tc-2021-255 - On the energy budget of a low-Arctic snowpack

Responses to Reviewer #2

The manuscript “On the energy budget of a low-Arctic snowpack” by Lackner et al. presents measurements and modeling of the surface energy balance for a site near Umiujaq, Canada. The authors evaluate a unique data set on the snow surface energy balance including measurements of turbulent fluxes with the eddy covariance technique. The manuscript is well written and I recommend publication in TC following revisions.

Major comments:

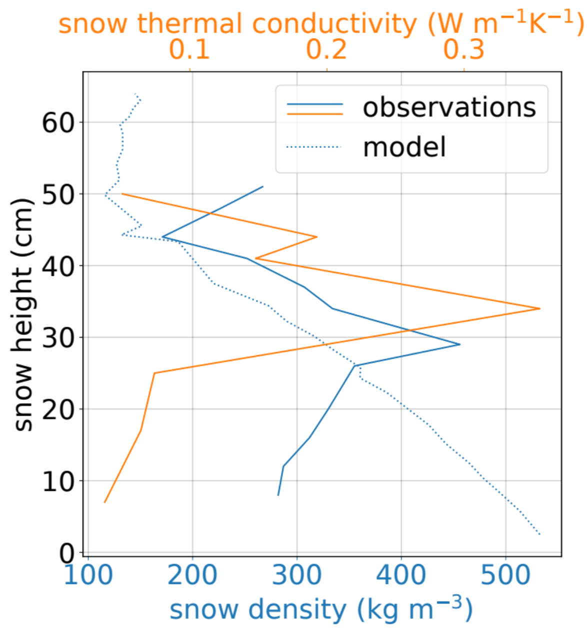
1. I am missing a dedicated section on the observed snow pack structure, including density and grain size profiles, as well as presence/absence of depth hoar, wind slab and melt layers. I am aware that such snow profiles exist only for a few points in time, but they are nevertheless important for the understanding of the snow pack processes. I am also missing an evaluation of CROCUS simulations against these snow profiles which in my view is critical for understanding model performance. In CROCUS, the snow thermal conductivity is directly related to snow density, which again is controlled by snow microstructure, wind compaction, melt, etc. Therefore, the heat flux into the snow pack and the heat storage within the snow pack are strongly related to the simulated density profile.

Thank you for recommending our manuscript for publication. We highly appreciate your helpful comments that will allow us to significantly improve the paper.

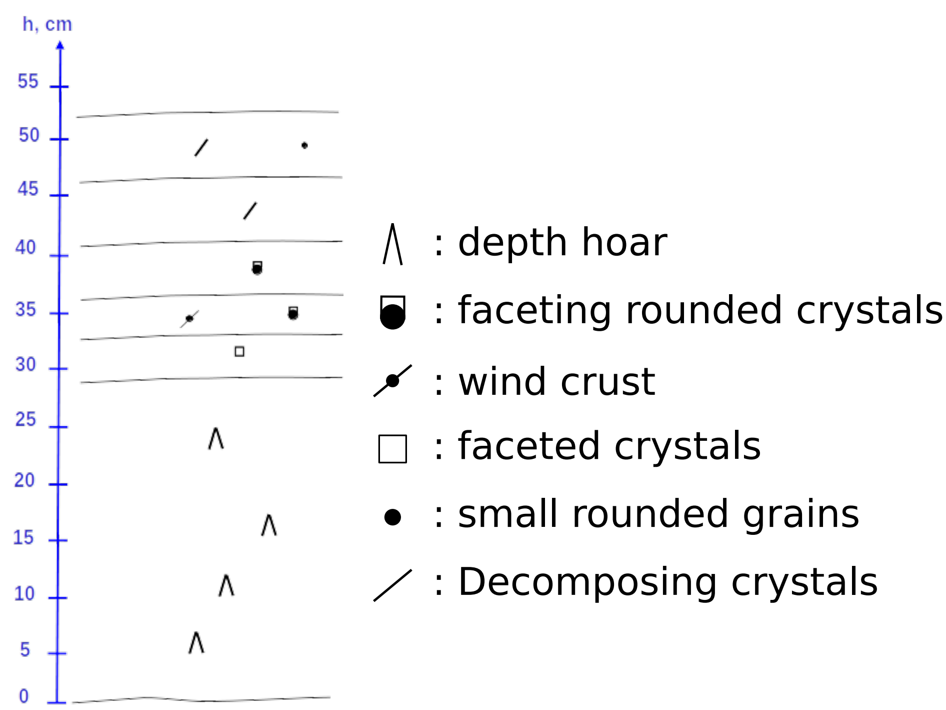
Yes indeed, information about the internal structure of the snow is important. For this reason, a second paper about this is in preparation and will be submitted soon. There, we compare the snowpack properties such as density, thermal conductivity, grain types, and snow temperature at this very site to simulations of Crocus. In order to avoid repetition, we did not include a section on this topic in this paper. However, following your concerns that some information is needed to understand the snowpack processes, we will include some figures in the supplementary material. As a density profile representative for this site is already present in the supplementary material, we will add a profile of the grain types and snow thermal conductivity as well as a simulated snow density profile (see figures below).

