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Discussions

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Interactive comment on "Characteristics and fate of isolated permafrost patches in coastal Labrador, Canada" *by* Robert G. Way et al.

Anonymous Referee #2

Received and published: 19 February 2018

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:

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

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.

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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.

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

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)?

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)?

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

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?

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".

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.

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

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).

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-271, 2018.