The Cryosphere Discuss., 4, C1801–C1820, 2011
www.the-cryosphere-discuss.net/4/C1801/2011/

The Cryosphere Discussions

Interactive comment on "Permafrost and surface energy balance of a polygonal tundra site in Northern Siberia – Part 2: Winter" *by* M. Langer et al.

M. Langer et al.

mlanger@awi.de

Received and published: 31 March 2011

We thank the reviewer for the constructive comments on our manuscript. We carefully considered all comments of the reviewer which are highlighted in bold. Changes done in the manuscript are marked in italic.

The authors present a useful and conclusive study about the energy balance at an Arctic permafrost site. This study has been submitted in two parts (part I: summer period; also submitted to TCD), and I agree with reviewer 1 that both parts cannot be analysed separately, as the publication of part 2 depends

on the successful revision process of part 1 (especially as serious reservations were expressed by one of the reviewers).

After the revision process, the first part of this study is now accepted for publication in "The Cryosphere". Hence, the successful publication of the companion paper as a prerequisite for the publication of this study (part 2) is fulfilled.

Furthermore, in my opinion, both parts would be better merged into one joint publication, as several of the chapters are similar (introduction, field site, methods, references), and the discussion and summary in part 2 was given for both parts anyway. By this, the comments of the reviewers of part 1 and 2 could be merged and the overall quality of both parts would be enhanced.

We agree that the review process of both manuscripts can not be handled separately. Therefore, we also carefully consider comments of the reviewers of part 1, if the content of this study is affected. In return, this also applies to the completed revisions of part 1 within which the present comments are also accounted for.
However, the first study comprises the surface energy balance of the polygonal tundra site from the beginning of snow melt until the beginning of freezing. The main focus is on temporal and spatial variations of the surface energy balance. This includes a detailed study on the effect of the polygonal surface structures on the surface energy balance. In contrast to the summer period, the landscape during winter is characterized by a persistent snow cover which completely changes the scale and magnitude of the landscape heterogeneities. The present study focuses on the temporal evolution of the energy balance while surface heterogeneities only have a secondary priority, as they only occur due to freezing water bodies. Both studies set different priorities and focus on different details in the surface energy balance. In our opinion, it would not be possible to merge both studies without loosing relevant information (e.g. energy partitioning at wet and dry surfaces patches, delayed freezing of soils and ponds), or

being much too long for a single article.

Generally, the manuscript contains a lot of justification about the importance of energy balance measurements in Arctic regions (e.g. in the Discussion section), which are not necessary (the data set itself is worth publishing) and are in the majority trivial (e.g. regarding the importance and influence of correct subsurface parameterisations for permafrost and atmosphere models in the Arctic). Also references stating this importance should be reduced to papers which have a real connection to the present study and not solely because they were conducted for the Arctic.

Due to the critical remarks of this and the first reviewer on the given justifications and the discussed implications for models, we considerables reduced the concerning paragraphs in the revised version of the manuscript. However, the authors believe that it is important to discuss possible implications of our measurements for modeling the soil-atmosphere interactions in the Arctic. This is especially true for the subsurface heat flux which is usually strongly simplified in land-atmosphere schemes of general circulation and weather forecast models, but nevertheless is found to be of outstanding importance for the surface energy balance during the arctic winter at the study site. We specified our intention by completely rephrasing Sect. 5.3.

Section 5.3 in the Discussion section describes the implications for permafrost modeling, which is not the topic of the paper, as no permafrost modeling was conducted. It is more a motivation for the study than a part of the discussion of the measurement results, especially as the majority of the (rather general) results has been mentioned before. This section can be shortened considerably.

We have completely revised the Discussion section in general and Sect. 5.3 in particular, thus sharpening the focus of the Discussion to the topics, where we see

implications of our study for larger-scale modeling efforts.

Finally, it is not clear to me why an interannual comparison was aimed, if many parts of the data were not available during the second year. The various differences in the methodological approach between the two winters could severely influence the interannual analysis if no comparison of the methods is done.

