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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

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Reply to the interactive comment of Anonymous Referee 2 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 comments provided. Here, we respond to each issue raised and outline how the comments led us to revise the manuscript. The entire original text

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of the interactive comment is shown in **bold font** and author responses in regular font. Each issue is identified with a code indicating "Referee 2" as well as a consecutive number, e.g., R2.1. 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 Summary

This paper is formally well written and presented, and describes unique data from an interesting environment that is under-represented in the literature. I think that the data from these cores should be published, but in its current form the paper does not do the material justice. The paper suffers from a mismatch between its stated goals and its delivered conclusions. There are also problems with the methods that are major enough to call some of the conclusions into question. I recommend that the paper be rejected and returned to the authors for re-working and potential re-submission after the problems have been addressed.

0.2 Criticisms

0.2.1 Approach

The context for this study requires more thinking and description. For example, the authors state that, contrary to published maps, there is significant excess ice in the "area" – what is the area? The paper requires a map showing the Keewatin Ice Divide, the "central Slave Geological Province", etc. Table A.1 would be informative if it were a map or, even better, mapped together with an optical satellite image to allow comparison with land cover. For what region do the authors believe that their results are representative? Demonstrate that the set of

cores is somehow representative for a region and communicate what region this is. How much of the region that you so identify is represented by the four terrain types for which you present data?

R2.1 – Maps and context: An overview map figure has been added to the manuscript and an additional map figure with the local study area to the new Supplement. We decided against optical satellite data as surface cover does not correlate well with sub-surface conditions.

“Comparison with other permafrost regions...” is listed as the 3rd goal of the paper, but is not addressed and represents a central weakness. As an example, any reader of this paper would expect to find permafrost ONLY in those regions of the NWT and Yukon in which the authors have already worked, as well as in unglaciated portions of Alaska. Such myopia is not unique: many authors cite only papers within their national borders, but this criticism is a grave one for geoscientists, who should have a natural curiosity for the variability of expressions of their study objects on Earth. European, Alaskan and Russian research on permafrost is extensive, and offers many studies that deal with ALL of the study objects (glaciation, permafrost, ground ice, ground ice/water chemistry, etc.) and processes (thawing, landscape change, deglaciation, solute exclusion, thermokarst, etc.) in this study. Willfully ignoring most of the research in your field makes it impossible for the authors to demonstrate rigor in their approach, their knowledge of their field or an openness to the existing and potentially alternative interpretations of similar data sets. Comparing your results (e.g. Table D2) to only 2 previous studies (BOTH from your own research group!) cannot fulfill your stated goal to “...compare excess-ice content, organic-carbon density and soluble cation concentration with other permafrost environments...”. It certainly reduces the relevance of your work to a broader readership and would alone be reason enough not to publish this manuscript.

R2.2 – Comparison with other permafrost regions: The full text for Objective iii was "compare excess-ice content, organic-carbon density and soluble cation concentration with other permafrost environments *and with compilations such as overview maps and databases*". Here, in order to keep the scope of the manuscript manageable, "compilations such as overview maps and databases" have been taken as current reflections of what may be expected in the study area. To avoid misunderstanding, we have adjusted our formulation to read "... other permafrost environments OR with compilations ..." in the revised manuscript. Beyond that, we do find it unreasonable to conduct a study where conditions are contrasted between two regions, especially if one is well studied and is similar in some but not all aspects. This is part of formulating tractable questions.

R2.3 – Comparison of solute contents with two studies is not enough: Few other studies exist in formerly glaciated and nearby environments that can easily be compared. In part, this is due to the broad diversity of approaches in extracting soil water and reporting (normalizing) analytical results. This is also addressed in our responses R2.7 and R2.12, and now described in the revised manuscript. It also explains why two studies with overlap in authorship were chosen: here the methods are comparable. One study (Lacelle et al., 2019) has now additionally been quantitatively included as data was available digitally and could be converted. At the same time, the impact of differing lab methods is difficult to assess. Two additional studies from the vicinity of Yellowknife have also been included, although direct quantitative comparison is difficult.

The study makes the a priori assumption that categorization of the soil cores by terrain types is justified and that averaging all borehole results within one terrain type is justifed. How were terrain types identified? Why were these four chosen? Why do the authors think that terrain type has an influence on ice content, organic carbon, and total soluble cations? Such questions are fundamental to study design and the means to reaching conclusions of broader

significance. Tellingly, there is no significance to the terrain types in the 6 conclusions reached. At this point, the authors should have re-examined the basis for their choices. The results for all four parameters as a function of depth do not provide enough information (Fig. 3-6) for the reader to assess whether the cores really differ between terrain types. It is not clear why only total soluble cations are analyzed and presented. Why not other dissolved species? Why not present profiles for individual boreholes and/or cations? This work is left to the reader.

