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

Responses to the reviewer #3

Overall comments:

Overall this paper should be published. There are earlier papers looking at snow energy balances in the subarctic/low arctic (I refer to Anthony Price' early work in the Schefferville area when he was a PhD student at McGill). In the Lackner et al paper there are notable discrepancies between the real world and the model. Though I thought the discussion was quite good it might be an idea to add a few words directed at the model and why it doesn't seem to model Qh well. There is acknowledgment of this but I wondered if the authors, after their experience with this data set have suggestions to better the model? I have made a few comments below some out of curiousity and others more specific. In tundra environments herbs and shrubs in the snowpack can play a role in the energy balance at I assume they can...especially in the late winter when the sun is becoming more intense and in the early spring as they absorb radiation (I understand coniferous plants can photosynthesize under the snow surface).
Overall I would accept the paper with some minor modifications.

Thank you for your positive comments on our manuscript.

Crocus depends on complex relationships between snow, atmosphere, vegetation and soil. Since the model has been little compared to observations of the surface energy balance in the Arctic environment, we believe that the first logical step, before any proposal for improvement, is to perform a rigorous evaluation of its performance highlighting its strengths and weaknesses. This is thus the main objective of the modeling part of the paper.

It should be noted that some of our instruments (e.g. snow temperature measurements) were deployed in a shrub-free zone, thus preventing us from targeting their impact on snow properties. Also, all the shrubs in the valley are deciduous, so there is no photosynthesis in winter. Finally, we wish to remind the reviewer that we excluded spring from our analyses, where the contribution of shrubs to e.g. albedo is certainly more important than in wintertime.

Below, we answered all your comments and have proposed modifications when requested.

Specific comments:

1. Page 3: line 50: they measured ground heat flux under the glacier? Is that right?

No, at the glacier site the ground heat flux was not measured.

2. Line 53: small point: would be slightly clearer to say that 50% of the winter precip is lost to sublimation

Indeed, thanks. We will change the wording to:

“[...], **water losses due to sublimation** in the Arctic can make up to 50% of the total winter precipitation.”

3. Line 59: though in the subarctic, the earlier study by Price (PhD at McGill) was a detailed energy budget of a snowpack. (Water Res. Res Vol 12:4: 686-694)

Thanks for the reading suggestion, it is indeed a very interesting article. Unfortunately, we believe it is best not to cite it here for a few reasons:
- the authors are interested in a forest site and here we are aiming for a tundra;
- the period under study is spring whereas here we are targeting winter;
- not all components of the energy balance are measured and therefore there are no conclusions on the turbulent heat fluxes, which occupy a prominent place in our article.

4. Page 7: line 145 or so: as the density of the snowpack changes the amt of air space changes...would this not have an impact on calculating the heat capacity of the snowpack?

Good point, thanks. Only the heat capacity of the snow (ice) was used for the calculation here, as the air space and the associated heat capacity only make up a negligible fraction of the one of ice.

5. Page 8: Line 179: 12 m or 1.2m? earlier you state that you have thermocouples at 4 and 14 cm...so not sure what you are doing here...assume this is a typo

Yes, the thermocouples were installed at 4 and 14 cm depth, but the 12 m soil column refers to the model. Here, a thick soil column is needed in order to also include heat storage effects of deeper soil layers. No measurements are present at these depths but as stated in L. 183, a spin-up of 5 years was used to obtain an equilibrium of the thermal regime in these deep layers.

6. Line 180: did you measure field capacity in the lab

No, unfortunately no such measurements were performed. However, we used time series of the soil water content at several depths to estimate the field capacity and the saturation water content. The field capacity was then taken as the values of the soil water content some time after rain events.

7. Line 194: a small question (though it probably makes little difference) do you adjust specific heat wrt temp – I assume you are but would it make much difference? Thinking here too about air in the snowpack

L. 194 refers to the calculation of the turbulent fluxes in the model Crocus and to the best of our knowledge, the specific heat of the air is not adjusted for temperature variations. But as you mentioned, it should not make much difference. In the snowpack the air space is not included as detailed in comment 4.

8. Figure 2: Probably quite explainable...however there are places here where your snowpack drops significantly over what appear short periods of time...wind? (coupled with compression) -for example 2018-19 late Jan there is a snowfall (i assume) and on or about 7 FEb there is a sudden drop of snowpack height from about 55cm to 40cm (or

so)/ as well in 2019-20 late Feb snowpack is about 75cm or so and by mid March around 60cm...significant wind at this time..assume it is wind scour

Yes, in fact blowing snow is a common phenomenon at the site and wind transports the snow to a site further down the valley where snow height is significantly higher. In L. 371/372 the approximate frequency of blowing snow is stated:

"[...] blowing snow events, which frequently occur at the site (observed several times per week on time-lapse images) [...]"