We agree with the reviewer that the objective of an complete interannual comparison is too ambitious regarding the available dataset. The study mainly focuses on the winter period 2007-2008, while interannual comparison can only be given when data are available. Consequently, we revised the wording of the introduction (Sect. 1), parts of the discussion (Sect. 5), and the conclusions (Sect. 6).

Specific comments:
p.1392, l.21-23: spatial variabilities: was that really investigated ? Depends on the scale...

In the revised version we avoid using “spatial variability”, as it is only partly investigated in the present study. The wording of the abstract has been changed to:
In this study, we present the wintertime surface energy balance at a polygonal tundra site in northern Siberia based on independent measurements of the net radiation, the sensible heat flux and the ground heat flux from two winter seasons. The latent heat flux is inferred from measurements of the atmospheric turbulence characteristics and a model approach. The long-wave radiation is found to be the dominant factor in the surface energy balance. The radiative losses are balanced to about 60% by the ground heat flux and almost 40% by the sensible heat fluxes, whereas the contribution of the latent heat flux is small. The main controlling factors of the surface energy budget are the snow cover, the cloudiness and the soil temperature gradient. Large spatial

differences in the surface energy balance are observed between tundra soils and a small pond. The subsurface heat flux released at a freezing pond is by a factor of two increased compared to the freezing soil, whereas differences in net radiation between the pond and soil are only observed at the end of the winter period. The observed inter-annual differences in the surface energy balance are related to differences in snow depth and cloud cover which strongly affect the temperature evolution at the investigated pond. The freeze-up of soils and ponds displays a high sensitivity to the snow cover evolution and the incoming long-wave radiation.

p.1393, l.24-26: unclear sentence: "new model schemes, which aim to incorporate permafrost" is too unspecific...what kind of models (land-surface schemes in GCM's, 1D energy balance models, spatial distribution models...)
p.1394, l.2-3: ground ice content, soil moisture ?
p.1394, l.5-6: first part of this study comprising the summer season
p.1394, l.10: better: In this second part, we focus...

We addressed all above comments above in the the revised version of the introduction:
In scenarios of the future climate obtained from current state-of-the-art General Circulation Models (GCM's), the Arctic experiences a much more pronounced warming compared to the global average. The strongest warming is expected to occur during winter which is already confirmed in current climate observations (Moritz et al., 2002; Johannessen et al., 2004). This warming trend is already reflected in widely increasing soil temperatures in arctic land areas underlain by permafrost (e.g. Osterkamp, 2005). The diversity and complexity of the processes governing the arctic climate constitute a major challenge for climate modeling, so that predictions are associated with a great uncertainty. The scientific report of the "Arctic Climate Impact Assessment" concludes that "much of the uncertainty in arctic climate change projections can be attributed to an insufficient knowledge of many of the physical processes active in the arctic

domain. (...) To validate coupled high-resolution models in the Arctic, improved and extended observational datasets are required. In situ observations exist for a few locations and restricted time periods, but more such datasets are needed." (ACIA, 2004). The latter is especially true for field datasets on the heat and moisture turnover at the land-atmosphere interface, which must be parameterized in an adequate way in climate models. For sea ice on the Arctic Ocean, the SHEBA study has compiled a comprehensive data set on the surface energy balance (Persson et al., 2002), which has been used extensively for model validation (e.g. Bretherton et al., 2002; Tjernstrom et al., 2005). For arctic land areas, Westermann et al. (2009) documented the annual course of the surface energy balance for a permafrost site on Svalbard, which, however, is not representative for the vast permafrost regions in Siberia, Canada or Alaska. These permafrost areas have received increased attention through scenarios, which suggest massive emissions of greenhouse gases from microbial decomposition of organic material thawing in the course of permafrost degradation. Permafrost models driven by the output of climate models (GCM's) predict a sizable reduction of the permafrost area until 2100 and an increase of the active layer thickness in the remaining area (e.g. Stendel and Christensen, 2002; Lawrence and Slater, 2005; Nicolsky et al., 2007; Lawrence et al., 2008). In order to improve the accuracy of such projections, it is desirable to obtain more regional datasets that can characterize the physical processes and serve as model validation.
This study is the second part of an extensive investigation on the annual surface energy balance at a polygonal tundra site in northern Siberia, which represents the first effort of that scope in the vast tundra regions of Northern Siberia. While the first part (Langer et al., 2011) focuses on the summertime energy balance and its spatial variability induced by heterogeneities in the polygonal tundra, this study focuses on the surface energy balance during the winter periods from October 1, 2007, until March 30, 2008, and October 1, 2008, until March 30, 2009. The objectives of the study are to (i) identify the controlling and limiting factors of the winter time surface energy balance, (ii) evaluate differences in the surface energy balance between the most

