**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 for 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 specific comments:

C389

1. Abstract
Results are summarized in the abstract but no information on conclusions, impacts, and outlook is given.

Statement on the significance of the presented data set added:
"As such comprehensive data sets on the surface energy budget are sparse for the Arctic, they are of great value to support modeling efforts on the present-day and future arctic climate and permafrost conditions."

2. Aim and background of the study
The importance of surface energy balance data in the context of climate observation and modeling is outlined in the introduction and at the end of the section it is briefly mentioned what data is presented in this paper. The authors should give more information on the frame or context in which the measurements are performed, as well as describe the specific aim of the paper and what is actually new.

We agree with the reviewer, that this is an important issue which deserves to be presented in more detail. We have subsequently restructured and added new information to the Introduction. We have put the surface energy budget models in a more general context of atmospheric circulation models, as the treatment of the surface energy budget is in principle identical with the one of the lower boundary of atmospheric circulation models. A partly new paragraph has been included:

"The redistribution of energy at the surface is one of the driving forces for the global climate system. The basic contributions of the surface energy budget are the short- and long-wave radiation, the sensible and latent heat fluxes and the ground heat

flux. The adequate representation of this surface forcing is one of the challenges in atmospheric circulation models, on which predictions on climate change are based. The models make use of mostly semi-empirical parameterizations of the different fluxes of the surface energy budget, which have usually been developed and validated for non-arctic regions (Viterbo and Beljaars, 1995). In the Arctic, the perennial snow cover and the annual snowmelt, which greatly modify the surface processes for a large part of the year, constitute additional challenges for modeling which have not yet been fully resolved (Douville et al., 1995; Slater et al., 1998). Another unresolved problem is the parameterization of the sensible and latent heat fluxes during stable atmospheric stratification conditions which frequently occur in the arctic winter (Zilitinkevich et al., 2002; Lüers and Bareiss, 2009a). The same problems occur in process-orientated permafrost models (Hoelzle et al., 2001), which in principle use the same parameteri- zations of the surface energy budget to evaluate the ground heat flux and the thermal conditions of the subjacent permafrost (Hinzman et al., 1995; Ling and Zhang, 2004). Direct measurements of the entire surface energy budget in arctic regions are therefore indispensable to evaluate the performance of the employed flux parameterizations and surface parameter sets, especially if the study can provide the entire annual cycle and thus a complete picture including snow-associated processes. Great efforts have been initiated to study the annual cycle of the surface energy budget over arctic sea ice (Persson et al., 2002; Uttal et al., 2002), while comprehensive long-term studies are still missing for arctic land areas."

The introduction concludes with a partly new paragraph which is more focused on the context and aim of the study than in the previous version:

"This study presents eddy covariance measurements of the sensible and latent heat flux at a high-arctic permafrost site on Svalbard, which were conducted over a full seasonal cycle from March 2008 to March 2009. The eddy covariance measurements

are complemented by measurements of the radiative parts of the energy budget and the ground heat flux, so that a complete set of independent measurements of all contributions of the surface energy budget is accessible at a temporal resolution of one hour for an entire year. In this study, we focus on the annual and diurnal cycles of the surface energy budget. This not only allows to identify the driving parameters of the coupled permafrost-snow-atmosphere system, but also provides a basis for further investigations and modeling efforts, e.g. on the impact of small-scale variations of the surface cover on the local energy budget and the thermal conditions of the subjacent permafrost. While the current study extends the sparse data set on the surface energy budget in the Arctic, we hope to encourage similar studies at other circumarctic locations, which would greatly improve the understanding of the climate of high-latitude ecosystems and its susceptibility to climate change."

3. Section on radiation
Why can albedo be inferred from measurements at the BSRN station? Several lines above it is stated, that radiation from the BSRN station cannot be entirely assigned to the study area because of significant differences in snow and surface cover, which are both important factors determining the albedo. This seems to be a contradiction.

The paragraph has been revised, which should clarify the objections. We have included the direct albedo measurements, which in the previous version was only used in the Discussion on the energy balance closure. These considerations show that we have to accept an uncertainty of 0.05 on the albedo and thus 5% on the net short-wave radiation.

It is not very clear from the text here, and only mentioned in Section 3.4, that long wave radiation is measured and not derived from surface temperature

measurements.

We have added the missing information:
"The Bayelva climate station is located about 100m from the eddy covariance site (Fig. 1c), where measurements of S_{in} with a Skye Pyranometer SP1110 and L_{out} with a Kipp & Zonen CG1 long-wave radiation sensor are performed."

4. Section on the eddy covariance method
Although it is a standard method, a short description of the eddy covariance method at the beginning of Section 3.3 should be included to help the reader. From the text alone, it is difficult to understand what parameters are actually measured, what assumptions are taken, and how the turbulent fluxes are eventually calculated.

