

## ***Interactive comment on “Variability in Above and Belowground Carbon Stocks in a Siberian Larch Watershed” by Elizabeth E. Webb et al.***

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NOTE: Please see Supplement pdf for the updated text and pdf version of these comments.

We appreciate the input from the editor and from both reviewers, which we feel has made this a stronger manuscript. Our responses to the reviewers' comments follow each comment and are in italicized font. The line numbers referenced in our responses refer to the updated manuscript text, which is attached.

There were some comments by reviewer #2 that seem to have mismatched line numbers (based on the submitted version of the manuscript). We were able to resolve many of these comments, but for some comments it was not clear what text the com-

C1

ments referred. We would be grateful for an opportunity to address these comments after further clarification. Best, Sue Natali

Responses to Reviewer 1 It is clear throughout the text that the main emphasis was on the aboveground biomass. What I lack is the same accuracy and description for the fewer belowground carbon samples, especially for the 7 surface permafrost cores. As a reader I want to know for example: was the coring and the analysis of the 60 cm cores in short increments?

//The text has been edited at lines 227 to clarify that the cores were sectioned into ~10cm increments, and a Supplement Table has been added with depth increment data.

How were the 7 sampling sites selected? //The seven sites where surface permafrost was sampled were a subset of the 20 sampled stands; these seven sites were selected based on accessibility and distribution across the catchment. The 20 stands (i.e. 'sites') were selected to span a range of tree aboveground biomass, as inferred from tree shadows mapped using high-resolution (50 cm) WorldView-1 satellite imagery (Lines 134-136).

Any signs of cryoturbation, data on soil texture, etc. //We did not collect data on cryostructure or texture, unfortunately.

Why do you think there is so little carbon in your top meter compared to the results from many other studies? //We noted in lines 382-382 that these soil C pool estimates fall within the range of published assessments that characterize this area (i.e., forested area around Cherskiy). However, they are at the low end of the larger region, although within one SD of the regional mean. This may be a result of variation in parent material, disturbance (fire or harvest), or other soil conditions. This assessment, however, is beyond the scope of this study.

In addition, I suggest that you add the SOC data from the first meter from the two deep

C2

cores which are part of the watershed to the other permafrost cores. This additional data will most likely increase the 1m average which will then be similar to many other cited studies. //We added the 1m SOC data from the two deep cores to the average SOC value presented in the text (line 333-334). In the text, we presented average (+SE) SOM both with just the yedoma deep core added (because much of the manuscript focuses on yedoma C pools), and then with both the yedoma and alas deep core data added. Figure 2 has been updated with the additional data from the deep yedoma core. You have nice supplementary data but I lack the information from the permafrost cores in the data. These have been added in Supplement Table 4.

Since the samples were also analyzed for nitrogen (Line 223), why did you not further incorporate this data in the text? //We were only able to analyze a subset of soils for C and N because of challenges of transporting international soils. We were able to extrapolate %C, based on C-SOM relationship, to the full dataset, but not possible for %N. Inclusion of N analyses in the methods section was done in error, and we have removed this text.

Also, given the sampling and measurement uncertainties, I think is unnecessary to present the soil C values in grams, especially since you shift to kg from line 308 in the text. //Agree. The soil units in the text are in kg, and Table 4 now also is in kg C/m<sup>2</sup>.

Specific Comments - Line 288: Comma used for decimals //These actually should be commas, not decimals. No change made.

- Line 296: I suppose the SE should be  $\pm$ ? //Yes. We have corrected.

- Line 346: Please remove the word "slightly". //Done.

- Line 385: How many permafrost soils were sampled: 21 or 7? I miss this information in the section 2.7. Since it is stated in the description of Table 4 ". . .at selected sites, but not on the transects. . ."? //We collected three cores at 7 sites for a total of 21 'surface permafrost cores. We edited the table description to read: " Permafrost cores

### C3

were sampled to 1 m at 7 sites (3/site). ", and clarified the number of samples per site in section 2.8 on line 212.

- Line 640: Typo "Author(s)" //Corrected-thanks.

- Table 2 & 3: Site number 18 is not forested as stated in table 1, why are there values for larch/larch density? //We corrected table 1 description to read: " All sites were in forested areas except #17 (riparian); Site #18 (alas) had few scattered trees located along one end of the transects."

- Table 4: Would be good to indicate the n for the values since they are different //We have indicated the sample size in the table description.

- Table 4 & 5: Please add in the title "of the mean" as in others tables //Added.

### Responses to Reviewer #2

The weak points in the manuscript are in my opinion a somewhat confusing sampling scheme or its description, and an underdeveloped discussion that does not challenge the perspective of the authors. In particular, the authors see vegetation as a primary driver for total C storage, despite the fact that the vast majority of the C is stored in soil and moisture is identified as a major driver of C stocks. To round up the discussion, the authors should also consider that vegetation is merely a reaction to ground conditions and soil forming processes or topographic drivers. Further there is clearly a bias towards the description of vegetation analysis, while the description of the soil sampling and the discussion on soil related aspects is underdeveloped. Please rewrite the sampling description and/or provide a graph outlining the sampling procedure. This is important, because the C variability is one of your major conclusions. //The two main suggestions of this reviewer focus on description of the soil sampling and discussion of the drivers of soil C stocks. To address these concerns: 1. We edited the methods section and added a supplemental figure, Figure S1. 2. Text discussion of soil moisture effects and reference can be found on lines 442-445

### C4

Minor comments: //L 22 What is snag? //A snag is standing dead or dying tree.