prominent landscape elements, namely the snow-covered tundra soils and freezing water bodies, and (iii) assess differences between the two winter seasons. The results are discussed with respect to modeling the arctic boundary layer and permafrost.

p.1394, l.14: the variability between two winters cannot be summarised as "interannual" variability - you are not showing results from a full "evaluation". Similarly, the term "evaluation of spatial differences" and "spatially distributed measurements" may be interpreted as a large number of spatial measurements. Please rephrase or add the spatial scale and/or the number of different spatially distributed measurements that were applied.

We recognize that the the study cannot deliver a full evaluation of the ineran-nual variability. Therefore, we rephrased this and further paragraphs.

p.1395, l.1-11: in this paragraph, it is not clear where the data come from - are all numbers given (permafrost depths, ZAA, soil temperature etc) taken from Grigoriev 1960 ? Are recent data available and discussed in the paper ? Are the 0.4-0.5m thaw depth the result from the present study, a mean of the area or a mean over time ?Where do the snow cover data come from ? Especially for a region which is so large, and contains many different permafrost features with only very few data available, it is important to be very specific what is meant.

We agree with the reviewer to give a more specific description and background information of the thermal ground characteristics. The given data stem from the results of the companion paper during the summer period, which is now cited. The snow depth data are results of this study, which are now moved from the study site description to the result section. The revised paragraph "Study site" now is:
The continental climate conditions are also reflected in the thermal regime of the soil, which is characterized by continuous permafrost reaching depths of 500 to 600 m *in*

the region of Lena River Delta (Grigoriev, 1960). During the observation period, the soil temperature is about -10C at the depth of the zero annual amplitude (ZAA $\approx$ 15 m*) and the maximum thaw depth ranges from 0.4 to 0.5* m *at the study site (Langer et al., 2011). The tundra surface is highly fractionated due to polygonal structures typically 50 to 100* m^2 *large. The rims of these polygons are elevated by about 0.2 to 0.5* m *compared to the centers. The polygonal centers consist of water-saturated peat soils or constitute ponds which frequently occur at the study site. During the winter period, the tundra soils are covered by a shallow snow layer, which has been observed to persist from October until May (Kutzbach et al., 2007).*

p.1395, Eq. (1): is snow melt not included because it does not play a role in winter ? It would be more consistent to inlcude the term formally, but then neglect it, as it is zero duricng the observation period.

We changed the Eq. 1 and the added an explanation for the neglected term of snow melt Q_{melt}.
The equation of the surface energy balance can be written as

$$Q_{\mathrm{net}} = Q_{\mathrm{H}} + Q_{\mathrm{E}} + Q_{\mathrm{G}} + Q_{\mathrm{melt}} + C, \quad (1)$$

where Q_{net} is the net radiation, Q_{H} the turbulent sensible heat flux, Q_{E} the turbulent latent heat flux, Q_G the subsurface (ground or snow) heat flux, Q_{melt} the energy consumed by the melting of snow, and C is the residual of the energy balance which accounts for inaccuracies of measurement. In the following the term Q_{melt} is neglected, since only temperatures well below the freezing point are considered during the observation period. The energy consumed by sublimation of snow is accounted for by the latent heat flux Q_{E}.

p.1395, l.17: use "cf." instead of "compare" throughout the whole manuscript.

Done.

p.1396, l.7-8: "Data from the net radiation sensor at the tundra site are not available for the ..."

Wording changed accordingly.

p.1396, l.11: "...radiation sensor during winter..."

