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

**Anonymous Referee #2**

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The authors report on the analysis of the organic carbon mineralization of soil and Yedoma material from numerous sites in the Northern hemisphere making use of two different incubation experiments. The authors cluster the samples into different source material (eolian, alluvial) and ecosystem / soil types (peatland, Turbels), aiming to provide estimates for the bioavailability of SOC in different Arctic terrestrial OC pools. As there is only scarce knowledge on the vulnerability of the tremendous OC pools in the Arctic, the overall objective of the manuscript to come up with such estimates is of great interest especially to refine carbon modelling. Using a large data set of OC mineralization rates/data is a very straightforward approach to obtain estimates for the potential bioavailability of OC. However, the manuscript appears a bit like the attempt of a group of authors to get a manuscript out of existing data sets using correlations



of the least common multiples which are stated to be %C, C/N and bulk density. The explanation why a single day mineralization rate at the end of a long-term incubation, and a short-term incubation go together is questionable. After the rewetting of dried material it is known that the first flush of CO2 within the first days is mainly derived from OC additionally available due to the physical impacts of the drying (disintegration of SOM, lysed microorganisms) especially as it was done at higher temperature. Given the highly seasonal DOC content in permafrost affected soils (the material presumably mainly driving the CO2 evolution), this short term incubation is also more like a snapshot in time. The authors should explain much better why they use these two incubations, and what oven dried inoculated vs. fresh material can tell us about the bioavailability of soil organic matter under natural conditions. Furthermore, it would be interesting if the authors give the cumulative OC over the full period of the long term incubation. Besides these technical aspects, the manuscript appears very descriptive. There is a number of studies on the distribution and composition of OC in permafrost affected soils that demonstrate possible OM vulnerability to increased microbial decay. It would be interesting to discuss the data in more detail especially in view of the composition of the OM, even it would just be C/N ratios as given by the authors. line 195-201 - The drying-rewetting of this approach lead to an increased respiration due to lysed cells, physical breakdown of soil material etc. Thus it may serve as a proxy for potential amount of 'artificially' labile OC, but does not reflect the natural amount of labile OC. Detailed comments: line 309-310 - Something to be expected, the more substrate the higher the respiration. But it neglects all other factors driving C-release, like pH etc. line 317-319 - If I got your M&M section right, you measured the long term incubation samples at one point in time after almost a year. Of course its much lower, the short term got a higher CO2 due to rewetting effects plus the flush in mostly labile OM, and the long run incubation represents more stable OM mioties. How is the cummulative OC release in the long term experiment, and thus the overall OC release? line 572-573 - This is normal, you have in most soils systems not matter if arctic, temperate or tropic an exponential decay of the respiration rates. For the long

term incubation the total amount of released C would be interesting. line 575-577 - Didn't you state before that it is not possible to compare the mineralization rates due to the different sampling, sample treatment and incubation? line 578-579 - How did you come up with this assumption? What makes the data robust? line 581-582 - You may be able to relate the studied soil samples to larger scale OC inventories, but how do the lab incubations relate to the natural systems with differing pH, active layer depth, soil humidity etc.? line 619-629 - How does this deep OC rather stable OM relate to C/N ratios? line 632 - Please use a other word than "restistance", SOM does not "actively" resist decomposition/mineralization. line 632-633 - There is already some work trying to elucidate the underlying mechanisms on SOM stabilization in permafrost affected soils (e.g. Gentsch et al. EJSS 2015; Mueller et al. GCB 2015). line 633-643 - Besides a solely microbial driven decomposition, there are also some more soil physical and chemical constraints to SOM mineralization (see comment above). line 637-643 - Peat decomposition is dominated by the water regime. Drained peatlands can loose substantial amounts of OC on very short timescales. Thus, this only explains retarted decomposition in intact peatlands, not so much in other peat-like soil materials. line 644-657 - In natural systems such short term flushes are known to happen very often (freeze-thaw; drying-rewetting), thus for the labile OC the short term incubations gives for one moment in time (sampling date) a good insight. For a more solid OM material proxy the long term incubation is still of some use, but it would be nice to get either the overall OC and not just a rate at day x, or k-values for the long term decay curves. line 661-663 - This holds true for most soils, amount of substrate means low DBD, this linked to N availability determines OC mineralization. I would have wondered if its different in colder soils. line 664-667 - What about other proxies like pH? line 668-669 - Do you have other soil parameters that could be used to fine tune the multiple regressions?

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