

Interactive comment on “Distributed Temperature Profiling System Provides Spatially Dense Measurements and Insights about Permafrost Distribution in an Arctic Watershed” by Emmanuel Léger et al.

Anonymous Referee #1

Received and published: 17 March 2019

Dear authors,

I have read the manuscript submitted to The Cryosphere and think that it provides an interesting approach to surveying shallow ground temperatures, allowing to obtain a good spatial snapshot, which improves the understanding of detailed variability. The manuscript is well-written, although sometimes needing more objective comments and some polishing. There is a strong focus on the low cost of the instrumentation, but that is really not very novel, since this type of devices have been developed and applied by numerous teams over the last two decades. However, they are becoming increasingly

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cheaper due to the reduction of hardware costs, development and miniaturization of sensors and datalogger and higher availability of open-source software. I therefore suggest that the authors put more focus on the novelty of the concept of moving the probes along the measuring site, rather than on the monitoring system itself, especially since this approach (monitoring) has not been conducted in this case-study.

Still on the measuring method (profiling), this type of approach was frequently used on local and urban climate studies, at least until the 1990's, for measuring air temperatures across large areas, with sampling measures at specific sites, which were then temperature-corrected for time, in order to allow for comparison. Such a correction might even be interesting to be done here, to make use of the near-surface temperatures, which are affected by the diurnal changes (>25 °C following your manuscript). Examples of such application should be mentioned, since they link to the proposed method, that can be seen as precursor of the method. Here are some examples, but others also exist: <https://doi.org/10.4113/jom.2010.1112>, <https://doi.org/10.1007/s00704-005-0152-1>.

Temperature profiling has also been done by other authors on permafrost settings. For example, Goyanes et al (2014) (<http://dx.doi.org/10.1016/j.geomorph.2014.04.010>), measured temperatures from 5 to 70 cm depth in a grid in Deception Island to detect the effect of geothermal anomalies on permafrost distribution. They have also compared the results with ERT surveying. Although not with dataloggers and with a scarcer number of temperature measuring depths, but the approach is comparable to the one presented here, especially since in this manuscript the datalogging has not really been used. So, please check references and extend the review also to CALM-related publications, since I would think this has been done elsewhere.

As you will find in the detailed review below, I think that you need to improve the characterization of the site in order to better assess the results. I think a larger-scale map derived from the UAV survey (or high resolution satellite) would be helpful, and also a window showing the setting of the studied slope in the watershed context. A dis-

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cussion on the spatial variability of soil characteristics is needed, especially since the site is not homogeneous (as you mention, there are possible rock outcrops). Following this rationale, a description of the geomorphic units is also lacking, since concavities and convexities, may possibly be explained by different dynamics and also reflect soil (deposit?) types and hence will have an effect on soil temperature.

I also think you should be more cautious in what concerns to permafrost distribution, since it seems that you present no single direct observation of permafrost, other than the indirect measures by ERT. Please clarify this and indicate if there are other observations that show the characteristics of permafrost at the site or in its vicinity.

As a conclusion, I think the manuscript is of good quality and should be published after a thorough review. The results show that the approach can be of wider application, especially with multiple datalogging systems installed, but the main-added value related to the way you apply the surveying.

Detailed comments: Title: I think the title is probably too ambitious for the contents. I would suggest a more focused title, such as “A distributed temperature profiling method for assessing spatial variability of ground temperatures (Nome, Alaska)”. Page 1, l. 10. Please clarify/rephrase the sentence, since temperature does not contain information on the properties modulating the soil thermal flux; it rather reflects these properties. Page 1, l. 18. Replace AK by Alaska. The acronym is poorly known outside the US. Page 3, l. 25. I would suggest deleting the mention to Arctic permafrost regimes and write only “permafrost regimes”, and add a few citations from non-Arctic regions (e.g. mountain and Antarctic). Page 4, l. 6. I recommend extending this review to non-Arctic permafrost sites, since significant advances have been presented, for example, in the European Alps. Page 4, l. 19-20. Consider rephrasing the sentence, since it doesn't make too much sense to distinguish between soil minerals themselves and the organic component that may be mixed with the soil minerals, since you are aiming here to monitor the soil as a whole. Page 4, l. 35 – delete “low-cost”. It has been mentioned earlier. Page 5, l. 7 – Replace AK by Alaska. Page 5, line 8 – Consider replacing “This

