Editor Decision: Publish subject to minor revisions (review by editor) (23 Apr 2018) by Christian Hauck

Comments to the Author:

Dear authors,

thank you very much for your revised manuscript and your author's comments, which respond very well to the points and queries raised by the two reviewers. I re-read the manuscript myself, and have some minor comments/questions to add, which are detailed in the following. I would like to ask you to respond to them point-by-point, and revise your manuscript accordingly. Hereafter, the manuscript can be accepted for publication in The Cryosphere.

Kind regards,
Christian Hauck
Editor

[Authors' response] We agree with most of the commentary and have responded accordingly. Likewise, we have amended the manuscript in accordance with these suggestions.

Uncertainties associated with ERT and permafrost thickness measurements.
Detailed comments:
p4, line 25-26: "sensitivity values < 0.1". This point was raised by reviewer 1 ("what is meant by low sensitivity areas and how does it reflect uncertainties ?"), but was not really answered in the author's response. Where does this sensitivity value come from and how is it calculated ? Why do you use a limit of 0.1 ? This has to be justified and the explanation also given in the paper (or referenced).

[Authors' response] As model blocks are more effective at showing contrast, we decided to use this approach but using the RES2DINV software it is necessary to use a means (such as sensitivity values) to limit the area considered for analyses. Model block sensitivity values are a measure of the amount of information about the resistivity contained in the measured block and are discussed in detail in the RES2DINV software manual that was produced based on Loke and Barker (1996) and Loke et al (2003). Higher sensitivity values correspond to more reliable model resistivity values, as such using sensitivities for limiting the area visualized. The selection of values less than 0.1 was on the basis of manual interpretation of the profiles which led to the selection of 0.1 given that values below that point were consistently representing areas that were at the deepest portions of the profiles.

p6, line 9-16 (and in the following analysis of ERT results and EIC calculations):
The uncertainty analysis of the ERT/T-based permafrost thicknesses is only given for P3. Why not for all examples in this paragraph and even more, all values/data in Table 3 ? Without additional information, these highly accurate estimations

Again, this was mentioned also by reviewer 1 ("P6, L10-12: I do not understand how the accuracy of the loggers and the inherent uncertainty in ERT is considered in estimating this very precise thickness value without an uncertainty range. Is this an estimate of maximum likely thickness?").

Are such accurate numbers (e.g. 5.4m - p8, l28) drawn from the rather coarse ERT results alone ? Not to speak of the inherent uncertainties of the inversion process...maybe using thicknesses such as 5, 5.5m etc would be more appropriate, and/or the use of uncertainty ranges.

This uncertainty would propagate also to the EIC values given later in the manuscript.

Table 3: Inferred maximum permafrost thickness: in a similar way as reviewer 1, I do not understand how these very accurate thicknesses were derived from ERT without giving an uncertainty range ? See my comments above.

[Authors' response] We have amended our approach for calculating uncertainties associated with the estimation of permafrost thicknesses. We agree that we have provided too high of a precision. As such, we conducted some additional analysis and have settled on a permafrost thickness error of ±0.5 m as being reasonable as this would constitute approximately one model block for most profiles. As such, we have provided ranges for values provided throughout the text, manuscript SI, tables and excess ice fraction figure. Where ground temperatures have been used to estimate permafrost thicknesses in conjunction with the ERT data, we have highlighted this.

However, to use ERT alone to help delineate the error associated with the direct evaluation of permafrost thicknesses, we have generated a logistical regression curve which estimates the probability that ground is frozen for a given modelled resistivity value (Ω.m).

First, we matched the xyz block resistivities observed at the near-surface layer (or layer closest to the depth of frozen ground – typically 0.5 m block) to the frost table measurements taken along the ERT profiles. Where frozen ground was determined as present in the upper 1 m, we coded the location as 1 and where it was absent we coded it at 0. Data from all the sites were then pooled and a logistic regression analysis was performed enabling generation of a probability of frozen ground curve for modelled resistivity values (see figure below).

