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Discussions

Interactive comment on "Permafrost and surface energy balance of a polygonal tundra site in northern Siberia – Part 1: Spring to fall" *by* M. Langer et al.

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We take note of the critical remarks on our manuscript. In the following we carefully consider all comments of the reviewer which are marked in bold. Changes in the manuscript are marked in italic.

Originally I was enthusiastic to learn more about the permafrost and surface energy balance of a polygonal tundra site in northern Siberia at Samoylov Island. Actually, the locality by itself seems to warrant a publication in the scientific literature. However what I had to read was more of an experience with

the Department of the Seemingly Obvious – a nice Master's thesis approach but without the scientific rigor that I expected.

We agree with the reviewer that the locality itself warrants publication, since energy balance studies in the Arctic are still rarely available. This is especially true for large areas in Siberia where only a few studies are published. This study aims to narrow the gaps of knowledge about the magnitudes of energy turnover at a polygonal tundra landscape, its temporal variabilities during the summer half year, and its spatial variabilities according to typical surface heterogeneities. We make use of "state of the art" measurement techniques such as eddy covariance and go beyond this state by the application of multiple and mobile senors for the detection of spatial variabilities. The study is strengthened by modeling approaches for maximizing the data density and quality. Furthermore, important surface and -subsurface parameters such as soil thermal properties and aerodynamic characteristics are evaluated. In the response to this review, we will to show that the study is performed with due diligence and "scientific rigor".

The present manuscript goes a couple steps behind what Ohmura already published (see Ohmura (1984), Ohmura (1982a), Ohmura (1982c)), Ohmura (1982b), Ohmura (1982d)). Most of the statements are either textbook knowledge or qualitative statements that are so broad and all-inclusive that they cannot really be wrong. This does not really add anything to our current understanding of Arctic energy budgets of polygonal tundra. And some of the rather bold statements are not really based on profound quantitative analyses of available data.

We acknowledge the classical papers, especially Ohmura (1982a) and Ohmura (1984) which we now cite in the introduction and the discussion. However, our study adds new insights to:

- the temporal evolution of the energy balance at a polygonal tundra site including the shoulder seasons (snow melt and freezing back). In particular, our findings concern the triggering factors of snow melt and the timing of freeze back.
- the spatial variability of all energy balance components. In the revised manuscript, we have added a new evaluation of the differences of sensible and latent heat fluxes between dry and wet surfaces, which demonstrates that the energy partitioning is distinctly different over distances of a few meters.
- the impact of small water bodies on the net radiation, which is distinctly different from the net radiation at tundra surfaces. This is of particular importance since small ponds are frequent landscape features in the polygonal tundra, but are difficult to identify with e.g. remote sensing.

More general, there are important differences between the studies by Ohmura which have been performed on Axel Heiberg Island, and our study. An Axel Heiberg Island, the landscape is characterized as semi desert heath tundra, and thus representative for many High Arctic landscapes. In contrast, our polygonal tundra site is representative for a large part of the wet tundra of the Siberian Arctic, presently relatively unknown, and with different climate characteristics. Thus, our results are of great importance not only for understanding of the local energy balance turnover, but also as a complementary study to other arctic landscapes. It is our firm belief that even more regional studies are required in order to better resolve and understand the complex factors controlling the energy turnover at the surface in arctic landscapes.

Paragraph changed in the introduction:
In order to support and validate modeling, it is desirable to obtain regional process studies, which deliver important information about the landscape-specific energy balance characteristics and their determining factors. Many publications have demonstrated the value of such regional studies, which show that measurements on energy

and water balance are still needed to improve the parameterizations of climate models (van den Hurk et al., 2000; Betts et al., 2001, 2003). Several energy balance studies are already available for the North American Arctic (e.g., Ohmura, 1982a, 1984; Eaton et al., 2001), and more are contained in the comprehensive reviews by Eugster et al. (2000) and Lynch et al. (1999). For the European Arctic, including Svalbard, energy-balance studies are published by Lloyd et al. (2001) and Westermann et al. (2009). However, Siberian sites are not included and generally few published studies are available for the Siberian tundra (Kodama et al., 2007; Boike et al., 2008).
In this study we present data on the surface energy balance of a typical tundra landscape in northern Siberia collected between April and September during each of 2007 and 2008. This article comprises the first part of a long-term energy balance study over the entire annual cycle of two consecutive years. The study aims to (i) compile the surface energy balance at a polygonal tundra site during the summer half year period, (ii) determine the seasonal and spatial variability of each energy-balance component which gives insight in the driving processes of the coupled permafrost-snow-atmosphere system, and (iii) identify the dominant factors that determine the energy partitioning and subsurface heat budget (active layer dynamics, permafrost temperatures).