Concerning the evaluation of Crocus against these profiles, as mentioned above, this is the subject of another complementary paper, which examines this issue in much more detail. It has already been shown on a few instances that Crocus is not able to reproduce Arctic snow density profiles, and this study is no exception. We will add a note in section 2.4.2 that the internal properties of the snowpack were not well simulated by the model as observed in other studies:

“Similar to previous studies where Crocus was run in an Arctic setting (Barrere et al. (2017) and Royer et al. (2021)), the density profile simulated by the model did not match the observations. While the observed vertical snow density profile was rather constant with height, Crocus showed a strong decreasing trend towards the snow surface.”



Supplementary Figure 1: Profiles of observed snow density and thermal conductivity on March 22, 2019 and the corresponding simulated density profile. The stratigraphy on the same day is shown on the right.



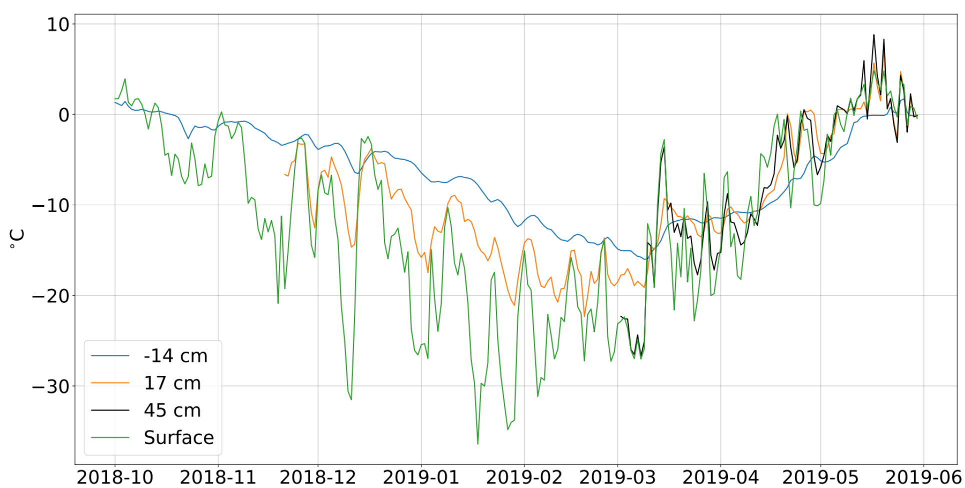
Supplementary Figure 2: Stratigraphy on March 22, 2019 corresponding to the density and thermal conductivity profiles shown in Figure 1.

Along the same lines, there should be results and a discussion on the role of the ground below on the snow energy balance. The authors refer the reader to Lackner et al., 2021, for a more thorough description of the ground properties, but there are many critical aspects that the reader needs to know, for example: Is there permafrost at the specific location of the measurements? If yes, what is the active layer thickness, if not, what is the thickness of the seasonal frost layer? Is there a water table on top of the permafrost, which first has to refreeze in fall/early winter, thus confining ground surface/snow base temperatures to close to zero degrees during this time? What is the difference between the ground surface temperature and the snow surface temperature which defines the overall temperature gradient over the snow pack? It should be possible to check all these aspects in both the measurements and the model. Some of the points raised might help explain the discrepancy in Qg (L. 332).

