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

Responses to Reviewer #1

Overall Comments:
The authors have collected a nice dataset and have produced a potentially very informative and useful paper, examining the snowpack energy balance in a low-Arctic snowpack. I appreciate the difficulty of working in such an environment and I believe that the measurements and modelling have the ingredients for a paper that warrants publication. However, I think there needs to be some additional analysis in order to obtain the best possible interpretation of the data. […] There are not a lot of comprehensive energy budget studies on low-Arctic snowpacks and so making the most use of the data would result in a more useful paper.

Thank you for your encouraging words. We highly appreciate your comments, which will clearly help us improve our manuscript. Please find our answers to your comments below.

1. Some of the time series presented would be much easier to interpret if temperatures were included. Important temperatures would include, air temperature, snowpack surface temperature, average or bulk snowpack temperature, and soil temperature at 4 and 14 cm depth. Soil temperatures would provide an indication of when the soil water was freezing.

We agree with you and plan to add the requested time series. Please see our response to comments 18 and 19.

2. It would be helpful to be able to either see the separate shortwave and longwave radiation balances, or to augment the net radiation with observed and simulated albedo values.

Please see our answer to comment 18.

3. The authors need to state whether the precipitation data were corrected for gauge undercatch and if so, describe the procedure. The precipitation data cannot be used to force a model in their raw, uncorrected state.

Please see our answer to comment 15.

4. Some time series of simulated SWE (with points for late winter observations) and simulated and observed snow depth would be interesting. Since there is no mid-winter melt happening, I am interested in knowing how the observed snow depth time series are affected by snowfall events, whether some depth increases are caused by snowfall or drifting, and whether depth decreases are caused by drifting, sublimation or settling and wind packing. Definitive answers may not be possible but evidence in the data may produce some answers. There are not a lot of comprehensive energy budget studies on low-Arctic snowpacks and so making the most use of the data would result in a more useful paper.

We understand very well your curiosity to learn more about other aspects of Crocus modeling, which have more to do with the properties of the snowpack itself, and not only the energy balance simulation.

In fact, there is so much to say about this that we have compiled all our analyses in second paper that will be submitted shortly to *The Cryosphere*, and that will echo this one. In connection with comment 12, we agreed that it is relevant to compare an observed density profile with a simulated one, and still plan to present some answers to these questions, but which will be substantiated in the upcoming paper.

Specific Comments:

5. Line 7-10: I find the following a little confusing: "At the snow surface, the heat flux into the snow is similar in magnitude to the sensible heat flux. Because the snow cover stores very little heat, the majority of the heat flux into the snow is used to cool the soil." I understand that the sensible heat flux is usually downward and I assume that the heat fluxes calculated from the temperature gradients near the top of the snowpack showed a similar heat flux. However, I find "the majority of the heat flux into the snow is used to cool the soil" to be confusing. A downward sensible heat flux into the snow would not cool the soil. The upward soil heat flux into the snowpack would cool the soil. I would reword this part.

This wording is confusing indeed. We will make the following modifications to the sentence (in bold):

"[...] the majority of the **upward** heat flux **in** the snow is used to cool the soil."

6. Line 15: One could surmise that the flora and fauna as well as the local populations have adapted to the conditions, which is why there is such concern about changes to the environment affecting the flora, fauna and the traditional way of life of the local inhabitants.

We will modify the sentence to illustrate the dynamic nature of these changes and the constant search for equilibrium that ensues (modifications in bold):

"[...] conditions to which local populations, flora, and fauna **are adapting**."

7. Equations 1 and 2: If Qs is derivable for equation 1, it could inform the results from equation 2.

Yes, deriving Q_s from equation 1 can inform the results from equation 2. However, Q_s is further partitioned into the rate of change of internal energy of the snow dU/dt and the ground heat flux Q_g, for which equation 2 is still needed.

8. Line 40-42: I agree that lack of energy balance closure in eddy covariance systems is not restricted to Arctic environments or winter conditions. However, the ability of eddy covariance systems to measure fluxes has been documented in many papers as being severely limited under periods of low wind speed and strong stability, which damps turbulence. The prevalence of such conditions may therefore affect the degree to which observations at a given site are affected, even if the underlying mechanisms are the same. Was energy balance closure worse under clam, stable conditions? Figure 1 appears to show a ridge close to the site and I wonder whether drainage flows are affecting the energy imbalance because of the topography.

