Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-89-RC1, 2019
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Biogeosciences Discussions

Interactive comment on "Lability classification of soil organic matter in the northern permafrost region" *by* Peter Kuhry et al.

Anonymous Referee #1

Received and published: 7 June 2019

The manuscript 'Lability classification of soil organic matter in the northern permafrost region' by Kuhry and co-workers describes results from two large incubation experiments with soils from the circum-arctic region. The authors grouped their samples according to landscape units and further properties such as soil depth or carbon content and subsequently compared the CO2 production between these groups. The manuscript is concerned with the very relevant question on organic carbon decomposition in northern, permafrost affected soils. It contributes to our understanding on how fast permafrost organic matter may be decomposed to the greenhouse gas CO2. Furthermore, the authors present a novel concept by relating CO2 production to different landscape units and soil classes that are represented in circum-arctic databases. The author's findings are novel and the conclusions drawn largely supported by the

presented data. The manuscript is well written and easy to read, however, there are some points that should be addressed before publication.

First of all, the authors compare data from two very different incubation experiments. While the PAGE21 experiment presents CO2 production rates after almost one incubation year, the CryoCarb experiment(s) lasted only for four days. The difference in incubation time means that the different experiments give information about very different carbon pools. The authors are certainly aware that conceptual models divide soil organic matter into different pools characterized by different decomposition rate constants. This concept has also been applied to a wide variety of permafrost soils grouped into different classes, similar as in the current paper (Schädel et al., Circumpolar assessment of permafrost C quality and its vulnerability over time using long-term incubation data. Global Change Biology 2014; 20: 641-652). Latter paper demonstrates that all different kinds of soil (organic, surface mineral or deep mineral) the most rapidly degradable carbon pool represents only a very small fraction (< 5% of initial C). Hence, the CryoCarb experiment might give information about this small pool of rapidly cycling carbon in these soils but not on the overall 'available' pool. In contrast the PAGE21 experiment will likely give information on the much larger 'slow' carbon pool, which substantially differ in size between the different classes defined by Schädel et al.. The meaning of the data from the two incubation experiments for the different carbon pools should be clearly explained, e.g. in the introductin. The large dataset form the Schädel et al. paper in part supports the findings of the current manuscript (organic matter in deeper permafrost deposits is less degradable than surface organic matter) and in part contradict them (Schädel et al. find high decomposability of peat, the current paper not) and the results from this previous paper should be discussed as well.

Furthermore, the PAGE21 incubation follows a widely used approach, which makes the obtained data comparable to those from previous studies. In contrast, the CryoCarb experiment uses a novel approach, which appears to have some critical issues. First of all

the authors used samples dried at up to 50°C, which will most likely substantially harm if not kill the microbial community in these samples from very cold soils. The peak of CO2 production after rewetting and inoculation of these samples with viable organisms likely results from the rapidly degradable organic matter from (lysed) microorganisms. Also the study cited by the authors (Fierer and Schimel, 2003) concludes that this CO2 pulse does not come from the soil organic matter but from microbial carbon. In this case, what do the data of the very short CryoCarb incubation experiment mean for the decomposition of soil organic matter? Furthermore the authors should give some more information on the setup of the CryoCarb experiment and the preparation of the samples. If the samples were stored without freezing for up to two weeks before drying, a substantial fraction of the active carbon pool will have been decomposed before drying. Is it possible to account for this bias? And which inoculum was used for the different incubations? Were the samples incubated with inoculum from the same sampling site? And how was accounted for the carbon introduced by the inoculum? Was there a control incubation only with inoculum? Please explain this experiment in more detail and please also address the limitations of the CryoCarb experiment.

The different experiments present data from different sampling areas that were grouped into different classes. However, it remains unclear from which area samples are grouped into which class. It appears e.g. that all peat samples in the PAGE21 experiment originate from Storedalen Mire. To evaluate the significance of the data, it is important to know where the samples of the different classes come from and if they are representative for the whole landscape class they stand for. Therefore, I suggest presenting a table in the supplementary information with the origin of the samples grouped into the different landscape and soil classes.