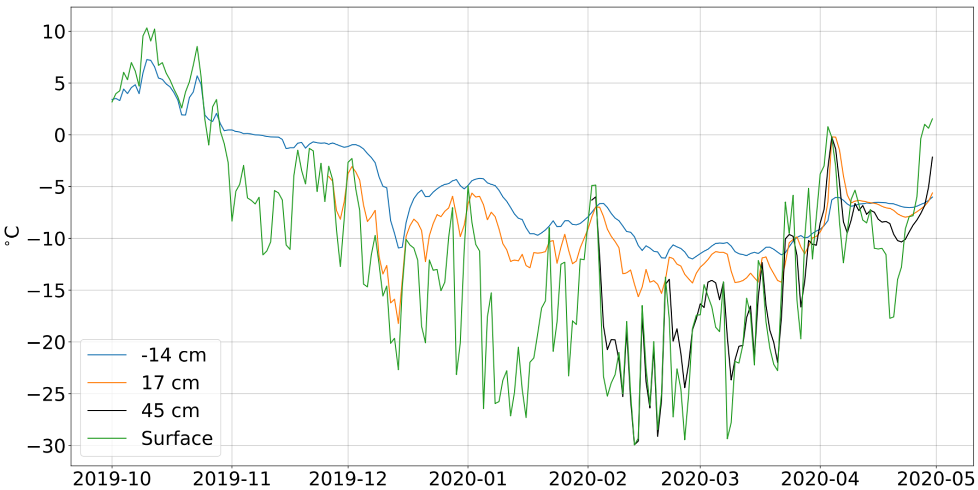
There is scattered permafrost in the valley in the form of lithalsas, but our very site is free of permafrost. We will clarify this in the manuscript:

"Permafrost is discontinuous to sparse and is rapidly degrading (Fortier et al. 2011)**, and at the precise location of the experimental setup, no permafrost was present.**"

The Figures shown below depicting the temperatures of the ground and the snow surface are going to be included into the supplementary material for winters 2018-19 and 2019-20. The deepest soil temperature measurement is at 50 cm depth (see figure below). The temperature roughly varies between –10°C in winter and +10°C in summer. Thus, the freezing depth is probably a few meters deep.



Supplementary Figure 3: Temperatures of the ground (at 14 cm depth), the snow (at 17 and 45 cm height), and at the snow surface during winter 2018-19.



Supplementary Figure 4: Temperatures of the ground (at 14 cm depth), the snow (at 17 and 45 cm height), and at the snow surface during winter 2019-20.

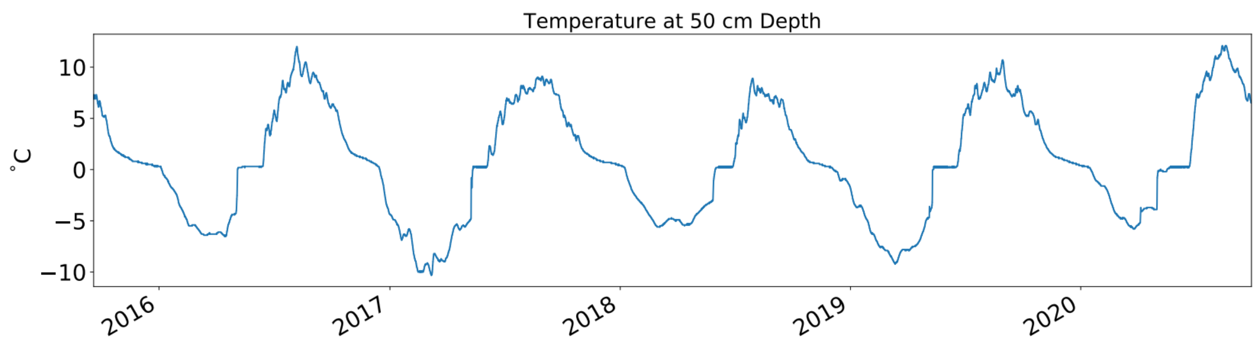


Figure: Soil temperature at 50 cm depth.

Specific comments:

1. Fig. 1: A site map would be nice, which for example shows the distance to the coastline.

An inset map will be added to Figure 1. We will specify that the site is 4 km from the Hudson Bay coast and 4 km from lake Tasiujaq.

