snow?

No, not necessarily, alpine environments usually feature an increased amount of snow and are not as cold as the Arctic tundra. This is why alpine snow covers show some resemblances with those in the boreal forest.

9. L 55 I would like to see the authors directly specify the research questions. This has a good start, but I would like this clearly stated and then answered in the discussion+conclusion

We will rephrase the last section of the introduction to make the research questions (formulated as objectives) more apparent:

“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.”

10. L 72 Figure 1 I really like this figure

Thank you. We appreciate your comment.

11. L 95 This section needs a description of how canopy interactions (mass + energetics) are handled

With the vegetation used here, shrubs, there is no canopy interaction in Crocus. The only dependencies of the snow cover on the vegetation are the ones introduced in this study and which are described as *Compaction*, *Snowfall* and *Blowing Snow*. We will state in the manuscript that no canopy interception is present in Crocus for shrubs.

12. L180 "based on estimates" are these from just musing on it, or were these informed from soil pits, etc?

These are estimates based on soil pits dug on several occasions. We will clarify this in the manuscript:

‘[...] clay for FOREST based on estimates **from several soil pits dug around the station where higher fractions of fine particles where found compared to TUNDRA.**’

13. L181 Based on the met data availability I had expected a simulation period of 2012+ with a spin up period pre-2012. Why was the model not spun up prior to 2012 and run for evaluation 2012 onward? It would be good in this section to explicitly note "met data available for 2012-XXXX, model spin up was YYYY-ZZZZ, and evaluation was PPPP-QQQQ".

As stated in the manuscript, the precipitation data was available from 2016 onward (there is a typo in the manuscript, it has to be 2016 instead of 2015). Precipitation has a huge influence on the simulation, for instance when it comes to the compaction due to the overburden. For this reason, we did not use the meteorological data prior to 2016 for the evaluation itself but for the spin-up. It is already stated that this spin-up was from 2012 to 2016 and we will add a phrase stating the start and end of the evaluation.

“[...] raw ERA5 data had to be used for the 2012–2016 period**, for this reason the evaluation period is from 2017 to 2020.**”

14. L182 Does this not contradict the 2012 statement on line 172? "Observations of these variables at each of the two sites have been collected since 2012, except for atmospheric pressure,"

Yes, indeed we will add precipitation as an exception as well.

“[...] except for atmospheric pressure, which was available since June 2017**, and precipitation, which was available since May 2016.”**

15. L182 "corrected the precipitation" is this the ERA5 data? Or the obs? Please explicitly state.

Only the raw precipitation data needs to be corrected for undercatch particularly of snow. We will clarify this:

“We corrected the **observed** precipitation data [...] “

16. L184 "fixed threshold of 0.5" There are a plethora of threshold methods that are physically based and indeed the choice matters significantly

1.Harder, P. & Pomeroy, J. W. Hydrological model uncertainty due to precipitationâ phase partitioning methods. _Hydrol Process_ **28**, 4311–4327 (2014).

2.Jennings, K. S., Winchell, T. S., Livneh, B. & Molotch, N. P. Spatial variation of the rain–snow temperature threshold across the Northern Hemisphere. _Nat Commun_ **9**, 1148 (2018).

We agree that the choice does matter significantly. However, as we do not consider the snowmelt period, the threshold only affects the onset of the seasonal snowpack in fall, e.g., its lower layers. As the air temperature remains well below any reasonable rain-snow threshold during winter, the vast majority of the snowpack does not change when varying the threshold.

Furthermore, in L.185, we state that we obtained the threshold used in this study by comparing air temperature with observations on the type of precipitation from an observer at the nearby airport. The type of precipitation highly depends on quantities such as the atmospheric lapse-rate rather than on quantities measurable at surface level. Thus, we think that a threshold determined from several years of observations on the type of precipitation at the site is sufficiently robust.

17. This would be a good candidate for inclusion in additional uncertainty estimates to understand how impactful this was in the fall and spring seasons.

