

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.

Anonymous Referee #2

Received and published: 2 September 2010

General comments: Langer et al. are presenting a descriptive analysis of the spring, summer, and fall energy balance at a polygonal tundra site in Northern Siberia. While the data is arguably of better quality than earlier work that relied on the Bowen Ratio techniques, much of the analysis is rather routine. The results indicate that many of the conclusions of previous work are robust, despite different geographical locations (and techniques). Still, the authors are not highlighting classical papers (Ohmura’s for example). I am currently not seeing a new contribution to the field but rather a confirmation of previous investigations.

While the available dataset provides the opportunity to assess the spatial variability

C728

of the surface energy balance, the authors are focusing on the temporal variations at the stationary site. I would like to see the manuscript having an increased attention to the geographical differences/similarities in the energy fluxes even though the eddy covariance measurements represent a mosaic of the polygonal ground surface.

Specific comments: The manuscript is generally well organized apart from sections under “results” that includes portions suited for the discussion. I find several sentences awkward to read (see “technical corrections”). It would also help me as a reader if the authors avoided long paragraphs. Some sections are confusing. For example, the authors are discussing their second paper (winter energy balance) in the abstract and introduction. Also, the labeling of the sub-periods of the snowmelt period is extremely confusing. I find the language somewhat “wordy”. In particular, I am not a fan of value laden phrases such as “essential”.

The authors are describing the landscape as heterogeneous at the scales of meters. If the data is there, why not focus on the spatial (and temporal) variability in the partitioning of ground heat flux into its latent and sensible components for example? More efforts in data mining of the results from the mobile towers and how they compare to the stationary site could result in some new findings. One mobile tower has 79% dry area. Why not focus on that one?

I appreciate their efforts to produce a continuous description of the surface energy fluxes by modeling any missing data. In particular, I find the total evaporation rates (summer and snowmelt) of interest. On the other hand, I would appreciate if it was clear what sections are measured data and modeled in the continuous time series.

In many places it seems like the authors could strengthen their interpretations with their own data.

The authors invested in an extended description of the errors in the measurements and calculations, but I am missing a presentation of the the absolute errors when results are presented. The scientific methods are clearly outlined with the exception that I am

C729

missing a clear description of how the ablation was monitored. I would also appreciate a reference to the corrections applied when the fluxes were calculated. It is not fair to the reader to assume a familiarity with the “TK2” program.

As the study is located at sites that are at least temporarily flooded, I am missing a description and a discussion of the heat storage component in the standing water column. Harazono et al. (1998) showed that a substantial portion of the diurnal surface energy exchange can be located to the energy dynamics of the shallow water column.

I am missing a profile figure describing the soil thermal regime (“permafrost” is nonetheless part of the title. . .)

What is typical wind direction? See impact of wind direction on the energy balance at a coastal location in the work by Rouse et al. 1987.

Technical corrections and specific comments: P. 902 Apart from being used in every other sentence, the word “essentially” does not provide any additional information. I suggest removing it. L 2-4: Awkward sentence. Too busy. L. 5: Annual surface energy balance? The title focuses on the spring and summer. I am confused. The paper I am asked to review only includes the April to Sep. Please remove the “annual” and winter analyzes to avoid confusion. L. 9: Missing a comma before “and”. Please check the rest of the manuscript. L. 10: Unclear. Dominant factor of the magnitude of the absolute fluxes? L. 13-14: The second part of that sentence is unclear. Expand the second part, i.e. divide the present sentence into two sentences. L. 17-19: Statistically significant? If not, remove “significant”. Are the measurements flux measurements? If so, please say so. L. 19: Remove “at different locations”. It is obvious. L. 20-21: Wordy. A suggestion: “However, spatial differences in the partition between sensible and latent heat flux only exist during conditions of high radiative forcing, which only occur occasionally.” L. 24: Remove “fundamentally” (same issue as I have with “essential”). L 24: I wouldn't call 1997 “recent”.

P. 903 L. 1: Affected by what? Remove “processes” (it makes is unnecessarily awkward

C730

to read). L. 12-14. Awkward sentence.

P. 904 L. 3: Annual? This paper is about the spring and summer. L. 5: Define is low-centered or high-centered polygonal tundra. L. 8: Annual? If you have a Part 2 study that represents the winter, you can always mention that at the end of the introduction. L15: Annual? See comment above. L 19: Define landscape scale (100's meters?)

P. 905 L. 1-2: Maximum air temp in September? Does not seem correct. L. 9-11: Does this belong to the site description? Isn't this result? L. 12: Say low-centered polygons. L. 15: Are they inundated (or saturated) throughout the summer or only after snowmelt? Please clarify. L. 24. Provide units.

