

Interactive comment on “Transport of branched tetraether lipids from the Tagus River basin to the coastal ocean of the Portuguese margin: consequences for the interpretation of the $MBT^I/$ CBT paleothermometer” by C. Zell et al.

C. Zell et al.

claudia.zell@nioz.nl

Received and published: 25 May 2014

We would like to thank both anonymous referees for their constructive comments, which certainly help to make the revised manuscript more comprehensible. Below we are responding in detail to each comment of reviewer #1.

Referee #1: In this study, the authors compare concentrations and distributions of GDGTs in soil, river bank sediments, river SPM, marine sediments, and marine SPM to understand the production and transport of GDGTs to determine the efficacy of using

C1844

the brGDGT paleothermometer in marine settings. The authors find that brGDGTs are produced in both river and marine settings, complicating their use as a soil-derived paleotemperature proxy. Quantitative paleotemperature reconstructions are needed to understand past climate change, and studies like this one are crucial for understanding both the potential and limitations of the brGDGT paleotemperature proxy. This study was well thought out, the data were well analyzed, and the interpretations follow from the data presented. While the writing in the manuscript was fairly written, it could use some modifications to make it more clear to the reader, as the sentence structure and language were lacking in some areas. Overall, I think this manuscript should be accepted with minor revisions. Reply: We thank the reviewer for his/her positive comments. We will modify the manuscript where necessary to make the manuscript more comprehensible.

General Comments Referee #1: Pages 21-22: You note that there is an increase in cyclization between soils and marine sediments and attribute this difference to large pH discrepancies between ocean water and soils. However, there is also a significant increase in cyclization between soils and SPM/river bank sediments, as has also been noted between lake sediments and soils (e.g. Tierney and Russell, 2009; Loomis et al., 2011) and pH between these two depositional environments is very comparable. How do you explain this variability in absence of pH differences? Reply: The referee is right that this is remarkable. The difference between the soil pH (7.4) and river water pH (7.8) is relatively small. For the soils one would certainly expect to see a higher DC. The reconstructed pH underestimates the actual soil pH in many of the soils of this sample set (reconstructed pH=6). We assume, that this might be related with the problems related to the use of brGDGT in dry environments. The river SPM samples have a DC of ca. 0.28 (reconstructed pH=7.1), while the measured pH in the SPM sampling site was 7.8. Riverbank sediments have a slightly higher DC, but we lack pH data of river (and pore) water from these areas.

Referee #1: Your marine SPM core brGDGT concentrations increase with depth, which

C1845

you attribute to bottom water transport in the nepheloid layer. However, this profile is similar to lakes (e.g. Sinninghe Damste et al., 2009; Bechtel et al., 2010; Blaga et al., 2011; Woltering et al., 2012), many of which would have very different circulation patterns and lack a nepheloid layer. How do you reconcile these, and do you think we can treat marine and lacustrine systems similarly? Reply: The high concentrations in the bottom water can only be seen close to the river input. This likely indicates input of brGDGTs adsorbed to particles in the river. The turbidity data of the CTD clearly shows that a nepheloid layer is present on the shelf (see figure below) and thus we feel this is the most likely explanation. Although it is true that in stratified, anoxic lakes one often also observes a higher brGDGT concentration in the bottom waters, we feel it is hard to compare this with the Tagus River system since lateral transport is not playing a role in lakes whereas it is a dominant process in the river system. In lakes the high concentration in bottom waters is often attributed to the much longer residence time of particles in the deep, stratified waters (e.g. Buckles et al., GCA, in press)

Specific Comments Referee #1: Page 17, Lines 20-end: Your weighted mixing model here is flawed, as you are comparing downstream soils (even after removing the lowest two soil samples) to river sediment upstream. The fact that the DC in river bank sediments is different from the DC of soils is enough to say that the distributions are different, but if you wanted a more rigorous way of distinguishing between rivers and soils, you should do an iterative weighted average of soils upstream from each river bank sample and compare those weighted averages to the sediment. However, this approach may also be complicated by the fact that there are dams along the river, so it may be best just to stick to the differences in DC between the samples. Reply: It is true that the soil weighted average brGDGT data should not be compared to those of the upstream river sediments. However, we compared the soil weighted average data to those of the river SPM, which were only sampled close to the river mouth. We agree that dams might influence the brGDGT signal in the downstream part, as they likely block at least part of the brGDGTs derived from the upstream region. Therefore, the brGDGTs from the downstream soils might have a stronger influence on brGDGTs in

C1846

our SPM samples. We will make this clearer in the revised manuscript.

Referee #1: Also, you note that there is no altitudinal trend in soil GDGTs, but is there a trend in river bank GDGTs with altitude? If so, this could be another line of evidence that river bank GDGTs are produced in situ if they follow some sort of altitudinal/temperature gradient. Reply: We do not observe an altitudinal gradient in the river sediment GDGTs.