Done.

p.1396, l.14: "cf Langer et al 2010" – see comment above

Done.

p.1396, section 3.1: Did you conduct a comparison of the two methods for estimating the outgoing thermal radiation in the two winters ? Otherwise the difference between the two years could well be a results of the different methods for measuring/calculating the outgoing radiation.

A sensor comparison has been performed during the summer period (cf. Langer et al. 2011). This comparison revealed a relative uncertainty of about 20% with an offset of less than 10 Wm^{-2} between both sensors. Therefore, we do not evaluate differences in the net radiation lower than10 Wm^{-2} which were measured with different sensors. Due to the critical remarks of this and the first reviewer, we added additional information on the measurement accuracy of the used instrumentation.

p.1396, l.25 – p.1397, l.6: this is unclear: more details have to be given or the reference to the other paper (Langer et al. 2010) must be more explicit. What

is already explained there and how does the approach in winter differ from the one in summer and why ? If all is the same then you should write this and do not go into detail at all.

In the revised version, we provide more details on the turbulent heat flux measurements and modeling under the winter conditions. Furthermore, references to the companion study are now more specific.
Based on these measurements the momentum flux u_*^2 and, in first order approximation, the buoyancy flux Q_{HB} can be inferred (Liu et al., 2001). These turbulent fluxes are calculated for 30 minute intervals with the "QA/QC" software package "TK2" including standard corrections and quality tests (Mauder and Foken, 2004; Mauder et al., 2007). For a more detailed description of the corrections and quality tests applied in the data post-processing please refer to the companion study (Langer et al., 2011, Sect. 3.2). In principle, the buoyancy flux Q_{HB} must be corrected according to the flux of water vapor in order to obtain the true sensible heat flux Q_{H} (Schotanus et al., 1983). With an expected Bowen ratio of $Q_{\mathrm{H}}/Q_{\mathrm{E}} \approx 0.5$, the true sensible heat flux is less than 3% lower than the measured buoyancy flux in the temperature range from -10 to -45C and about 10% lower for a Bowen ratio of 2 at 0C. Hence, we accept the buoyancy flux to be a good approximation of the real sensible heat flux for most of the observation period. According to a quality check and the exclusion of the lee wind sector (263to 277), about 18% of the flux measurements must be discarded. In carefully designed experiments, the applied quality criterion is found to be associated with a relative accuracy of about 15% which we assume to be appropriate for the measured buoyancy flux (Mauder et al., 2006).

p.1397, l.7: "modeled by an approach similar to the one used in the first part" Why "similar" ? do you mean "the same" or does it differ? And if yes, in what respect does it differ and why ?
p.1397, l.7-17: the model must be explained in detail or it must be cited, where it

is shown in detail! This paragraph is too unspecific in the details

We extended the description of the used model approach and provide a more specific reference to the companion study where details are given:
The latent heat flux Q_E at the tundra site is modeled for winter 2007-2008 by the same approach which is described in detail the companion study during the summer half year (cf. Langer et al., 2011, Appendix D). The model makes use of the available eddy-covariance measurements of the momentum flux u_*^2 and the buoyancy flux Q_HB, from which the turbulent transport coefficient is inferred. The atmospheric stratification is calculated based on the often applied parametrization introduced by Høgstrøm (1988). The near surface gradient of the specific humidity is inferred from measurements of relative humidity RH in 2 m height and the surface temperature T_surf from which the water vapor pressure above the snow surface is calculated using Magnus formula. The relative accuracy of the latent heat flux is estimated to be on the order of 25%, assuming Gaussian error propagation with a relative accuracy of 15% on the transport coefficient (inferred from the eddy measurements) and 20% on the gradient of the specific humidity.

p.1397, l.15-17: Do you have any indication from other sites that the relative humidity does not vary, in order that you can assume a humidity of 70 +/-5 each year ? Only because it did not vary in one year, it can not be assumed that it is like that every year! Especially if you want to address "interannual variability" !

We agree with the reviewer. We removed the concerning value from Tab. 2, as and none of the conclusions in the paper are based on this value.

p.1397, l.19: ": are calculated for both sites."

