

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

Anonymous Referee #2

Received and published: 8 May 2017

General and specific comments

The authors describe a set of permafrost cores from Bol'shoy Lyakhovsky Island in the north-east Siberian Arctic that cover a time span from Eemian to Holocene, with the aim to assess the potential of organic matter from different periods as a substrate for microbial greenhouse gas production. However, I cannot fully follow the approach. The authors investigate organic matter quality using pyrolysis-GC-MS and Rock Eval, past and present microbial biomass using GDGTs and PLFAs, as well as concentrations of free and organic matter-bound acetate:

(1) I recommend to be careful with conclusions on organic matter degradability based on organic matter chemistry (see e.g. Schmidt et al., 2011, Nature).

[Printer-friendly version](#)

[Discussion paper](#)



(2) I do not understand the importance of past and present microbial biomass for this research question. There is no doubt about the presence of a living, active microbial decomposer community in the active layer, and the data are not interpreted to more detail. I would also like to point out that I am not aware of studies testing how fast PLFAs are degraded in continuously frozen soils, and that we therefore do not know for sure if PLFAs in permafrost really represent the living microbial community.

(3) As the authors themselves acknowledge (page 11, lines 12-14), acetate concentrations say little about acetate availability as this depends on the production rates of acetate from organic matter. I am therefore not sure about the value of this parameter in this context.

(4) I would appreciate more details on the applied methods that are also not contained in the cited previous publication (Stapel et al., 2016). In particular, what compounds were detected with pyrolysis-GC-MS and how were they evaluated to generate Figure 3? What PLFAs were detected and used to quantify total PLFAs? Were only bacterial or also fungal markers considered?

(5) The authors further present some interesting correlations between individual parameters, and the statistical approach should be described in the methods section. The underlying correlation matrix could also be presented in a separate table in the manuscript to give the reader a better overview.

(6) The authors use European terminology to describe the glacial cycles that is technically not correct for Siberia. I do not object in general since the European terms are well known and the authors also use Marine Isotope Stages to identify these periods, but I suggest to add at least a comment on the terminology to the text.

(7) If I understood correctly, the authors imply that old organic matter from deep, continuously frozen permafrost deposits might move upwards into the active layer and stimulate microbial activity there (e.g., page 10, lines 10-13; page 11, lines 20-24; Conclusions). While it is correct that an influx of additional organic carbon can stimu-

[Printer-friendly version](#)[Discussion paper](#)

late microbial activity in soils (“priming effect”), I do not see by what mechanism organic compounds could move upwards from frozen into non-frozen parts of the soil.

(8) The manuscript contains many grammatical mistakes and would overall profit from some language polishing (some sentences are very long and difficult to understand).

(9) As my last general comment, I want to mention that I think the authors do a good job in keeping overview of the different depositional ages across the different cores. This is not an easy task.

Technical corrections

Page 1, line 19: What do you mean with “representing at least a future substrate potential upon release during OM degradation”?

Page 1, lines 28-30: I find it difficult to follow the causalities here, please rephrase.

Page 2, line 1: The upper 0-3 m.

Page 2, lines 2-4: I suppose you mean that the freeze-locked OM might thaw and/or be converted into CO₂ or CH₄, potentially inducing a positive feedback to global warming. Since the consequences of permafrost thaw are described in more detail later in the paragraph anyway, I suggest deleting this sentence. If you want to keep the sentence, please be more concrete.

Page 2, line 6: What do you mean with “drastic changes in the ecosystem”?

Page 2, lines 8-10: The sentence is not clear to me. The term “re-mobilization” usually refers to the export of previously frozen OM or nutrients into aquatic systems (e.g., in the cited Vonk et al. reference), but this is not the cause for increasing decomposition rates or accessibility of OM for microbial degradation. Rather, permafrost thaw leads to increased microbial activity and consequently increased decomposition rates, as well as to increased export into aquatic systems.

Page 2, line 10: “preserved”.

Page 2, line 10: Delete “the”.

Page 2, lines 12-14: This sentence mainly repeats the statements in the sentences before, I suggest to include the experimental information there.

Page 2, line 28: Delete “of”.

Page 3, lines 9-10: Reference missing.

Page 3, lines 12-13: What do you mean with “feedback effects on permafrost deposits”?

Page 3, lines 15-16: I don’t understand, please rephrase.

Page 4, line 30: “Grinding” instead of “grounding”. See also page 5, lines 2 and 15.