L 23- 24 rephrase //Done. Sentence now reads: "Thaw depth was negatively related to stand age, and soil C density (top 10 cm) was positively related to soil moisture and negatively related to moss and lichen cover."

L 45 – 50 How about thermokarst? //We changed 'microtopography' to 'topography', and one of the references following that is a thermokarst reference.

L 58 See also Vitharana et al. (2017) AGU:bgs //Thanks for suggesting. We added this reference to this manuscript and changed that sentence to read: " Furthermore, permafrost regions are characterized by high heterogeneity in soil C stocks due to variability in soil-forming factors (Vitharana et al., 2017) and at small spatial scales due to cryogenic processes (i.e., cryoturbation at the sub-meter scale)."

L 63 – 65 What do you mean by high resolution sampling and what does this have to do with circumpolar estimates? Also, Walter Anthony et al. (2014) is a paper on thermokarst lake deposits and C accumulation over the Holocene and has nothing to do with soil. //By high resolution we mean that spatial resolution of the sampling should match the spatial resolution of the variability. We edited the sentence, and we deleted the Walter Anthony reference.

L 70 Yedoma is a sedimentological Suite and not soil, or do you mean the soil developed on top of these Yedoma deposits? //We changed 'soil' to 'deposits'.

L 72 25m: clearly you cite a number that in Tarnocai et al. 1999 is cited as Zimov et al. 2006 ! then cite Zimov et al 2006, or find a more up-to-date number //If you are referring to the reference to Tarnocai 2009 on line 66 then we have made the suggested change. If that is not correct, then please clarify and we will make further changes as suggested.

Section 2.2 I am sorry, but this section is a bit confusing. Add reference to Fig 1. //Agree. First, we moved the stand age sampling into its own section and moved the

C5

stand age and density results into the results section. We cleaned up and clarified the rest of the text in this section. We also added in reference to Figure 1.

Did you sample random? If not, then please justify why not and how this could be a bias in your study. //We clarified section 2.2 to note that sites were selected based on biomass distribution; while plots within sites were established based on slope or N-S direction to avoid bias.

L 142 what is the logic behind this? Please explain. //I think there is some confusion regarding line numbering; please clarify so that we can address this comment. Lines 141-143 read: " Wood samples were dried at 60 °C and then sanded sequentially with finer grit sizes to obtain a smooth surface. Each sample was then scanned and the annual growth rings were counted using WinDendro (Regent Instruments, Inc., Ontario)."

L 145 – 147 Please rewrite and provide a figure that explains your sampling scheme. //We added a supplemental figure and edited the text.

Section 2.3 what is the motivation for this? //To explore effects of slope and solar insolation on soil C pools.

L 171 Did you correct your allometric functions for reduced C content in decomposed dead trees? (see for instance Smith et al 2003 GTR report:Forest volume-to-biomass models ...) //We did not for snags but did for downed dead trees (line 183-185). Dead standing larch had little observable decay.

L 193 Are these values also valid for Larch trees? //We used value for similar structured trees, following methods in previously published studies, as cited; ideally, if available, we would use for larch.

L 218 What soil How did you select the sampling location with regard to microtopography. Did you have hummocks in the soil? See also Ping (2013) Soil Horiz. //There were no hummocks at these locations. Soils were sampled at either end of each of the

C6

three transects (line 213-214; Figure S1) so they were distributed across each site at ~10m distance.

L 213 Please provide more precise constraining dates for the active layer thickness //We added dates to the text at line 204.

L 222 again, it is very unclear how you sampled this and how many samples and soil profiles go into one site. This is important to be clarified because an important part of your discussion and your conclusions are based on the variability of these values. What do you mean by 6 samples one at each end of a transect? //We edited the soil sampling and analysis section to clarify and added a Supplemental figure.

L 224 If you only collected the top 10 cm of mineral soil you have a bias towards C enriched upper soil. This can be problematic if you interpolated to deeper depths. If this is the case, please discuss this and outline potential impacts on your statistics. //We did not interpolate. At the 7 sites where we sampled frozen soils, we collected the full mineral soil profile (lines 217-218) as well as frozen soil. We only used these deeper samples for the deeper estimates.

L 239 Which guidelines did you follow for this? //I think there is some confusion regarding line numbering; please clarify so that we can address this comment. Lines 238-239 on the submitted manuscript read: "For the deep permafrost samples, sub-samples used for %C, %OM, and BD measurements were collected from adjacent depth increments"

L 297 and 301 Please use the same units for masses throughout the article. I suggest kg C m<sup>-2</sup> //All soil units have been changed to kg C m<sup>-2</sup>.

