The Cryosphere Discussions

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Comment on tc-2021-255

Anonymous Referee #2

Referee comment on "On the energy budget of a low-Arctic snowpack" by Georg Lackner et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-255-RC2, 2021

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:

- 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.
- 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).

Specific comments:

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

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

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

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

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?

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

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.

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.

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

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

L339: Switch order of references.

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.

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.

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.

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