

Interactive comment on “Stable carbon isotope discrimination and microbiology of methane formation in tropical anoxic lake sediments” by R. Conrad et al.

Anonymous Referee #2

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Review of bg-2010-316 “Stable carbon isotope discrimination and microbiology of methane formation in tropical anoxic lake sediments”

GENERAL COMMENTS

The paper attempts to quantify pathways for methane (CH₄) production in sediments from lakes in the tropical Brazil. This is a very important scientific endeavor. Tropical lakes are thought to be large sources of atmospheric CH₄, but fluxes and processes controlling CH₄ production are quite uncertain. Therefore, the data are timely. The subject is appropriate for publication in BG

The approach employs stable carbon-isotope techniques. The basic premise is well

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described in the introduction section. This is not a new approach; it was pioneered in the 1980s, and the author (R. Conrad) has used the technique in studies of lake sediments in Germany and in tropical Brazil. The present study extends the technique to 16 sediment cores.

I have one comment on the structure and organization of the paper. The basic data are presented in four tables, which are fine. However, after that, these data are compared and contrasted in several linear-regression plots. It seems to me that the plots belong in a discussion section, since they help to interpret the basic data. Indeed, while reading the results section, I struggled a bit to follow the logic behind some of the figures, whereas the logic was explained in the discussion section. The paper, as written, is fine for a technically adept audience, but I suspect that a general audience will struggle a bit. A better organization presents the basic data in the results section and saves the interpretation for the discussion section.

SPECIFIC COMMENTS

1) The abstract is fine.

2) The introduction section is a bit wanting to me. As written it is a rationale and justification for using stable isotopes to unravel pathways for CH₄ production. However, there is no justification for doing the work in tropical lakes. There is no conundrum or question guiding the research. After reading the introduction, I asked myself why go to the tropics? The research could have been done in any lake. I would like to see a better rationale and justification for studying the specific tropical lakes. What question is being addressed by the study?

3) The incubation experiments, chemical analyses, and calculations are essentially identical to those given in Conrad et al., 2010, *Limnology and Oceanography*, 55, 689-702. Therefore, the present study merely extends to approach to a new site. You need a strong justification for merely studying more sites.

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4) Moreover, the choice of study sites seems haphazard, to me. A justification for site selection would be helpful. Also you need a justification for sampling in different months, given the strong seasonal hydrology in the region.

5) Although the sediment cores were 40 cm in length, the experiments used only the upper 3 cm of each core. Therefore, the results are limited to the sediment water interface. Unfortunately, we learn little about chemical characteristics in situ, other than the data presented in Table 1. Do you have values for concentrations of dissolved CH₄ and carbon dioxide in situ? A better understanding on chemical characteristics in situ would help to constrain results from studies in vitro presented in the remainder of the paper.

6) Is it possible that CO₂ in the headspace underestimates the total amount of CO₂ produced? Given the near neutral pH, it seems to me that a significant amount of CO₂ will dissolve. Did you measure dissolved inorganic carbon (DIC) and, the δ -13C of DIC?

7) It also seems likely that organic matter is only partially decomposed, resulting in production of dissolved organic carbon (DOC) during the incubation assay. Is DOC production relevant to the discussion on page 8634?

8) I was a bit surprised by the terse discussion and interpretation why the two methods, CH₃F versus isotope fractionation, gave different values for fraction of CH₄ production from hydrogen and CO₂. This seems like an important distinction, to me, which deserves a better interpretation.

9) Does acetate accumulated by microbial biomass influence the isotopic values?

10) The conclusions on page 8643 are actually a summary of the major findings rather than a conclusion, (except for the last sentence). You could summarize briefly and elaborate more on the implications.

11) The supplemental material is fine.

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TECHNICAL COMMENTS

- 1) Page 8628, line 21: you should acknowledge no effect of CH₃F on CH₄ production in sample 15.
- 2) Page 8629, line 9: this sentence is confusing, to me. What do you mean by 'calculated thereof'?
- 3) Page 8630, line 13: avoid using 'etc.' at the end of sentence. I doubt that unacquainted reader will follow your logic.
- 4) Page 8636, line 13: avoid using 'this' in a sentence. I doubt that unacquainted readers will follow your logic.
- 5) Page 8638, line 13 – 25: would you expect fractionation of Corg? Since only a small amount of organic matter is degraded during the incubation, fractionation seems unlikely, to me. Why such detailed discussion here?
- 6) Page 8639, line 12: where are the data for 'other intermediates'?
- 7) Page 8639, line 20 -23: is the discussion of methane emission relevant? You measured methane production in 3 cm of a 40 cm deep core. I would limit the discussion to the data at-hand.
- 8) Page 8640, line 10: the statement that bicarbonate content is negligible is incorrect. I believe that the Bjerrum plot for the sum-CO₂ system indicates that the bicarbonate is at least 50% of the total carbon at pH = 6. Bicarbonate is the dominant ion at pH greater than 6.4. Since you know the headspace and liquid volumes, the calculation is straightforward.

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