As we illustrate in Table 1, the cases where the atmosphere is stable ($\mathrm{Ri}_b > 0.25$) are relatively marginal (11.4% of the time), as the site is rather windy. As such, a clear trend of lower energy balance closure during calm wind conditions has not been observed. Even though the eddy covariance system is a source of uncertainty, the other heat flux measurements (e.g. in the snow and ground) are associated with an uncertainty similar to if not greater than the eddy covariance system itself during the winter conditions presented here. Thus, a lower closure not only arises just from problems with the eddy covariance system and thus, there is no clear relation with calmer wind conditions.

We recently published a paper looking at the summer energy budget at this site (Lackner et al. 2021). We indeed observed a clear drop of the energy budget closure when the winds came the ridge, probably because we then find ourselves in a recirculation zone that is hardly compatible with the assumptions underlying the application of the EC approach.

9. Line 95: The authors should specify that the CO2/H2O analyzer is an open path system which may experience interference from snow and blowing snow.

Good point, we will expand on this (modifications in bold):

“The setup included a 10-m flux tower equipped with a sonic anemometer and a CO_2/H_2O gas analyzer located 4.2 m above ground (IRGASON, Campbell Scientific, USA) on a 5°- slope with a SE aspect. **Due to the open-path nature of the EC sensor, it was subject to interference in the presence of precipitation and during blowing snow events.**”

10. Line 111: Is a 10 cm spacing of thermocouples starting at -4 cm sufficient to compute the ground heat flux accurately?

Indeed, more refined spacing, as well as deeper soil measurements would have been desirable. For example, in a study at the same site but covering the snow-free period, we estimated the ground heat flux using three ground temperature measurements, at a station some 15 m from the one used here (see Lackner et al. (2021)). Even though there are only two measurement levels, they have the advantage of being directly below the snow temperature profile measurements, which ensures consistency in our analysis. Also, as mentioned in the manuscript, we measured the heat flux at the very bottom of the snow cover and compared it to the ground heat flux, and we found that both fluxes agreed very well. This suggests that the method we use is sound.

11. Lines 125-130: Was coordinate rotation applied to the eddy covariance data to account for the slope? A brief summary of procedures for processing and QA/QC would be informative and useful. Were any u* thresholds applied?