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“Teller watershed” by “The studied watershed along Teller Road” Page 5, line 16-17 – Delete cumulative twice; it is not necessary, since annual precipitation data is always cumulative. Page 5, lines 20-23 – You should rephrase and clarify if the most frequent situation is a thaw depth greater than 1 m, or a thaw depth of 0.4-0.6m. It is not clear (at least for the reader at this stage of the manuscript) why you are emphasising on such thaw depths, when there are also deeper ones. Page 7, l. 1-5 Simplify text and make it more objective. Excessive use of adjectives associated to costs, etc. Page 7, line 17 . Where were those probes installed and what are their characteristics? Page 7, line 15-20. I find it difficult to accept that a system that was specifically designed to be tested for measuring soil temperatures, especially using such minute and fragile sensors, has not been calibrated. It is also not clear why temperatures at 80 cm depth were used, since not all sites were measured down to that depth. How many sites were extrapolated and what is the quality of the extrapolation? The assumptions that “no sensor-specific calibration curve could easily be defined while ensuring that it would increase sensor accuracy significantly” needs a better framing. If the system is to be applied in the future, calibration is needed and mentioning that it could not have been done because it is 80 cm long, reveals some limitations for future developments. Page 8, l. 1 – Please clarify the setup. Is the pvc tube buried vertically with the sensors at different depths? What is the thickness of the tube? Page 8, line 26 – indicate where in the Arctic, since conditions are variable. Page 8, l. 31 – replace “topographic trends” by “topography”. Page 9, l. 1 – replace “inferred” by “prepared”. Page 9, line 3. Delete “More details on UAV-based imaging...” and keep only the reference to Dafflon et al 2016. Page 9, l.9-11 – delete. Not necessary. Page 9, l.-13-15 – This should be moved to the methods. It answers the question raised above. Page 11 l. 6 – You mention vegetation and topography, but for such a detailed spatial and vertical analysis, you will certainly need to characterize geomorphology and soil type to better understand spatial variability. Add these characteristics to the study site description. Page 12, l. 2-5 – consider revising: “quite perpendicularly to the general slope aspect...”. Improve phrasing “...possibly related to ground erosion and/or ground settlement”. Why? This

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should be better discussed under the presentation of the study site. Page 12, Line 12-13 – “...at many other locations for each depth...” - Clarify. Page 12, line 14 – “with a few exceptions for the shallowest measurement...” – clarify. Page 12, line 24-25 – Clarify what you mean with this sentence. Page 14, line 16 – How can you associate directly the presence of permafrost with temperatures at of below 0.2 °C in mid-july? Please rephrase the sentence carefully, since you are using ERT data as a proxy for permafrost, which should work fine, but check the phrasing. Page 14, line 20 – I would prefer indicating that the temperature values “suggest” the presence of permafrost, rather than “indicate”, since you have no direct observations of permafrost along the transects. Page 15, lines 14-15. Why does this happen? A scatterplot or at least R2 values could help understand the described co-variability, since the figure is really small. Page 18, line 22 – Replace “temperature flux” by “heat flux”. Page 19, line 9 – Please rephrase the sentence reconsidering the novelty. Page 19, line 32 – You mention remote sensing data, but I think you mean satellite data, since with UAV surveys one can very well characterize structure of shrubs (multispectral, LIDAR and low elevation flights at multiple angle surveys).

Figure 1 – Consider beginning the caption with a title that applies to both windows (i.e. Location and general setting of the study area) and only after mention “a) Aerial view...”. Enlarge the dots of the two stations which are very difficult to depict in the image. Add graphic scale in Figure A. Although the UTM coordinates are there, they may not be self-explainable to many readers. A larger map of the UAV survey with topography would be important in order to assess the topographical and geomorphological setting of the slope section under analysis. That is very important to frame possible subsurface flow paths, geomorphic units and processes that may be active and which influence soil properties. Figure 2. Add graphic scale. In caption, remove the mention to being overlain in figure 1. Figure 3 – The permafrost areas identified with the rectangle are based in which data? The date is still not the one of maximum thaw depth, I suppose. Please clarify in the caption and in the text. The Y-axis for TDR is difficult to read. Check colour and font size. You mention that temperature above 25 cm were influenced by

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diurnal oscillations. Hence, I think they should not be plotted. Figures 4 and 5 – If only the temperatures deeper than 25 cm should be used for comparison, I would suggest removing them from the figures.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-264>, 2019.

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