A paper comparing the snow heights of this site with another one further down the valley is in preparation. There we plan to study this phenomenon closely.

9. p10. Line 255: I assume that this pattern of precip is tied somewhat into the proximity of Hudson's Bay....does the drop in precip in December tie into ice covering a large part of the bay?

We also think that this precipitation and wind pattern are strongly influenced by Hudson Bay. As stated in L.222, Hudson Bay freezes around mid-December and subsequently wind speed and precipitation rates drop. Thus, one can assume that this is due to the freezing of Hudson Bay. We will add this to L. 255:

"Just as for the wind speed, precipitation rates **are presumably also influenced by Hudson Bay and** drop at the end of December and remain rather low until March, [...]."

10. Figure 3: net radiation in 2018-19 in early January show a slightly positive balance out of curiousity what is happening here; similarly 2019-20 in early March; in both cases longwave in and out is balanced ...significant cloud cover? Thinking that in years ahead with more of Hudson bay staying open longer there may be increased cloud cover...might be interesting to speculate how this may play a role in the energy budget of these low Arctic snowpacks?

A significant cloud cover in combination with relatively warm air temperatures (still below 0°C) was responsible for the peaks in net radiation. There might be an effect of climate warming and Hudson Bay being open for longer periods on cloudiness but our site is already very cloudy so it is hard to speculate in this topic based on the experiences from this site.

11. Figure 6: you are inferring a linear relationship here...is there any point? are these relationships significant? What might be interesting is to look at (for example) in (b) at Ts-Ta 1°C to about 2.3°C the range of Qh is very large, though focused primarily between ~+50 and -45 W/m2...for more or less similar Ts-Ta values you get a very large range of Qh: is there anything of interest here: similarly for (d) between ~.15VPD and .18 VPD a very large Qe range

In many equations used to calculate the turbulent fluxes in land surface models, wind speed, Ts-Ta and VPD enter linearly, as can be seen in equation 6 and 7 in the manuscript. Thus, here we wanted to compare those quantities with observed fluxes and look whether the linear dependence can be observed.

12. P 14 line 280: are these relationships statistically significant? Though when looking at these

relationships the important thing is the visual message that the model in Qh and Qe under/overestimates for a reasonable range of the W/m2 range

We agree, the importance of Figure 7 is the visual message that there are some issues with the performance of the model over the observed heat flux range. We therefore did not test the statistical significance of the model.

13. P15 Line 301-302
You refer here to residual snowpack heat flux. In our experience in subarctic and low arctic snowpacks there can be a notable concentration of coniferous shrubs that absorb energy and appear to be photosynthesizing (we were not measuring this but colleagues mention this goes on). Is the snowpack in any way impacted by energy absorption by shrubs in the snowpack at all? Seems it might...of course depending on the characterisitics and density of the shrubs. I see no mention here of shrubby veg...so assume this isn't the case here?

The occurring shrub types here, mainly dwarf birch (compare L. 84-86), are all deciduous shrubs, so there is no photosynthesis in winter. The shrubs probably have an impact by absorbing radiation, which then heats up the snowpack. This, however, is more crucial in spring (April, May and June) when the shortwave radiation is much larger and the twigs of the shrubs are closer to the snow surface or even stick out of the snowpack. Furthermore, a significant bending of the shrubs was observed in the snow pits, which were dug in areas covered by shrubs.

14. P 15 line 294: would the sensors be in any way impacting the energy balance? as they are close to the
surface of the snowpack/ what about blowing snow along the surface...impact the ability of the sensors in any way?

Indeed, it is hard to measure the temperature of the snow close to the surface as the sensors may suffer from solar loading. We discuss these limitations in lines 393 to 397:

For this reason, we used only measurements where the sensor had a certain distance (3-5 cm) to the snow surface. Also, the sensors were enclosed in a white tubing to minimize radiative effects, as shown in Figure 1. But obviously certain influences cannot be ruled out making the snow heat flux very hard to measure accurately.

