

### Referee 3

This manuscript describes the influence of temperature on competition for electron donors between iron reducers and methanogens in microcosms constructed from Ljubljana marsh soil. The primary objective of the study is not particularly interesting; it is expected that iron reducers would outcompete methanogens for electron donors, assuming sufficient Fe(III) is available. The effects of temperature on the competition are interesting, although temperature does not change the outcome of the competition. The study is tight, however, with a significant amount of supporting data that strongly support the author's conclusions. The coupling of kinetics of organic matter decomposition with the production and accumulation of products (CH<sub>4</sub> and CO<sub>2</sub>) and reactants (H<sub>2</sub> and acetate) are strong points of the manuscript, as is the linking of acetate and methane data with *mcrA* genotypes. I have a few suggestions and questions.

Some wording is awkward. For example:

1. p. 2358 line 23: Change to "For centuries, most European wetlands. . ."

Changed as suggested.

2. p. 2359, line 8: Change to "Fermentation products such as acetate and H<sub>2</sub> are substrates for . . ."

Changed as suggested.

3. p. 2360, line 2 (Fey and Conrad, 2000) Remove the comma after Fey.

Changed as suggested.

4. p. 2360, line 11: "This will result in anoxic conditions. . ."

Changed as suggested.

General comments:

1. In the Introduction, briefly introduce the importance of two component models and their use in analyzing processes.

To avoid repetitions we didn't change. We are mentioning the role of two-component models in the discussion: "Soil organic matter is a complex mixture of different components. In short-term experiments, CO<sub>2</sub> and CH<sub>4</sub>, the end-products of carbon mineralization, often increase linearly with time. For this long-term experiment, lasting 115 days, two-component decay models describing the different degradability of a labile and a refractive fraction gave a better fit (Murayama and Zahari, 1996b)."

2. Throughout the manuscript, replace "decomposition" with "mineralization." Decomposition can refer to a variety of processes and products, whereas only mineralization was measured in this study.

Replaced as suggested.

3. Ammonium was measured and data displayed in Fig. 2, but very little information was given on why it was measured and for potential implications for its changes in concentration. Some deeper comment would be appreciated.

We use ammonium only to support the temperature dependency of mineralization writing: “Above 10°C, the concentration increased reaching an optimum at 32°C (3.04  $\mu\text{mol NH}_4^+ \cdot \text{g dry wt}^{-1}$ ).” Further discussion would be beyond the scope of this paper that is focused on methanogenesis, iron reduction, and the microbial populations involved.

4. Interestingly, the *mcrA* primers designed by Luton and used in this study are biased against *Methanosarcina* and towards the *Methanomicrobiales* and *Methanobacteriales*. The authors may want to mention this bias; it might serve to support their data.

This would indeed be a good argument, but unfortunately, this view is not supported by our database of about 2500 *mcrA*-sequences (Metje, unpublished)