

Interactive comment on “Storage and transformation of organic matter fractions in cryoturbated permafrost soils across the Siberian Arctic” by N. Gentsch et al.

Anonymous Referee #3

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The paper reports a huge dataset of an indepth study of Siberian permafrost soils. These soils were sampled along a longitudinal gradient down 30–40 cm depth into the permafrost horizon. The samples were subjected to physical fractionation and chemical analyses. The data were exploited by statistical analyses. As a result the authors conclude about the importance of cryogenic processes for SOM accumulation in subsoils. They emphasise the importance of mineral associated C, which however, seems to contain a significant proportion of more labile C in terms of chemical accessibility. Elemental and isotopic data seem to suggest the occurrence of microbial alterations despite unfavourable conditions. From the statistical analyses the authors further conclude about the mechanisms of OC accumulation in the mineralassociated fraction.

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The study was carried out carefully and the data advance our understanding about soils of the arctic region, however, the presentation of the data is somewhat incomplete:

No basic parameters of the soils were reported. For exemple it is only within the text that the reader learns about the high pH values of these soils, which are likely influencing all the chemical and even biological transformations occurring. I think that this may be a problem of data allocation to different manuscripts, as another one is submitted right now. However, such basic information needs to be included in the manuscript for the different soils/horizons sampled along with their elemental content and some information on mineral parameters, i.e. different extraction results.

The manuscript would have benefitted from the inclusion of the ^{14}C data, to give at least some indication on microbial activity/decomposability. As such, the manuscript provides a lot of detailed information of SOM association to different fractions, which in the end does not lead to any additional progress. Also, it is not evident from the data presentation, why a longitudinal gradient was sampled? Is there any added value from the analyses of these well chosen samples or is it just nine separate sites? Can any longitudinal trends be indicated?

I have a problem with the sample preparation procedure. The samples were airdried. However, the authors mention in the text in some place the importance of changing redox conditions for the processes operating in these soils. What is the potential impact of airdrying on the results obtained?

Moreover, I found curious that you used $(\text{Fp}+\text{Alp})/(\text{Fed}+\text{Ald})$ as an indication of complexed OM. In my opinion the determination of C in the pyrophosphite extract would give a much better proxy.

As the pH values of the soils were apparently very high, I wondered why the occurrence of inorganic carbon in these soils was never discussed.

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Heavy and light fractions are compared in terms of C/N. However, this may be not advisable, considering that the heavy fraction may always contain a substantial amount of inorganic N.

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