Section 5.4 added to the discussion:
Only few studies on the surface energy balance exist for arctic regions, most of which only cover short periods or do not include all components of the energy balance. The most comprehensive summary for a number of sites in the Arctic and Subarctic (except Siberia) is given by Eugster et al. (2000). In addition, studies on the surface energy balance have been published for Alaska (Ohmura, 1982a; Harazono et al., 1998; Mendez et al., 1998; Lynch et al., 1999; Vourlitis and Oechel, 1999; Chapin et al., 2000), Greenland (Soegaard et al., 2001), Svalbard (Harding and Lloyd, 1998; Westermann et al., 2009) and Siberia (Boike et al., 1998; Kodama et al., 2007). Since most of these studies only provide flux values for the summer season (July - August),

a meaningful comparison is only possible for this time. Here, we use the averages of the summer period in 2007.
The Bowen ratio of around 0.35-0.5 observed in this study for polygonal tundra in Siberia demonstrates the high evapotranspiration rates of a typical wet land and is well within the given range from other arctic wetland locations. The total evapotranspiration rate of 1.4 $\mathrm{mm\,d^{-1}}$ *falls between the lower ranges reported from Svalbard (*$\approx 1\,\mathrm{mm\,d^{-1}}$*) (Lloyd et al., 2001; Westermann et al., 2009) and higher values for wetland sites in Greenland (1.5*$\mathrm{mm\,d^{-1}}$*) and Alaska (1.5 to 2.3*$\mathrm{mm\,d^{-1}}$*).*
With a fraction of 20% of the net radiation, the ground heat flux observed at the study site is among the largest values reported for Alaskan, Greenland and Svalbard sites (Eugster et al., 2000; Westermann et al., 2009), while ground heat fluxes from other Siberian sites reported by Kodama et al. (2007) and Boike et al. (1998) fall in the same range. The main reasons for this are most likely similarly cold permafrost temperatures and a similarly large annual temperature amplitude due to the continental climate conditions.

We also removed all statements which might be too general or do not contain important information such as:

- *The increasing temperatures are accompanied by steadily increasing short-wave radiation after the end of the polar night and the shrinking snow cover towards the end of the period.*
- *We emphasize that the correct representation of the snow melt must be considered crucial in permafrost modeling. It is therefore highly desirable to gather a better understanding of the triggering processes of the snow melt and their representation in models.*
- *As the contribution of the ground heat flux to the surface energy balance is significant even in the summer months, an adequate representation of the soil domain*

in global climate models seems mandatory, if the land-atmosphere exchange processes in permafrost regions are to be modeled correctly.
- *In the second part of this study (Langer et al., 2010b), the representation of the soil and particularly of soil freezing processes in models is discussed in more detail.*
- *Since the long-term averages of the turbulent heat fluxes are almost equal for the stationary and the mobile eddy covariance system, we conclude that the presented sensible and latent heat fluxes are representative on the landscape scale.*

For example, page 923, lines 22-26: "As the contribution of the ground heat flux to the surface energy balance is significant even in the summer months, an adequate representation of the soil domain in global climate models seems mandatory, if the land-atmosphere exchange processes in permafrost regions are to be modeled correctly." – this is not supported by your data. Anyone who knows about global climate models also knows the costs associated with it; before you can show that any process that is not covered in the model must be included, it is important to show the level of significance. It is a misconception to assume that a more detailed model will automatically achieve a more correct representation of reality.

Considering our results, we belief that it is appropriate to state the importance of the ground heat flux and discuss its potential impact on climate modeling in permafrost regions in the discussion section. However, the corresponding statement has been replaced (see above) to clarify our intention:
In general, the high contribution of the ground heat flux to the surface energy balance indicates that it must be considered as an important factor in larger-scale model approaches. This is clearly different to the mid latitudes, where the average ground heat

flux usually ranges between 0 to 6$\mathrm{W m^{-2}}$, which is about 5% of the net radiation and hence is often considered to be of less importance in modeling and validation studies (e.g., Baker and Baker, 2002; Boone et al., 2009). It should be carefully checked, whether the parameterizations of soil thermal processes (which have been developed in and for the mid-latitudes) employed in current large-scale circulation models can adequately account for the prominent role of the ground heat flux in permafrost areas. This issue is of particular importance, as permafrost areas with continental climate, where high ground heat flux can be expected, occupy vast areas in the Arctic. This improvement of modeling results by employing more realistic parameterizations of the soil processes has been outlined in a number of studies (e.g., Peters-Lidard et al., 1998; Cox et al., 1999; Viterbo et al., 1999; Pitman, 2003).

Other aspects on the experimental side of the manuscript seem similarly flawed. For example, page 924, lines 8–11: "With albedo differences between wet and dry areas on the order of 0.05, the net short-wave radiation can be on average by up to 7 W m2 higher at wet compared to dry areas, while the differerences can exceed 25 W m2 for high radiative forcing during midday." – if you ever read the manual of such an instrument you will find the following specifications for the Kipp and Zonen CNR1:
Pyranometer: Zero offsets at 200 $\mathrm{Wm^{-2}}$ thermal radiation: +15 $\mathrm{Wm^{-2}}$ and for 5 $\mathrm{Kh^{-1}}$ change in ambient temperature: $\pm$ 4 $\mathrm{Wm^{-2}}$. The directional error is specified at $\pm$ 25 $\mathrm{Wm^{-2}}$ (at 1,000 $\mathrm{Wm^{-2}}$).

