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Biogeosciences Discussions

Interactive comment on "Effect of peat quality on microbial greenhouse gas formation in an acidic fen" *by* M. Reiche et al.

Anonymous Referee #2

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Review of BGD paper "Effect of peat quality on microbial greenhouse gas formation in an acidic fen" by Reiche et al.

The authors measured CO2 and CH4 formation rates in the lab and correlated them to thermal stability indicators and results from pyrolysis. Overall, the paper is clearly structured and the statement of objectives is understandable. Development of a thermal stability index is, however, the reinvention of the wheel and the authors do not seem aware of the literature. In some instances the text needs clarification and structuring.

The functional relationship between thermal and biochemical stability is, from my point of view, questionable as it lacks a mechanistic understanding though often empirical relationships are found. Thermal indices of OM stability and their meaning have been most recently discussed in a review paper (Plante et al. 2009, Geoderma 153: 1-10)

and I suggest having a look into it, particularly in their Table 2.

Detailed comments The quality index is already mentioned in the abstract without explanation and should either be removed from it or introduced here. The relationship between thermal stability and chemical composition is not clear; what are the substances that evolve first in the TG?

In line 25, p. 8776, authors suggest that the new peat quality index should aide the estimation of GHG formation potentials. This is something I miss in the study: Application of the index to a range of samples from the same site to resolve the issue of spatial heterogeneity. It is, in addition, quite ambitious to generalize the index as it has been developed for one single site only.

P. 8778 l. 1-5. A more elaborated statement on whether peatlands are CO2 sources or sinks would be helpful.

P. 8778 l. 10. One reason for not having a general accepted definition of OM quality is that quality can only be expressed in conjunction with functioning. For the current paper, authors may more clearly state that their quality is related to biological functioning.

L. 12. I find that sentence misleading. In the cited study by Rubino et al. (2007), the authors discuss that 'On this basis, we do not advise the use of Tg to produce an integrated index of litter quality, at least until the correspondence between thermal and microbial degradation is better understood ...'.

P. 8780, 2.2. Incubation was done with field-moist samples and the water content varied between samples. Without estimation of water content effects on decomposition rates, correlation of the latter to other influencing factors such as quality may be biased.

L. 18. Heating rates are usually given per minute. What is the reason to first pyrolyse and then to combust? Needs explanation.

P. 8783, lines 12-29. These two sections may be re-organized. They overlap with discussion and I suggest to first referring to the previous work on classification of rates

before it is applied.

P. 8784 l. 21. Authors refer to 'highest' and 'increased'. Is their any statistical analysis to support these terms, i.e., are there significant effects?

P. 8786 l. 11. It is argued that a high percentage of labile pyOM in 0-10 cm stems from input of fresh, less decomposed litter. I suggest to measure litter from that site to compare litter thermal stability to peat thermal stability.

P. 8787, first paragraph. Several pyrogram patterns are discussed. I miss a discussion about the only consistent pattern visible in Fig. 4, namely a decline in carbohydrates with depth. A pattern typical for peat seems the steady increase in lignin-derived phenols with depth (e.g. Zaccone et al. 2008, Org. Geochemistry 39: 830-838); is this in contrast to the current findings?

Interactive comment on Biogeosciences Discuss., 6, 8775, 2009.