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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

Rupesh Subedi

stephangruber@cunet.carleton.ca

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Reply to the interactive comment of Anonymous Referee 1 concerning the manuscript "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 R.Subedi, S.V. Kokelj and S. Gruber.

We are grateful for the constructive and detailed comments provided. Here, we respond to each issue raised and outline how the comments led us to revise the

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Discussion paper

manuscript. The entire original text of the interactive comment is shown in **bold font** and author responses in regular font. Each issue is identified with a code indicating "Referee 1" as well as a consecutive number, e.g., R1.1. This will help to revisit key issues raised by each of the three interactive comments in a summary reply that outlines the most important changes to the manuscript.

0.1 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).

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 sub-section 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).

0.2 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

R1.1 – Deep cores: Interpretation of the term 'deep' varies between academic communities and, as such, we prefer to not use it in the title. The abstract clarifies "Twenty-four boreholes with depths up to ten metres...".

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.
Done

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

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

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

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.

R1.2 – Selection of study site: More detail on the selection of the study site and its relationship with surrounding areas is now provided, including new map figures.

P3, L70: ...characterization of active layer and deep permafrost materials in...
We prefer to avoid the term 'deep permafrost', see R1.1.

P3, L82: ...and 14 C, respectively, and...
Done

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

R1.3 – Calibrated years: The two references cited do not specify whether these are calibrated radiocarbon years. Presumably they are, but we prefer not to make that determination.

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).

R1.4 – Map of study area: This figure is now provided.

P4, L100: Organic soils cover 5% and...
This has now been replaced by table because the response to other comments required more specific data on spatial abundances.

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

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.

R1.5 – Site selection and sampling strategy: The original strategy for site selection is now explained in more detail.

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 !

R1.6 – Volume of core: Many of the recovered core sections were irregular due to reaming or partial melting. This is now clarified in more detail and with an additional figure in the revised text.

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

R1.7 – Volume of pit samples: Pits were sampled, where possible, at depths of 10 cm, 20 cm and 30 cm. The use of sampling cylinders deviated from the intended protocol and the volume cannot be reconstructed with confidence. Clarified in revised text.

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?

R1.8 – Bias from clasts larger than 5 cm The average volume of clasts coarser than the drill barrel has now been estimated visually from the core photographs and applied as a correction in the aggregation of organic carbon densities and storage, accordingly.

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.

R1.9 – Uncertainty from LOI estimate when no mineral content visible: Yes. This now stated and referenced in the revised manuscript. Estimates actually only affected the pit samples in the top 0.3 m and this is also clarified, now.

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).

R1.10 – Carbon density bias from 0.5–5 mm grain-size fraction: After drying the samples at 105 C, they were crushed with mortar and pestle before sieving; most root or plants residues would have been crushed and passed the sieve.

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
Agreed. At the same time, this is a unique set of data and we intend to make it as useful as is possible.

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

R1.11 – Arbitrary uncertainty ranges They are based on reasoning and subjective decisions because no objective values can be determined. This is why we wrote 'The potential magnitude of this effect...' rather than calling it an estimation of uncertainty. More explanation has now been added in the revised manuscript to make the reasoning behind those ranges traceable, and therefor the resulting values more valuable.

P8, L183-184: This section/subsection should be moved prior to subsection 3.1., starting with an explanation about the selection of sites (see my point P5, L116)

R1.12 – Moving Section 4.1 Study Sites We have expanded the section on field observation and sampling to also describe the original sampling protocol and difficulties encountered.

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)

R1.13 – Expanding Table A1: The table has been expanded and the Supplement now contains plots per borehole.

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

R1.14 – Abundance of surficial geology classes: This is now in a new table.

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.

R1.15 – Individual borehole profiles: This has been added as supplementary material.

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

P18, Table A1: Add depth of core (and reason to stop drilling)
Yes, see R1.13 above.

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
Yes, see R1.9, above.

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