Changed.

p.1398, l.10: Why were the borehole and the TDR probes not available during the second winter? If most of the data were not available during the two winters why do you aim at comparing both years? Would it not be better to concentrate on the one year and add the results from the second year only when it explains some additional detail?

We agree that this study mainly focuses on the winter half year period 2007-2008. Data from the winter 2008-2009 are used as complementary information whenever available. In some cases, these data reveal a strong interannual variability of important processes, e.g. the refreezing of the active layer. However, as a general assessment of the interannual variability of all processes cannot be given, we rephrased the objectives of this study (see Sect. 1) in the revised version of the manuscript.

p.1398, l.14: inferring liquid water content from soil temperature assumes that no additional water flows into or out of the system. Is this the case at your site ? (lateral, vertical)

At the study site, horizontal and vertical water flow can be neglected during the freezing period. We added the following explanatory sentences:
In both years (before the onset of freezing) the soils are water saturated featuring a volumetric liquid water content of about 75%. During freezing, we assume the peat soils to stay saturated, since subsurface drainage is assumed to be negligible. In addition, the vertical water exchange is largely impeded by the formation of an ice layer at the soil surface with the beginning of freezing. Hence, it can be assumed that changes in the liquid water content are mainly attributed to the phase change of water during the observation period.

p.1398, l.19: the reference to Table 6 is unclear – Table 2 ?? (or Table 6,

Langer et al. 2010 ?)

References to the tables have been corrected in the entire manuscript.

p.1398, l.24: unclear reference of Table 6 - Table 2 ?

Done. Please see above.

p.1399, l.12: rephrase: Secondly, for winter 2007-2008 when the snow temperature profile was not available:

Done.

p.1399, l.23: again: this difference in methodological approach between the two winters might influence your interannual comparison !

For the identification of inter-annual differences in the surface energy balance we always consider the accuracy of the applied instrumentation. Details on the expected error ranges are now given in the method sections (see Sect. 3.1 and 3.2).

p.1400, l.5 and l.25: Table 2 instead of Table 6 ?

References to tables have been corrected.

p.1400, l.12-13: Did you compare the ultrasonic measurements with the Lewkowicz method for the data set in 2007/2008 to obtain a reference uncertainty between the two methods ?

A direct comparison between both method of snow height detection was not

possible with the available dataset. However, a comparison to standard snow depth measurements is given by Lewkowicz (2008) to which we now refer more specifically in the revised version (cf. Sect. 3.4).

p.1401, l.27: "inter-annual variability of the snow depth. "

Done.

Figure 4: AMSR-E shows a steady increase in snow cover thickness, whereas the on-site measurements show constant snow cover thickness between Nov 2007 and Feb 2008 and Dec 2008 and March 2009. This discrepancy is not mentioned in the text ("good agreement"). Why do you need AMSR-E in this context ?

We recognize that our statement of a "good agreement" is not appropriate in its generality. AMSR-E provides very large spatial averages of the snow water equivalent (12km), while ranging sensors deliver point measurements. Therefore, certain discrepancies between the both methods are not surprising. Substantial differences in the snow cover evolution already occur between the tundra and the pond site (cf. winter 2008-2009). Due to this high spatial variability of the snow cover, it is our intention to provide some independent information on the snow cover evolution in the wider area. In this regard, AMSR-E is used to verify that the measured inter-annual differences in snow depth are not solely caused by spatial inhomogeneities, but correspond to a regional tendency in the snow cover evolution.

p.1402, l.7, 15, p.1403, l.4, 6, 21, p.1404, l.18, 26-27, p.1406, l.24, p.1408, l.14, p.1411, l.12: reference to table 6 wrong

References corrected.

p.1404: it is not necessary to cite the Table with the results (2 and 3) after each sentence describing some of the results, if no misunderstanding is possible. A few times would be sufficient.

References have been reduced.

p.1405, l.14: "air masses"

Done.

p.1406, l.14: "by about 5-10cm"

Corrected.

p.1406, l.19-21: "The sensible heat flux: ": this sentence is partly a repetition to lines 8-10

Repetitions removed.

p.1406-1407: too many unnecessary references to Figure 7

References reduced.