The two experiments used a very different number of samples, and also the number of samples grouped in the different landscape and soil classes seem to be very different. I suggest presenting the statistical information (R2, p, n) including the number of data used for the presented regression analysis side by side, e.g. in the respective figures.

Furthermore, some of the datasets are fitted with a linear regression some with an exponential, power or polynomal regression but the reason for these different regression types are not explained. Some of the results are not reasonable even if the regression is statistically significant and the authors should give a comprehensible justification for the model they selected for fitting their data. Why should a linear increase of the C concentration result in an exponential increase of the CO2 production?

One strength of the manuscript is that it relates CO2 production to different landscape classes and soil materials. However, more and more classes and sub-classes are introduced for the two experiments and it gets more and more difficult to follow. I suggest that the authors critically review if all the different classes, sub-classes and groups of classes are required to come to the main conclusion of their manuscript.

One of the surprising results of this manuscript is that the decomposability of peat organic matter seems lower than that of organic matter in mineral soils, which partly contradicts previous findings. It is also surprising that the variability of CO2 production from peat organic matter is very low (Fig. 6). I suggest giving this result more attention in the discussion. Do the samples represent peat from drained peat plateaus that are exposed to long-time of aerobic decomposition? How representative are the data for the Histosol/Histel class in the Northern Circumpolar Soil Carbon Database? What might be the reason for the low decomposability? Does this peat only represent ombrotrophic bogs or also minerotrophic fens?

The discussion is mainly considering previous studies of the authors working at the sites studied in the current manuscript. However, there is a wealth of recently published data from aerobic incubation studies considering a wide range of circum-arctic soils. To put the data of the current manuscript in a wider perspective I suggest stronger considering data from previous incubation studies from other sites, which might support or contradict the findings of the current manuscript.

The results section contains a substantial amount of discussion and also some descrip-

tion of methods, and I suggest to move this text to the respective section (see specific comments).

The line graphs (Fig. 2-Fig. 5) may be improved by using clearly different symbols and colors, and by inserting a legend to each of the panels.

specific comments:

L75: Should read 'permafrost zone' instead of 'thawing permafrost'. The estimate describe C fluxes from soils of the permafrost zone not of the thawing permafrost alone.

L 90: Schädel et al., 2014

L150ff: Please also specify how many replicates were used and how CO2 production rates were measured.

L153: Please specify from which study areas the samples were collected.

L 157: Please specify from which sampling site the peat samples were collected.

L 168f: If only the rate after 363 days is considered in this manuscript it is not necessary to mention how often rates were measured (or cite the respective study of Faucherre, JGR, 2018; doi:10.1002/2017JG004069).

L197: I suggest not using 'available' in this context. The CryoCarb experiment give information on the very fast cycling C-pool, which is much smaller than the carbon pool that is available for microbial decomposition (see general comments).

L 205ff: This paragraph is mainly an explanation why the authors used %C as the main explanatory parameter for the measured CO2 production rates. I suggest shortening this paragraph in the M&M section and shift the main part into the discussion, if necessary.

L285: Schädel et al., 2014

L282-291. A large part of this paragraph contains discussion and should be moved to

the respective section.

L 354f: Does this mean that CO2 production rates were similar in samples from peat deposits and mineral soils if C/N ratios were below 20? Please rephrase.

L357ff: please move to discussion.

L368 – 378: This paragraph rather describes methods and should go to the respective section, e.g. at the end of 2.5.

L378: Please explain to which subclasses you refer.

L420ff: Please explain what is meant by 'C-enriched' or 'organically-enriched'. To my understanding, every soil horizon that contains organic carbon is 'C-enriched'. How do the authors differentiate between 'C-enriched' and not 'C-enriched'?

L536ff: A large part of this paragraph belongs to the discussion, please move to the respective section.

Fig.2: I suggest omitting Fig. 2 since it gives the same data as Fig 3 including the fit of the total dataset.

Fig. 3: The different greens are difficult to differentiate. Please also use clearly different symbols.

Fig. 4 and Fig. 5: Please add legends to the figure and please use clearly different colors and symbols.

Fig. 6: Significant differences between the groups should be better indicated here and Table 4 could than go to the supplementary material

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