Considering the reviewer's remark, we believe that it is necessary to clarify some principles of radiation measurements and accuracies. A "Pyranometer" is, by definition, a short-wave radiation sensor, while the description of accuracies given by the reviewer rather holds for a "Pyrgeometer" which is sensitive to thermal radiation.
Compare Glossary of Meteorology (http://amsglossary.allenpress.com/glossary).

The suggested error margins are therefore not applicable for the discussed albedo difference between wet and dry surfaces, which only concerns short wave radiation.

Pyrradiometer: The Kipp and Zonen manual sais "On a sunny windless day with a solar irradiance of 1000 $\mathrm{Wm^{-2}}$, an error of 25 Watts per square metre can be expected."

A "Pyrradiometer" is a net radiation sensor sensitive to the entire spectrum of solar and terrestrial radiation and thus is not relevant for the discussion on albedo differences.
Compare Glossary of Meteorology (http://amsglossary.allenpress.com/glossary).
However, we guess that the given accuracy should refer to the CNR1 "Pyranometer", since similar values can be found in the manual for this short wave radiation sensor. As described in the manuscript, for the detection of albedo differences we use a mobile short wave radiation sensor (SP1110, Skye Instruments, USA) for measuring outgoing short-wave radiation, while the incoming radiation is measured by the short wave-sensor of the CNR1. According to the manuals, the relative accuracy of the SP1110 is $\pm 5\%$, while the relative accuracy of the short-wave sensor of the CNR1 for daily sums is $\pm 10\%$.
(SP1110: ftp://ftp.campbellsci.com/pub/csl/outgoing/uk/manuals/sp1110.pdf)
(CNR1: ftp://ftp.campbellsci.com/pub/csl/outgoing/uk/manuals/cnr1.pdf)
Assuming Gaussian error propagation the resulting uncertainty on the measured albedo value $e(\alpha)$ can be calculated as follows:

$$e(\alpha)^2 = e(Q_{S\uparrow})^2 + e(Q_{S\downarrow})^2, \tag{1}$$

where $e(Q_{S\uparrow})$ and $e(Q_{S\downarrow})$ are the relative uncertainties of the measured outgoing and incoming short wave radiation. For the given values, this results in an relative uncertainty of $\pm 11\%$. Hence, for an albedo value of 0.2 the absolute accuracy is ± 0.02 and

± 0.015 for an albedo of 0.15. The absolute error on the albedo difference $E(\Delta\alpha)$ can be calculated as

$$E(\Delta\alpha)^2 = E(\alpha_{\text{wet}})^2 + E(\alpha_{\text{dry}})^2, \tag{2}$$

where $E(\alpha_{\text{wet}})$ and $E(\alpha_{\text{dry}})$ are the absolute errors on the measured albedo values at a wet and dry location respectively. This results in the albedo difference of 0.05 with an uncertainty of ± 0.025. Applying this difference of the surface albedo on the average value of incoming solar radiation, we expect a difference of about $7 \pm 4\,\text{Wm}^{-2}$ in the net short wave radiation budget between wet and dry surfaces, which could exceed values of $25 \pm 13\,\text{Wm}^{-2}$ during conditions of high radiative forcing. To clarify the accuracies of our radiation measurements we revised the concerned paragraph as follows:
To get a more differentiated picture of the short-wave radiation balance, spatially distributed measurements of the outgoing short-wave radiation are performed with a mobile short-wave radiation sensor (SP1110, Skye Instruments, USA). The measurements are performed under clear-sky conditions over a period of 3 to 4 hours around solar noon. Based on these time series, average albedo values for wet and dry surfaces are inferred using incoming short-wave radiation, as measured by the CNR1 sensor. Following the accuracies of the sensors given in the manuals, the uncertainty for the calculated albedo value should be on the order of 10%.

Your text on pages 906/907 seems to partially express this knowledge, but then you argue (on page 919): "This high closure term can be partly explained by an underestimation of the net radiation: a comparison of the NR-Lite and the four component sensor during fall 2008 reveals an offset of 6 Wm^{-2}, which is substantial considering the generally low radiation budget." – in my view this is a misconception: if something is not statistically significant (6 Wm^{-2} is way below the accuracy of any of your radiation instruments!) then you cannot claim at the same time that it is substantial. In reality your measurements are unable

to support the hypothesis that the null hypothesis (difference is zero) can be rejected.

The reviewer's statement concerns the relatively high closure term during the fall period. The in-situ sensor comparison between the NR-Lite and the CNR1 revealed an average offset of about 6 Wm^{-2} between both instruments. This value is within the accuracy level of the sensors found in other studies, and within the specifications given in the manuals. This offset between the instruments might explain the observed closure term which is in the range of 10 Wm^{-2}.
We believe that invoking the concept of the null hypothesis is not of any help here. As stated by the reviewer, in this case, a possible null hypothesis would be: "The two sensors receive the same amount of radiation and the difference in the amount of radiation received at the sensors is hence zero." The reviewer is fully right, that we cannot reject this null hypothesis, as the errors associated with both measurement devices are too large. However, this is not the question here. The two sensors are directed towards the same surface spot, so we KNOW, that the amount of radiation received at the sensors is equal. The two sensors, however, yield different measured values, with an offset of on average 6 Wm^{-2} between both instruments. What we have done here, is a simple comparison of the two sensors, as it is a standard procedure in meteorological applications.