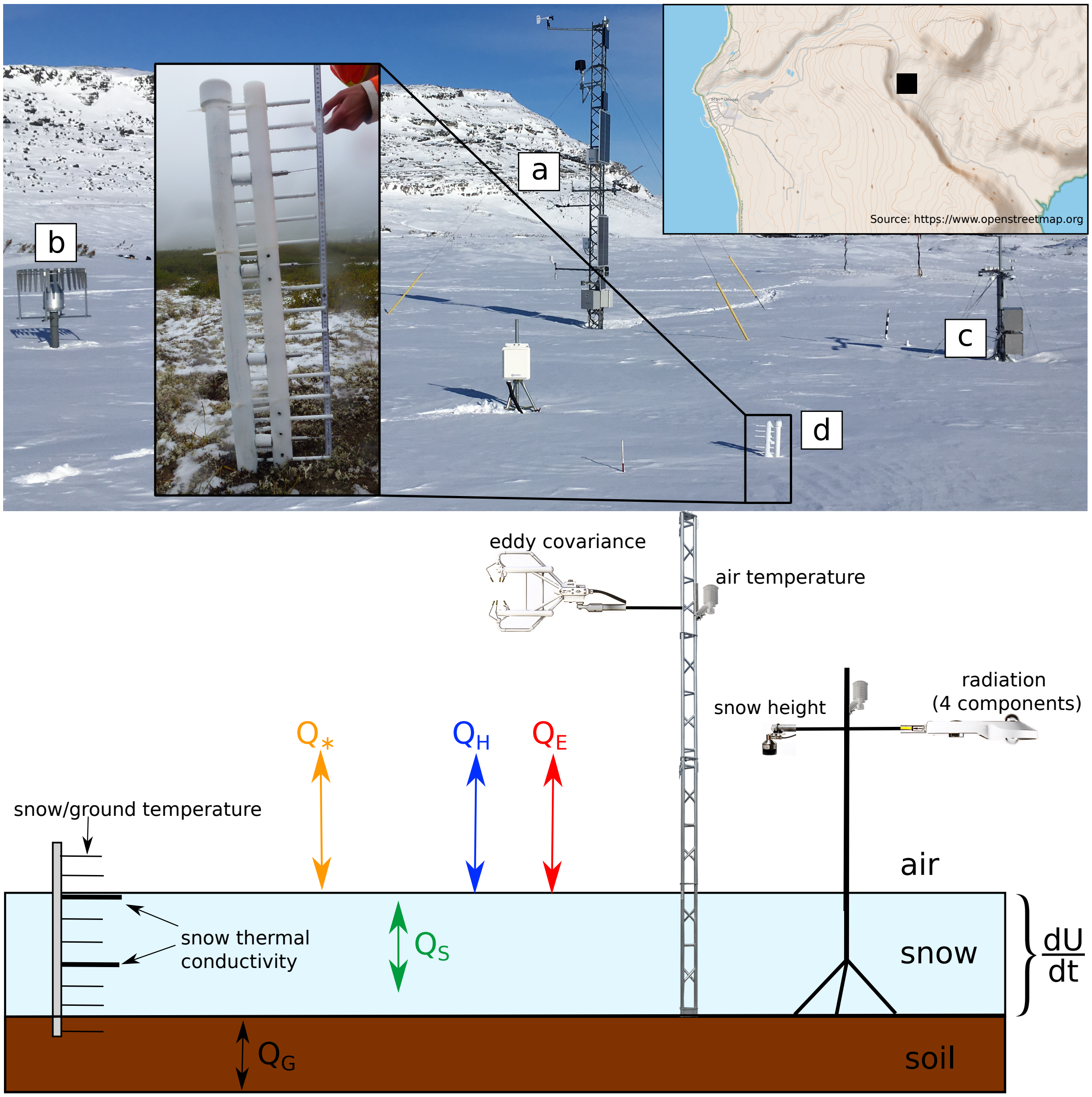


Figure 1: Upper panel: Study site with a) the main 10-m flux tower with the eddy covariance setup, b) a precipitation gauge, c) a 2.3-m high mast hosting the 4-component radiometer and d) a vertical pole holding an array of thermocouples and heated needles. The inset map shows the location of the site in the Tasiapik valley, some 4 km east of Hudson Bay. Lower panel: Schematic of the study site illustrating the main instruments monitoring energy balance terms. The whole experimental setup is contained within 20 m.

