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The Cryosphere Discussions

Interactive comment on "New ground ice maps for Canada using a paleogeographic modelling approach" *by* H. Brendan O'Neill et al.

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REVIEW

H. Brendan O'Neill, Stephen A. Wolfe, Caroline Duchesne "New ground ice maps for Canada using a paleogeographic modelling approach"

This manuscript presents a new modelling approach based on surficial geology, permafrost extent, and paleogeographic reconstructions, which allows for general evaluation of distribution of ground ice in Canada. The proposed approach has some obvious limitations that were described in details by the authors. Nevertheless, this approach is an important step towards better understanding of permafrost conditions and ground-ice distribution in Canada. Considering the lack of field data, which is common for

many areas, the modelling approach gives an opportunity to fill the gaps in ground-ice information and delineate potential areas with high ground-ice contents.

The manuscript is clearly written and the authors' approach looks very interesting and helpful for future permafrost studies in Canada and other countries, so I definitely support publication of this paper. However, the manuscript needs some revision. These are my major concerns and recommendations:

1. I recommend to change the title because "a paleogeographic modelling" is only a part of your approach and does not reflect the entire process. Maybe just "a modelling approach"?

2. I recommend to clarify some terms and definitions. You often use the term "massive ice and icy sediments" (page 2, line 5; page 4, line 30; page 5 line 15, etc.) to describe tabular massive-ice bodies of either glacier or intrasedimental origin. This approach is common in the permafrost literature (e.g., Heginbottom et al., 1995; Brown et al., 1997; French, 2018) but the term "massive ice" itself is very general. This term covers many types of ground ice (including wedge ice) so I recommend to specify what particular type of massive ice you are talking about.

3. The term "intrasedimental ice" is also very general (for example, see French, 2007, p. 182) but you apply it only to tabular massive ice bodies.

4. When you use the term "segregated ice" (e.g., page 6, line 5), it's better to specify that here you are talking not about massive ice bodies formed by segregated ice but about relatively thin lenses and layers that form cryostructures of the frozen soils.

5. Page 4, lines 9-11. I cannot see a big difference here: both maps (Heginbottom et al., 1995, and Brown et al., 1997) do not present numerical estimates for massive ice, pingos, and ice wedges using different symbols for their general distribution (abundant and sparse) instead.

6. Page 4, lines 33-35. Increases in active layer thickness cannot reach 5 m (in such

case, it's already permafrost degradation), so I recommend to talk about thermokarst and thaw subsidence to explain why these top 5 m of permafrost are so important.

7. Page 5, lines 8-16. I recommend to rewrite this paragraph. When you describe formation of segregated ice at the top of permafrost (mechanism #2), it's better to talk about aggradational ice (e.g., Mackay, 1972; Cheng, 1983; French and Shur, 2010) – the process of its formation describes this mechanism better than just downward water migration from the active layer. Presenting examples of the mechanism #1, you mention only lithalsas, palsas, and peat plateaus but this mechanism covers any epigenetic freezing: for example, freezing of recently exposed sediments in the continuous permafrost zone (e.g., lacustrine sediments after the lake drainage). Your description of the mechanism #2 and corresponding examples (lines 11-15) look rather complicated. I recommend to start with formation of syngenetic permafrost because this is the best example of this mechanism. Second, you may also mention formation of the intermediate layer (quasi-syngenetic permafrost) – this process is also very common (see Shur, 1988; Shur and French, 2010; Shur et al., 2011). Line 16: I totally agree that both mechanisms can occur within the same landform but I'm not sure that you give the best example (palsas and lithalsas). Actually, such combinations occur widely within the entire permafrost region – in the areas where syngenetic and/or quasi-syngenetic permafrost is underlain by epigenetic permafrost, which is really very common.

8. Page 5, lines 17-21. I recommend to omit or simplify this paragraph: instead of talking about beds of intrasedimental ice, it's more important just to emphasize that here you are talking about relatively thin lenses of segregated ice that form various cryostructures and not about massive ice bodies formed by segregated ice.

9. I recommend you not to use terms "extensive discontinuous" and "sporadic discontinuous zones." These terms were used in PMC (Heginbottom et al., 1995) but now synonymous terms "discontinuous" and "sporadic" are much more common. Anyway, you use both sets of terms in the same paper, and it doesn't look good – it's always better to be consistent.

10. In your manuscript, you do not describe yedoma in Canada. I understand that you ". . .model ground ice evolution over the last 17 ka in Canada. Ground ice formed in unglaciated terrain prior to deglaciation is thus not considered" (page 7, lines 3-4). When you discuss some limitations of your models, you also state that "the models do not account for the difference in ground ice accumulation due to the mode of permafrost aggradation, i.e., epigenetic or syngenetic" (page 17, lines 19-20), and talking about syngenetic permafrost you mention that "distribution of loess is limited in Canada" (page 18, lines 4-5). To prove it you cite the paper by Sweeney and Smalley (1988), which describes only thin (<1 m thick) loess deposits in permafrost regions of Canada, but you do not cite several important papers describing occurrence of the late Pleistocene syngenetic permafrost with large ice wedges in some parts of Canada (Fraser and Burn, 1997; Kotler and Burn, 2000; Froese et al., 2009; Stephani et al., 2014; Fortier et al., 2018). Of course yedoma is not so common in Canada (in comparison with Siberia and Alaska) but considering importance of these very specific deposits, which have extremely high ice contents due to both segregated and wedge ice, I strongly recommend to write more about yedoma. And I also believe it would be good to include the modelling of yedoma distribution in Canada in your future studies. Talking about syngenetic permafrost, you may also mention formation of modern syngenetic permafrost within floodplains of Arctic rivers.

MORE COMMENTS AND SUGGESTIONS ARE PROVIDED IN THE ATTACHED FILE.

Good luck!

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Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-200/tc-2018-200-RC1-supplement.pdf

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