This approach assumes that the substrate of the upper layers is representative to those deeper down which is not necessarily applicable in all cases but for the purposes of these thin permafrost features it should be reasonably representative. Permafrost probabilities for each site were then estimated from the ERT block data using the logistic regression fit and the modelled apparent resistivities (see figures below as an example). This analyses allowed us to examine what the typical level of ambiguity was for permafrost determination at the base of palsas and led us to the conclusion that it was reasonable to assume an uncertainty of 1 block.
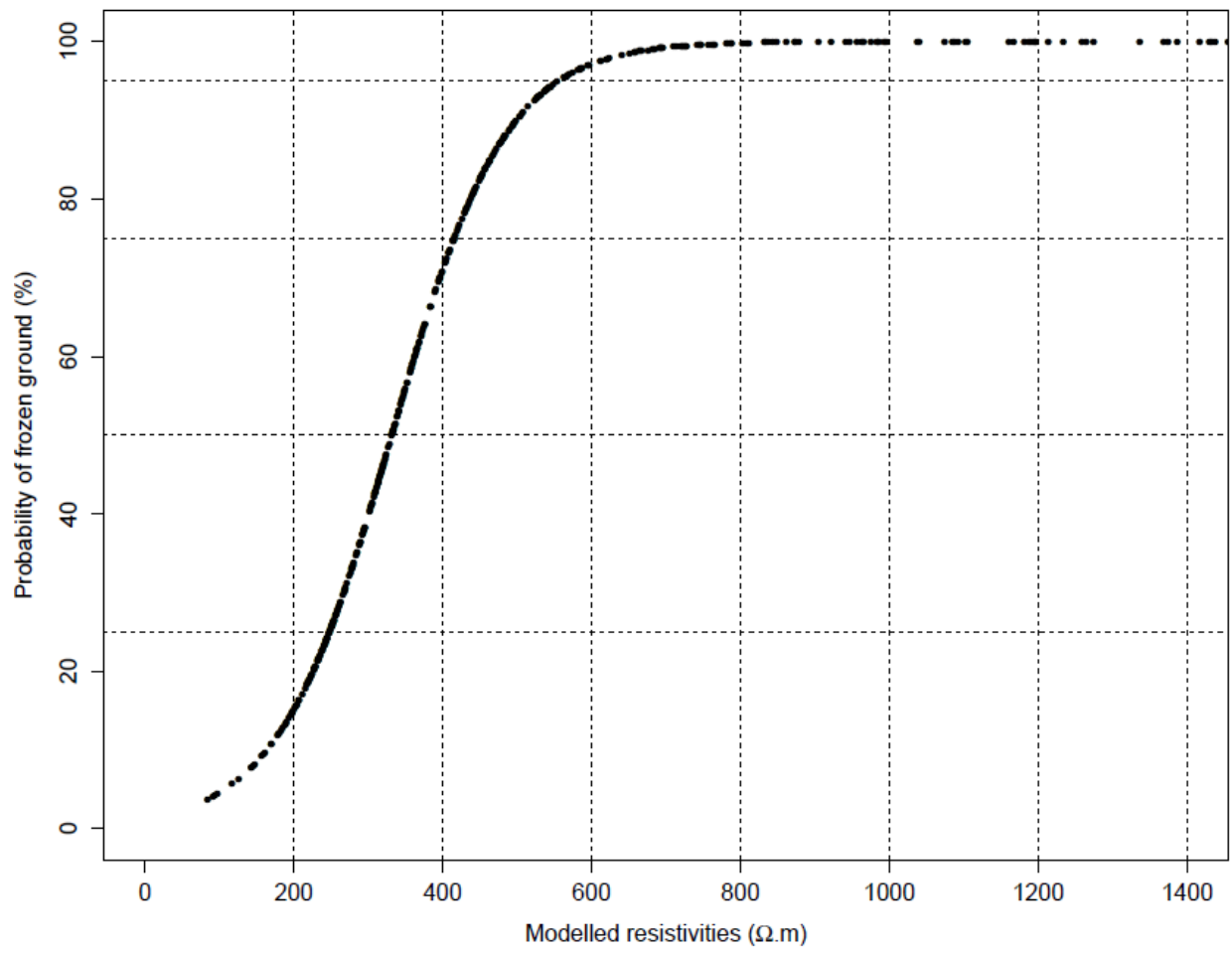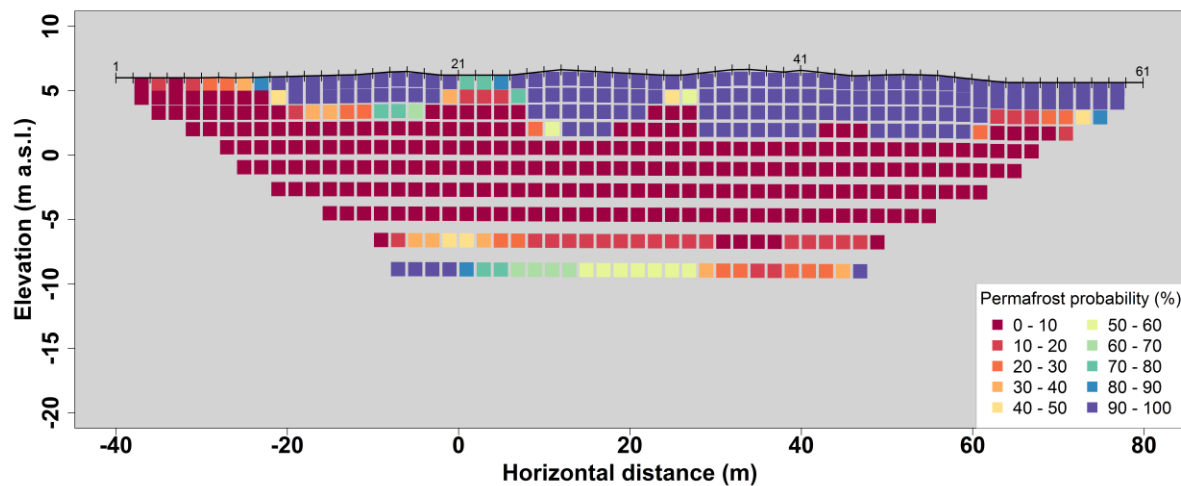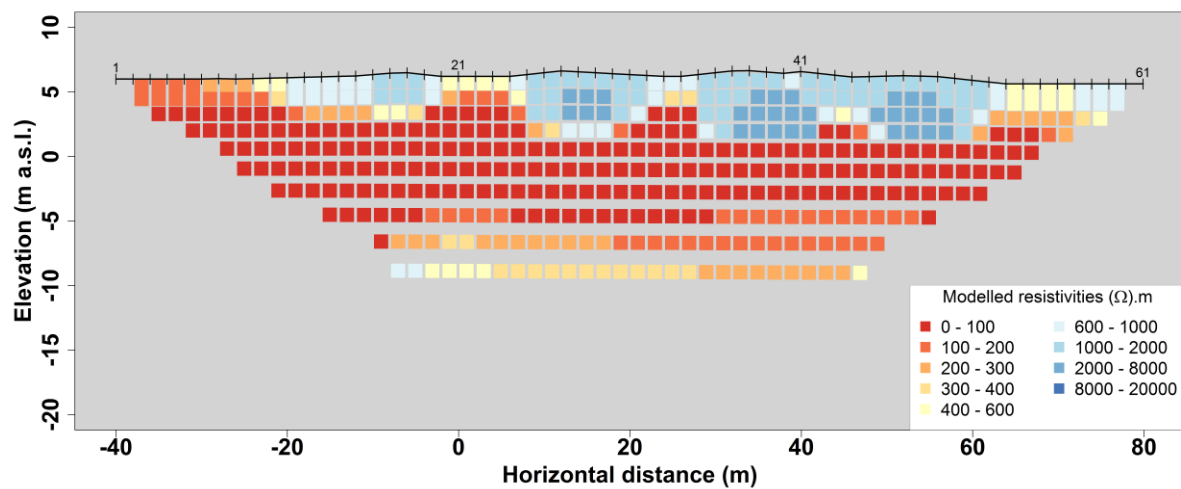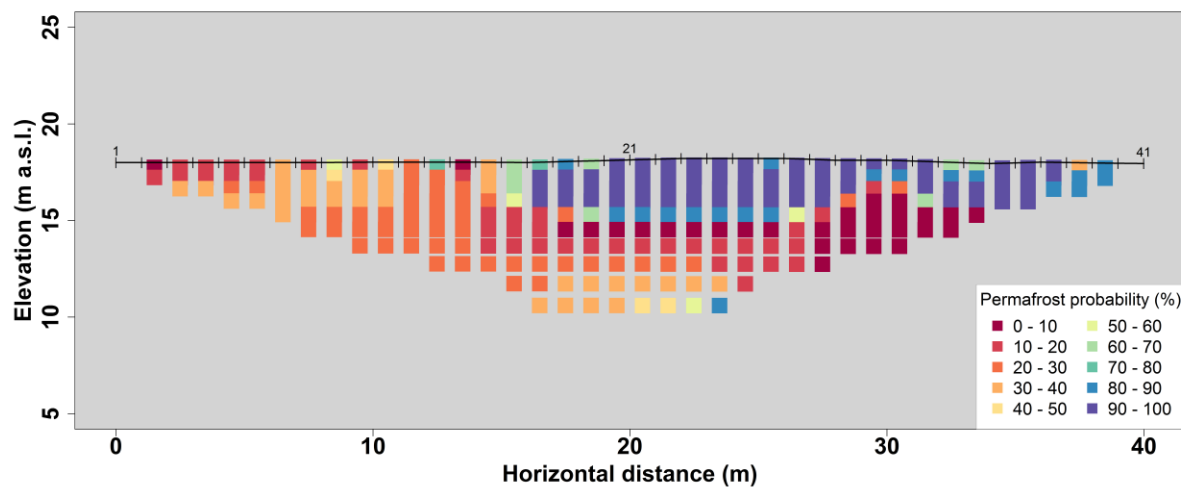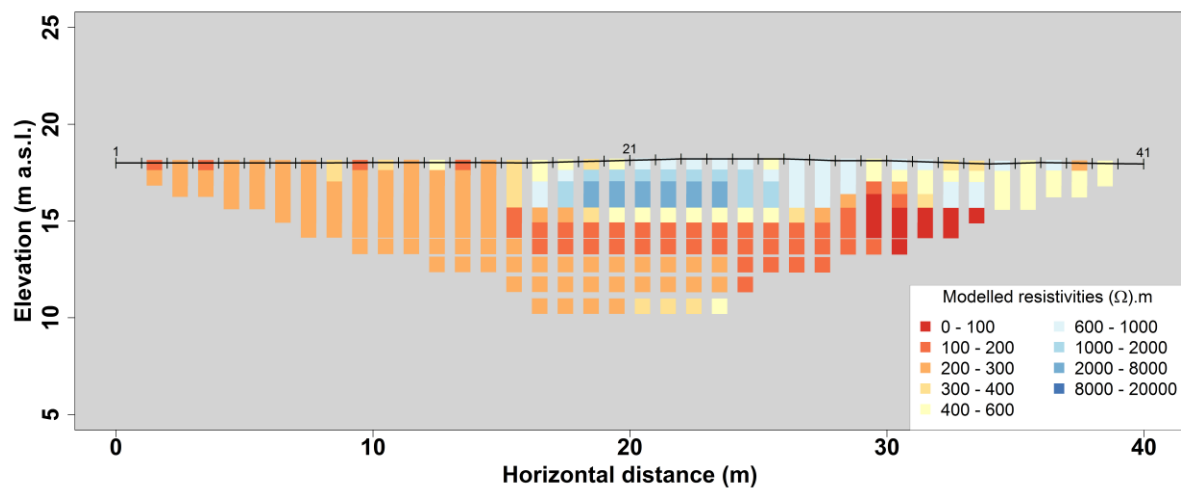
p4, line 33ff:
This is not clear to me: do you mean "divided into layers of 0.1 m thickness within the uppermost meter" ?

