

# ***Interactive comment on “Improved landscape partitioning and estimates of deep storage of soil organic carbon in the Zackenberg area (NE Greenland) using a geomorphological landform approach” by Juri Palmtag et al.***

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Anonymous Referee #2

We are grateful for the detailed and constructive comments by the reviewer which were helpful in improving the scientific quality and clarity of this manuscript.

Anonymous Referee #2

Comment: Page 1 L1 (title): Very long title, but the nitrogen is missing. What about

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“Deep storage of organic matter in Zackenberg, Greenland”. The methodical approach (“geomorphological landform approach”) is not needed here.

Response: We agree that the title was very long and used partly used the suggestion by the reviewer. But since the actual aim of this paper is the used approach, we think that landform partitioning should be still part of the title.

Comment: L13: Please add an introductory sentence before stating the paper aims. General comment: are all the included samples soils in the sense of soil science? If yes, SOC is an appropriate abbreviation, otherwise please change to OC. Please add to the abstract a short discussion sentence including “what is deeper than 3 m” (you did this in the paper text already for fan/delta) and “how representative” could this study area be for Greenland/or other Arctic areas (missing right now).

Response: As asked we added an introductory sentence: “Soils in the northern high latitudes are a key component in the global carbon cycle, with potential feedbacks on climate.” We also changed for the deeper deposits the abbreviation to OC and added which type of sediment it is “alluvial and deltaic deposits”. We also added in the discussion section rough estimations on SOC storage in deposits below 3m.

Comment: L30: Please add a reference here Response: Thank you for your comment. We have added additional reference of Schuur et al., 2015.

Comment: L31: Please concretize “most regions”. Permafrost regions, Arctic regions? Despite IPCC, please add a primary reference here.

Response: Thank you again for your comment. Primary reference added and the regions are now concretized with: “northern circumpolar regions”.

Comment: Page 2 L4: As you know first assessment was not Tarnocai et al 2009, there have been a lot more before, like Post et al 1982, Tarnocai 2003, than Zimov 2006.

Response: We agree and thankful for the comment. This part is now corrected with the proper references regarding the first assessment: “In 2009, Tarnocai et al. linked

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circumpolar SOC data (e.g. Kuhry et al., 2002; Zimov et al., 2006; Tarnocai et al, 2007; Ping et al, 2008) and presented a new total estimate of 1674 Petagram C (PgC) stored in soils and deep deposits of the northern circumpolar permafrost region.”.

Comment: L16: add a dot after 2016). . . Response: Typo corrected and dot added.

Comment: L29 and following: I like the concise presentation of the specific aims. Please be consistent and use this order and number for the conclusion sections

Response: The conclusion section is now restructured and the aims are following the same order.

Comment: L36: please delete the c. or introduce this abbreviation for the coordinates.  
Response: Agree that c is unnecessary and gladly removed from the text.

Comment: L38: Please add the rain/snow percentages of the precipitation (if available)  
Response: Asked information on rain/snow added to the site description.

Comment: Page 3 L15: Please add the GPS uncertainty here Response: Approximate GPS uncertainty added.

Comment: L19: Not the motor head, but the core barrel system is of relevance here. Please add this information here.

Response: As asked we added the additional information on the core barrel in the methods section.

Comment: L32: How many samples were measured for TOC/TN. TOC and TN in one run and same device? Or did you decarbonise the samples before to get rid of the TIC? Did you calibrate your 950 TIC measurements as well?

Response: Added and explained in more detail the questions raised by the reviewer #2 on the number of samples, device and sample preparation.

Comment: Page 4 Line 9: Please add the units to the formula, this would explain the

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percentage /100 and g to kg as well as the cm<sup>3</sup> to m<sup>2</sup> conversion factor

Response: The units are explained just above the equation which is a common way to do it. It seems very unnecessary to repeat it again.

Comment: L16: Please explain the mentioned “data limitations”

Response: The text part is now reworked explaining the data limitation for the lack of deeper TN values.

Comment: L22: please delete “time or” Response: Same comment as from reviewer 1, so we removed time from the text.

Comment: Page 5 L3: Is your data Gaussian distributed? Did you test this (e.g. Shapiro-Wilk test). Why not using more robust median and interquartile ranges? Please explain and justify your decisions here.

Response: Thank you for your comment. Yes, in most cases there are too few sites to do a proper test for the normal distribution. However, yes I did test this using Shapiro-Wilk for example in the alluvial fans based on 15 sites. Regarding the usage of standard deviation: We follow a standard protocol used by our group and others. For that reason and for comparability between sites we use SD.