Figure 8: a reference to the time period of the data is missing in the caption (all winter measurements ? 2007/8 or 2008/09 etc)

Reference to the displayed period has been added to the caption.

Figure 9: caption: reference to Table 6 wrong

References corrected.

p.1410, l.8-27: this is a useful summary of the findings of the surface energy balance – however, it refers primarily to the summer period, which was discussed in Part I. In order not to double the publications, I recommend merging Part I and Part II, as the real significance of the study will only be obvious when discussing the surface energy balance of the whole year (as was done in section 5.2 and Fig. 9).

In the revised version of the manuscript, we focus the paragraph "Summary and Conclusions" on the results of this study.

p.1411, l.20-27: the importance of the ground heat flux is a rather trivial implication drawn from the measurement results: this has been recognized in many many studies (and a reference to them is not really necessary), and the reason why it is not included in many atmospheric models is rather technical, than based on insufficient knowledge. This paragraph can therefore be shortened considerably.

We do not comment on the reasons, why a realistic representation of soil thermal processes is not included in atmospheric models. However, considering that more than 60% of the energy lost through net radiation in winter is provided by the ground heat flux, we do believe that a realistic representation of soil thermal processes is key to a realistic modeling of the surface energy budget. We agree that the general effects of the ground heat flux are known, but such high ground heat fluxes (compared to the net radiation) over such a long period (6 months) have not been reported yet. As this could be the case for vast areas dominated by permafrost in Siberia, improving the representation of soil thermal processes could result in improvements of the atmospheric models, which we believe is an important discussion point in the paper. However, we restructured the entire discussion (see Sect. 5) and focused and shortened the implications drawn from the magnitude of the measured ground heat flux (cf. Sect. 5.3).

p.1412, l.1-6: Similarly, the reference to the thermal offset due to the snow cover is a well established concept, and can not be seen as a major implication. As written by the authors themselves, the "impact of the snow cover on the atmospheric conditions:" has been demonstrated in numerous studies – it would be more interesting to discuss the reliability of the given 4C difference, and/or the range of this estimate. If these ranges are not available, this paragraph can be merged with item 1, and shortened considerably.

According to the comments of the first reviewer and in order to sharpen the focus of study, we concentrate on the surface energy balance and reduced the implications for the thermal state of permafrost. Hence, the issue raised by the reviewer is not longer included in the revised version.

p.1413, Summary and conclusions: as written before, the summary is based on the first and the second part of the study. This again confirms the possibility to merge both parts, by this reducing unnecessary repetitions. No reference to permafrost is made. As this was also only marginally discussed throughout the manuscript I suggest changing the title to "Surface energy balance of a..."

The given annual summery has been rewritten so that the focus is shifted to the winter season. Furthermore, in the direct comparison we discus and give new insights into the annual energy and water budget (see Sect. 5.2).
In accordance with this and the first reviewer, we agree that permafrost is just a secondary issue in this study. Therefore, we changed the title of this and the companion manuscripts to: "The surface energy balance of a polygonal tundra site in northern

Siberia ..."

References

ACIA: Impacts of a Warming Arctic-Arctic Climate Impact Assessment, Impacts of a Warming Arctic-Arctic Climate Impact Assessment, by Arctic Climate Impact Assessment, pp. 144. ISBN 0521617782. Cambridge, UK: Cambridge University Press, December 2004., 1, 2004.

Bretherton, C., de Roode, S., Jakob, C., Andreas, E., Intrieri, J., Moritz, R., and Persson, P.: A comparison of the ECMWF forecast model with observations over the annual cycle at SHEBA, J. Geophys. Res., 2002.

Grigoriev, N.: The temperature of permafrost in the Lena delta basin – deposit conditions and properties of the permafrost in Yakutia, chap. 2, pp. 97–101, Yakutsk, in Russian, 1960. stroèm1988non

Høgstrøm, U.: Non-dimensional wind and temperature profiles in the atmospheric surface layer: A re-evaluation, Boundary-Layer Meteorology, 42, 55–78, 1988.

