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Interactive comment on "Ground ice, organic carbon and soluble cations in tundra permafrost and active-layer soils near a Laurentide ice divide in the Slave Geological Province, N.W.T., Canada" *by* Rupesh Subedi

Anonymous Referee #1

Received and published: 6 March 2020

General comments

This study presents unique and highly valuable data on ground ice, organic carbon and soluble cation contents in deep permafrost cores of the Central Canadian Arctic. The surprisingly high ground ice content found in some cores makes the landscape susceptible to potential (differential) ground subsidence and thermokarst formation allowing the remobilization of deep carbon and soluble cation stocks (as well as affecting infrastructure).

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Discussion paper

The authors should better explain why this particularly study area (Lac de Gras) was selected, as well as which approach was used to select specific core sites. This is important information to evaluate how representative this study is for the wider Slave Geological Province.

There are some issues with field and laboratory procedures, regarding the logging of field volumes collected in the active layer of soil pits, the application of a mean LOI value of 80% to organic samples in the top meter of the cores, disregarding coarse clast volume, the inferred zero organic carbon content of the soil fraction 0.5-5.0 mm, the indirectly inferred fine soil fraction (< 0.5 mm) for about half of the samples, and the indirect derivation (regression) of dry bulk density values (when known volume samples are, or could have been, available for most of the samples). Particularly, SOC estimates for the 0-1 m depth interval are prone to large uncertainties and should not be the focus of the analysis. I feel it necessary to mention these concerns, even though in most cases they cannot be addressed any longer.

The structure and use of language are adequate. I propose to move one subsection on field sampling to Methods. Figures and tables are generally fine, but I suggest to add an additional map to Figure 1 as well as a new figure in the Appendix with properties of a few selected individual permafrost cores.

Despite some methodological issues, this study is a very important scientific contribution that addresses important gaps in the knowledge of ground ice and organic carbon content in deep permafrost cores (other than deltaic and Yedoma deposits).

Specific comments

Title: ... and soluble cations in deep tundra permafrost cores near a Laurentide ... Note: the 0-1 m (and active layer) SOC estimate is highly uncertain (see below), the authors should focus on the valuable deep data

Page 1 (P1), Line 13-14 (L13-14): ... and 0-3 m, respectively. Deeper deposits have

C densities ranging from X-Y Kg C m-3, representing a significant additional C pool.

P1, L16: . . . and slightly less 0-3 m organic carbon stocks and fewer . . .

P2, L45: . . . consequences of permafrost thaw.

P2; L47: (Hugelius et al., 2014)

P3, L79: The authors should clarify why this particular study area was selected, especially since they compare their results to more generalized maps of permafrost/ground ice conditions and SOC storage for Canada and the northern circumpolar region. Is it a simple issue of accessibility, or was this area chosen because of special features of potential interest to infrastructure development (e.g., the occurrence of fossil thaw slumps as depicted in Fig. C1) ?. This is important in order to evaluate the representativity of the study area for the Slave Geological Province.

P3, L70: . . . characterization of active layer and deep permafrost materials in . . .

P3, L82: . . . and 14.x(0?) C, respectively, and . . .

P3, L83: cal yr BP ? (2 times)

P4, L95: I propose that the authors include a (simplified) surficial geological map of the 50x50 km study area as Fig. 1B, with location of the 24 permafrost cores. The current Canada map can be a small inset (Fig. 1A).

P4, L100: Organic soils cover 5% and . . .

P5, Fig. 2 caption (and related references in text). Shift B and C, see Figures 3-6

P5, L116: As with the study area, the authors should explain their selection of core sites. Were sites selected because of easy access, or because they were considered typical for the different surface geology units, or was there a degree of randomness in site selection. This is important to assess how representative sites are for scaling to the study area as a whole. See my point P8, L183-184.

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P5, L116: Permafrost cores with a diameter of 5 cm were obtained in mid-July 2015 using a . . .. . . Note: So, these samples had a known volume that could have been used for DBD calculations !

P5, L118: How was the active layer in soil pits sampled ? Sample depth interval ?, using fixed volume cylinders (for DBD) ?

P6, L129: The coarse clasts >5 cm that could not be recovered by the drill are not considered. The authors refer to this on P12, L238. This could result in a significant overestimation of OC densities, particularly in till. I wonder, are there no natural/excavated deep exposures in the general study area from which the volume proportion of large clasts can be (visually) estimated and then computed ?

P6, L149: Using a LOI of 80% for those samples with no visible mineral component is highly questionable. Peat deposits will normally have a higher LOI, whereas topsoil organics in mineral soils will have generally a lower LOI. This introduces high uncertainty, which is one reason why the authors should not focus on the SOC 0-1 m stock.

P6, L150: The LOI applies to the fine soil fraction (<0.5 mm), whereas the volume of the coarse fraction >5 mm – 5 cm is accounted for (P6, L129). But what happens to the fraction 0.5-5 mm, is this all considered 100% mineral ? It could include roots, or other plant remains / organic aggregates, etc. ?. Furthermore, the fine soil fraction (<0.5 mm) is only available for half of the samples and very indirect approaches are used to calculate this value for the remaining samples (P7, L166-167).

P7, L160: It is rather unfortunate that DBD was not computed directly from dry weight and field volume of samples, at least for those samples in which no ground ice/materials were lost

P7, L174-175: The fine fraction and DBD deviations for calculating uncertainty ranges seem to be quite arbitrary

P8, L183-184: This section/subsection should be moved prior to subsection 3.1., start-

ing with an explanation about the selection of sites (see my point P5, L116)

P8, L187+: For all boreholes, it should be indicated why coring was discontinued (hitting bedrock, logistical/time constraints, etc.). See also comment on Table A1 (below)

P8, L218: Please add area proportion for each surface geology class (see P15, L316-317)

P9-12: In Figs. 3-6 the authors have grouped samples from all profiles belonging to one class in one and the same graph. The information from single profiles is lost. I propose to add graphs in the Appendix, providing data from Figs. 4-6 for a single/typical/most complete core for each surface geology class (New Appendix Figure C1-C4). It should be considered that data from individual profiles are more valuable than composites that cannot be disentangled anymore in its individual components.

P13, L264-265 and P14, L274-275: (currently Fig. C1)

P18, Table A1: Add depth of core (and reason to stop drilling)

P20, Fig. B2 caption. The peat in (A) would normally have a LOI of c. 95%. The default value of 80% does not generally apply

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