Referee #1: Page 19, Lines 14-17: How do the soils near the river mouth compare to river bank and SPM? From your figure 10, these samples look the most different from sediment/SPM samples, which will help your argument for in situ production as well. Reply: This is a good point. The brGDGTs in soils collected closest to the river SPM sampling sites (sites below 200m) are distinctive from those of the riverbank and river SPM. If the three samples investigated in this study are representative for the downstream soils and if they are the mayor source of soil derived brGDGTs to the downstream river, it might be another indication for in situ production. We will mention this in the revised text.

Referee #1: Page 20, Lines 5-11: You should mention here that the increase in brGDGT concentrations in the bottom waters may derive from in situ production. The IPL fraction increases dramatically at depth, and it is possible that the core lipids at these depths are derived from the breakdown of the IPL fraction. This is supported by the distribution changes in SPM (as you mention in your next paragraph). Reply: We don't agree with the reviewer for this point. The brGDGT concentration increases in the bottom water was only observed close to the river influenced coastal site and higher concentrations were in general observed in the upper water layers. We will clarify this point in the revised text.

Referee #1: Pages 22-23: While the trends in BIT do follow the original soil organic matter interpretation of Hopmans et al., you have strong evidence for in situ production of brGDGTs in the marine system. Given this, I would strongly caution it's use as a ter-

C1847

restrial organic matter tracer. Reply: We agree with the reviewer. We also mentioned that the BIT index should be used with caution, since brGDGTs can be produced in the marine environment and crenarchaeol is also produced in soils. However, the general trend of higher brGDGT concentrations in the terrestrial environment and higher crenarchaeol concentrations in the marine environment is clearly seen in our study area. Hence, the BIT index in this setting can serve as a tracer for continental (soil and river) derived organic matter, but not as a tracer for soil organic matter since we suggest that an important amount of brGDGTs are produced in the river itself.

Technical Comments Referee #1: Page 2, Lines 1-2: Due to the evidence of in situ production of brGDGTs in rivers (including your study) change to “which are thought to be transported from soil: Reply: We will modify the sentence according to the reviewer’s comments.

Referee #1: Page 3, Line 20: Reference should be Niemann et al., 2012 (not 2005; also change in your references); also add Loomis et al., 2012 Reply: We will modify the sentence according to the reviewer’s comments.

Referee #1: Page 5, Line 23: When are the dry and wet seasons? Reply: We will clarify this in the revised version.

Referee #1: Page 14, Line 6: “closed” should be “close” Reply: We will modify the sentence according to the reviewer’s comments.

Referee #1: Page 19, Line 26: add a comma after “In SPM” Reply: We will modify the sentence according to the reviewer’s comments.

Referee #1: Page 22, Line 8: “depended” should be “dependent” Reply: We will modify the sentence according to the reviewer’s comments.

Referee #1: Page 24, line 16: Add Loomis et al., 2012 to the calibration list Reply: We will add the citation in the revised version, according to the reviewer’s comments.

Referee #1: Figure 6: Labeling/caption is not clear on this figure. Is the top row CL, C1848

middle row IPL, and bottom row percent IPL? Reply: Top row is CL ng L⁻¹, CL ug gOC⁻¹ and last row IPL-derived. CL will be added to the top and middle row panels.

Referee #1: Figure 9: The color bars for the CL plots aren’t labeled. Are the same as the color bars for IPLs? If so, remove the color bars for CL, if not, please label them. Reply: Color bars at the CL plots will be removed.

Interactive comment on Biogeosciences Discuss., 11, 3731, 2014.

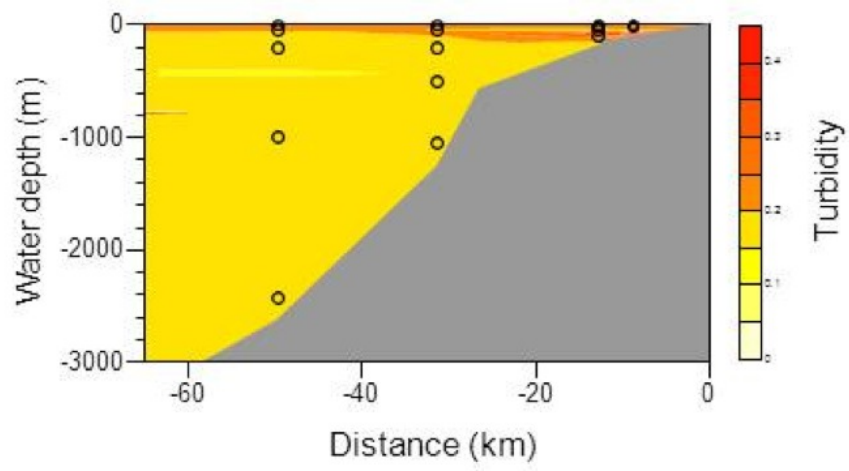


Fig. 1.

C1850