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Interactive comment on "From fibrous plant residues to mineral-associated organic carbon – the fate of organic matter in Arctic permafrost soils" *by* Isabel Prater et al.

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Dear master students,

Thank you for your helpful comments and for the effort you put into your review. And congratulations that you had the chance to prepare reviews as exercise during a seminar – that is a really useful training. We appreciate your comments; please find our answers below in italics:

- The research question addressed by the authors is important due to the lack of knowledge on the topic. Indeed, researchers only recently started to understand the impor-

tance of cold soils for the global carbon cycle, and thus global climate. As a consequence, only a few studies related to this topic have been made so far.

We are happy that you share our view on the current rather sparse knowledge on the topic of our research.

- The authors did not explicitly state any hypotheses. They described their intent of investigating the effect of climate change on the carbon stabilization in permafrost-affected soils, but they remained vague and did not state any kind of expected results. Therefore, it is difficult to understand to what extent the research contributed to their question.

We are happy that you raised this issue, we now better clarified our objectives and added our expectations in the final part of the introduction.

– The study site is situated in the river delta of the River Lena. Chemical composition and structure of the soil could be the result of flooding which is not the case for typical arctic permafrost soils. In general, the isle may be more affected by the Lena itself than by the rising temperature. In addition, the closeness of the Siberian sea will have an influence of the isle too, as the ocean moderates the temperatures. Therefore, the study site on the isle Samoylov maybe not representative for arctic permafrost soils in general.

As described in section "2.1 Site characteristics and soil sampling", we took the samples from the Holocene river terrace that is rarely flooded. To avoid the impact of regular flooding, we did not take samples from the active floodplain. Your remark regarding the influence of the river and the Laptev Sea is very important. We address the differences between the climate on the island and on the mainland in our site description. However, the studied Cryosol types can be found throughout the Arctic and thus we assume the demonstrated properties and suggested processes can be applied in a general sense.

- Do you think is the d15N a suitable method? There are many uncertainties related to

it, which could be elaborated upon.

The use of d15N as an integrator of the N cycle is a semi-quantitative method, which does not allow to derive quantitative process information. Thus, we agree that this method requires a careful discussion of the various processes/fractionation factors and isotopic signatures of N sources that jointly determine d15N, which is precisely what we do in this manuscript. Taking this into account, it is a very powerful tool to fingerprint dominating N cycle processes and general N cycle patterns (e.g., open N cycle, closed N cycle) without the need for disturbance by e.g., adding enriched isotope tracers. In this context, it is also important to relate d15N to other measured parameters to support its interpretation, which is what we do in this study as well.

Specific comments and suggestions: L. 75-78: Here the authors write about their approach and the aims, which are basically to gain better knowledge on the topic. Since this section is at the end of the introduction, we think that this part is the most suited for adding the research questions and hypotheses. We think this is important, especially because the authors took four soil cores in a vast area that might be highly heterogeneous. Therefore, having expectations related to the SOM fractions you expect to find in this area, including also the stratification of the soil layers could help determining how representative the four soil cores are with respect to the whole study area.

We agree that hypotheses are one possible way to communicate expectations at the start of a study. We have added a sentence stating what we generally expected to find without naming this explicitly as hypothesis. The cores were taken from a Holocene river terrace and were cryoturbated. We are giving the detailed information on the soil cores in table S1 in the supplement. As we sampled in depth layers, we have no particular information about the stratification, however, there were no indications of flooding events that might have resulted in stratigraphic shifts.

L. 94-97: In this part the methods are described. However, the authors then state that "a detailed description of the study area and the sampling of the soil cores can be found

in Zubrzycki et al. (2013)".We advise that the authors include all relevant information also in the presented manuscript. Otherwise, the readers have to go into the literature to find this relevant information.

Thank you for this remark; we added more information in the text. As we did not want to overload this section, we focused on the information that is relevant for our study. For readers who want to have more information, we added the respective references.

L. 101-105: Here, the authors describe how samples were collected, but omitted to state how many samples were collected for each SOM fraction and from which soil core they were collected. We advise to provide the number of samples of each SOM fractionation type, because otherwise it might be difficult to interpret the graphs. We also checked the literature but found any information about the number of samples in Zubrzycki et al., 2013.

Thank you for this very important remark. A detailed list of the samples and their properties can be found in table S1 and we added this information (l. 113). In addition, we have to excuse that we had a mistake in the reference (it said table 1 instead of table S1 before). We also added the number of selected layers in the same line.

Fig. 4: The three graphics (figure 4. a, b, c) could be made more similar. Further, for what concerns figure a and b, the authors represented only the two extreme values C3on the x-axis (10 and 100), which makes it difficult to infer the values of the dots in the middle of the graph. Please include more labels on the x-axis to make it more continuous and improve readability. We would also advice to put the x-axis on the bottom for both graphs (a and b) and not once on the top and once on the bottom. Further, we noticed a clear positive correlation between the C/N ratio and the d13C, however, since the C/N ratio usually decreases during ongoing decomposition, we were expecting the opposite trend. We therefore advise to further explain the meaning of this positive correlation.

Thank you for this hint. We reworked all of our figures and made them easier to read

and took care of a better quality. We also implemented a color scheme for all figures, changed the values on the x-axis and put all x-axes to the bottom. Regarding d13C and C/N ratio, we rewrote the respective section (4.3) to better emphasize the differences between the SOM fractions.

Fig.7 & 8: Graphs 7 and 8 are difficult to interpret and would require more information in the captions to make the graphs understandable without the reader having to look up more information in the main text.

We reworked both figures and think they are easier to read and understand now.

Title: Why was the word “fibrous” included in the title? Almost all plants residues are fibrous, except for plant exudates. Do you specifically looked at fibrous plant residues omitting exudates? Further, the fate of organic matter sounds somewhat dramatic. We think the title could be shortened to, for example: “From plant residues to mineral-associated organic carbon in Arctic permafrost soils”.

We use "fibrous" because we see a clear difference between the large, fibrous POM fractions and the oPOMs fraction that is not fibrous anymore – we describe this at length, but the difference becomes especially obvious in figure 9. We want to emphasize the specific macroscopic nature of the cryoturbated materials referring to the occurrence that one is experiencing when sampling. And the fibrous litter residues foster the bulky soil structure that drives the specific oPOMs formation, which is clearly different from less organic temperate soils. We want to use the word “fate” as it summarizes a development from initial litter residues to more transformed soil compartments, which we think best describes the partitioning of compounds to various processes or pools/compounds in biogeochemistry.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2020-52/bg-2020-52-AC3-supplement.pdf