P. 906 L. 6-8. Unnecessary to provide eq. 2 if you will be using eq. 3.

P. 907 L. 17: “We assume emissivities _ of 0.98 for wet and 0.96 for dry tundra surfaces (Langer et al., 2010a).” This assumption can drastically affect the spatial comparisons. A discussion/assessment of this assumption is preferred later in the paper. L. 21: Define solar noon. Is it a time period or a specific our?

P. 908 L. 23: “For Bowen ratios of approximately 0.5 and average air temperatures of T_300 K, the offset is on the order of 15%...” This uncertainty should be included when the results are presented. L. 17-24: Separate the analyses into the data that is not missing the vapor flux and data that is.

P. 910 L. 22: Written weirdly. “Ground heat flux is essential for permafrost”??

P. 911 L. 16: Evaluate (no “d”). L. 17: The reference is incorrectly formatted. A parenthesis is missing? L. 26: Use same units (Celsius or Kelvin) throughout the manuscript.

P 913 L. 16-17: “With an almost identical snow depth, a similar snow-water equivalent is assumed for 2007.” If no density was measured in 2007, write so. L. 21-25: It is unclear to me what definitions you are using. Please clarify what defines the three periods.

C731

P 914 L. 4: May is misspelled. L. 7-9: Basic. Remove sentence. L. 10-13: The method of SWE measurements was never explained earlier. Please do. How many samples, how many locations etc. L. 14: Write "mid-May". L. 20-21: The naming is confusing when it is used later on. Give neutral names such as 1 and 2 or A and B. L. 23: Unclear what the range -40 to 50Wm⁻² actually represent. The diurnal amplitude?

P 915 L. 1: Unclear what you mean with "surface radiation budget". L. 13: Do you mean a total of 12 mm? If so, clarify. L. 18: Define the duration of the observation period (dates in parenthesis). L. 14-16: "As the snow water equivalent of 2008 amounts to approximately 57mm, about 20% of the snow cover sublimate or evaporate during the last spring days. " I think this finding is worth to highlight in the abstract.

P 917 L. 14-15: I assume you mean a B below 1. L. 16: This seems like a low number, especially since the area is continuously saturated. L. 20: What is the model? L. 21: Boring usage of space. Instead, reference the figures in parentheses and tell the reader what the take-home message is. In general, try to follow that same style throughout the manuscript. L. 24-25. Awkward sentence.

P 918 L. 8-10: Awkward sentence. Need to be cut into several sentences. L. 25-17: Why don't use the measured short-wave radiation when discussing cloudiness?

P 919 L. 4: Why discussing in net short-wave and not net all wave? L. 27-28: A circular statement. Awkward as written.

P 920 Would be neat to tie in the snow depths with the energy balance discussion. For example, how does the net radiation change with a thinning snow cover? I want to see extended results on the spatial variability in fluxes. Currently, there is only only ~1 page about spatial variations. When do these differences occur/not occur?

L. 18: Data quality assessment should go before any other results. It is results too. L. What about heat storage in ponding water?

P 921 L. 20: Nowhere did I see the results highlighting the importance of the thermal

C732

state of permafrost.

P. 922 L. 2: Remove "out". L. 27-29: Contradictory statements. L. 28 "surface cooling", L 29 "ground heat flux only marginally affected", L 1-3 (P. 923) "surface temp not affected.."

P. 924 L. 22: "Albedo differences in spring are not likely..." Can't you show that with your data? L. 26: Why the reference? Isn't that what you are presenting here?

P. 925 L. 12: I would not call eddy cov measurements micro-scale as they represent a mosaic of surface conditions, which are tied to the polygonal features. L. 19-22: Just because the long-term average is similar does not allow you to make that broad of a conclusion. I believe you were constrained in your analysis by a) that your two footprints were too similar and b) that the uncertainty in the calculated fluxes too large.

P 926 L. 1-3: Repetition of comprehensive. L. 7: "... intended to be incorporated into climate models..." infer that you are planning on such, which is not relevant for this paper. L. 9-13: The connection between cloud cover and soil freeze-up is interesting. L. 22-: You said earlier that there was no difference in the larger scale surface energy balance measurements among the two sites. ? I am confused.

Table 2. Provide ratios (sensible/net rad), to aid the comparison.

Figure 4. What explains the high night time ground heat fluxes and also high night time latent heat fluxes? It does not match with the measured net radiation.

Figure 5. How large percentage of the presented results is based upon modeled values?

Figure 5. Confusing; the direction of the sign for net radiation has suddenly changed since Figure 4.

Figure 6. Define the labels in the figure caption.

Figure 7. Did you only compare to one mobile tower? Why not present results from all

C733

three locations?

Figure 7. Clarify what E and H stands for.

Figure 7. The labeling of the y-axis is confusing.

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