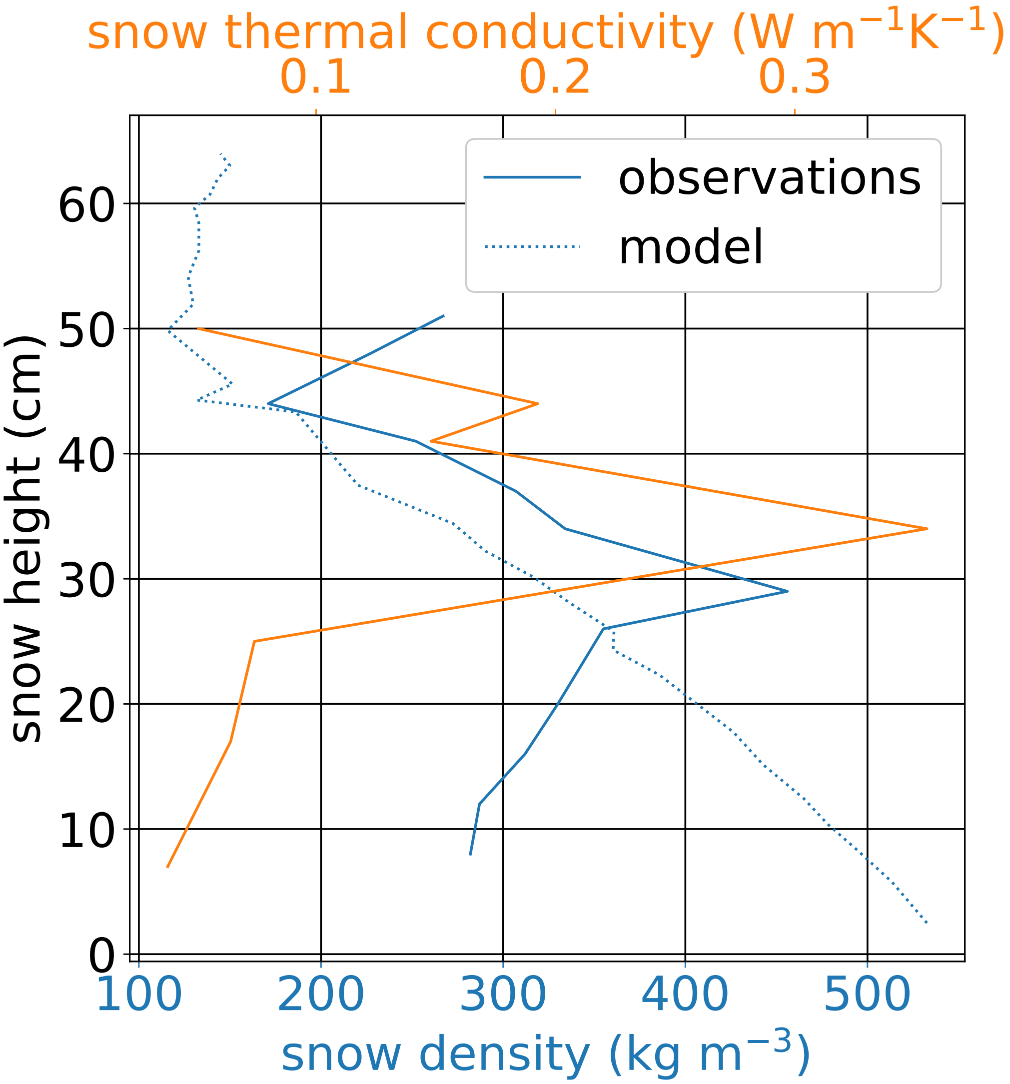
Indeed, we applied the double rotation method to the raw turbulence data. The planar fit method was tested as well, but yielded occasional large spikes and unrealistic results. We surmise that the presence of snow cover constantly changes the surface, making it impossible to use the planar fit method. The processing routine with EddyPro handles an important part of the QA/QC processing, the rest being done by the PyFluxPro program. As for the u* threshold, no filter was applied to the turbulent fluxes in order to maximize the amount of data available for the analysis. To make this information more obvious, we will add the following information (in bold):

“A detailed explanation of the procedure for obtaining the turbulent heat fluxes from raw eddy covariance data is provided by Lackner et al. (2021). In short, turbulence data were processed using EddyPro® (ver- sion 7.0.3; Li-COR Biosciences, USA), a software package that computes fluxes from raw 10 Hz data, while accounting for several corrections, **including the application of a double rotation on the raw data to align the coordinate system with the current snow surface. EddyPro**

also includes a thorough QA/QC procedure. A program called PyFluxPro (Isaac et al., 2017) was also used to remove spikes and erroneous data that persisted despite the EddyPro® processing."

12. Line 147: What does the model suggest for the snowpack density evolution? How much did it vary from year to year in the snow pits and in the models? I see later that the error is considered greater than assuming a constant density but the sign of the error and reasons are not discussed.

Snow models such as Crocus intrinsically have difficulty simulating the physical properties of Arctic snow (vertical density profiles, stratigraphy, etc.) as demonstrated by many past studies (Barrere et al (2017), Gouttevin et al 2018 and Royer et al 2021). In our case, this is shown by comparing an observed density profile with the density profile simulated by Crocus (see Supplementary Figure 1).



Supplementary Figure 1: Profiles of snow density (blue) and thermal conductivity (orange) at the 22 March 2019. The simulated density profile at the same time is also shown.

13. Line 156: I would not classify a temperature error as a percentage. Is that in °C or K? I suspect that a percentage error for thermocouples would refer to the temperature difference between the thermo-junction in the snow and the reference temperature junction. An error in the accuracy of the reference temperature thermistor would likely be expressed in fractions of a degree Celsius over a range of temperatures.

