The Cryosphere Discussions

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Comment on tc-2022-19

Charles Fierz (Referee)

Referee comment on "Snow properties at the forest–tundra ecotone: predominance of water vapor fluxes even in deep, moderately cold snowpacks" by Georg Lackner et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-19-RC3, 2022

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.

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

Remarks on terminology (to be considered throughout the manuscript)

- '*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.
- '*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.

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

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

Lines 113 & 116: Consider switching from '*accuracy*' to '**uncertainty**'.

Specific comments

- 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*'.
- 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.
- 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.

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

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.

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?

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.

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.

Table 1: Why was the long wave component not included?
'*difference*': Was it TUNDRA - FOREST or vice versa? It matters wrt the mean and you need to add a 'Δ' in the table header.

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

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.

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.

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.

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

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

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.

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.

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.

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?

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

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?

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

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.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2022-19/tc-2022-19-RC3-supplement.pdf