Page 4, lines 30-31: Were samples acidified before TOC analysis?

Page 5, line 9: Change “were” to “was”.

Page 5, line 23: Change to either “using medium-pressure liquid chromatography” or “using a medium-pressure liquid chromatograph”.

Page 6, line 1: I suggest using “includes” instead of “consists”.

Page 6, line 10: Change “marker” to “markers”.

Page 6, line 19: Do the PLFA and TOC concentrations also correlate?

Page 6, line 23: Change “is” to “are”.

Page 6, line 23: “Significantly”: Was this statistically tested, and if yes, how? Otherwise, I would use another word.

Page 7, line 2: What do you mean with a partial correlation between past bacterial and archaeal markers?

Page 7, line 6: Should be plural, “concentrations”.

[Printer-friendly version](#)[Discussion paper](#)

Page 7, line 7: “Rise”.

Page 7, line 23: Do p-value and R² refer to correlations of both bacteria and archaea with TOC?

Page 8, line 8: “Scatters”.

Page 8, line 23: I suggest using “weakest” instead of “smallest”. Same for line 26 (“strongest” instead of “highest”).

Page 8, line 30: What do you mean with “assigned”? How does OM degradability depend on the amount of OM? And how do you distinguish OM composition and OM quality?

Page 9, line 10: The main mechanism by which TOC/TN decreases during OM decomposition is the faster loss of C than N due to microbial respiration.

Page 9, line 13: Change to “... while OM with low HI contains ...”

Page 9, line 19: Please add a reference here.

Page 9, lines 27-29: Are you referring to the last interstadial here? I also noticed that Table 2 suggests both a dry climate and moist soils during that period, this seems rather strange. There is also something wrong with the grammar in the first part of the sentence.

Page 9, lines 30-31: “Moisture increased depositional settings”: please rephrase.

Page 9, line 33: Change “minor” to “lower”.

Page 10, line 11: The Fontaine paper is not about permafrost.

Page 10, lines 11-12: I do not understand.

Page 10, line 12: Which is not surprising considering that the active layer is seasonally thawed and the deeper permafrost is continuously frozen.

BGD

Interactive
comment

Printer-friendly version

Discussion paper



Page 10, lines 13-14: PLFAs inform about microbial biomass, not activity.

Page 10, line 20: Please add a reference for GDGTs as indicators of microbial activity. Also, the data presented show TOC concentrations, not accumulation rates.

Page 10, line 23: “increased”.

Page 10, lines 23-25: The observed coincidence of high GDGT and OM concentrations does not necessarily imply certain environmental conditions. Microbial biomass is often correlated with OM concentrations since most microorganisms use OM as substrates.

Page 10, line 32: I suppose you mean microbial metabolism, not turnover.

Page 11, line 15: What do you mean with “soil biogeochemistry composition”?

Page 11, lines 16-17: Speculation.

Page 11, line 19: It is true that input of fresh OM by plants might additionally stimulate the microbial community in active layers, but I would expect that the main reason for the higher microbial biomass is the fact that the active layer is thawed in summer (i.e., provides liquid water).

Page 11, lines 20-21: What do you mean with the incorporation of frozen permafrost carbon into the active layer? I also think it should be “freeze-locked”.

Page 11, line 25: Change to “. . . differences also affect the PLFA concentration.”

Page 11, line 26: “thermos”?

Page 11, line 27: Change to “continuous”.

Page 11, line 31: Change “is” to “are”.

Page 11, line 34: What do you mean with “OM composition deposited”?

Page 12, line 1: Microbial consumption in the active layer is an ongoing process and not restricted to the “time of deposition”.

[Printer-friendly version](#)[Discussion paper](#)

Page 12, line 2: What do you mean with “stronger pronounced”? I’m afraid I cannot follow the entire sentence.

Page 12, line 5: Singular: “reason”.

Page 12, line 10: I am not sure “implied” is the right word.

Page 12, line 11: What do you mean with “microbial acetate consumption on a regional scale”?

Page 12, line 20: I suggest changing to “. . . a significant substrate pool for future microbial greenhouse gas generation might become accessible within thawing permafrost.”

Page 12, lines 22-26: I think this sentence is incomplete. I certainly cannot follow the grammar.

Page 12, line 27: “appears”.

Page 12, line 32: “a strong impact”.

Page 13, line 8: “guided”.

Page 27, line 6: “Eglinton”

Page 29: How were the plus and minus signs assigned?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-89, 2017.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