Another serious conceptional error is found in Fig. 8 and text on page 921: "If only random measurement errors would be involved, the expected EBR distribution should be more similar to the normal distribution displayed in Fig. 8." - this is not correct. If you look at the uncertainty of a ratio where both the denominator and enumerator are sums or differences (your Eq. (8) for EBR), then it already becomes clear where the conceptual flaw is: already Wikipedia knows (http://en.wikipedia.org/wiki/Ratio_distribution) that ratios of normally distribute variables are not normally distributed: "When X and Y are

independent and have a Gaussian distribution with zero mean the form of their ratio distribution is fairly simple: It is a Cauchy distribution. However, when the two distributions have non-zero mean then the form for the distribution of the ratio is much more complicated." Now, also the difference and sum between normally distributed variables are most likely not normally distributed (one finds plenty of hits by just googling for "probability distribution of sum of two variables" gives a good starting point to learn about unknown topics. My view is that the scientific literature should be more rigorously founded than Wikipedia and other sources, and hence I would reject such an article that does not really comply with available knowledge and does not advance our understanding of the system under investigation. I hence strongly disagree with your statement on page 921, lines 3–8: "The normalized EBR distribution of the entire data set roughly resembles a normal distribution featuring a mean value of 0.86, a standard deviation of 0.34, and a slightly positive skewness of 0.47 (Fig. 8). If only random measurement errors would be involved, the expected EBR distribution should be more similar to the normal distribution displayed in Fig. 8."

Following the critical remarks of this and the second reviewer, we revise the entire section and provide an extended error discussion in Sect. 5.1. The issues raised by the reviewer do not appear in the revised version.

Another critical issue is the reference to Langer, M., Westermann, S., Muster, S., Piel, K., and Boike, J.: Permafrost and surface energy balance of a polygonal tundra site in northern Siberia – Part II: Winter, in preparation, 2010b. (pages 904, 922, 924, 925). I could not find this paper and it appears that a fair assessment of Part I would only be possible if the reviewer had access to Part II. In fact, normal journals tend to reject manuscripts that make such extensive references to unpublished material.

We apologize for the delay in the submission of the second part of this study, which is now available at TCD (Langer et al., 2010b).

Your argumentation with respect to atmospheric stability in the arctic region is inconsistent. On page 909, lines 3–6 you write ". . . which is based on tests for stationarity of the turbulence and the integral turbulence characteristic (ITC). In this study, the latter is not applied, since the quality criterion of the integral turbulence characteristic is not well defined in arctic region, where stable atmospheric stratification and intermittent turbulence are common (Lüers and Bareiss, 2009)." but then on page 916, lines 25–28 you claim that "In both years, unstable stratifications ($\zeta < 0$) occur frequently during the day, but usually do not last longer than 12 h. The nights are dominated by neutral stratifications ($\zeta \approx 0$), while stable atmospheric conditions ($\zeta > 0$) are only observed occasionally under calm conditions and highly negative values of the net radiation." – this is a direct contradiction. Moreover the Lüers and Bareiss (2009) article cannot be found in the databases I have access to. . .

Due to the critical remarks of this and the second reviewer on the description of the processing of the eddy covariance data, we revised this paragraph. The inconsistent statement and the concerned reference do not longer appear. Description changed to:
The applied quality assessment follows the scheme of Foken et al. (2004) based on tests for stationarity of the turbulence characteristic. The stationarity criterion is considered to be sufficiently fulfilled (quality flags 1 and 2) if the average covariance inferred from 5 minute sub-intervals do not deviate by more than 30% from the covariance value over the entire 30 minute interval (Foken et al., 2004).

Minor issues

902/7: sounds like 1.5 years, not like "half year period"

We changes this part of the abstract to:
The study was performed during half-year periods from April to September in each of 2007 and 2008. The surface energy balance is obtained from independent measurements of the net radiation, the turbulent heat fluxes, and the ground heat flux at several sites.

904/3: here you say "This study presents the annual energy balance" but title and abstract only focus on spring to fall

The sentence has been changed to:
In this study we present data on the surface energy balance of a typical tundra landscape in northern Siberia collected between April and September during each of 2007 and 2008. This article comprises the first part of a long-term energy balance study over the entire annual cycle of two consecutive years.

Eq. (3): this is misleading - the emissivity (ϵ) is not necessarily the same for the local surface and the sky; you imply that the surface emissivity also applies to the sky, which is incorrect in most cases

Eq. 3 does not contain the "emissivity of the sky", but only the surface emissivity ϵ. The displayed equation results from Kirchhoff's Law, which states that the radiation $Q_{L\uparrow}$ originating from a gray body with surface emissivity ϵ is

$$Q_{\mathsf{L}\uparrow} = (1-\epsilon)Q_{L\downarrow} - \epsilon\sigma T^4_{\mathsf{surf}}. \quad (3)$$

The fraction $(1-\epsilon)$ of the incident thermal radiation is reflected, plus the fraction ϵ of the black-body radiation emitted by an ideal black body with temperature T_{surf}. In the revised version of the manuscript, we have added the above equation to avoid

confusion.

