<u>Reviewer responses</u>

Reviewer 2

The manuscript describes detailed field studies of isolated bodies of permafrost along a transect in southeastern Labrador, Canada, as well as corresponding numerical simulations. I agree with reviewer #1 what the manuscript is generally well written with a clear structure and adds significant new knowledge to our understanding of isolated permafrost bodies in peatlands, but with more information needed especially on the modelling part of the study. I also recommend publication after minor revision, with the following additional comments to be addressed:

[Authors' response] The authors' would like to thank reviewer 2 for taking the time to review this manuscript. We agree with reviewer 2's comments and have amended the manuscript accordingly.

P1, L13: consider adding "in this region" after permafrost, as it might otherwise sound like thick peat is generally critical to permafrost.

[Authors' response] Added.

P1, L17: Here you mention "downscaled global warming scenarios", but there is no mentioning of downscaling in the methods section, only using multi model mean values.

[Authors' response] The original climate model projections were regraded to common 1° by 1° global grid by Environment and Climate Change Canada and we downscaled the monthly climate scenarios to daily. This probably is not important therefore we delete this phrase.

P3, L13-19: how does this relate to the information in P2L27, that regional air temperatures have been rapidly increasing over the last 50 years? Was the study period colder than for instance the mean of the last decade?
P3, L13-19: consider adding some more information here about the climatic conditions, like mean annual precipitation.

[Authors' response] We have added additional discussion of climate variables and a comparison with the most recent decadal period to this section.

P5, L6-12: which parameters were calibrated and how should be more clearly stated.

[Authors' response] We calibrated only two parameters: snow wind-scouring factor and geothermal heat flux. We revised this the first sentence in the paragraph to clearly indicate that.

P5, L8-9: Is this the multi-model mean from the CMIP5 archive? Were these values used directly, or just the trend? If these were used directly, how did the values correspond to the measurements in the overlap period (e.g. 2006-2016)?

[Authors' response] We used the anomalies (difference for air temperature and relative difference for precipitation) with respect to the reference period of 1976-2005, then we derived the future monthly values based on the anomalies and the averages during the reference period at each sites. We added a new section in SI about the climate data compilation for the model.

P6, L13-15: Here and elsewhere (e.g. P6, L30) the authors describe more (seasonal) ice than expected. Is this an indication that the study period was colder than the previous years (see comment P3, L13-19)?

[Authors' response] We have added some additional commentary on this point.

P7, L20: Drop "s" in "Tables 3".

[Authors' response] Done.

P11, L9-10: I find the explanation for the high geothermal heat fluxes needed reasonable. However, if this is really heat flow from the surroundings, is it reasonable to keep this constant throughout the simulation? Also, what is the error introduced by adding this heat at the base of a 120m soil column?

[Authors' response] The model has only one parameter to add heat flux from the lower boundary. We calibrated it based on modelled and observed soil temperatures observed at 1m and 3 m for Cartwright site, and at 2.0m and 4.5m at Blanc Sablon site during 2014-2016 (Figure S1 and S2). It is hard to consider its variation with time using this 1D model. A two- or three-dimensional model with detailed hydrological component is needed to quantify these changes such as the studies of Kurylyk et al. (2015, doi: 10.1002/2015WR018057) and Sjöberg et al. (2016. doi:10.1002/2015WR017571). We added this limitation of the model in the discussion.

P12, L23: TTOP should be explained here. What is it and how is it derived? If these are values derived with the TTOP model I would not call these "recorded".

[Authors' response] At an earlier mention of TTOP we have added the definition of TTOP used in this study and have referred to Way and Lewkowicz (2018) where there is a fuller discussion of this definition and these values. We have added a reference to that study here as well.

P15, L2-3: I would add the snow feedback to the reasons why these simulations might be too optimistic: When the PF thaws (and excess ice melts) less snow should be removed, and the wind-scouring factor should decrease, which is not accounted for here.

[Authors' response] We have added the following discussion:

Degradation induced changes in palsa morphology due to melting of excess ice may further warm ground temperatures via changes in snow accumulation (increases) which are not considered in the model simulations. Correspondingly, the modelling results are likely optimistic in terms of permafrost persistence.

P15, L22: Koven et al. (2013) does not describe regional model simulations, but global.

[Authors' response] Agreed and amended accordingly.

Table 3: It would be useful to have the locations in this table as well, so one would not have to go back and forth between this table and table 1. Consider adding this as an extra column here or naming the ERT profiles according to the locations (e.g. BS1, BS2, RB1, RB2 etc).

[Authors' response] We had added a column delimiting the location for each profile.