As mentioned in comment 16., spring is not considered in this study. Concerning the impact in fall, we tested limits between 0.3 and 0.8 °C and did not see any significant impact on the snow quantity in fall. In the manuscript we will add this point:

‘Thresholds between 0.3 and 0.8 °C were tested with little impact on the amount snow.’

18. An adjacent question is how was precipitation temperature estimated due to its impact on developing snowpack cold content?

In the model, precipitation temperature was taken to be equal to snow temperature. This will be stated in the manuscript:

‘The temperature of new snow is taken to be equal to the temperature of the uppermost snow layer.’

19. L188 "specific humidity" this one may not be identical

1.Flerchinger, G. N., Reba, M. L., Link, T. E. & Marks, D. Modeling temperature and humidity profiles within forest canopies. _Agr Forest Meteorol_ **213**, 251–262 (2015).

It is true that there might be differences in specific humidity, however, relative humidity was measured at both sites but unfortunately at FOREST there is only a short period when the instrument worked properly. Thus, we decided not to take those measurements for the model forcing at the FOREST site, but we used available measurements at FOREST to compare measurements at both sites (see Figure 2 below) and concluded they were sufficiently similar to use the same values for the forcing at both sites.

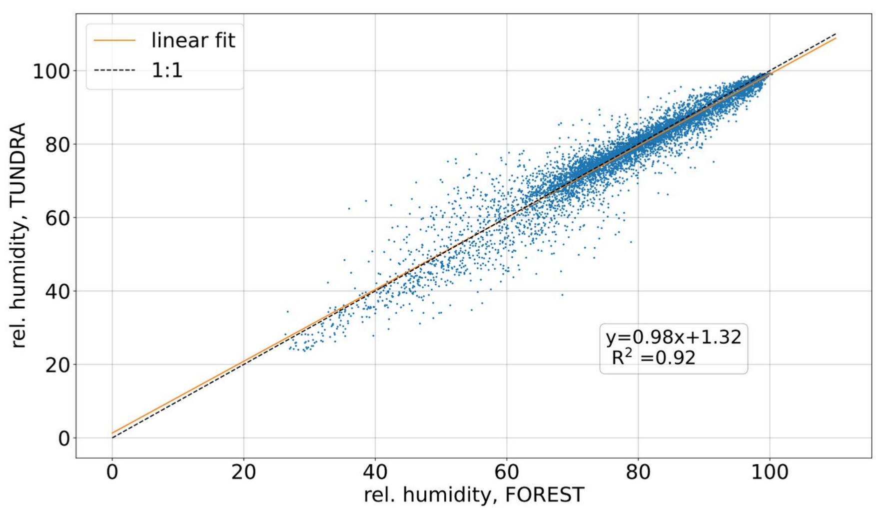

Figure 2: Scatter plot of the relative humidity at TUNDRA versus the relative humidity at FOREST for the period October 2015 to July 2016 when measurements at both sites were available. Also included are a linear fit with the corresponding equation. This fit is very close to the 1:1 line, also plotted.

20. L198 "mean difference" And I assume gusts too, important for blowing snow

That is correct, for blowing snow gusts are important and just as the mean difference of the wind speed gusts are also lower at FOREST.

21. L199 "downwelling shortwave" is this sub canopy? Shrubs or in the forest proper? If the latter I would have expected substantially more difference late season. It may be worthwhile breaking this into a few periods as the long, dark winters will heavily bias the mean.

Both measurements were performed above the canopy (shrubs) and as the trees at the FOREST site are sparsely distributed, their impact on the radiation is likely not significant.

22. L201 "remained comparable throughout the winter" This seems expected due to low solar angles?

Indeed, this was somewhat expected.

23. L202 Or due to the canopy + higher solar angles?

As mentioned in comment 21., the canopy at FOREST is very sparse and does not have a strong impact on the downwelling radiation.

24. L205 Figure 3, note the site and add units.

We will add the site (TUNDRA) in the caption. However, units are already present (mm).

25. L217 "depending on the maximum snow height" isn't this somewhat a tautology such that deeper, colder snow packs take longer to melt out than small snowpacks? Noting the impact of cold content development might help make this section stronger.