907/14–17: a thermal infrared sensor as I know them measures radiation, and by specifying an emissivity of the local surface (some sensors use a fixed value of ($\epsilon = 0.95$) one computes the surface temperature. Hence, if you want to backcalculate the radiation you have to use the emissivity that the instrument has hard-coded, not the emissivity of the surface (since this is irrelevant for the infrared radiometer)!

The employed infrared surface temperature sensors (IRTS-P) measure brightness temperatures (misleading wording has been corrected, see below) and hence do not use a predefined emissivity value (Bugbee et al., 1998). Based on the measured brightness temperature the surface temperature T_{surf} can be inferred by using different emissivity values for wet and dry surfaces. Due to the non-blackbody emissivity characteristics of the surface, it is further necessary to account for the reflected fraction of thermal sky radiation in the spectral range of the sensor ($7 - -14\mu$ m). Therefore, we use the down welling thermal radiation measured by the CNR1 sensor. The used method follows the scheme described by Langer et al. (2010a), which assumes blackbody characteristics of the emitted sky radiation. This approximation is acceptable for the majority of measurements due to prevalent cloudy conditions at the study site (compare Langer et al., 2010a). The corresponding paragraph has been changed to:
According to instrument specifications, the IRTS-P sensors measure over a spectral range of 7-14 µm and deliver brightness temperatures with accuracies of about ± 0.5 C (Bugbee et al., 1998). The true surface temperature T_{surf} and upwelling thermal radiation $Q_{\mathsf{L}\uparrow}$ are calculated similar to the approach described by Langer et al. (2010a) which accounts for the surface emissivity ϵ and the back scattered fraction of down welling thermal radiation. In accordance with Langer et al. (2010a) we assume emissivities of 0.98 for wet, and 0.96 for dry tundra surfaces. For averaging periods

longer than a week the expected error on the true surface temperature is smaller than $\pm 1\,\mathrm{C}$ *(Langer et al., 2010a). This relates to an error in* $Q_{L\uparrow}$ *of about* $\pm 5\,\mathrm{W m^{-2}}$ *in the relevant temperature range from -10 to 30* C.

908/25: the wording "the internationally standardized QA/QC software package TK2" is misleading – TK2 was not internationally standardized, it was used as a reference for the Mauder et al. 2007 comparison without establishing that it conforms to some absolute standards. Moreover, the comparison was only done within the Carboeurope project. Hence a more cautious wording is highly recommended.

In accordance with the reviewer, we believe that cautious wording is recommend in scientific literature and discussions. We changed the concerned sentence to:
The turbulent fluxes are calculated for 30 minute intervals with the 'QA/QC' software package 'TK2' (Mauder and Foken, 2004; Mauder et al., 2007), which includes standard corrections and quality tests. Besides the aforementioned correction of the buoyancy flux, processing the data involves an adjustment of the horizontal and vertical wind speed components using the planar fit correction (Wilczak et al., 2001), and an adjustment due to the displacement between anemometer and gas analyzer (Moore, 1986).

911/17: brackets around citation missing

Done.

915/23–24: "During the pre-melt period, the atmospheric stratification changes frequently between stable and unstable conditions (Fig. 2)." – first, this cannot be seen clearly in Fig. 2 with the scaling you use, and second it is quite normal that the diurnal cycle of stability goes from unstable during daytime to stable

during the night. This is so ubiquitous a statement that it really does not advance our understanding of polygonal tundra.

We believe that the scale and resolution of Fig. 2 are sufficient to see the frequent changes between stable an unstable conditions during the pre-melt period which ranges from April 1 to May 10. For better visualization of the pre-melt and melt period we added a separation line to the figure. In the following text, we stress that this behavior of the atmospheric stratification changes suddenly with the onset of snow melt. This behavior reveals that the atmospheric stratification and hence the larger scale turbulence characteristics are strongly related. This aspect becomes important when the triggering processes of snow-melt are discussed. As argued in the discussion section, the immediately increased turbulent exchange and the positive temperature gradient towards the surface suggest that snow-melt is at least partly related to larger scale changes in the atmospheric conditions (influx of warm air masses). This process is important for modeling, as well as for the understanding of the energy and water balance beyond the local scale and requires more attention in future work.

Try to get rid of "essentially" – in most cases the word is used inappropriately. E.g. page 915, lines 21–22: "The atmospheric stratification is reflected in the stability parameter ζ, which is essentially zero during neutral conditions" – this is the definition of neutral conditions: $\zeta = 0$, and hence is not a finding of this study.

Done. We revised the entire manuscript with respect to wording.

917/7: "dew fall" – although widely used in the trivial literature this expression is scientifically unsound; dew cannot fall, it can only form on surfaces (from which it can drip off)! Use "dew formation" instead.