R2.4 – Justification of terrain types The initial field sampling design and reasons for it having limited utility for interpretation of results is now described briefly in the revised manuscript. Based on this, the distinction of the four terrain types used is justified as a better way to group the locations analysed. Plots of individual borehole logs and analytical results are included in a new Supplement to prevent the manuscript from becoming too long. We do not agree that conclusions specific to each terrain type are a measure of their utility. Along the same lines, one could ask why individual boreholes should be shown. Terrain types are a useful grouping to add explanation to observations.

R2.5 – Why only total soluble cations? This study has unique data and insight to offer in several domains (ice, carbon, cations). As it is subject to a number of imperfections, focusing on total soluble cations and few salient features of their distributions keeps this robust and concise.

0.2.2 Methods & Data treatment

The field photos of the cores are very impressive and show a wide variety of compositions and cryostructures – I think the paper would be strengthened by more of these and closer examination of the results. The methods used in this paper are beset with problems. The authors should refer to standard texts and

guides on soil analyses for methods of analyses. It is not sufficient to cite previous studies by the same set of authors (using "cf."?) to establish that standard accepted methods have been applied. Specific problems with the analytical methods are:

R2.6 – More core photos: The published core photos are referenced in the description of the study region in the original manuscript. The data (more than 2.5 GB) is well organized by borehole and depth interval. Rather than include many more photos and increase the length of the manuscript, we point to this information with more specificity in the revised version and point-out sections with telling photographs in borehole plots. Photos of frozen ice-poor till have now been included as an additional important example in a new figure.

R2.7 – Standard accepted methods: Janzen (1993) and Dean (1974), cited in the original manuscript, are standard texts. At the same time, these and other standard texts on soil analyses known to us do not describe methods for permafrost materials specifically. This is relevant because the extension of standard procedures (usually based in agricultural considerations) to permafrost materials is not straightforward. In many instances, no consensus on how this is best done is apparent from the permafrost literature. Often, this problem is due to extreme ranges of water content when excess ice is present. For example, expressing water content on a dry gravimetric basis, as proposed in standard texts, can lead to high values that are difficult to interpret (Phillips et al., 2015). This is now also clarified in the revised manuscript.

The are 2 sets of cation concentrations: those which underwent an arbitrary 1:1 dilution with water and those which did not. Diluting the solution affects the equilibrium between dissolved and adsorbed species. It is not acceptable to treat these data sets as equivalent expressions of some kind of concentration without at least testing for equivalence.

R2.8 – Comparing samples with and without added water: In the revised manuscript, the known issues with differing extraction ratios are now described in the methods section. Additionally, Interpretation and Discussion now include test results about the effect of extraction ratio on our results.

The creation of means as a function of depth is problematic and has not been justified – how is the 95% confidence limit calculated? In many cases MOST of the measurements lie outside of the 95% confidence limit. In what are the authors 95% confident? Anyone drilling in a similar location can be confident that most values lie outside of these limtis. With this kind of variability over depth, drawing a smoothed mean variation (the blue lines) over depth is nonsensical and masks real variability. It certainly requires examining each set of measurements on a per-borehole basis and on evaluating the data in detail.

R2.9 – Mean profiles: Mean profiles make broad patterns emerging from multiple boreholes and/or samples more easily visible. Because the actual sample values are shown in the same graph, the real variability is not masked. The standard error of the average expresses the confidence in the average falling within this range, accounting for sample abundance and vertical extent. The majority of samples may be outside the standard error at 95% confidence for the mean profile.

Similarly for Table D2: it is not clear how many samples are used to form the means, whether they are means, what the variability is, etc.

R2.10 – Data in Table D2: The values from Kokelj and Burn (2005) are now identified as mean values in Table D2 of the revised manuscript. The number of samples or measures of spread are not included in the table of the original publication but, if desired, can be appreciated from the figures. Values from Kokelj et al. (2002) are now identified as 'estimated from figures'. For both, we prefer not to indicate statistical results that we estimate after the fact but rather report and interpret the publications' content in the

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simplest way that supports our interpretation. The difficulty outlined here underscores the fact that finding more data for quantitative comparison is not straight forward.

Presumably data from multiple cores is combined for each of the four regions – it is not clear and has not been established that these groups of cores are similar enough to be grouped, that the cores cover similar depth ranges, that the sampling frequency is similar, or that the sample sizes per terrain type have no effect. Present the data for individual cores, do not create means, etc. and then establish that the groups of cores are different using a test of significance. At the moment, all the work is left to the reader.

R2.11 – Combining multiple boreholes in a terrain type: The justification for the terrain types is outlined in more detail in the revised manuscript and individual borehole plots are included in the Supplement. We also clarify that mean profiles are described for aiding quantitative description, rather than quantitative prediction, based on sometimes sparsely and unevenly sampled boreholes.

