Biogeosciences Discuss., 7, C1962–C1967, 2010
www.biogeosciences-discuss.net/7/C1962/2010/

Biogeosciences Discussions

Interactive comment on "Carbon isotopic composition of branched tetraether membrane lipids in soils suggest a rapid turnover and a heterotrophic life style of their source organism(s)" *by* J. W. H. Weijers et al.

R. Smittenberg (Referee)

smittenberg@erdw.ethz.ch

Received and published: 22 July 2010

1. Does the paper address relevant scientific questions within the scope of BG? * Yes. The paper discusses the source and stability of a certain type of lipids derived from soil bacteria, that have their use as a paleo-environmental proxy.

2. Does the paper present novel concepts, ideas, tools, or data? * The techniques and ideas behind it are not novel, the data are new.

3. Are substantial conclusions reached? * Some more indicative, although no conclu-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

sive insights are reached concerning the life style of the source organisms of branched GDGTs. The calculation of their decay rate, (higher than n-alkanes but similar to carboxylic acid in the same soils) is a substantial finding.

4. Are the scientific methods and assumptions valid and clearly outlined? * In almost all cases

5. Are the results sufficient to support the interpretations and conclusions? * Yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? * Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? * Yes

8. Does the title clearly reflect the contents of the paper? * yes

9. Does the abstract provide a concise and complete summary? * Yes

10. Is the overall presentation well structured and clear? *Can be improved to some extent.

11. Is the language fluent and precise? * In general yes but not fully. The authors may take a fresh look at the text and here and there improve some style issues and take out the last mistakes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? * Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? * See suggestions

14. Are the number and quality of references appropriate? * Although the references appear all to be appropriate, their number is high and the authors could try to remove some.

15. Is the amount and quality of supplementary material appropriate? * not applicable

Other comments

The beginning of the introduction describing the 'history' of the lipids can probably be shortened as this is covered in other publications. Table 2. The last column does not have a heading and structure 'd' is missing in the sequence. Given the information in the text I assume that for 'd' no 13C values were obtained and that the last column should be 'h' ; Note the indent of 'C4 vegetated soil'.

3.3.

HPLC/MS quantification: The authors assume a 1:1 response factor ratio between crenarchaeol and the branched GDGTs. This may not be true but by absence of pure GDGTs the authors make the best assumption possible. However, please acknowledge this assumption in the text.

4.1.

In the discussion about the GDGT concentrations/distributions I would have expected that the authors would have calculated the proxies based on the branched GDGTs, i.e. the BIT index and the CBT/MBT indices. No need to discuss these numbers in great detail, but it might be useful in light of the expanding community that uses these new proxies.

4.4.

p3710,line 13. Start a new paragraph with 'First' (and use 'Second', not 'Secondly' to start the next one) p3710,lines 16-25 are confusing, together with figure 4. In the figure it appears that heterotrophically derived CO2 is the same as respired CO2, that then diffuses to become soil CO2, with a fractionation of +4.4 permil. This is not the case, heterotrophic derived CO2 should be the same as soil CO2, which then diffuses away to become respired CO2 with a fractionation of -4.4 permil. The latter is measured in many cases and may serve as an approximate value in case soil 13C-TOC values are

BGD

7, C1962–C1967, 2010

not known. However, it is not part of the carbon flow towards chemo-autotrophy. I don't see a reason why the right and left side of figure 4 should be any different for the 13C value of 'heterotrophy', being ca 4.4 permil heavier than TOC (and respired CO2 above the soil). p3710,lines 15-19/table 2. Given the measurement errors, I would not claim that all the crenarchaeol-derived biphytanes have lower 13C values except for one: I count two being equal within error, two lower and one higher. p3710,lines 19-23. This is a confusing end statement of the discussion of the potential of chemo-autotrophy. Overall, the most essential piece of information in the 'first' part is the comparison between the 13C values of (the alkanes of) crenarchaeol and brGDGT, which is non-conclusive since they are equal. Other than that, the mechanisms behind it (C flow and associated possible fractionations) are informative, but since they are also non conclusive, can probably be summed up concisely, i.e. in a clear and structured way than is now the case. The most important part - the potential difference between acetogenic and isoprenoid lipid 13C, may deserve more than the confusing half sentence it has now been given (p3710lines21-23). 'second' and final part

I would place the references to Pancost&S.Damsté and Opperman at the beginning of part 4.4, stating the current understanding of the matter. In the final part (p3712 lines 3-24) where one would expect an evaluation of two possibilities based on new data and insights, they only (re)discuss the insights of these two papers, and do not take at all their own new data into consideration. The final line of paragraph 4.4 should in my opinion be the one of [p3711 line 28 – p3712 top] (By expanding this relationship . . ..)

4.5

p3714 line 4. I suggest to start with the 13C values of structure 'e'. Table 1: add a column with the X/XV ratios p3714 line 22/23. Suggest to replace the words 'only slightly' with 'somewhat'. (subsequent line: improve writing: two times 'seems' after 'suggest')

* Consider some re-ordering of the discussion, for instance swapping 4.4. and 4.5

BGD

7, C1962–C1967, 2010

and discuss first the 'known' crenarchaeol 13C values, so that the 'unknown' brGDGT 13C values can be discussed better in light of that (see e.g. . The same is true for the heterotrophic vs chemo-autotrophic discussion. Since the now 'second' part starts discussing heterotrophy – the basis for soil CO2, it may also be useful to swap these, but the basic discussion about heterotrophy – the source of soil CO2 would need to stay at the beginning.

* Consider adding a graph that shows all the GDGT 13C data as well as TOC and one or average n-alkane value). Fig 4 may be integrated in that (leaving out the 'respired CO2' point)

4.6.

page 3717 line 2. Replace 'educated guess' with 'estimate' line 15: add '(IPL)' line 20 onwards: I do not agree with the conclusion that none of the bacterial biomass / br GDGTs is accumulating. A turnover time (1/k) of 17 years means a decay rate k of 0.058 yr-1, and thus a half time of 11.8 years, assuming that the decay is constant and first order. The calculated rate is based on a measurement performed over a relatively short time scale, concerning organic matter decomposition: only the fist half to three-quarter of the original amount disappeared ($\sim$44% and $\sim$75%). As the authors correctly state later, on longer timescale other, preservational, factors may start to play a more important role than first order decay.

Table 3: please specify the carbon chain length of the carboxylic acids.

Conclusions

p3718 line 18 . The 'conclusion sentence' speculating the role of of br GDGTs producing bacteria in OM degradation come out of the blue, this was not discussed in the discussion part of the paper. I do not agree with the conclusion that brGDGTs would not accumulate over time. Rather I would state that they appear more susceptible than long chain n-alkanes and similar to carboxylic acids. I would also refrain from extrap-

BGD

7, C1962–C1967, 2010

olating turnover times in arable, managed soils to all soils that see suffer much less disturbances.

Interactive comment on Biogeosciences Discuss., 7, 3691, 2010.

BGD

7, C1962–C1967, 2010

Interactive Comment