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The Cryosphere Discussions

Interactive comment on "The annual surface energy budget of a high-arctic permafrost site on Svalbard, Norway" *by* S. Westermann et al.

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We are thankful to the reviewer for the thorough reading and the thoughtful suggestions to improve the quality of our manuscript. We revised the manuscript according to all issues raised by the referee in the following comments (reviewer statements marked by bold). We submit a modified manuscript where the changes are marked in bold. The paragraphs 1, 3.2, 5.1 and 6 have been restructured and extended and are thus largely marked as "changed".

Response to General comments:

General comments:

This manuscript presents independent measurements of all components of the surface energy budget at a high-arctic site near Ny-Alesund, Svalbard, over a complete annual cycle. These observations are described and analyzed in a convincing manner, followed by a discussion of implications and consequences for the local permafrost. The observational data of this study in combination with additional measurements from nearby located stations (e.g. BSRN site) constitute a very valuable data set which is quite unique for a high-latitude field site in terms of record length, temporal resolution, and completeness (standard meteorological, micro-meteorological, snow and soil observations). Both the selected sensor systems and the data analysis methods and techniques are appropriate and state-of-the-art. Despite the fact that the paper does not include novel or innovative measurement or analysis tools and approaches, the presented results merit high consideration since they constitute valuable information in the context of understanding physical processes and climate change in high-latitude regions where such complete observational data are very scarce. The acquisition of this complete set of measurements, the thorough and consequent application of well established analysis tools, and the qualitative and quantitative discussion and interpretation of findings form the originality of this work. The results provide a quantification of the different components of the surface energy balance at a high-arctic lowland site which I trust will be of considerable interest and use for the scientific community. The paper would have even more impact if the authors briefly described how their findings affect and support current and future research on climate change in the Arctic.

We have extended the abstract and partly restructured and extended the introduction and conclusion to include this proposition. We believe that comprehensive long-term data sets on the surface energy budget are indispensable to enhance the process understanding and support modeling efforts in the Arctic. We have added a statement on the significance of such data sets for the validation of atmospheric circulation models, especially in the Arctic, where such studies are extremely sparse. While it is not within the scope of the present study to come up with validations and possible improvements of modeling, we have mentioned a number of aspects, such as the strong near-surface inversion during winter, where the presented data set could be used for a more detailed study. Furthermore, we hope to stimulate similar efforts at other arctic locations, which would create a more complete picture and thus greatly enhance the process understanding of the climate of arctic permafrost areas.

Overall, the paper presents the topic in a clear, complete and concise manner; it is well written, clearly structured and organized. I recommend the manuscript be accepted for final publication in 'The Cryosphere', pending a few minor changes and edits as indicated in the specific and minor comments below.

Response to Specific comments:

1. p.638, lines 23-25: What is the reason or justification for the assumption of 0.65 for the albedo of melting snow?

We have included clarifying information in the sections "'3.1 Radiation"' and "5.1 Measurement errors and energy balance closure": 0.65 is the average albedo measured at the BSRN station for the time when the snow melt occurred, i.e. the time between: the albedo starts to decrease from the winter value of 0.8 (approx. 1 June) and: the time before the albedo decreases within two days from 0.5 to the summer value of 0.15 (approx. 15 June). It is clear that the value must be considered an estimate only.

2. p.642, line 21: "The fit is strongly influenced by..." Not clear if this is specific for this study (then it should be "This fit...") or generally applicable. Please clarify.

text modified to "For the fit, we exclude periods with measurement errors or strong rain events, which may induce non-conductive transport of heat."
We have seen this effect in the present study, but would certainly expect it to be a general effect, given similar data quality and precipitation/infiltration intensity. The agreement between modeled and measured curve is usually very good, if the heat transport is conductive and an accurate measurement for the spacing between the sensors is used (which we do). The RMS-error can then be dominated by a single spike or outlier of the middle temperature measurement. In such a case, the fit will then tend to minimize the deviation between the outlier and the fitted curve, resulting in a considerably wrong diffusivity value. The same happens if infiltration of rain violates the assumption of purely conductive heat transfer. In both cases, the wrong diffusivity can be clearly seen, since the modeled phase shift between upper boundary and middle temperature does not correctly reproduce the phase shift of the measured temperatures.

