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Interactive comment on "Carbon and nitrogen pools in thermokarst-affected permafrost landscapes in Arctic Siberia" *by* Matthias Fuchs et al.

Anonymous Referee #2

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The authors of "Carbon and nitrogen pools in thermokarst-affected permafrost landscapes in Arctic Siberia" have executed and interesting study and presented a well written manuscript. The data are interesting and provide valuable measurements of permafrost C and N stocks. Some of the field sampling methods should be explained further to ensure the data merit scaling up using the landscape mapping approach described. The inclusion of N pools in this manuscript represent a novel contribution to the field of permafrost mapping but the implications for the author's findings should be more fully developed in the discussion. Details regarding the analysis of soil samples for %N must be clarified as well- the low N content of these mineral soils suggests that separate analysis for %C and %N would be necessary given the limitations of most

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Discussion paper

elemental analyzers.

Detailed comments and questions are listed below:

PG3, Line 11-15: Koven's study did an excellent job comparing two potentially limiting factors on ecosystems carbon balance (SOM-C decomposability, plant N limitation) but it doesn't present a strong argument against plants accessing N in newly thawed permafrost. They assume aboveground plant phenology reflects belowground plant phenology and say that they do not capture the microbial community/decomp dynamics observed in manipulative field experiments with the simplified N cycle they included in CLM. There is a growing body of field studies looking at plant access to N from thawing permafrost- Keuper et al 2017 (Global Change Biology) and references herein would be a good place to start.

PG3, Line 30: The introduction should include background information and literature references regarding the CN ratio of permafrost soils and how it relates to the past and future decomposition of SOM (ie, Schaedel et al 2014, Global Change Biology). The addition of N pool data is interesting component of this paper but seems underexplored.

PG5, Line 20-31: The field methods should be clarified to ensure the data represent the landscape adequately and will bear the scaling approaches utilized. How can the sampling point locations be equidistant from one another along a transect and reflect a stratified sample scheme? Does "stratified sampling" refers to the choice of transect location? How were the number of samples from baydzherakhs decide? What landscape features were baydzherakhs and DTLB data points grouped with for the average %C and %N values given in Table 1?

PG6, Line 9-13: Were separate samples run for %C and %N analysis? A larger weight of sample might have been necessary to determine %N numbers, especially on these mineral soils. A 5mg sample with only 0.1% N (Table 1 data) for instance would only have 5ug N which is likely below the detection limit for many elemental analyzers. Please provide detection range and sensitivity for this machine and/or specify sample amounts for the separate analyses if indeed samples were run separately.

PG7, thoughout: Please clarify how ground-truthing was conducted. Were training areas selected based on observations made in the field? Did any of the field observations points overlap with the high-resolution imagery described to check landform classification?

PG8, Line 4-8: Somewhat confusing to have this discussion of ice wedge calculations when the reader does not yet know the source of ice wedge content data... consider moving to later in the methods section.

PG8, Line 27: Is the difference described here significant? Seems unlikely given the variation in the Bykovsky samples.

PG14, Line 12-17: Move this background info to Introduction. Would it be possible to use the C:N data in this paper to estimate C losses from these sites using the models in Schaedel et al. 2014? Some more developed discussion of potential N mineralization with decomposition of these soils would be warranted. This paper's inclusion of N stocks is novel but the discussion does not delve into the implications of the results.

Table 1: Please include symbols or alternate font styles to denote statistical differences between sites and landscape forms.

Figure 2: Model summaries for decreasing C:N with depth and summary statistics should be included here.

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