Many publications indicate that the term “dew fall” is widely used in scientific literature (e.g. Oke, 1987; Garratt, 1994). However, we appreciate the effort for a consistent and scientifically sound wording. We changed “dew fall” to “dew formation”.

917/10: Fig. 4 comes after Fig. 5, rearrange your figures chronologically

Done.

917/21: equally spaced intervals is always a good idea for time series, but why do you use a 20-day averaging? The natural intervals for times are seconds, minutes, hours, days, weeks, . . .

The twenty day interval was found to be good compromise between intervals that are large enough to include an adequate data density and resolution for the visualization of the course of the energy balance during the summer half-year period. Different intervals would not change the information content of the figure and would also be somehow arbitrary. We add the following explanatory sentence to the caption of Fig. 3:
For the visualization, the averaging intervals of 20 days are found to be a good compromise between temporal resolution and data density.

918/17–18: what should my imagination tell me about a zero-curtain? Please use dictionary words!

The term zero-curtain is well establishes in periglacial science (compare: Multi-language glossary of permafrost and related ground-ice terms (Van Everdingen, 1998))

921/18–21: these are purely qualitative statements that are unrelated to your measurements. Dialectic argumentation without any quantitative measurements would have produced the same views.

These statements just summarize the three major factors that control the local energy balance characteristics. In the following paragraphs these factors and their impact on the local energy balance are further explained and related processes are discussed based on the measurements presented in the result section. This discussion would not be possible without the performed measurements.

922/15–21: why not just do it? I quickly browsed a literature data base I have access to to give you some potentially relevant literature citations that you might find useful to explore the surface energy budget of sites where snow plays a role. Also the abovementioned Ohmura citations may be helpful. With respect to snow modeling there might be work by Glen Liston which might be of interest.

We carefully considered the suggested literature and refer to it, whenever a closes relation to our study was found. Further more Section 5.4 now includes a comparison of our results with available arctic energy balance literature (see above). However, the concerned paragraph refers to the triggering processes of snow melt. Many studies report net radiation to be the dominant factor controlling snow melt (e.g. Ohmura (1982)). In this study, we show a distinct contribution of sensible heat flux which is related to larger scale advection of warm air masses. Therefore, we suggest to further explore this issue, since it directly concerns the timing of snow melt which has an drastic impact on the surface energy balance. In order to reduce redundancies, the concerned sentence has been removed (please see above).

922/24–27: “During the spring period, our results indicate that the observed

interannual differences in the ground temperatures are caused by different air temperatures, which are presumably related to the general synoptic conditions." – well, what other explanation would you have expected? In my view such a statement is so ubiquitous that it cannot be wrong, but does not relate to the specific conditions of polygonal tundra nor a Siberian site.

The fact, that during the spring period inter-annual differences in the subsurface heat budget are related to differences in the air temperature and associated with negative sensible heat fluxes indicates that the local springtime warming is subject to warm air masses, which must be advected from other locations. This in turn indicates that the rewarming of permafrost during spring is rather attributed to larger scale processes of atmospheric circulation than to the local surface characteristics e.g. snow cover. We used the expression "general synoptic conditions" with the intention to describe the non-local characteristics of this process. Sentence changed to:
During the spring period, our results indicate that the observed inter-annual differences in the ground temperatures are caused by different air temperatures, which are presumably related to the lager scale atmospheric advection processes.

923/21: "Stefan equation" is confusing, please use "Stefan-Boltzmann equation" instead

As explained in the manuscript, the Stefan equation calculates active layer thickness by assuming that the entire ground heat flux is consumed by the thawing of soil and has nothing to do with the Stefan-Boltzmann law. The terms "Stefan equation" or "Stefan solution" are commonly used in the scientific literature. To avoid confusion we added a citation and changed the sentence to:
As already shown by Romanovsky and Osterkamp (1997), this clearly limits the value of the widely used Stefan equation, which evaluates the active layer dynamics by assuming the ground heat flux to be used entirely for thawing.

926/2,5: I cannot confirm that I found this study "comprehensive" – my dictionary defines this term as "complete; including all or nearly all elements or aspects of something"

This study depicts the energy balance of a polygonal tundra site in northern Siberia during the summer half year period. The study comprises measurements of all energy balance components, the determination of important parameters such as soil thermal properties, roughness lengths and resistance to evapotranspiration. While temporal variabilities of the energy balance components are in the main focus of this study, it also comprises spatial variabilities of the surface energy balance. The work is based on extensive field measurements and also makes use of modeling. We do not claim completeness which would not be possible in the scope of an article. For the sake of a better wording, we avoid the phrase "comprehensive" in the revised version of the manuscript.

Figs. 2 and 4: dates are not in ISO format; odd tick spacing of 2.5 days; labels of dates must be at 00:00 hours of the respective day

Done.

Fig. 3: check uppercaselowercase writing in the two y-axis labels

Done.

Fig. 5: dates not in ISO format

Changed.