15. Figure 7. so with Qh you have wide scatter in the model. might be interesting to look at some observed values of Qh and investigate the very large range of modelled responses...to isolate what other variables might be playing a role in producing a large range/
for example at ~+10W/m2 (observed) you have a range of modelled responses between ~ -5 and +35 ....in the upper range of the modelled responses versus the lower range were there any standout differences in other variables (wind speed?)

Atmospheric stratification is a very complex subject that is not very well captured by the model and consequently it is the largest source for the scatter in Figure 7. Also, we checked some variables such as temperature and wind speed and they already showed substantial scatter for observed values around 10 W m-2 of Qh, so no variable could be identified that causes the large scatter in the model.
The same is true for very high modeled values.

16. Figure 7 and Figure 8
I see why you have included Fig 8 but there are some interesting differences between these figures which leads to a couple of questions. 1. In Fig 8 where you are confident you have good control on all measureable variables there is very good agreement between the modelled and observed Qe. However in Fig 7 there is consistent underestimation (I think I am reading this correctly). So what did you learn (and what could we learn) from this discrepancy with respect to the model and where the issues are?
2.With respect to Fig 7 if it wasn't for a number of values >25 W/m2 or below -5 W/m2 the relationship could well be close to vertical as I would guess 90+% of the values fall within the large lump. In Fig 8 the model again overestimates under very good controlled conditions. Any idea why the model seems to overestimate sensible heat? Any physical explanation?

1. During the period shown in Figure 8, the conditions were favourable for the measurements, including no issues related to blowing snow. Thus, our confidence in the measurements is higher. But most importantly, in Figure 7 the bulk of the point is close to 0 (between –10 W m^{-2} and 5 W m^{-2}) thus, even with the underestimation of the model, the absolute difference between the observations and simulations is in the order of a few W m^{-2}, so rather small. This small difference is visible in Figure 8. We have chosen to present all fluxes in Figure 8 within the same range, unfortunately in the case of Qe this choice negatively affects the ability to compare modeled and observed Qe in more detail, but the differences are in the same range as the ones presented in Figure 7.

2. Even for the bulk of the points the relation between observed and modeled is not vertical, in fact, for the range between 0 and 10 W m^{-2}, the correlation is quite good with a slope close to 1 as indicated by the black line in Figure 7. For higher values, there is a substantial overestimation, it seems that the snow surface temperature difference between the model and the observations tends to be higher than the average difference. Thus, a possible reason might be that the simulated snow surface temperature is a cause for the high values.

17. P17. Line: 315: I assume crossing out the Qa term simply means that while you identify it is important it is not included here...the arrow to the zero?

The arrow over Qa means that although we acknowledge the advection to be part of the energy budget, we assume it to be sufficiently small to be neglected. And even in periods where this term is higher, we cannot measure it and have to neglect it. We will add a statement clarifying the meaning of the arrow:

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

18. P17 Line 326: so you have more energy coming in than can be accounted for in heat loss from the system/ possibilities? (a bit of guess work here)underestimating energy used to raise the temperature of the ice in the snowpack to melting point? Early in the melt as melting water infiltrates the frozen ground (is it?) the runoff refreezes complicating the issue? Some issue with calculating heat loss from the snowpack re: turbulent fluxes.....and going out on a limb here but are there periods when you may have laminar flow and underestimating heat loss// later I see that isn't an issue...but thought I would leave this comment in

It is very hard to say where we miss energy fluxes. Issues related to melt can be ruled out as we did not include periods when melting occurred. In fact, this was an important reason for the choice to leave out these periods. Turbulent fluxes are a probable source for missed energy. Even in summer, most energy budget studies report unclosed energy budgets where more energy is coming in than going out. During the winter conditions here, measuring the turbulent fluxes is even more challenging with the atmospheric stratification being mostly stable (see Table 1).

19. P17 line330: the phrase: “slightly less”... are you being generous using this term especially for Qh? re: fig 7

We will change the wording here to:

“[...] is well simulated, but this is less true for [...]”

20. P17 L333: issue re: observations of instrument location an issue...

The location of the instrument does change the measured ground heat flux, but given the large discrepancy between the observed and modeled ground heat flux the location of the instrument is likely not the dominant factor here.

21. P 19: line spelling of recommend

Thanks, we will correct this typo.

22. P20 Line 397: again, if herbs and shrubs are present are they playing a role here?

For the measurement of the snow heat fluxes at this site the shrubs don’t play a role as the instrument was placed on pure lichen (compare L. 112).

23. P21 line 436: spelling of word: consisted

Again, thanks for the remark. We will fix the typo.