We agree. We will change our description of the error (modifications in bold):

"[...]while the temperature measurements **of the type-T thermocouples have an accuracy of 0.5°C in the temperature range under study**."

14. Line 176: Again, a percentage error in a temperature is difficult to interpret.

Please see comment above.

15. Line 178: Is the error for precipitation 0.15 mm per half-hour, 0.15 mm per precipitation event, or 0.15 mm per time interval during which precipitation was recorded? Okay I looked it up. Accuracy is specified as 0.1% of full scale, and 0.15 mm is the repeatability, while sensitivity is 0.1 mm. If the 1500 mm version of this gauge was employed, then the accuracy is 1.5 mm over a season, based on what actually entered the gauge and closer to 0.15 mm per event, but this does not account for snow undercatch caused by wind deflection around the gauge. Were the snowfall data corrected for undercatch of snow. There are equations available for correcting this gauge with a single Alter shield based on wind speed at the gauge height. Smith (2006) found that a Geonor T200B with a single Alter shield caught only 36% of the snow caught by a Double Fence Intercomparison Reference (DFIR) gauge (the WMO standard) in Bratt's Lake Saskatchewan. Were the precipitation gauge data corrected for undercatch, and if so, which equation was employed?

Yes, the precipitation data were corrected for undercatch. We used the equation from Kochendorfer et al. 2017. The following sentence will be added to the manuscript:

"**Precipitation data were corrected for undercatch of solid hydrometeors using the transfer function of Kochendorfer et al. (2017), which depends on wind speed and air temperature.**"

Concerning the error for precipitation, in Kochendorfer et al. (2018), they report an RMSE of 0.25 mm for the 1500 mm gauge used here, after correcting the data for undercatch. Thus, we feel that this is the best way to state the error, as it includes both the accuracy of the gauge and the data post-processing. Note that we initially reported an error of 0.15 mm, which was incorrect and will be corrected in the next version.

16. Figure 2: Do the tick marks for each month represent the start of the month or the mid-point? It appears to be the start.

To clarify this, the following sentence will be added in the figure caption: "Ticks marks on the x-axis indicate the start of each month."

17. Regarding discussion of sensible and latent heat fluxes: Rather than using the terms 'increases' and 'decreases', it may help to include direction, such as strong upward or strong downward or weak upward or weak downward fluxes.

Good point. Indications of the direction of the fluxes will be added in the discussion on the sensible and latent heat fluxes.

18. Line 298-302: Errors in the simulated snowpack albedo could cause differences in daytime net radiation, and this could be checked and plotted, although I suspect there is more error in the longwave component. A phase shift may be a result of poor thermal conductivity simulations or issues related to simulated fluxes and stability corrections. Figure 8 would be easier to interpret and would be more informative if air temperature and the radiative skin temperature of the snow surface (based on outgoing longwave radiation) were also plotted. QG while not at the surface, could inform Figure 8.

The modeled albedo was in the same range as the observed albedo with only slight differences (mean difference of 0.0006 for winter 2017-18). Such small differences do not strongly affect the energy balance in winter as the incoming shortwave radiation is not that high. Thus, we agree that the longwave component is more important here.