We have included a brief section on the eddy covariance method and the basic formulae.

I further suggest renaming Section 3.3 to "turbulent fluxes" or similar, since the other sections in Chapter 3 are named after the different parts of the energy balance and not the method used.

Paragraph renamed to "Turbulent fluxes".

5. Uncertainties and spatial and temporal variability
The authors thoroughly discuss the energy balance closure problem and give error estimates for some of the energy fluxes. In my opinion this is a very

important part of the study, and I suggest to extend it to a section discussing uncertainties. What are the uncertainties/error estimates for each part of the surface energy balance and each of the six segments due to a) measurement errors, b) assumptions taken and parameters estimated (e.g., albedo for snow melt period, emissivity, thermal diffusivity, is partly done), and c) spatial variability and distance of different measurement instruments (eddy covariance system, temperature profiles, radiation measurements are not measured at the same location, also partly discussed). Also a visualization of error estimates in the Figures 2,5,6,10-12 would be valuable. The temporal variability should be discussed.

We agree that the considerations on the errors are valuable, particularly when a similar study should be repeated for another year or for another area. We have therefore extended the section on the "Energy balance closure" and renamed it to "Measurement errors and energy balance closure". We have further placed this paragraph at the beginning of the Discussion, as it is relevant for the following sections "Annual surface energy budget" and "Implications for permafrost". We have decided against relocating the error considerations to the "Methods"-section, where such consideration would be normally placed, as the energy balance closure problem, despite of recent progress, is still an open topic for research. Therefore, potential measurement errors must be taken into account when discussing the closure of the energy budget in our study.
We argue that the qualitative classification of uncertainty error levels a)-c) must be extended by one important additional aspect: the systematic bias of the obtained fluxes inherent in some of the employed methods. We believe that this error source is qualitatively different from a random or statistical measurement error, which might (in the best case) cancel out in the long-term averages or at least could be positive or negative in sign, depending on the conditions. The systematic underestimation of sensible and latent heat fluxes by time-averaging eddy covariance measurements, which has been identified as the most likely candidate for the energy balance closure

in a number of studies, belongs to this latter level of uncertainty and must be clearly distinguished from the points a)-c).

Furthermore, we don't think, that the errors should be included in Fig. 2,5,6,10-12. First of all, the different qualitative levels of uncertainty are impossible to visualize, as e.g. the turbulent fluxes can be associated with random measurement errors and a systematic bias, or the ground heat flux with an uncertainty due to the conductivity and an uncertainty due to inconsistencies between eddy covariance and point measurements. Secondly, a visualization in the figures could create the impression, that a standardized error evaluation technique was applied, which would ensure a strict comparability of the different error estimates. However, such a strict error analysis has not been applied, and various error estimates had to be used (literature, error due to spread of measured parameters, etc.). Such a strict error analysis is even difficult to accomplish in carefully planned turbulence experiments, which focus on short-term precision measurements rather than long-term studies.

All in all, we have attempted to assess the uncertainties of our study as comprehensively, as this is possible with the employed data basis. Furthermore, we have included more citations of literature on the active research topic of the closure problem, which are related to and might help to further clarify the uncertainties of our study.

In section 2.1, meteorological parameters are compared to a long-term reference period. In what sense and magnitude can this influence the results presented?

With the present data set, we can only speculate on this issue, which we do in "5.2 The annual surface energy budget": "Given the present data set, an earlier termination of the snow melt, e.g. by end of May instead of end of June, would not only lead to an increase of the net short-wave radiation in the annual budget, but also to an enhanced flux of latent heat. In case of the sensible heat flux, the ratio between summer conditions with atmospheric warming and winter conditions with atmospheric cooling

would be shifted, resulting in a smaller, but presumably still negative net sensible heat flux."

While this interpretation is relatively straight-forward due to the comprehensive multi-year study on albedo and snow melt by Winter et al. (2002), other interpretations, e.g. on the effect of the wet fall conditions would be highly speculative due to cloud-radiation feedbacks, modification of turbulent fluxes, etc. We have included further information on the long-term variability of the fall and early winter precipitation in Sec. 2.1: "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 "wet conditions" rather than an extreme exception."

6. Conclusions
The first two paragraphs of Section 6 start with some concluding, or rather summarizing, sentences. I miss a paragraph or section where the outcomes of this paper are clearly presented in a "take-home-messages" way.

We have renamed Section 6 to "Summary and outlook" and partly restructured it to better separate the part "Summary" and the part "Outlook". In the first paragraph, we summarize the main findings on the surface energy budget in three numbered statements as a "take-home-message". We then continue with implications and possible impacts on modeling. In the third paragraph, we emphasize the significance and prospects of continued monitoring of the surface energy budget for the understanding of climate change and propose improvements for future studies. We have then inserted a concluding paragraph, which stresses the general necessity of comprehensive long-term data sets of the surface energy budget to enhance process understanding and support modeling efforts.

