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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'/CBT paleothermometer" *by* C. Zell et al.

Anonymous Referee #1

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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 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.

General Comments

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 variably in absence of pH differences?

Your marine SPM core brGDGT concentrations increase with depth, which 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?

Specific Comments

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 com-

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plicated 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. 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.

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.

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 form the breakdown of the IPL fraction. This is supported by the distribution changes in SPM (as you mention in your next paragraph).

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 terrestrial organic matter tracer.

Technical Comments

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. . ."

Page 3, Line 20: Reference should be Niemann et al., 2012 (not 2005; also change in your references); also add Loomis et al., 2012

Page 5, Line 23: When are the dry and wet seasons?

Page 14, Line 6: "closed" should be "close"

Page 19, Line 26: add a comma after "In SPM"

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Page 22, Line 8: “depended” should be “dependent”

Page 24, line 16: Add Loomis et al., 2012 to the calibration list

Figure 6: Labeling/caption is not clear on this figure. Is the top row CL, middle row IPL, and bottom row percent IPL?

Figure 9: The color bars for the CL plots aren’t labeled. Are the the same as the color bars for IPLs? If so, remove the color bars for CL, if not, please label them.

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