Fig. 6: in caption please explain what Qnet,p is; what is Qnet, and why not Qnet,t to be consistent in notation? Are date labels end of 20-day periods? These kinds of bar graph are problematic for the data that you want to show; a line graph with symbols would be correct (bar graphs are typically only used for categoric variables)

We changed the figure caption and figure accordingly (see now Fig. 7). The caption changed to:
Differences between the net radiation measured at the pond and the tundra surface ΔQ_{net} *during the observation periods in 2007 and 2008.*

Fig. 7: I think the y-axis label should say △Q, not just Q (you seem to show a difference in Q, not Q itself); horizontal bars of whiskers are too wide (could be eliminated). Conceptionally I do not think that this display is correct: if QH goes up, then the footprint gets smaller, but it appears to me that you treat it as a pseudo-constant in this comparison.

We changed the label of the y-axis and removed the horizontal bars of whiskers (see now Fig. 8).

We checked the composition of the footprint area for each half hour value using the analytical footprint model of Schmid (1994). The results revealed that the composition of the flux source areas is basically consistent for each location of the eddy covariance system. Measurements differing more than 5% from the average footprint composition have been automatically discarded. This why we assume not the footprint, but the composition of the flux source area to be constant.

The footprint area depends on the turbulence characteristics which can be parametrized by the horizontal wind speed components, the surface roughness

and the Obukhov length (Schmid, 1994). The latter is only partly related to the buoyancy flux, so that a simple relation between Q_{H} and the footprint area, as suggested by the reviewer, can not be assumed.

Fig. 8: NO!

Fig. 8 has been removed from the manuscript (see above).

Final Remark

You see my frustration: I hoped to learn something that relates to polygonal tundra at an exciting remote site in northern Siberia, but what I had to read sounded like "the snow melts in spring and the sun shines brighter during summer". Sorry, but I do not see how this approach advances our understanding of the energy balance of tundra ecosystems.

We fully agree with the reviewer that this remote site is exciting. The more we do know about these sites, the better, since as discussed before, there are almost no accessible studies. We are disappointed by the reviewers final remark. Our paper makes a clear advance in the understanding of the energy balance of a tundra ecosystem by applying 'state of the art' measurement methods at a remote arctic site. Furthermore, we demonstrate in our paper the impact of surface heterogeneities, which clearly determine the partitioning of the turbulent heat fluxes at the polygonal tundra. In addition, we are able to relate larger scale synoptic factors such as cloud cover to the evolution of the local energy balance (snow melt, freeze back). Thus, the present paper provides numerous new insights into the energy turnover of the polygonal tundra in northern Siberia.

References

Baker, J. and Baker, D.: Long-term ground heat flux and heat storage at a mid-latitude site, Climatic Change, 54, 295–303, 2002.

Betts, A., Viterbo, P., Beljaars, A., and Van den Hurk, B.: Impact of BOREAS on the ECMWF forecast model, Journal of Geophysical Research. D. Atmospheres, 106, 33, 2001.

Betts, A., Ball, J., and Viterbo, P.: Evaluation of the ERA-40 surface water budget and surface temperature for the Mackenzie River basin, Journal of Hydrometeorology, 4, 1194–1211, 2003.

Boike, J., Roth, K., and Overduin, P.: Thermal and hydrologic dynamics of the active layer at a continuous permafrost site (Taymyr Peninsula, Siberia), Water Resources Research, 34, 355–363, 1998.

Boike, J., Wille, C., and Abnizova, A.: Climatology and summer energy and water balance of polygonal tundra in the Lena River Delta, Siberia, Journal of Geophysical Research-Biogeosciences, 113, G03 025, doi:10.1029/2007JG000540, 2008.

Boone, A., de Rosnay, P., Balsamo, G., Beljaars, A., Chopin, F., Decharme, B., Delire, C., Ducharne, A., Gascoin, S., Grippa, M., et al.: The AMMA Land Surface Model Intercomparison Project (ALMIP), Bulletin of the American Meteorological Society, 90, 1865–1880, 2009.

Bugbee, B., Droter, M., Monje, O., and Tanner, B.: Evaluation and modification of commercial infra-red transducers for leaf temperature measurement, Advances in space research, 22, 1425–1434, 1998.

Chapin, F., Eugster, W., McFadden, J., Lynch, A., and Walker, D.: Summer differences among arctic ecosystems in regional climate forcing, Journal of Climate, 13, 2002–2010, 2000.

Cox, P., Betts, R., Bunton, C., Essery, R., Rowntree, P., and Smith, J.: The impact of new land surface physics on the GCM simulation of climate and climate sensitivity, Climate Dynamics, 15, 183–203, 1999.

Eaton, A., Rouse, W., Lafleur, P., Marsh, P., and Blanken, P.: Surface energy balance of the western and central Canadian subarctic: Variations in the energy balance among five major terrain types, Journal of Climate, 14, 3692–3703, 2001.

Eugster, W., Rouse, W., Pielke Sr, R., McFadden, J., Baldocchi, D., Kittel, T., Chapin, F., Liston, G., Vidale, P., Vaganov, E., and Chambers, S.: Land–atmosphere energy exchange in Arctic tundra and boreal forest: available data and feedbacks to climate, Global Change Biology, 6,

84–115, 2000.