[Authors' response] Amended for clarity.

p5, line 4 (and at many places in the manuscript: I think the term"gridded dataset" is more common, at least in the climate community.
I would suggest using "gridded dataset" instead of "grid dataset" throughout the manuscript and in the supplementary material text.

[Authors' response] Agreed and amended.

p15, l30: "global climate model predict"

[Authors' response] Amended to:

"as predicted by global climate models"

p16, line 13: "as well as possible heat transport from sub-permafrost groundwater" - this has strictly speaking not been mentioned before. If it appears in the conclusion, it should also appear in the results or discussion section.

[Authors' response] We had added a mention of this to the discussion. See amended manuscript.

"heat transport from sub-permafrost groundwater"

Supplementary material:
Appendix S1, first paragraph, last sentence: "the daily climate station data backwards to 1900"

[Authors' response] Amended to:

"The year 1900 was infilled by linearly extrapolating the monthly data for model initialization purpose."

S1, second paragraph: "based on the anomalies and the averages during the reference period at each sites" --> "site" instead of "sites"

[Authors' response] Amended.

S1, general: This seems to me a very basic downscaling approach (delta approach, if I understood it right), which does neither take into account local conditions (or were the 1° climate data somehow downscaled to local conditions via meteo stations ?) nor the possibility that minimum and maximum temperature trends will develop differently in a future climate.

Do you have any justification/prior studies which use this approach ?

Otherwise, in my opinion, the scenarios used can be seen rather as a sensitivity study (what would happen under different climate conditions) than a real projection into the future. If this is the case, then it should be stated accordingly in the manuscript.

[Authors' response]

The climate record of the coastal Labrador region is highly variable and its future evolution is uncertain due to broad-scale uncertainties associated with ocean-atmospheric teleconnections and other factors (Brown et al., 2012; Way and Viau, 2015; Grenier et al., 2015). The authors have made the choice to use a simple downscaling method (delta) instead of higher complexity approaches because in coastal Labrador there is considerable potential for introducing additional sources of error through ocean-atmospheric-cryosphere feedbacks and other interactions that are not consistently represented by GCMs (Grenier et al., 2015; Ekstrom et al., 2015). Way and Viau (2015) showed that over the historical record air temperatures followed the CMIP5 multi-model mean with a higher fidelity than for several other widely-used climate models. As such, we feel that the best estimate of the climate for the periods analyzed will follow this overall trend. We do acknowledge that the evolution of Tmin and Tmax and year-to-year variability may impact the potential evolution of permafrost but we also disagree that using non-GCM downscaled data is equivalent to a sensitivity study. Further, there is not a consistent set of evidence that more complicated downscaling methods are inherently more accurate relative to delta approaches for simple variables like air temperature (Ekstrom et al., 2015). We agree that the scenarios differ with the climate models and their new development and configurations. We added a sentence to show the changes of projected air temperatures to the main manuscript and have added more information on our downscaling method to the SI.

Added to the methods:

"Under the scenarios of RCP 2.6, 4.5 and 8.5, air temperature was projected to increase 1.0, 2.3, and 5.5ºC from the 2010s to the 2019s, respectively."

S1, last sentence: "wind speed data was filled..."

[Authors' response] Amended to:

"Wind speed during 1954-2005 are from observations at Cartwright climate station. In other periods, we directly used the observed wind speed during 1954-2005 to fill the data gaps."