Yes, it is expected that deeper snow covers take longer to melt. However, here we specify quantitatively what impact the increased snow depth has e.g. that the meltout date might be half a month earlier or later.

26. L241 "similar environments" unclear what this is referring to.

This refers to environments with a similar canopy.

We will explicitly state this and exchange ‘similar environments’ with ‘environments with a similar canopy cover’.

27. L241 Figure 6 these captions are Proper Name case Tundra and Forest. Either change the text to proper name case or change these to TUNDRA/FOREST for consistency.

Thank you for the remark, we will change the captions to TUNDRA/FOREST.

28. L253 "for the upper 80%" It looks closer to 50-40%?

It is true that the decrease is less pronounced from 0.2 to 0.5 of the normalized height. We therefore change the phrase to:

‘there was a clear decrease in density for the upper **50% and a very slight decrease between 15 and 50 %**’

29. 48 Figure 7 Suggest adding uncertainty regions for these observations to match the 29% noted in the text.

Our focus here is more on the gradient of the thermal conductivity which differs between TUNDRA and FOREST. As you suggested in comment 3., we will revise our discussion of the errors. There we will underline also that our focus is on the gradient rather than the specific value.

30. L281 Best remind the reader quick what was adjusted

We will state the processes included:

'Simulations with the adjusted version of Crocus **(including the processes Compaction and Snowfall)** [...]'

31. L283 "between the two versions" adjusted v. Non-adjusted?

Yes, this is what we meant. We will clarify this:

'One striking difference between the **adjusted and the default** version is [...]'

32. L286 Canopy impacts?

As we stated in comment 11., there is no canopy impact in Crocus prior to our adjustments with this type of canopy.

33. L295 Without doing a falsification experiment of one with and one without can you know this 100%. Please describe how it is known with such high confidence that it is related to these events.

In the model, snow accumulation can only be observed due to precipitation events. So, every rise in snow height is caused by snowfall. In reality, blowing snow can increase snow height even without a precipitation event or accumulated snow during a precipitation event can be eroded leading to a decrease in snow height. This was observed with time-lapse cameras which were installed at TUNDRA. So if there is an increase in snow height in the simulation (due to snowfall) but not in observations, the only possible cause can be erosion by blowing snow. Vice versa, if there is no increase in snow height in the simulations (and no precipitation) but an increase in the observations, blowing snow is also the only possible reason.

34. L321 "as for the low[...]there." Is this not canopy as well?

No, as mentioned in comments 21. and 23., the forest is very sparse thus, the lower elevation relative to the ridge confining the valley has a greater influence than the trees.

35. L397 This water vapour transport finding seems to be a major conclusion I think you should better highlight

Indeed, we agree and will change the discussion to highlight this finding more:

'Hence, water vapor transport is the dominant process in shaping the density and thermal conductivity profile at our site, exceeding the importance of compaction due to overburden weight. Domine et al. 2016 and Domine et al. 2019 have shown this for thin high-Arctic snowpacks but here, snow depth is often more than 1 m and thus, considerably deeper than in the high-Arctic. Thus, water vapor transport is prevalent for all Arctic regions, even for those with relatively large amounts of precipitation.'

36. L403. Same as above

We agree that this is a major conclusion but the importance of it is already highlighted by the subsequent paragraph where we detail the impact of the finding for Arctic and boreal biomes.

37. L423 "Arctic-like" this is unclear, previously you had noted Alpine-like. Is that what you mean here?

Here, we refer to the snow at TUNDRA which is rather typical of the Arctic. The snow at the FOREST site is more alpine-like. We will clarify this in the subsequent phrase:

'At FOREST, the density gradient is towards slightly lighter layers at the top, indicating a role of overburden compaction more similar to alpine-like snow.'

38. L435 by publication-time I hope?

Yes, the data files will be deposited in a repository with a DOI before publication of this paper.

References

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.

Groisman, P. Y. and Easterling, D. R.: Variability and Trends of Total Precipitation and Snowfall over the United States and Canada, *Journal of Climate*, 7, 184 – 205, https://doi.org/10.1175/1520-0442(1994)007<0184:VATOTP>2.0.CO;2, 1994.