The Cryosphere Discuss.,
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The Cryosphere Discussions

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 #2

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Review: tc-2020-33

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

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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.

Criticisms

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?

"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.

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 justififed. 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.

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:

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- 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.

- 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.

- 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.

- 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.

- 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.

- 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.

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.

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.

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?

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.

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.

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

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