Comment: L16: Same for CI, which requires Gaussian distributed data Response: See response above, where tested data is normal distributed.

General comments results section: I understand you n is the number of sites, but could you add a n for the number of measurements as well? Is the percentage a result of Cable 2017 or this study? I recommend transforming the major information of Table 2 into a boxplot figure (which is allowed for a n<5 for n (measurements, nit sites) and include table 2 into a supplementary chapter. Is the data available (embargoed, e.g. PANGAEA). Then you can add a doi of your data.

Response: As asked by the reviewer, we added the number of measurements in Table

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2. Question regarding the percentage landform cover. As stated in the text, we amalgamated small classes from Cable et al., where we didn't have any sampling pedons into larger landform classes. However we use the same aerial extent of the study area. Table 1 shows the geomorphological landforms and their proportion of total surface area in Cable et al., into the amalgamated classes. As stated in a former response, we would like to keep the table with all the data as it is which enables a direct comparison with other sites dealing with SOC storage. Also, the data is not yet available in e.g. PANGAEA but is planned in the future.

Comment: Page 7 L7: First estimated for Zackenberg or Greenland or similar landscapes? Response: Additional information added to clarify this point.

Comment: L24: please delete c., which is not coordinates for which this abbreviation was used before Response: c which was used for circa now removed from several places.

General comments discussion: Please include and discuss a number of how much is perennially frozen/seasonally unfrozen of you carbon and nitrogen stocks. Please include comparisons of your and other comparable case studies, which you all know because of author overlap (e.g. Fuchs et al. Biogeosciences, Discussions, Hugelius et al. 2009, etc.) To my opinion 5 references (4 actually, as Palmtag 2015 is mentioned twice) for a discussion are not enough. I do not care about the number of references, but this shows that the authors should discuss their findings a bit bigger context. Frozen/unfrozen percentages, what are the consequences for the carbon pool → modelling...any back on the envelope calculation of the <3m pool of the fans etc. Discuss the satellite product comparisons (Bartsch and other ESA DUE or GlobPermafrost-related publications). Rough discussion of carbon qualities (using C/N) and compare this to literature concepts (for C/N Schaedel et al 2014) Maybe an additional “discussion figure” (right now just 1 intro and 1 results figure) would be helpful.

Response: Thank you for these comments. Several more references were now added

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and their findings discussed and compared to our results. We also included comments on the perennially frozen proportion. However, there are too few sites at all available with comparable mountainous permafrost environment and to my knowledge none of those which deals with carbon stocks deeper than 1m of depth. We also included the publication from Bartsch et al. using SOC estimated based on synthetic aperture radar. But since the soil penetration is only a few cm, it has the similar problem as LCC and cannot capture the long-term depositional history. And we believe that a discussion of carbon qualities is beyond the scope of this paper.

Comment: Page 8 L18: Keep using introduced abbreviations (here CI) and do not introduce them twice. Response: Mistake corrected and the secondary introduction of abbreviation removed.

Comment: Page 9 L6: Making an rough estimation by stating the assumption of a <3m pool could be a nice first guess for future work Response: We added a rough estimation for SOC storage in deltaic deposits below 3m.

General comments for the conclusions: Please repeat the paper's aims here and answer this in the same order like in introduction. Please include your nitrogen calculations here as well. Response: The comment implemented and the aims reordered following the introductions order. Also, the nitrogen was added in this section.

Comment: L9: "new additional" sounds strange Response: The "additional" is now removed from the text.

Comment: Figures and Table: Figure 1: hard to read the site labels. Maybe a hillshade and transparent colours could improve the geomorphological understanding for the reader. According to figure 2: use A and B instead of "top right" Response: Both comments implemented.

Comment: Figure 2: A is redundant with Table 1, right? An option could be deleting a and put the spotlight on B-C.

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Response: Figure 2 A is redundant with Table 1 but we would still like to keep it as it, since it illustrates graphically the proportional contribution of each landform in comparison to the SOC storage for each landform.

Comment: Table 2: please add n measurements to the table. Moreover switching from CI (in the manuscript text) to SD could be puzzling for the readers.

Response: As suggested we added in the table 2 the number of samples measured for each landform. Regarding the use of SD and CI: In the manuscript text and Table 2 it is clearly stated if we used SD (only for the landform means) or CI which was used for the whole Zackenberg area. The decision to use CI is because it account for the relative spatial coverage, storage variability and degree of replication of each upscaling class and therefore much more confident.

Please also note the supplement to this comment:

<https://www.the-cryosphere-discuss.net/tc-2017-255/tc-2017-255-AC2-supplement.pdf>

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-255>, 2017.

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