The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-271-RC1, 2018

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

Anonymous Referee #1

Received and published: 5 February 2018

General comments: The manuscript describes characteristics of palsa peatland permafrost in coastal Labrador, Canada, derived from electrical resistivity studies and local observations of ground temperature and climatic variables. These descriptions, from several sites in the area, are complemented with numerical transient modeling of the fate of permafrost under future warming for two studied boreholes in the study area. The results show that thin permafrost exist in isolated patches in palsa peatlands in the relatively warm coastal area of Labrador (-1.1° to 1° C average annual air temperature) and that it is in equilibrium with present climate due to a combination of thin snow cover and large thermal offset caused by the peat cover. The simulations show that the permafrost would degrade in most or all of the study area for the range of tested climate warming scenarios.

The dynamics of permafrost in the discontinuous, sporadic and isolated patches permafrost zones is challenging to predict with today's simulation tools, which generally focus on coarse scales and vertical heat fluxes. This study adds valuable information on how and where permafrost appears in isolated patches, which is needed for understanding of how climatic changes can affect these areas. It further addresses the challenges associated with using numerical permafrost models, which do not represent lateral heat fluxes, for simulating this type of permafrost.

The manuscript is well-written and structured. The introduction is brief but relevant. The methods section could need some expansion and clarification on some details, in particular the simulation procedure should be better described (see detailed comments below). The results are presented straight-forwardly. The discussion puts the results in context of current knowledge and highlights the relevance and impact of the findings, but I lack a mention of the implications of the assumptions that the modeling is based on (see detailed comments below). The manuscript contains many figures which I think is of value for a study that presents this type of geophysical data for describing permafrost.

As the manuscript presents a significant contribution to our current understanding of palsa and peat plateau permafrost characteristics and dynamics, and is generally well-written, I recommend that it is accepted after minor revisions.

Detailed comments:

P4, L1: Way and Lewkowicz full citation should be available when this is published.

P4, L7: What is the water-jet method?

P4, L25: What is meant by "low sensitivity areas (<0.1)" and how does it reflect uncertainties?

S1: In general, it would be good to state what the different parameter values are based on (local data, literature. . .?)

“Degree of decomposition” increases from 0.1 to 0.4 – is this linearly with depth?

“Organic matter content” – same as above

“Degree of decomposition” again. . . Is this for mineral substrate?

“Fraction of quartz” and “Thermal conductivity of rock” – how were these values chosen?

“Geothermal heat flux” – these were calibrated, right? This should be clearly stated in the table.

“Water table reduces 10% when above ground surface” – This sentence is really difficult to understand. I have no idea what it means. 10 % of what? In what way does the model include water above the ground surface, and how is a lateral flux of water incorporated?

The vegetation type is listed as shrub, but in your vegetation description you state that no shrubs are present on the palsas, only on the sides. How is this choice motivated?

P4, L33: Please clarify that all thermal properties and other necessary parameters and their motivations (literature, field observations) are listed in S1 and make sure that they are.

P4, L30 – P5, L12: A presentation of the model discretization/mesh is lacking. It is also unclear how initial conditions were set up and how/if any spin-up procedures were performed. Was the model parameters calibrated after a spin-up from year 1900? If so, what were the initial conditions at year 1900? Was daily air temperature the only data needed for running the model for all time periods? If there is a snow wind-scouring factor, I would assume that the model also takes in snow/precipitation data. Please formulate this more clearly than “climatic inputs” (P4, L8).

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?

P8, L30: TTOP?

P11, L4-15: So, the higher magnitude of the geothermal heat flux is used to compensate for the lack of horizontal heat fluxes in the model. How can you assume that the influence of the horizontal fluxes is stable over time, i.e. for keeping the calibrated geothermal heat flux values steady over the simulated warming periods? I understand that it is probably not possible to test this within this study, but the importance of this assumption should be noted in the text, especially as you argue for a higher geothermal heat flux in Cartwright due to smaller palsas in this location.

P11, L9: Do you mean horizontal (instead of vertical)?

P12, L20: Another relevant reference about how snow influences palsa ground temperatures in Scandinavia is Sannel et al., 2016:

Sannel, A.B.K., Hugelius, G., Jansson, P., Kuhry, P., 2016: Permafrost warming in a subarctic peatland – which meteorological controls are most important? Permafrost and Periglacial Processes, doi:10.1002/ppp.1862.

P12, L26-30: Why could it not be both? The simulation tool applied here does not take into account potential feedback processes that could speed up warming/thawing with time, such as increases in lateral heat transport as permafrost bodies decrease in size, and feedback from changing topography (with melt of ground ice) to decreased wind-scouring and subsequent warming/thawing.

P13, L8: Why does these heat flows need to be advective? See for example Kurylyk et al., 2016:

Kurylyk, B. L., M. Hayashi, W. L. Quinton, J. M. McKenzie, and C. I. Voss (2016), Influence of vertical and lateral heat transfer on permafrost thaw, peatland landscape transition, and groundwater flow, Water Resour. Res., 52, 1286–1305, doi:10.1002/2015WR018057.

Fig. S4: This figure needs some more explanation. It would be helpful if a definition of the thermal offset was given, that was used for these calculated values before and (most of all) after permafrost thaw.

P15,L1-3: A couple of references of relevance in a discussion on the relative importance of these processes when modeling permafrost in palsa peatlands are the above listed article by Kurylyk et al. (2016) and an article by Sjöberg et al. (2016). In both publications, numerical models that couple heat and water fluxes in 2D (thus incorporating advective and lateral diffusive heat fluxes) are applied for similar environments. Kurylyk et al. find that horizontal thaw rates are much higher than vertical thaw rates, for a simulated peat plateau and that most of this is due to horizontal diffusive heat fluxes. Sjöberg et al. studied lateral heat fluxes through a permafrost-free fen separating two peat plateau, and found that incorporating advective heat flow in the model changed the spring thaw date of the fen by a month.

Sjöberg, Y., E. Coon, A. B. K. Sannel, R. Pannetier, D. Harp, A. Frampton, S. L. Painter, and S. W. Lyon. (2016), Thermal effects of groundwater flow through subarctic fens: A case study based on field observations and numerical modeling, Water Resour. Res., 52, 1591–1606, doi:10.1002/2015WR017571.

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