

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

Anonymous Referee #1

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General Comments

The authors present a very detailed description of independent measurements of the annual cycle of surface energy fluxes at a lowland permafrost site on Svalbard, Norway. The manuscript is prepared very carefully and I enjoyed reading it: it is clearly written and structured, the illustrations are of good quality, and the reasoning is sound. The results presented are of interest for researchers dealing with measuring and modeling the surface energy balance in cold regions and the paper fits well the scope and topic of “The Cryosphere”.

There are few general remarks and questions (see specific comments) and some minor comments (see minor comments), which I think need to be addressed before final publication.

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Specific Comments

1. Abstract

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

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.

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. 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.

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.

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.

5. Uncertainties and spatial and temporal variability

The authors thoroughly discuss the energy balance closure problem and give error es-

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timates 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.

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?

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.

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.

Minor comments

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

p. 636, l.17–19: What is the long-term mean temperature at this depth? Is there any trend? And, is an active layer of 1.5 m representative for the area and the past measurement years?

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

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

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

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

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

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

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

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.

Interactive comment on The Cryosphere Discuss., 3, 631, 2009.

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