Foken, T., Göckede, M., Mauder, M., Mahrt, L., Amiro, B., and Munger, J.: Post-field data quality control, in: Handbook of Micrometeorology: A Guide for Surface Flux Measurement and Analysis, Kluwer, 2004.

Garratt, J.: The atmospheric boundary layer, Cambridge Univ Pr, 1994.

Harazono, Y., Yoshimoto, M., Mano, M., Vourlitis, G., and Oechel, W.: Characteristics of energy and water budgets over wet sedge and tussock tundra ecosystems at North Slope in Alaska, Hydrological Processes, 12, 2163–2183, 1998.

Harding, R. and Lloyd, C.: Fluxes of water and energy from three high latitude tundra sites in Svalbard, Nordic Hydrology, 29, 267–284, 1998.

Kodama, Y., Sato, N., Yabuki, H., Ishii, Y., Nomura, M., and Ohata, T.: Wind direction dependency of water and energy fluxes and synoptic conditions over a tundra near Tiksi, Siberia, Hydrological Processes, 21, 2028–2037, 2007.

Langer, M., Westermann, S., and Boike, J.: Spatial and temporal variations of summer surface temperatures of wet polygonal tundra in Siberia - implications for MODIS LST based permafrost monitoring, Remote Sensing of Environment, 114, 2059–2069, 2010a.

Langer, M., Westermann, S., Muster, S., Piel, K., and Boike, J.: Permafrost and surface energy balance of a polygonal tundra site in Northern Siberia–Part 2: Winter, The Cryosphere Discussions, 4, 1391–1431, 2010b.

Lloyd, C., Harding, R., Friborg, T., and Aurela, M.: Surface fluxes of heat and water vapour from sites in the European Arctic, Theoretical and Applied Climatology, 70, 19–33, 2001.

Lynch, A., Chapin, F., Hinzman, L., Wu, W., Lilly, E., Vourlitis, G., and Kim, E.: Surface energy balance on the arctic tundra: Measurements and models, Journal of Climate, 12, 2585–2606, 1999.

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., 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.

Mendez, J., Hinzman, L., and Kane, D.: Evapotranspiration from a wetland complex on the Arctic coastal plain of Alaska, Nordic Hydrology, 29, 303–330, 1998.

Moore, C.: Frequency response corrections for eddy correlation systems, Boundary-Layer Meteorology, 37, 17–35, 1986.

Ohmura, A.: Climate and energy balance on the arctic tundra, International Journal of Climatology, 2, 65–84, 1982a.

Ohmura, A.: Evaporation from the surface of the Arctic tundra on Axel Heiberg Island, Water Resources Research, 18, 291–300, 1982b.

Ohmura, A.: A historical review of studies on the energy balance of arctic tundra, International Journal of Climatology, 2, 185–195, 1982c.

Ohmura, A.: Regional water balance on the arctic tundra in summer, Water Resources Research, 18, 301–305, 1982d.

Ohmura, A.: Comparative energy balance study for arctic tundra, sea surface glaciers and boreal forests, Geojournal, 8, 221–228, 1984.

Oke, T.: Boundary layer climates, Methuen, 1987.

Peters-Lidard, C., Blackburn, E., Liang, X., and Wood, E.: The effect of soil thermal conductivity parameterization on surface energy fluxes and temperatures, Journal of the Atmospheric Sciences, 55, 1209–1224, 1998.

Pitman, A.: The evolution of, and revolution in, land surface schemes designed for climate models, International Journal of Climatology, 23, 479–510, 2003.

Romanovsky, V. and Osterkamp, T.: Thawing of the active layer on the coastal plain of the Alaskan Arctic, Permafrost and Periglacial Processes, 8, 1–22, 1997.

Schmid, H.: Source areas for scalars and scalar fluxes, Boundary-Layer Meteorology, 67, 293–318, 1994.

Soegaard, H., Hasholt, B., Friborg, T., and Nordstroem, C.: Surface energy-and water balance in a high-arcticenvironment in NE Greenland, Theoretical and Applied Climatology, 70, 35–51, 2001.

van den Hurk, B., Viterbo, P., Beljaars, A., and Betts, A.: Offline validation of the ERA40 surface scheme, European Centre for Medium-Range Weather Forecasts, 2000.

Van Everdingen, R.: Multi-Language Glossary of Permafrost and Related Ground-Ice Terms, revised May 2005. National Snow and Ice Data Center/World Data Center for Glaciology, Boulder, CO, 1998.

Viterbo, P., Beljaars, A., Mahfouf, J., and Teixeira, J.: The representation of soil moisture freezing and its impact on the stable boundary layer, Quarterly Journal of the Royal Meteorological Society, 125, 2401–2426, 1999.

Vourlitis, G. and Oechel, W.: Eddy covariance measurements of CO_2 and energy fluxes of an Alaskan tussock tundra ecosystem, Ecology, 80, 686–701, 1999.

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.

Wilczak, J., Oncley, S., and Stage, S.: Sonic anemometer tilt correction algorithms, Boundary-Layer Meteorology, 99, 127–150, 2001.

Interactive comment on The Cryosphere Discuss., 4, 901, 2010.