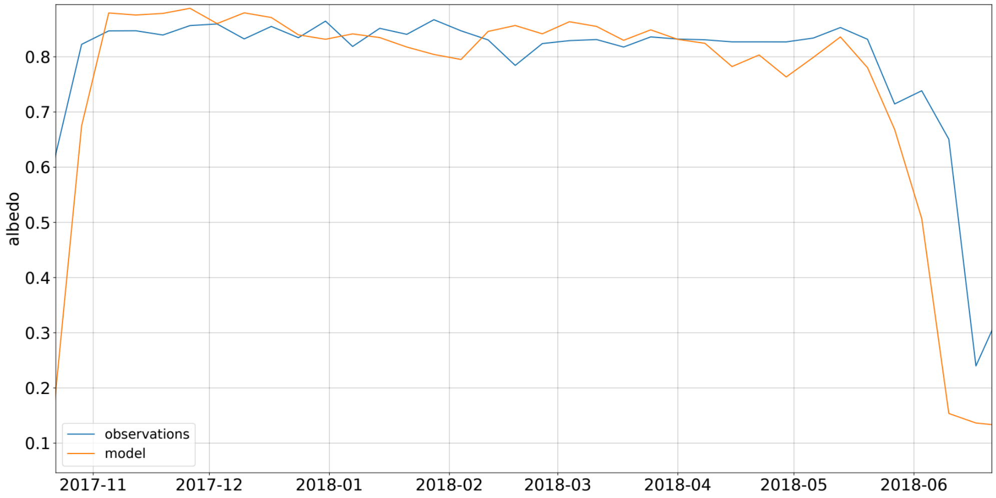


Figure: Observed and modeled albedo during winter 2017-18.

We will add a note to the manuscript describing the difference of the modeled and observed albedo:

“**During the period shown in Figure 8 and throughout the study period, the modeled albedo was always in the same range as observations (differences <0.05) with a mean difference between both of less than 0.01**.”

Furthermore, since thermal conductivity depends on snow density, the fact that the density profile is not modeled correctly certainly brings its own set of errors, as shown in several other studies (e.g., Barrère et al. 2017; Royer et al. 2021).Thus, part of the phase shift could be due to this error in thermal conductivity. A note stating this possibility will be added to the manuscript:

“At certain moments, a few hours of phase-shifting between the residual and the snow heat flux can be observed, **which might be due to an inaccurate simulation of the snow thermal conductivity.**”

As requested by the reviewer, we will add the air and surface temperatures to Figure 8.

