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Biogeosciences Discussions

Interactive comment on "Peat decomposability in managed organic soils in relation to land-use, organic matter composition and temperature" *by* Cédric Bader et al.

Anonymous Referee #2

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This manuscript describes the decomposability of organic soils (previously peat) from 21 sites in Switzerland that are now managed as cropland, grassland, and forest. The study looks at CO2 emission rates from laboratory incubations and possible correlations to various soil characteristics and temperature. In general, the study is well performed and provides a lot of details, however I have same major comments:

1) I find the term peat and peat decomposability quite misleading. While it might have been peat at some point it is not peat anymore and drainage must have occurred a long time ago (according to Table 1 sometimes 150 years ago). The authors do not address the history of the peat in the sampled locations adequately or give the reader

the proper background in regard to changes from peat to grassland, cropland, and forest. Table 1 has some information on the drainage history of the sites but it is not mentioned anywhere in the text. The introductory part about peatlands becoming cropland, grassland, or forest (p. 2) is too general and does not specifically address the sites. I also find it confusing how peatland and organic soil is used interchangeable (so it seems to me) in the manuscript, not every organic soil is a peatland. I think referring to organic soils (and it needs to be clearly defined at the beginning of the introduction what organic soils are, which is not there right now) throughout the text would be more appropriate.

2) The statistical analyses are not well enough explained and from what I understand not the appropriate analysis is performed. Why not perform a full linear mixed-effects model that includes all soil characteristics as fixed effects (land-use type, pH, bulk density, C/N etc.) in the same model while including depth and sampling location as random variables? Then, the model could be reduced step by step and each sub-model gets compared to the full model and by using the smallest AIC as the model selection criterion, it will be possible to identify the variable that has the strongest influence on CO2 release. Of course the variables included need to be tested for collinearity (e.g. total carbon and C/N most likely correlate and only one variable can be included). Given the lack of detail for the statistical analysis I could not make much sense of all the tables but in general, I find it very commendable if so much detail is provided in tables.

3) Overall, I am missing a story line and focus that brings the message across in an easily understandable way. The result section reads like a listing of findings and there is no result that gets highlighted or seems particularly memorable. I am also missing a link to the global scale, which I was expecting since the authors start out the introduction with the importance of organic soils globally.

Smaller comments:

4) What type of cropland is represented in this study? It never says which crop it is and I wonder if wheat compared to corn or other crops might be different

5) I do not understand the usefulness for Figure 4

6) P. 5, l. 158, I assume thoroughly mixed means homogenized? If so, bulk density as a variable loses its meaning completely

7) Fig. 1. The y-axis for all panels is depth (cm) but it is not written anywhere

8) Fig. 1, the symbols are so small, I think using larger symbols and different shades of black and white would really help the readability of this graph

9) Fig. 3, I think it would be much more useful to keep the same order for each sampling location for the upper and deeper soil layer, maybe keep the left panel (upper soil layer) as is and only adjust the right panel.

10) What about CH4 from any of these sites? If hydrology matters as much as the authors write then I would expect to read something about CH4 release

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