Johannessen, O., Bengtsson, L., Miles, M., Kuzmina, S., Semenov, V., Alekseev, G., Nagurnyi, A., Zakharov, V., Bobylev, L., Pettersson, L., et al.: Arctic climate change: observed and modelled temperature and sea-ice variability, Tellus A, 56, 328–341, 2004.

Kutzbach, L., Wille, C., and Pfeiffer, E.: The exchange of carbon dioxide between wet arctic tundra and the atmosphere at the Lena River Delta, Northern Siberia, Biogeosciences, 4, 869–890, 2007.

Langer, M., Westermann, S., Muster, S., Piel, K., and Boike, J.: The surface energy balance of a polygonal tundra site in northern Siberia – Part 1: Spring to fall, The Cryosphere, 5, 151–171, doi:10.5194/tc-5-151-2011, 2011.

Lawrence, D. and Slater, A.: A projection of severe near-surface permafrost degradation during the 21st century, Geophys. Res. Lett, 32, L24 401, 2005.

Lawrence, D., Slater, A., Romanovsky, V., and Nicolsky, D.: Sensitivity of a model projection of near-surface permafrost degradation to soil column depth and representation of soil organic matter, J. Geophys. Res, 113, F02 011, 2008.

Lewkowicz, A.: Evaluation of miniature temperature-loggers to monitor snowpack evolution at mountain permafrost sites, northwestern Canada, Permafrost and Periglacial Processes, 19, 323–331, 2008.

Liu, H., Peters, G., and Foken, T.: New equations for sonic temperature variance and buoyancy heat flux with an omnidirectional sonic anemometer, Boundary-Layer Meteorology, 100, 459–468, 2001.

Mauder, M. and Foken, T.: Documentation and instruction manual of the eddy covariance software package TK2, Univ. of Bayreuth, Dept. of Mikrometeorology, 2004.

Mauder, M., Liebethal, C., Göckede, M., Leps, J., Beyrich, F., and Foken, T.: Processing and quality control of flux data during LITFASS-2003, Boundary-Layer Meteorology, 121, 67–88, 2006.

Mauder, M., Foken, T., Clement, R., Elbers, J., Eugster, W., Grunwald, T., Heusinkveld, B., and Kolle, O.: Quality control of CarboEurope flux data–Part II: Inter-comparison of eddy-covariance software, Biogeosciences Discussions, 4, 4067–4099, 2007.

Moritz, R., Bitz, C., and Steig, E.: Dynamics of recent climate change in the Arctic, Science, 297, 1497, 2002.

Nicolsky, D., Romanovsky, V., Alexeev, V., and Lawrence, D.: Improved modeling of permafrost dynamics in a GCM land-surface scheme, Geophysical Research Letters, 34, L08 501, 2007.

Osterkamp, T.: The recent warming of permafrost in Alaska, Global and Planetary Change, 49, 187–202, 2005.

Persson, P., Fairall, C., Andreas, E., Guest, P., and Perovich, D.: Measurements near the Atmospheric Surface Flux Group tower at SHEBA: Near-surface conditions and surface energy budget, Journal of Geophysical Research-Oceans, 107, doi:10.1029/2000JC000705, 2002.

Schotanus, P., Nieuwstadt, F., and Bruin, H.: Temperature measurement with a sonic anemometer and its application to heat and moisture fluxes, Boundary-Layer Meteorology, 26, 81–93, 1983.

Stendel, M. and Christensen, J.: Impact of global warming on permafrost conditions in a coupled GCM, Geophysical Research Letters, 29, 1632, 2002.

Tjernstrom, M., Zagar, M., Svensson, G., Cassano, J., Pfeifer, S., Rinke, A., Wyser, K., Dethloff, K., Jones, C., Semmler, T., et al.: Modelling the arctic boundary layer: an evaluation of six ARCMIP regional-scale models using data from the SHEBA project, Boundary-layer meteorology, 117, 337–381, 2005.

Westermann, S., Lüers, J., Langer, M., Piel, K., and Boike, J.: The annual surface energy budget of a high-arctic permafrost site on Svalbard, Norway, The Cryosphere, 3, 245–263, 2009.

Interactive comment on The Cryosphere Discuss., 4, 1391, 2010.