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

Cédric Bader et al.

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Answer to the reviewer comments on the article "Peat decomposability in managed organic soils in relation to land-use, organic matter composition and temperature" by Cédric Bader et al.

We thank the two referees for their profound study of our manuscript, their helpful suggestions and their positive perception of our work. We give here information on how we plan to revise our article and to proceed. R (referee comment), A (author reply)

Referee #1

Major comments:

R: In materials and methods, more information of the sites, sampling design and samples treat-ments is needed. The requested new information is listed in the minor comments.

A: Referee #1 suggests that the size of each peatland is added to Table 1. This information will be included upon revision. Further, the referee states that there is no information on water table or drainage depths. Unfortunately we do not have this information. Yet, we know that the extensively managed sites are drained only at the surface. This information will be added to Table 1. We will further add a supplementary table which describes the soil profile down to a "permanent" mineral layer.

R: Site codes must be uniform throughout the manuscript!

A: We thank the reviewer for careful reading and will change our manuscript accordingly.

R: Should add more literature about the organic matter properties and how it regulates/does not regulate decomposition processes.

A: We will add some more literature on this topic.

R: This study focuses mainly on the effects of drainage on decomposition and SOM characteristics, yet the differences in management (i.e. machinery tilling, fertilization) between sites will likely affect peat decomposability and decomposition as well. Now these management related differences are mentioned first time in the discussion section, but they should be mentioned already at the site description in the materials and methods. Especially, the current vegetation type (forest, grass, crops) undoubtedly has influence on the current peat properties due to differences in litter quality and quantity. Additionally, in cropland the fertilization will affect to nutrient availability, and thus likely influences on decomposition. At least the variation in litter input should be discussed

more detailed.

A: We will include a description of management practices in the materials and methods section. Regarding the effect of vegetation type on peat composition we already argue, that the litter inputs likely have caused a different chemical composition of organic matter in forest as compared to agricultural topsoil (H/C and O/C ratios). In our discussion of these effects we however stress, that they are no major driver for peat decomposability. Yet, we do not know the exact fractions of litter derived vs. peat derived SOM. Therefore, we find a more detailed discussion on possible effects of litter input quality on SOM decomposability speculative.

Requested minor changes:

R: Referee #1 calls for a number of minor changes and corrections that are not listed here separately.

A: We concur with almost all minor comments and will be happy to improve our manuscript accordingly, also by including missing information. One minor remark refers to our decision to omit negative CO_2 values (0.45 % of all measurements). We believe that this data treatment is plausible. The omitted values were mostly strongly negative and occurred as single events during very short time spans, suggesting that they represent electronic artifacts rather than being product of a biological process.

Referee #2

Major comments:

R: 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 sam-pled 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.

A: All of our soils are Histosols according to WRB, and organic matter accumulating in histic horizons is termed peat. This also holds true when the peatland is drained, i.e., degrading. In an earlier study, conducted at one of our sites (Bader et al. 2017, cited in MS), we showed that a major fraction of SOM still originates from old peat. Therefore, we find the terms peat and peat decomposability appropriate. We however agree with the reviewer that using the terms peatland and organic soils interchangeably can be misleading. The sites used for this study are, from an ecological viewpoint, no intact peatlands as they do not accumulate new peat anymore. Therefore we will consistently move to the term organic soils. While we can provide some information whether the peat was derived from a bog or a fen, being more specific on the land-use and drainage history as currently given in Table 1 is very difficult. We will however, also in response to the first reviewer, provide an estimate on the time sites are managed with the current land-use.

R: 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 submodel 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 CO_2 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.

A: We thank referee #2 for these suggestions. We will perform a full linear mixed-effects model using fixed effects such as SOC, nitrogen and oxygen content as well as the bulk density. Further we will implement the land-use type as fixed effect. We will use the data on the single elements rather than the ratios in order to avoid collinearity. We will not use pH values because we do have only pH measurements for the different profiles but not for each single sample used for incubation.

R: 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 high-lighted 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.

A: What we do highlight, and this will be stressed even more upon revision, is that none of the parameters we measured was able to explain a substantial proportion of the variability in CO_2 release rates. We also write in the abstract: 'This, in turn, indicates a relative accumulation of recalcitrant peat in topsoils. Hence, our data suggest that after exposure of subsoil peat in the future, carbon loss from agriculturally managed organic soils will be similar considering warmer climate conditions.' We carefully considered whether our data allow such a conclusion and believe, this is a fair range of interpretation. These sentences do not support the referee's viewpoint that no result seems particularly memorable. We, of course, introduce to the topic by starting at the global scale at which the relevance of peatlands and organic soils is well acknowledged. This gives the motivation for our study. According to our knowledge, there has not been a previous work that included such a wide array of samples in the analysis of peat decomposability and its drivers.

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Requested minor changes:

We thank the referee for his/her careful reading and will address all minor points when revising our MS. One point to be highlighted at this stage is the referee's question on the usefulness of our Figure 4. That figure, which includes comparison to CO_2 release rates from other studies including both, mineral and organic soils, is, from our viewpoint, particularly relevant. It puts our results into a broader context. We write: 'Therefore, the pools size of labile carbon, indicated by the decomposition rates, seem not to differ between these soil classes. This comparison suggests that accumulation of recent, labile plant materials that presumably account for most of the evolved CO_2 is not systematically different between mineral and organic soils.' The comparison of organic matter decomposability in mineral and organic soils is highly relevant because, at present, the discussion on mineral soil carbon sequestration is tightly motivated by various so-called stabilization mechanisms. Our data indicate that OM decomposability seems not to differ systematically between these major soil groups and will hopefully inspire future discussion on the mechanisms of action.