The expression of concentration per dry weight of sediment is almost entirely meaningless. It is meaningless in terms of processes affecting concentration during freezing, thawing or in general in the field. Concentration gradients, moisture migration, and any other relevant processes will depend on concentrations in the pore space or pore water or liquid water. Concentrations should AT LEAST be reported in terms of the water volume obtained by thawing the samples.

R2.12 – Normalizing concentration per dry mass of sediment: In the new Supplement to the revised manuscript, we now also plot concentrations in terms of the water volume obtained by thawing the samples, as requested (this was included in the supplementary materials digitally already before). Additionally, we explicitly mention the expression of results per dry weight in the conclusion to further prevent misunderstanding. We agree that this adds clarity as some results may depend on the method for normalization chosen. We do not, however, agree with the broad assertion about the utility of the approach we have chosen: Solute contents in the permafrost literature are expressed in a variety of ways, for example with respect to dry mass, volume, or soil water content. None of these ways, similar to the choice of solute extraction (R2.7), is obviously perfect. In our example, standardization by dry weight makes sense because it helps in comparing permafrost (some very ice rich) and actively-layer soils (some coarse-grained and in dry convex upland locations). Furthermore, assuming that some of our ice rich sediments partially derive from Laurentide ice implies that the majority of solute found there can be assumed to originate from its particle content, possibly after thawing. For clarification, we have expanded the glaciological background so that the possibility of finding solute poor ice mixed with frozen sediments becomes more obvious. In summary, the issue of normalizing results, together with the wide differences in extraction methods used (see R2.7 – Comparing samples with and without added water) makes comparison of soil chemistry difficult between individual permafrost studies. To further clarify, a brief summary of these problems has been included in the manuscript.

The calculation of excess ice content based on the ratio of volumes of thawed, saturated, settled sediment and supernatant liquid is problematic since the volume of supernatant liquid depends on soil texture.

R2.13 – Determination of excess-ice content: Certainly. Further refinement or discussion of the shortcoming of this method, however, are beyond the scope of the work presented and would not affect our conclusions. The method we use remains the accepted standard in permafrost science (Subcommittee on Permafrost 1988) and engineering (ASTM 2016). Furthermore, line 124 in the original manuscript used 'estimate' to acknowledge that this is not clear cut.

0.3 Conclusions

Each conclusion has problems:

1. Without placing your borehole sites in a geographical context, it is difficult to evaluate whether this qualifies as a new regional insight.

R2.14 – Geographic context and regional insight: The context has now been expanded considerably with new figures and much expanded background on glaciological setting.

2. The method of measuring excess ice does not allow the conclusion that thick occurrences of excess ice were found in tills. The photos show excess ice, but make it difficult to believe the volumetric values obtained by this method.

R2.15 – Excess ice amounts: We maintain (see R2.12) that the method chosen for estimating excess ice content does allow the conclusion that thick occurrences of excess ice were found. We hope that the inclusion of individual borehole profiles in the Supplement and the published core photographs will also help alleviate this concern. We have addressed the question of the aerial abundance of thick excess ice now explicitly. While this is speculative, it helps to avoid the perception that we claim thick sequences of excess ice would be found everywhere. Finally, the amounts of 84% and 71% shown in Figure B3 were an error, as can also appreciated from Figure 4A that has no values above 80%. The values have now been corrected and additionally, the estimated visible ice content is shown per borehole, all of which are near 40%.

3. The soil cores go down to ten metres and have maximal ice contents of 60%. If the deepest core had the highest ice content, you would have subsidence of less 6 m. How then is subsidence of “tens of metres“ possible? Is this based on some kind of unmentioned extrapolation of observations?

R2.16 – Tens of meters: The formulation of tenS of metres in line 326 was unintentional and has been corrected. Lines 9 (abstract) and 271, correctly read "metres to more than ten metres" and "up to more than ten meters".

4. These are potentially interesting values, but would be made relevant if there was someway to know for what region the authors claim they are representative.

R2.17 – Area represented: The field area shown on a map and specified by coordinates. The spatial aggregation of soil organic carbon storage has been dropped to keep the manuscript manageable.

5. The cation concentration data are used only to establish in general "lower concentrations" when compared to a two studies from one other region and are entirely incidental to the paper's conclusions. There is no need to present terrain types, variation over depth or any of the data to reach this conclusion.

R2.18 – Conclusion concerning cation concentration: Yes, we have chosen to remain with a simple indicator and analysis and a high level result that can be stated with some confidence. We report a summary that is true for all three terrain types that have mineral soils in permafrost and the active layer. We hope that the added detail in the explanation of terrain types will further alleviate the concern raised here.

6. I agree that geological legacy is important. The data here are insufficiently linked to geological legacy.

R2.19 – Linking with geological legacy: This should be more obvious now with more explicit geographic context, justification of terrain types, and individual profile data shown and discussed.

1 References

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