

Interactive comment on “Substrate potential of Eemian to Holocene permafrost organic matter for future microbial greenhouse gas production” by Janina G. Stapel et al.

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Responses to the reviewer's comments: “Substrate potential of Eemian to Holocene permafrost organic matter for future microbial greenhouse gas production”

By Janina G. Stapel et al.

We thank R. Sparkes for his/her thoughtful and very constructive comments and suggestions on our manuscript which will improve the clarity and the quality of the paper. Below, we will address all listed issues, which are relevant for discussion.

Reviewer: R. Sparkes

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Major comments

1) Methods section – there is no explicit mention of the technique used to measure acetates, or it's not mentioned clearly. Since this is the main purpose of the paper it should be obvious what has been done to measure the acetate compounds.

The method is described in chapter “3.2 Low molecular weight organic acids (LM-WOAs) analyses”.

2) A lot of the molecular concentrations are reported as per gram sediment, but this leads to depth profiles that mostly correlate with OC content. Reporting molecular concentrations per gram carbon may lead to more interesting comparisons along and between cores.

Relation to gTOC provides information on the abundance of a parameter relative to TOC. As outlined above (reviewer 1, comment 12) there was not much change in the observed trends. Since we want to show the total abundance of microorganisms and substrates we would like to keep the gSed relation.

3) Permafrost soils and Yedoma can have very different biomarker compositions. For example, GDGTs are being used as microbial biomarkers, but Sparkes et al., Biogeosciences, 2015 showed that GDGT concentrations are low in Yedoma sediments. Bacteriohopanepolyols may be better tracers of microbial activity in this region (see for example Bischoff et al., Biogeosciences, 2016). When linking timescales to substrate potential, the different sediment types within each core need to be shown in figures and discussed as well, since there could be a combination of climatic and sedimentological controls on OM quality and substrate potential.

Well, bacteriohopanepolyols have not been investigated. The appeal of using GDGTs and archaeol is that you get information on bacteria and archaea, which is not the case for bacteriohopanepolyols. Also we are aware that GDGTs and archaeol do not represent all microorganisms, we at least get some insights of bacterial and archaeal

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variations over time. As outlined in our response to comment 2 (reviewer 1) the depositional effect on the OM quality is not clear yet. However, we will add this into the discussion.

4) P9 Line 1 – it is asserted that the permafrost deposits are dominated by terrestrial OM. Since GDGTs were measured, the BIT index could be used to confirm this.

Please see comment 3 reviewer#1.

5) P1 Line 23 – The GDGTs seem to correlate with TOC in the core sections referenced here. Relative increases in these molecules may support increased bacterial productivity, but if the biomarkers are changing with TOC then it may just represent variations in preservation. Once more, other markers for microbial activity would add value to the study.

GDGTs are already degraded biomarkers from intact polar lipids. The core lipids (GDGTs) are regarded as relatively stable. Thus, we suggest that they represent the past abundance of microbial communities, although preservation aspects cannot fully be ruled out.

Minor comments

P5 Line 24 - Was an internal GDGT standard used?

An external archaeol standard is used for quantification. We will add this to the method chapter.

P5 Line 24 – The GDGT biomarker molecules being measured are not defined.

We will add a supplement table where all GDGTs are listed (table S1).

P9 Line 15 – Absence of a particular biomarker does not necessarily mean that it has decomposed; it may never have been present.

This sentence will be rephrased to: “Thus, the low TOC and TOC/TN values (< 5), in

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addition to the low HI in the Eemian samples (MIS 5e, Table 1) may point to less favorable conditions for OM accumulation and/or an increased degree of OM decomposition and therefore to a reduced OM quality.”

P10 Line 10-15. This section is hard to understand, rephrasing may help

This will be rephrased and shifted (see comments above) to: “However, the increased PLFA concentrations in all active layers indicate to a certain extent that the permafrost deposits at least from MIS 3, 4 and 1 can serve as good substrate providers when thawed. For MIS 5e this could not be evaluated due to the lack of MIS 5e deposits with an active layer on top.” Typos

We thank R. Sparkes for his/her comments and will follow all his/her suggestions on the other minor comments (not listed here).

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