3. p.643, lines 6-7 and lines 17-19: What is the vertical spacing of the three temperature sensors?

Information inserted in the text (see below).

Are three sensors only sufficient to get a good fit for the thermal diffusivity of snow? What happens if the snow depth changes substantially and the set of three sensors gets buried deep down in the snow pack? Add a short

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explanation/discussion here.

Explanation added:
"During the winter 2008/2009, a profile of three temperature sensors (located at the soil surface and 0.15m and 0.4m above the surface) in the snow pack located next to P3 is used to fit the thermal diffusivity of the snow. This is possible from December 2008 onwards, as soon as the snow pack has reached the uppermost sensor. Initially, the array is contained in the young snow pack, but gets progressively buried with increasing snow height. The obtained values range from $d_h = 4.5 \times 10^{-7} m^2 s^{-1}$ to $d_h = 7.0 \times 10^{-7} m^2 s^{-1}$, with a tendency towards higher values of d_h at the end of the considered period for the then older snow. However, we have no measurements of the thermal diffusivity of the overlying, not instrumented snow pack. For the evaluation of the snow heat flux, we therefore choose a constant value of $d_h = (5.5 \pm 1.5) \times 10^{-7} m^2 s^{-1}$ and include the found variability of d_h as uncertainty."'

4. p.643, line 16: The resulting snow thermal conductivity of $K_h = 0.45 W m^{-1} K^{-1}$ is interesting since this value is considerably higher than values commonly used in snow, sea-ice, or climate models ($K_h = 0.3 W m^{-1} K^{-1}$ for a snow density of about 0.3 g cm^{-3}). Do you have an explanation for that? I do not doubt the value, just curious.

A possible reason are rain-on-snow events, which lead to the formation of internal ice lenses and ice layers in the snow structure. They have been observed in all the investigated snow pits, and may explain the high bulk thermal conductivity of the snow.

5. p.646, line 29: Please comment on or discuss possible reasons for the residual of 22 Wm^{-2}.

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Explaining sentence and reference to Discussion given, where the energy balance closure problem and measurement uncertainties are now treated in more detail.

6. p.647, lines 4-10: Any thoughts on how representative the observed annual cycle is? For instance, here you mention that the September precipitation was twice the usual amount.

This one-year study of the surface energy budget is certainly not representative with respect to all different parameters. The impact of the variability of these parameters on the surface energy budget is essentially an open question calling for a sustained measurement effort. The year-to-year variability has been assessed for some aspects, for instance for the duration of the snow-free period (Winther et al. 2002), one of the most crucial factors for the surface energy budget. The information on the available long-term observations of air temperature, precipitation and duration of the snow-free period is included.
Concerning the September and December precipitation: Particularly in late summer, fall and early winter, the precipitation in Ny-Alesund is highly variable, depending on the cyclone activity. Between 2000 and 2007, the September precipitation was 90mm or higher in three years, and 30mm or lower (as low as 10mm) in the other five years (www.eklima.no). We have added a short comment on the precipitation rates in Sec. 2.1 Climatological conditions: "With almost 100mm of precipitation each, the months of September and December 2008 stood out with more than twice of the long-term average. However, similar precipitation rates have been observed at a number of occasions in fall and early winter since 2000 (www.eklima.no), so that the second half of the study period must be considered as "wet conditions" rather than an extreme exception."

7. p.650, line 6: Is it a gradient (Km^{-1}) or a difference (K)?

changed to "difference"; the same error occured in the caption of Fig. 8, which we also changed.

8. p.651, line 1: Where does this melting happen - at the snow surface or internally?

We have no direct measurements or observations, which might help to clarify this issue. For the pre-melt and the snow melt period, our definition of the surface (see 3.1) is not 100% correct. It is well known, that the incoming short-wave radiation deposits a part of its energy inside the snow pack. This critically depends on the constitution of the snow and particularly on impurity layers within the upper part of the snow pack. We have no measurements of short-wave radiation at various depths in the snow. Therefore, we treat both the ground heat flux and the flux consumed by the snowmelt with a bulk formulation for the pre-melt and snow melt periods, where the short-wave radiation is strong.