7. Figures
Figures 2, 5, 6, 10, 11, 12: Can they merged into one single figure (e.g., using a,b,c,. . .)? This would facilitate the comparison of the magnitude of the energy fluxes for different times of the year.

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. Furthermore, the difference in the absolute magnitude of the fluxes between e.g. summer and dark winter would make it very difficult to compare the fluxes during the dark winter, where the fluxes are lowest. However, we believe that this aspect is more important than the comparison of the flux magnitudes between the different periods, which to a certain extent is possible with Tab. 2. We have therefore not changed Figs. 2, 5, 6, 10-12.

Response to Minor comments:

p.636, l.4-6: Is this exceptional and why is it important?

Changed to: "The Kongsfjorden, located 2 km NE of the study area (Fig. 1b), was free or almost free of sea ice during the entire study period, which has been the case since 2006 (Gerland and Renner, 2007; Cottier et al., 2007, own observations)."
We think that it is important to include a statement about the presence of sea ice. In the winters 2005/06, 2006/07 and 2007/08, there was no sea ice in the Kongsfjorden off Ny-Alesund, which is unusual for this site (details in Gerland & Renner: Sea-ice mass balance monitoring in an Arctic Fjord, Annal. Glac. 46, 435-442, 2007; Cottier

et al.: Wintertime warming of an Arctic shelf in response to large-scale atmospheric circulation, GRL 34, doi:10.1029/2007GL029948, 2007). In the winter 2008/09, a sea ice cover started to form after 15 March and thus the period considered in this study. It is not clear whether the ice-free conditions will be the exception or the rule in the next years. It is an interesting question if and how the heat reservoir of the open water body located 2km from the study site affects the energy budget of the permafrost site, particularly the sensible heat flux during the winter period. Even when sea ice is present off Ny-Alesund, the fast ice edge usually forms less than 10km from the study site (Gerland & Renner 2007), so that it may experience the influence of open water even in this case.
These questions cannot be investigated in the present study, but could be addressed by the implementation of a regional climate model. We think that such a work could be highly rewarding, particularly since a variety of ground-thruthing observations is available in Ny-Alesund.

p. 636, l.17-19: What is the long-term mean temperature at this depth? Is there any trend?

The long-time record of the Bayelva station is still unpublished, so we do not want to give exact numbers for the warming in this paper. Roth & Boike (2001) provide the temperature data of the years following 1998, which would allow to approximately estimate the warming since then. We have therefore only added a short comment about the long-term trend:
"The Bayelva climate and soil monitoring station has provided a long-term record of climatological parameters and permafrost temperatures since 1998. At present, the permafrost at Leirhaugen hill is relatively warm, with a mean annual temperature around -2C at 1.5m depth. The maximum active layer depth in 2008 was on the order of 1.5m. Since the installation of the station, the soil temperatures have warmed

significantly at the observation site (compare to Roth & Boike, 2001)."

And, is an active layer of 1.5 m representative for the area and the past measurement years?

About 10 shallow boreholes drilled in late August 2008 in the wider vicinity of the observation site suggest, that 1.5m (approximate thaw depth at Bayelva station) is a good average value (range from 1.4 to 1.65m) for the thaw depth. After the submission of the first manuscript to TCD, we evaluated GPR measurements of the thaw depth, which suggest that the thaw depth can be as deep as 2m (S. Westermann, U. Wollschläger, J. Boike: Monitoring of active layer dynamics at a permafrost site on Svalbard using multi-channel ground-penetrating radar, in prep.). Since these measurements were performed 150-300m away from the eddy system and therefore outside the studied area (unfortunately, GPR didn't work properly in the more fine-grained material around the eddy system), we do not include this information.

p.638, l.6: The surface is defined. . . (not ist).

corrected

p.641, l.25: Convert instead of translate, also: p.644, l.18

corrected

p.642, l.1: "cannot is" one word not two

corrected

p. 643, l.2: With K_h known . . . (n is missing)

corrected

p. 645, l.12: . . . is divided into . . . (not in)

corrected

p. 650, l.17: Can you give the temperature at this time of the year? And a detail: temperatures are never cold but low.

modified and corrected: "At the end of the light winter period, the lowest soil temperatures are reached, with about -8C at the soil surface and -4C at 1.5m depth."'

p. 652, l. 13: The temperatures are within the freezing range not the freezing characteristics.

Corrected to "freezing range"

p. 657, l. 14: I suggest renaming the section "summary and outlook" or similar because it is not only about future work, but also concludes this paper.

changed to "Summary and outlook"

Thank you very much

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

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