2. L30: Consider adding a clarification to Qg, like "…, i.e. the energy flux through the snow-ground interface".

We will add the recommended clarification:

" Q_G is the ground heat flux, i.e the energy flux through the snow-ground interface."

3. L75: I recommend “Here, we measure…” instead of “Here, we attempt…”. You’ve done it after all, despite the obvious difficulties!

Yes, indeed “Here, we measure” is more appropriate. We will change it, thank you.

4. L129: How often was the gap filling needed, i.e. what overall fraction of the data set is not the original measurements?

The overall percentage of gap-filled data is 44% for the sensible heat flux and 61% for the latent heat flux. Note that this percentage includes also the longer periods of instrumental failure in winter 2017/18 and 2018/19 visible in Figure 4. We will add the following statement to clarify this:

“Including longer power outages, gaps were present at 44% of the study period for sensible heat flux and 61% for latent heat flux.”

5. L139: 1W/mK seems very low, this would correspond to a rather dry soil. If the soil pores were largely ice-filled, a thermal conductivity of >2W/mK would be more appropriate. How does this assumption affect the computed heat fluxes and the comparison to the model?

Indeed, the value for thermal conductivity of the soil used here is rather low. The span of possible values is rather high in the literature and thus, we compared the heat flux in the ground to the one measured in the snow just above the soil, as detailed in the manuscript. With a value of 1W/mK, the two fluxes match in magnitude. A value of 2 W/mK or higher would at least double the heat flux in the ground and it would therefore be far higher than the measured heat flux in the bottom of the snow and the modeled soil heat flux. For this reason, 1 W/mK seemed the most appropriate choice. There is a reasoning supporting the selection of this value. The water content of this sandy soil is very low, as the large grains and the large pores retain little water. Therefore, there is probably little ice, leading to a lower thermal conductivity than expected for frozen soils with smaller grains.

We will add a corresponding note in section 2.3.2:

“We compared the resulting ground heat flux to measurements of the snow heat flux 7 cm above the soil to test our selection of thermal conductivity.”

6. L156: What does an error of 0.75% mean for temperature? In which unit is temperature referred to here?

We will change the specification of the error to °C :

“[...]while the temperature measurements **of the type-T thermocouples have an accuracy of 0.5°C in the considered temperature range**.”

7. L158, Sect. 2.4.1: Briefly describe the physics of the ground module that is used in the simulations and as such provides the lower boundary for the CROCUS model. Some of the text from l. 179 could be moved to this description.

We will add a brief description of ISBA, the soil model coupled to CROCUS:

“The soil and vegetation model ISBA is coupled to Crocus and simulates all water and energy exchanges between the different soil layers and with the snowpack above the ground. For this purpose, the one-dimensional Fourier law and a mixed-from of the Richards equation are solved explicitly (Boone et al. 2000; Decharme et al. 2011). The characteristics of the vegetation are selected from a list containing 19 different vegetation types (ECOclimap; https://opensource.umr-cnrm.fr/projects/ecoclimap-sg/wiki) using the site coordinates, or alternatively they can be specified by the user. In this study, the latter option was used.”

8. L284: Please introduce Q* again for clarity. It is not used in the paragraph on net radiation, so readers have to go back to the initial sections if they are not familiar with the symbol.

Good point. We will change the sentence to:

“Daily means of the **turbulent heat fluxes** Q_H, Q_E, and **the net radiation** Q_* time series simulated by ISBA-Crocus [...].”

9. L315: Briefly state what the Qa with the arrow means.

A clarification after the equation will be added:

“[...], **where the advective heat input Qa is neglected.**”

10. L332: Here, an uncertainty analysis on the different factors used to calculate Qg, especially the thermal conductivities, could help. See also major comment 2.

Given the fact that the thermal conductivity was already chosen on the lower bound of the possible values, it is very unlikely that the discrepancy between the observations and modeled values can be attributed to uncertainties of the observations, as observations are already much higher than modeled values. We will add a sentence mentioning the comparison between the heat flux in the bottom of the snow pack and the ground (see comment 5).

11. L339: Switch order of references.

The order will be switched.

12. L364: Consider adding a statement on the timescales. The authors write themselves that the model is doing better for longer periods so that some applications of the model may be less compromised than this statement suggests.