L 304 what could this variability be related to? //Please clarify the line number or specify the text that you are referencing.

L 319 Do you mean you started sampling at 0 cm from the top to 10 cm depth or the top 10 cm of the mineral soil? //We are referring to the top 10 cm of the ground surface,

C7

not the top of the mineral soil. We have clarified this in the text (lines 322-323) to read: "Soil C density in the top 10 cm of the ground surface (0-10 cm soil depth, which may have contained both organic and mineral soils)..."

L 352 I don't see this. Please clarify the line number or text this is referencing; it's not clear what changes are suggested to line 352. //Line 350-352 read: "In addition, our larch AGB estimates fell within the low range of larch stands across other high-latitude (> 64° N) regions and were generally 3-10 times lower than other stands (Kajimoto et al., 2010) "

L 406 Also have a look at Siewert et al (2015) AGU:bgs for a comparable study to yours. //Thank you for the suggestion. Reference has been incorporated at line 425.

L 407 What explains this high variation in your case? //Assuming this refers to line 394, I don't think we have enough samples/information to conduct this analysis, but much of the variation may have been driven by high and variable ice content.

L 420 Please mention that Yakutia spans over a large area with many ecosystem types. //We added text to note that the region comprises a diverse range of ecosystem types.

L 428 again, Yedoma is not a soil type //We have corrected throughout the text.

L 448 What do you mean by geophysical controls? //We changed 'geophysical' to 'parent material and climate'. These factors were not the focus of the discussion as the sites were located within a small catchment with similar parent material and climate.

L 459 Please also consider the notion that moist sites support more vegetation that is more productive and stores more C, rather than vegetation driven differences in moisture and thus C //We edited the text at lines 442-445 to address this comment and added a reference to Berner et al (2013).

L 471 related -to- stand age //Corrected.

Fig 2: Organic Layer stocks would also be interesting //Organic layer carbon stocks are

C8

provided in Figure 4.

Table 4 What do you mean by soil classification? Mineral or organic? Or soil type (Podsol, etc...) //We clarified to read "soil type (mineral/organic)"

Please use the same units through the paper! Here it is  $\text{g C m}^{-2}$  before it was  $\text{kg cm}^{-2}$  //All soils are now in units of  $\text{kg C m}^{-2}$ .

Why is the standard error the same for both columns of the permafrost cores? Are the permafrost cores also including the active layer? //The SEs were an error, which have now been corrected—thank you for catching this. The columns under 'thawed soil cores' are thawed active layer. The permafrost core data presented are C pools in the top 0-30cm of ground or C pools in the top 100 cm of ground. We edited the table description to clarify.

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Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-88/bg-2017-88-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-88>, 2017.

C9

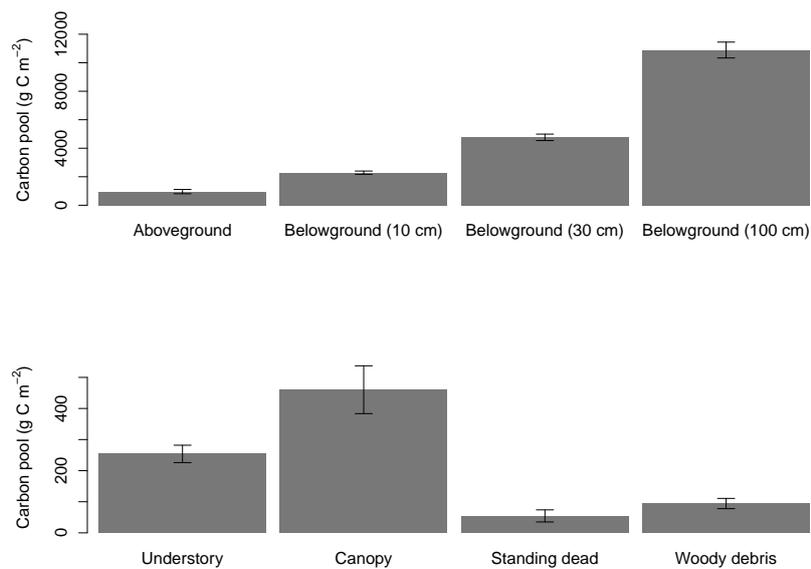


Fig. 1.

C10

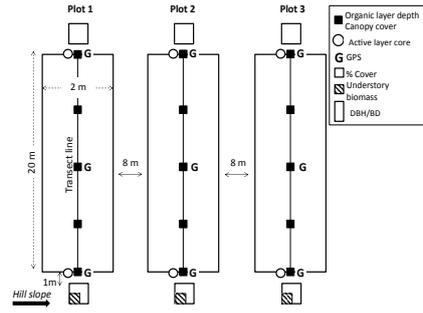


Figure S1. Sampling design for vegetation and soil sampling from 20 sites in the Y4 watershed. At the seven sites where surface permafrost was sampled, permafrost cores (to 1 m) were collected at three of the six active layer soil sampling locations. DBH: diameter at breast height; BD: basal diameter.

Fig. 2.

C11