9. p.652, line 13: What is meant by "within the freezing characteristics of the soil"?

changed to "within the freezing range of the soil" (see comment of Reviewer #1).
We mean the range, where a temperature change is associated with freezing of soil water.

10. p.658, lines 2-4: Did you already test the quality of the energy balance

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closure on a daily basis? I do not ask to include his analysis in this paper but I would love to see the performance.

We tested the energy balance closure on a daily basis. As to be expected, the closure term shows some variability, but it usually does not change sign (compared to the average value given in the paper). Two additional features of the closure term:
1) During summer, the closure term shows a clear diurnal course , which can be at least partly explained by the heat storage of the uppermost soil layer, which is not accounted for by the ground heat flux. As mentioned in the discussion, the day and night contributions of this effect cancel, so that it does not need to be taken into account on the timescales considered in the paper.
2) The closure term seems to correlate weakly with the sum of the turbulent fluxes, i.e. if the net radiation and hence the turbulent fluxes are close to zero, the closure term is close to zero, and if the net radiation has large values and the turbulent fluxes are correspondingly large, the closure term is large, too. This is true at least for the summer and the dark winter period and would support the assumption, that the closure term can be largely explained by fluxes contributions not measured by the eddy covariance system.

11. p.667, Tab.2: How was the cloud fraction (cf) determined?

The cloud fraction is determined visually by the staff of the Sverdrup station of the Norsk Polarinstitut, who feed their data into the "eklima" data base. The observation method is described (in Norwegian only) on
http://met.no/Meteorologi/A_male_varet/Observasjoner_fra_land/, which is linked from www.eklima.no. In the caption of the table, we now refer to section 3.6 Ancillary measurements, where we describe how the parameters are accessed and where we cite www.eklima.no.

12. p.668, Fig.1: I suggest removing Fig.1a and expand Fig.1b. People know the location of Svalbard (I hope...).

left unchanged, see Editor comment

13. p.670, Fig.3: The ground heat flux (yellow) is not very visible. Perhaps change color (also in Fig.8).

color changed to green

14. p.671, Fig.4: Give dates for the summer period also in this figure caption.

changed

15. p.672, Fig.5 (also Figs. 6, 10, 11, 12): Replace "See Fig.X." with "Notation as in Fig. 2".

changed

How does it look if the area of the arrows in Figs. 2, 6, 10, 11, 12 is scaled proportional in size? This way one could easily (inter-)compare the relative magnitude of the fluxes in different figures representing different periods or seasons.

We have tried to scale the arrows accordingly, but the difference in magnitude of the

flux contributions between e.g. the summer and the dark winter period would lead to a very small graph for the dark winter period. In such a graph, the relative magnitude of the fluxes of the dark winter period is hard to see for the reader, which is contrary to the intention of the arrow diagrams.
One way to overcome this problem is suggested by Reviewer #1: to merge all the arrow diagrams into a single one. However, in the final version we would like to place each arrow diagram as close as possible to the text section for the respective period, so that it can serve as an illustration of Tab. 2, which appears a few pages before and is somewhat cumbersome to read.

16. p.677 and 678, Fig.10 and 11: These 2 figures are not referenced/mentioned in the text in sections 4.4 and 4.5, respectively.

changed

Response to Minor comments and edits:

1. p.636, line 1: "...the first half of the study period..."

corrected

2. p.639, line 3: Remove this line; this is already defined in the notation part in section 3.1.

corrected

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3. p.643, line 2: "With K_h known,..." Letter "n" missing.

corrected

4. p.646, lines 11-13: Awkward sentence, rephrase.

changed to: "The average Bowen ratio is approximately one, but it shows strong variations, which are closely related to the soil water content of the surface layer (Figs. 3, 4)."

5. p.647, line 16: please write: "...i.e. the advection of relatively warm air...".

corrected

6. p.647, line 20: Replace "permanent" with "perennial".

corrected

7. p.652, line 6: Replace "at around" with "close to".

corrected

Thank you very much

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Sebastian Westermann, Johannes Lüers, Moritz Langer, Konstanze Piel, Julia Boike

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