

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

Anonymous Referee #2

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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).

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,

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the comments of the reviewers of part 1 and 2 could be merged and the overall quality of both parts would be enhanced.

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.

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.

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.

Specific comments:

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

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

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p.1394, l.10: better: In this second part, we focus...

p.1394, l.14: the variability between two winters cannot be summarised as "inter-annual" 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.

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.

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

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

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

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

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

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.

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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.

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

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" !

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

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 ?

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)

p.1398, l.19: the reference to Table 6 is unclear – Table 2 ?? (or Table 6, Langer et al. 2010 ?)

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

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

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

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

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 ?

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

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 ?

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

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.

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

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

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

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

Figure 8: a reference to the time period of the data is missing in the caption (all winter

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measurements ? 2007/8 or 2008/09 etc)

Figure 9: caption: reference to Table 6 wrong

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).

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.

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 4°C 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.

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 . . ."

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

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