

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

**Anonymous Referee #2**

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### **General comments:**

This manuscript presents independent measurements of all components of the surface energy budget at a high-arctic site near Ny-Ålesund, Svalbard, over a complete annual cycle. These observations are described and analyzed in a convincing manner, followed by a discussion of implications and consequences for the local permafrost. The observational data of this study in combination with additional measurements from nearby located stations (e.g. BSRN site) constitute a very valuable data set which is quite unique for a high-latitude field site in terms of record length, temporal resolution, and completeness (standard meteorological, micro-meteorological, snow and soil observations). Both the selected sensor systems and the data analysis methods and techniques are appropriate and state-of-the-art.

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Despite the fact that the paper does not include novel or innovative measurement or analysis tools and approaches, the presented results merit high consideration since they constitute valuable information in the context of understanding physical processes and climate change in high-latitude regions where such complete observational data are very scarce. The acquisition of this complete set of measurements, the thorough and consequent application of well established analysis tools, and the qualitative and quantitative discussion and interpretation of findings form the originality of this work. The results provide a quantification of the different components of the surface energy balance at a high-arctic lowland site which I trust will be of considerable interest and use for the scientific community. The paper would have even more impact if the authors briefly described how their findings affect and support current and future research on climate change in the Arctic.

Overall, the paper presents the topic in a clear, complete and concise manner; it is well written, clearly structured and organized. I recommend the manuscript be accepted for final publication in ‘The Cryosphere’, pending a few minor changes and edits as indicated in the specific and minor comments below.

### **Specific comments:**

1. p.638, lines 23-25: What is the reason or justification for the assumption of 0.65 for the albedo of melting snow?
2. p.642, line 21: “The fit is strongly influenced by...” Not clear if this is specific for this study (then it should be “**This** fit...”) or generally applicable. Please clarify.
3. p.643, lines 6-7 and lines 17-19: What is the vertical spacing of the three temperature sensors? Are three sensors only sufficient to get a good fit for the thermal diffusivity of snow? What happens if the snow depth changes substantially and

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the set of three sensors gets buried deep down in the snow pack? Add a short explanation/discussion here.

4. p.643, line 16: The resulting snow thermal conductivity of  $K_h = 0.45 \text{ Wm}^{-1}\text{K}^{-1}$  is interesting since this value is considerably higher than values commonly used in snow, sea-ice, or climate models ( $K_h = 0.3 \text{ Wm}^{-1}\text{K}^{-1}$  for a snow density of about  $0.3 \text{ g cm}^{-3}$ ). Do you have an explanation for that? I do not doubt the value, just curious.
5. p.646, line 29: Please comment on or discuss possible reasons for the residual of  $22 \text{ Wm}^{-2}$ .
6. p.647, lines 4-10: Any thoughts on how representative the observed annual cycle is? For instance, here you mention that the September precipitation was twice the usual amount.
7. p.650, line 6: Is it a **gradient** ( $\text{Km}^{-1}$ ) or a **difference** (K)?
8. p.651, line 1: Where does this melting happen – at the snow surface or internally?
9. p.652, line 13: What is meant by “within the freezing characteristics of the soil”?
10. p.658, lines 2-4: Did you already test the quality of the energy balance closure on a daily basis? I do not ask to include his analysis in this paper but I would love to see the performance.
11. p.667, Tab.2: How was the cloud fraction (cf) determined?
12. p.668, Fig.1: I suggest removing Fig.1a and expand Fig.1b. People know the location of Svalbard (I hope...).
13. p.670, Fig.3: The ground heat flux (yellow) is not very visible. Perhaps change color (also in Fig.8).

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14. p.671, Fig.4: Give dates for the summer period also in this figure caption.
15. p.672, Fig.5 (also Figs. 6, 10, 11, 12): Replace “See Fig.X.” with “Notation as in Fig.2”. How does it look if the area of the arrows in Figs. 2, 6, 10, 11, 12 is scaled proportional in size? This way one could easily (inter-)compare the relative magnitude of the fluxes in different figures representing different periods or seasons.
16. p.677 and 678, Fig.10 and 11: These 2 figures are not referenced/mentioned in the text in sections 4.4 and 4.5, respectively.

#### Minor comments and edits:

1. p.636, line 1: “...the first half of **the** study period...”
2. p.639, line 3: Remove this line; this is already defined in the notation part in section 3.1.
3. p.643, line 2: “With  $K_h$  known,...” Letter “n” missing.
4. p.646, lines 11-13: Awkward sentence, rephrase.
5. p.647, line 16: please write: “...i.e. the advection of relatively warm air...”.
6. p.647, line 20: Replace “permanent” with “perennial”.
7. p.652, line 6: Replace “at around” with “close to”.