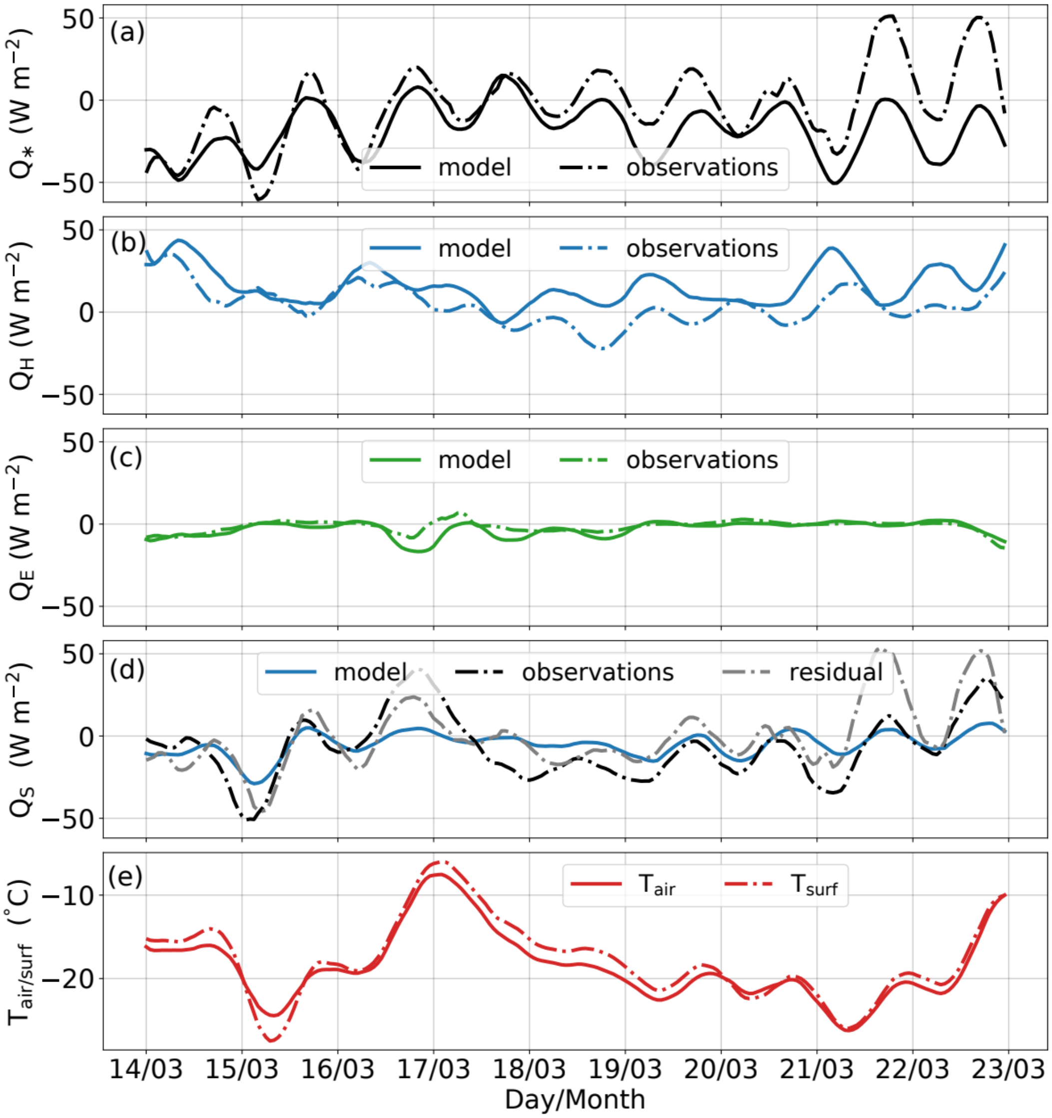


Figure 8: Comparison between observed and modeled hourly means of all constituents of the energy budget at the snow surface from March 14 to 23 2020. The residual snow heat flux is obtained by subtracting the turbulent heat fluxes from the net radiation. **Also shown is the air and surface temperature during this period.**

19. Figure 9: This figure would be easier to interpret if air, snowpack and soil temperatures (and snow surface radiative temperature) were plotted as points or lines. Early in the season, the sensible and soil heat fluxes to the snowpack are not enough to balance the radiative losses. Soil temperatures would provide information about when the soil water is freezing, which represents an energy source under the net radiometer although below the snow/soil interface. The differences between soil and snowpack temperature and the timing of soil freeze are important pieces of information that would help to interpret what is happening. Given that the soil heat flux is supposed to represent the flux at the ground surface, or in this case at the soil/snow interface, what sort of values for QG would be obtained by using the gradient between the lowest snow temperature and the 4 cm soil temperature with an average thermal conductivity?

We added the requested temperature in a third panel under Figure 9. However, we feel it is now a bit overloaded and thus, we will put it in the supplementary material and will leave the original version in the manuscript.