A statement on the time scales will be added to the sentence:

“Meanwhile, further studies are underway at the site targeted in this study to evaluate optimal turbulent heat flux parametrizations, **particularly for sub-daily time scales.**”

13. L365: I am missing three aspects in the section on sublimation and drifting snow. First, the percentage of SWE lost from sublimation also depends on snowfall/total SWE, so this aspect should be considered when comparing to previous studies (L. 370). Second, the authors should at least qualitatively comment on the intensity of the snow drift events. The daily average wind speeds (Fig. 2) seem fairly low and only marginally above the limit for snow drift of 5-6 m/sec, as e.g. assumed in

Crocus. In particular, prolonged storms with wind speeds >>10m/sec where snow drift is much more intense seem to lack completely. If correct, this could partly explain why the measurements do not show a higher sublimation. Finally, blowing snow events do not strongly change the constraints on energy availability and humidity that also apply for sublimation from flat surfaces, as mentioned in the manuscript. For very cold air and snow temperatures, for example, the humidity at saturation and thus the potential vapor deficit of the air are small which limits the latent heat fluxes and thus sublimation. The same is true for very moist air.

Thank you for the comment. It is true that the percentage of mass lost due to sublimation depends on total snowfall which is rather high at our site. We will add a corresponding phrase to the manuscript:

"**On the other hand, the winter precipitation is quite high at our site, which naturally decreases the fraction of sublimation losses to precipitation.**"

In L. 372 we state that blowing snow events are observed several times per week on time lapse cameras as the snow height decreases during these periods. Unfortunately, we cannot further specify the amount of snow blown away by the wind as we did not have instruments for this.

Right, blowing snow does not strongly change the constraints from the water vapor deficit and the air temperature on sublimation but as detailed in the manuscript, Mann et al. (2000) showed that due to the high density of snow particles in the air, it can become almost fully saturated. This represents conditions different to those found above a flat surface. Sublimation during blowing snow is still an active area of research with large uncertainties.

14. L416: This is an important finding which inspires confidence in the results and should thus be presented in more detail in the Results section. See also my comment L. 139.

A sentence highlighting the comparison between the heat fluxes in the bottom of the snow and the ground will be added to the manuscript in the results section. See comment 10.

15. References: the doi link to Lackner et al., 2021, points to a different paper

Well noticed! The correct doi link is the following (doi.org/10.1175/JHM-D-20-0243.1) and it will be integrated in the new version of the article.

References

Barrere, M., Domine, F., Decharme, B., Morin, S., Vionnet, V., & Lafaysse, M.: Evaluating the performance of coupled snow–soil models in SURFEXv8 to simulate the permafrost thermal regime at a high Arctic site, *Geosci. Model Dev.*, 10, 3461–3479, https://doi.org/10.5194/gmd-10-3461-2017, 2017.

Boone, A., Masson, V., Meyers, T., & Noilhan, J. The influence of the inclusion of soil freezing on simulations by a Soil–Vegetation–Atmosphere Transfer Scheme, *J. Appl. Meteorol.*, 39, 1544-1569, doi: https://doi.org/10.1175/1520-0450(2000)039<1544:TIOTIO>2.0.CO;2, 2000.

Decharme, B., Boone, A., Delire, C., & Noilhan, J. Local evaluation of the interaction between Soil Biosphere Atmosphere soil multilayer diffusion scheme using four pedotransfer functions, *J. Geophys. Res.*, 116, D20126, doi:10.1029/2011JD016002, 2011.

Fortier, R., LeBlanc, A.-M., & Yu, W.. Impacts of permafrost degradation on a road embankment at Umiujaq in Nunavik (Quebec), Canada. *Can. Geotech. J*. 48(5): 720-740. https://doi.org/10.1139/t10-101, 2011.

Mann, G. W., Anderson, P. S., and Mobbs, S. D. Profile measurements of blowing snow at Halley, Antarctica, *J. Geophys. Res.*, 105 (D19), 24491– 24508, doi:10.1029/2000JD900247, 2000.

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.

Mann, G. W., Anderson, P. S., and Mobbs, S. D. (2000), Profile measurements of blowing snow at Halley, Antarctica, *J. Geophys. Res.*, 105(D19), 24491– 24508, doi:10.1029/2000JD900247.