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Discussions

Interactive comment on "Vulnerability of soil organic matter of anthropogenically disturbed organic soils" *by* Annelie Säurich et al.

Anonymous Referee #2

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Review of MS BG-2017-127 by Saurich et al.

The authors report on the CO_2 release from incubated soil samples rich in organic carbon that stem from organic/peat soils or mineral soils rich in carbon. The results are related to possible, mostly chemical predictors to evaluate what drives decomposition of SOC in these soils. The topic is relevant and timely and I started reading with keen interest. However, the study has substantial methodological shortcomings which make it impossible to answer the aims listed at the end of the introduction. These are:

Soil samples were dried at 40 °C and then rewetted. This is a real shortcoming of the study for many reasons – destruction of part of the microbial biomass, shrinking and formation of hydrophobic surfaces in organic soils, stimulation of decomposition through rewetting in soils with smaller SOC contents etc. A sample from a Histosol

(samples from intact peatlands and other high SOM-soils are explicitly included in the study), dried at that temperature, can become a brick stone. It is unclear, how this influences the CO_2 release in different soils, particularly because wettability changes. Hence, how strongly drying effects SOC decomposability after rewetting depends on the sample's organic matter content! This is, in a negative sense, a good example that a uniform sample treatment does not guarantee the magnitude and direction of the analytical error to be the same. Further, how were intact, fibric peat samples passed through a 2 mm mesh?

The chosen incubation time is very short (40 – 90 hours). The authors speak about 'relatively constant basal respiration', but usually respiration rates decline exponentially over time (see also Fig. A1). What is measured during those 2-4 days is mostly the decomposition of fresh plant residues, which are abundant in the samples at different amounts. These amounts depend on sampling depth, land-use, and time of sampling, the latter also with reference to management activities (e.g., time passed since last cut of grass, position in the crop rotation etc.). It does not tell much about the decomposability of bulk SOC but this is, what the authors were interested in. For this purpose, incubations of many months are necessary in order to get a substantial contribution in CO_2 also from carbon not bound to fresh plant residues.

These factors might explain, why no correlations between soil properties and soil basal respiration could be identified and why "a decreasing trend of specific basal respiration with higher SOC was identified". To my opinion the latter finding clearly indicates, that high-C-samples suffered the most from drying, in accordance to the expected change in physical properties discussed above.

I am sorry, but these points are too important to let this manuscript pass. I therefore do not list further comments and suggestions.

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