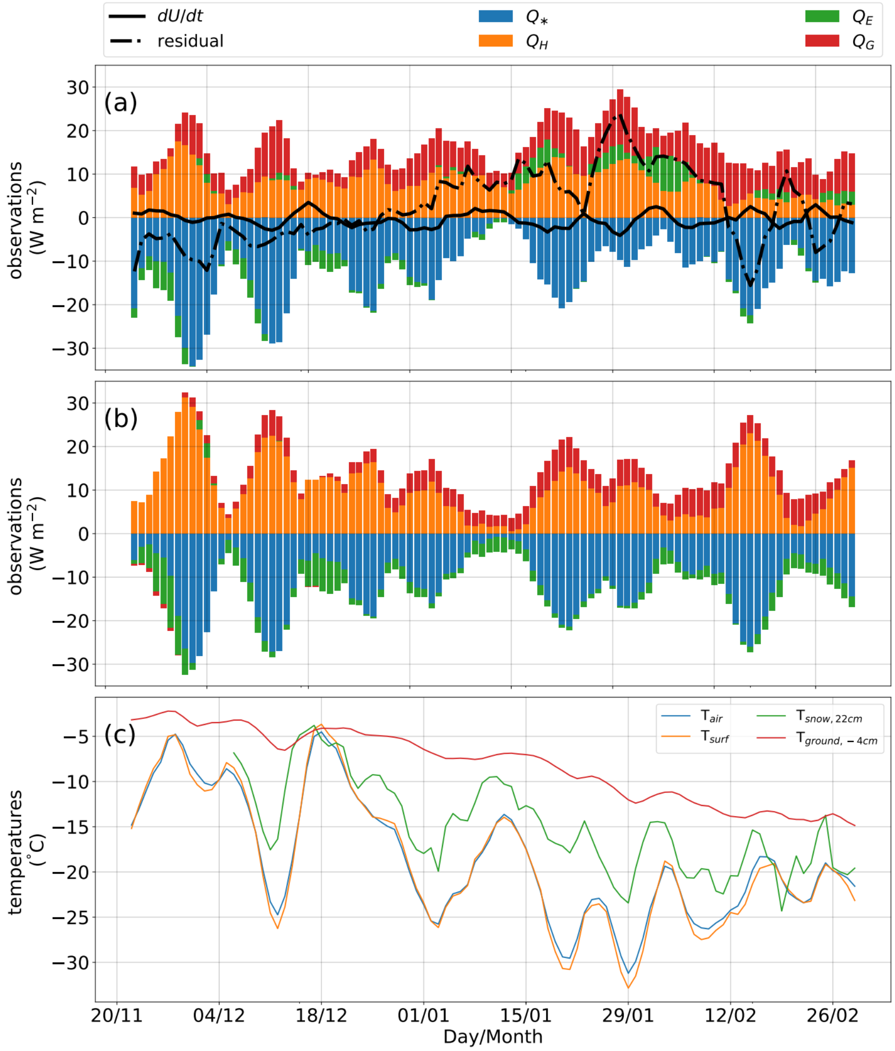


Figure 9: a) Observed and b) simulated daily snowpack energy budget terms comprising sensible (QH) and latent heat fluxes (Qe), net radiation (Q*), ground heat flux (QG, and the change in the internal energy of the snowpack dU/dt, during the first half of winter 2018/19. The modeled ground heat flux also includes the heat storage change dU/dt in the snowpack. The modeled snow enthalpy change is not shown because the modeled enthalpy includes changes due to precipitation and is therefore not comparable to observations. In the lower panel c) the temperatures of the air, the surface, the snow and the ground are shown.

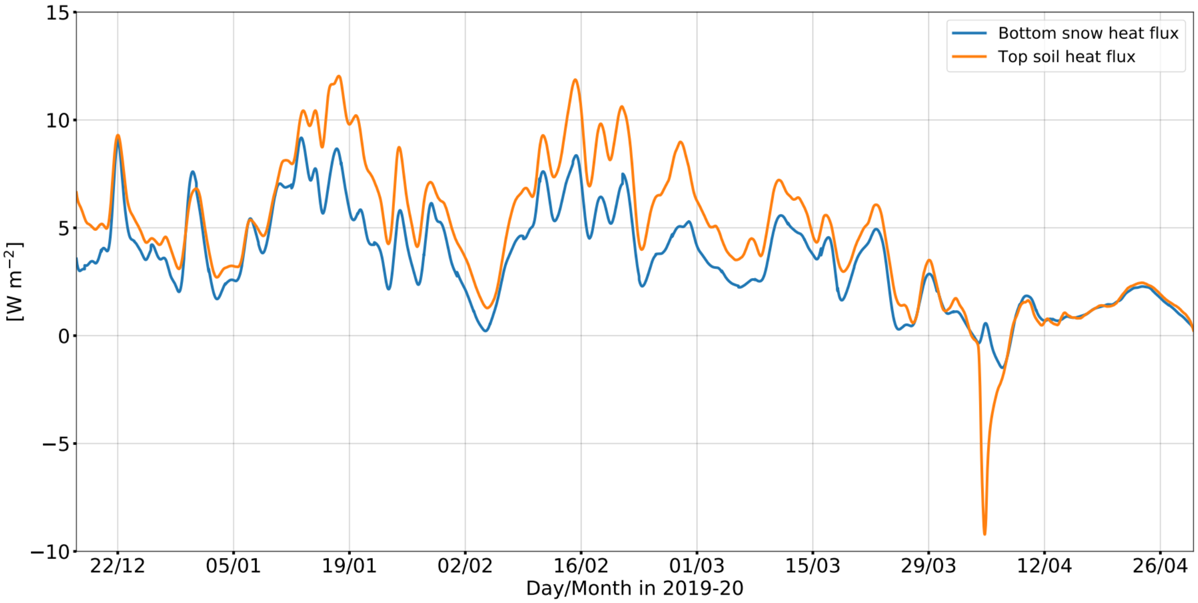
We do not recommend using one temperature measurement in the snow and one in the ground as the thermal conductivity between these two varies heavily. A test using an average thermal conductivity between the two media revealed inconsistent results.

