The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2022-19-RC1, 2022

Comment on tc-2022-19

Anonymous Referee #1

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-RC1, 2022

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:

- the higher snow height in the forest whereas lower height in the tundra.
- lower density at the top of snowpack in the forest whereas homogeneous density profile vertically in the tundra.
- 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.

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

- 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 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.
- 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.
- 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.

Specific comments:

- 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.

- L42: What does "this type of snow" indicate? Please clarify it.
- L44–46: This statement is inappropriate because the dominant process controlling the density profile is independent of the circumstances in the model development.
- 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.
- L46: "this deficiency" is unclear. Please clarify it.
- 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.
- 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.
- L64–66: For this, a topographic map is suitable. Could you add such kind of figure?
- 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".
- L67: Similar to my comment #9, the region of the "lower valley" is undefined.
- L69: Is "2-3 m" a typo for "2–3 m"?
- 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.
- L86–87: What was the time-lapse camera used for?
- L88–89: When did you conduct snow-pit observations eventually? Please add a table describing the dates of observations.
- L91–91: Please describe the vertical intervals in the measurement of density and temperature.
- L92: "some snow pits" is unclear. Could you specify how many snow-pits is?
- 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.
- L104–106: Is this really true? The effect of blowing snow on snowpack is generally separated into erosion and accumulation (, and sublimation sometimes).
- L107–108: Could you add a quick description of the parameterization of Gordon et al. (2006) here?
- (1): Do you mean that the viscus compression is ignored at the top layer?
- L102: Is 350 kg m-3 a value of the default Crocus?
- 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.
- 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.

- L123–127: This is verbose. Please make the statements shorter.
- (2): T_fus can be replaced with 273.15.
- 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?
- 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.
- 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.
- 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?
- L137: What is the stabilizing effect? Please describe it.
- 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).
- 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?
- 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?
- L147: The height of the canopy is undefined. Please specify its value. Related to my comment #29, is the vegetation height is more suitable?
- 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.
- L152–153: What are offline simulations? Does not the Crocus interact with a parent land surface model?
- 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?
- L156: Please remove PR because this abbreviation is not used anywhere else.
- (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?
- L158: Please add a unit for P_new and P_old.
- L158: Is P_old the observed precipitation rate, corrected using a transfer function (L183), at TUNDRA?
- L159–160: This pre-analysis should be given as supplemental information, at least.
- 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?
- 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.
- L165–166: Is this really true? As I pointed out in comment #37, Eq. (4) has only an effect to increase precipitation.

- L171–172: What is the time interval of forcing data? And please remove "(solid and liquid)".
- L174: Which grid point of ERA5 did you select?
- 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?
- 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.
- L192: Please concatenate this paragraph with the next one.
- L203: interannual variability?
- Figure 3: A color of 2019-20 is different from that of Figure 4.
- L208–209: Please concatenate this paragraph with the next one.
- 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?
- L222–223: How many samples did you take?
- 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.
- 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.
- 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.
- L242–248: However, there are very large variabilities among the profiles. I suggest depicting confidence intervals in the figure and verifying robustness.
- Figure 7 and L249–257: Same things as comments #57–59.
- 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?
- 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?
- L269–270: Same as comment #62. From mid-Jan., the height of snow-cover is exceeding far away from 64cm.
- 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?
- 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?
- L298: Please concatenate this paragraph with the next one.
- L305: The "residual" is unclear here. What is the residual from?
- L311: Same as comment #67. The residual is unclear.
- L326–334: Does sublimation, ignored in this study, affect the contrasted snow height between TUNDRA and FOREST?
- L332–333: I miss this observation result in the result section. Is this the snow height at TUNDRA?
- 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?
- 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.
- 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.

- L357–359: This is an inappropriate topic sentence. Probably the content of L362–363 is a suitable topic for this paragraph.
- 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.
- L375: non-linear equations?
- 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.
- 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.
- 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.