20. Line 409-414: Flow separation and drainage flows may be a factor at this site. Could a temperature profile be examined using the radiative temperature of the snowpack surface, the air temperature from the tower, and any unburied snow temperature sensors? It might give some idea of the level of stratification at night. Radiation errors might prevent the use of unburied snow temperature sensors during the day.

The atmospheric stratification is already quantified via the bulk Richardson number calculated between the snow surface and the measurement level of the EC sensor, a few meters above the snowpack. Using this approach allows us to have reasonable measurements of the Ri_b at all times, which would not be the case if we were to use unburied thermocouples that get heated by the sun during the day (as you are pointing out). A more detailed analysis of turbulent heat fluxes in winter and their parameterization for land surface models, in particular in relation to atmospheric stratification, is under way. For this current paper, we refer to Table 1 showing the stability regimes.

21. Line 415-418: It would be interesting to see the heat fluxes calculated at the bottom of the snowpack, compared with those calculated in the soil column (and those using the 4 cm soil temperature and the lowest snow temperature).

We will add a figure showing the heat fluxes in the snow at 7 cm and the ground heat flux in the supplementary material. For the reasons explained in comment 19, we have chosen not to include the heat fluxes. The heat flux using one temperature measurement in the snow and one in the ground will not be shown (see comment 15).



Supplementary Figure 5: Soil heat fluxes computed at a depth of 7 cm (orange) and snow heat flux computed 7 cm above the soil surface. Note that the large negative soil heat flux on 4 April corresponds to a rain-on-snow event. Positive values indicate fluxes from the soil to the atmosphere.

Corrections and minor suggestions:

We plan to make all the minor changes requested in comments 22 through 26, and 28 through 31.

22. Line 14: I would change “freezing air temperatures” to “air temperatures less than 0°C”.
23. Line 15: I am not sure that “constraints” is the right word here. Perhaps “challenges”.

24. Line 30: Just to be precise, I would state “…and QG is the ground heat flux at the snow/soil interface”.

25. Line 38: Change “snow” to “snowpack”.

26. Line 53: Since the term “sublimation” can refer to the conversion of water vapour to ice, even though the authors use “condensation” for this process, I would change the wording from “However, according to Liston and Sturm (2004), sublimation in the Arctic can make up as much as 50% of the total winter precipitation” to “However, according to Liston and Sturm (2004), sublimation losses in the Arctic can deplete as much as 50% of the total winter precipitation.”

27. Line 186-7: Do the authors mean that when there is snow on the ground in the model, the surface is always 100% covered with snow, as opposed to a fractional cover based on SWE or depth?

Yes, the ground is assumed to be completely covered with snow when the snow height exceeds 1 cm. Otherwise, the surface is considered to be covered to a certain fraction by snow, while the rest is snow-free. Consequently, the albedo and the turbulent fluxes are calculated separately for the snow-free and the snow-covered part. This, however, leads to very different values of the albedo and the turbulent fluxes compared to observations. For this reason, we have chosen not to use a fractional snow cover.

We will clarify this in the manuscript:

“We also used the option in Crocus that allows for the surface to be 100% covered with snow **once the snow height exceeds 1 cm**.”

28. Line 225: This sentence is written with the assumption that the reader is familiar with the low pressure systems in the region. I would reword it as: “Snow usually accumulates quickly in the fall as precipitation events are more frequent due to the large low pressure systems which are prevalent at that time of year.”

29. Line 226: Change “rates drops” to either “rates drop” or “rate drops”.

30. Line 362: Change “recomnd” to “recommend”.

31. Line 369: Change “sublimation accounts for only 5% of winter precipitation” to “sublimation losses represent only 5% of winter snowfall”.

References

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