Biogeosciences Discuss., 10, C4423–C4427, 2013
www.biogeosciences-discuss.net/10/C4423/2013/

Biogeosciences
Discussions
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Interactive comment on "Water availability determines branched glycerol dialkyl glycerol tetraether distributions in soils of the Iberian Peninsula" *by* J. Menges et al.

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Received and published: 20 August 2013

Anonymous Referee #2

General comments This is an interesting paper reporting the results of GDGT analyses for a soil transect across part of the Iberian peninsula, and comparing the analytical data with measured values for temperature, moisture, and pH. The study concludes that the MBT'/CBT temperature proxy may not be valid in soils from arid environments, and suggests that a soil aridity index represents a more significant control on the GDGT distribution. There is increasing interest in the more complex controls on GDGTs and as such this paper is timely, and the data are of value to the field. However, I feel

a number revisions would substantially improve the paper, and should be addressed prior to publication.

Specific comments 1) In discussing work on branched GDGTs in soils, the authors refer to only a limited set of literature, focusing particularly on the papers of Peterse, and ignoring some more recent work on soils such as Dirghangi et al., 2013 (OG). This is of particular importance as some of the missing papers deal directly with the questions addressed in this study.

*This and other papers have been added to the references and used in the introduction and discussion as also suggested by reviewer 1.

2) The authors use the MBT' index of Peterse 2012 throughout, which they justify on the basis that this avoids any distortion from low abundance GDGT IIIb and IIIc. However, this index is relatively new and not yet universally used or accepted. The paper would be more comprehensive and the conclusions more robust if the authors tested both MBT (as defined by Weijers et al., 2007) and MBT' (as defined by Peterse). Any differences, or lack of them, in the results would in themselves be of interest.

*Using the Weijers et al (2007) index and calibration we obtained larger residuals than by using the calibration by Peterse et al., in 2012. So, temperatures reconstructed using the original calibration do not fit better with the instrumental data. We feel that the inclusion of an extensive discussion on the differences between the two indices is beyond the scope of this paper, especially since this has already been carried out by Peterse et al. (2012).

3) The authors pretty much completely ignore the isoprenoid GDGTs and TEX86. In one way this is understandable as they are testing the MBT/CBT temperature proxy (and TEX86 is not used as a temperature proxy in soils). However, I think including the isoprenoid data, although not directly involved in the temperature measures, would be useful in assessing compositional differences in soils subject to different environmental parameters.

*As the reviewer acknowledges, the focus of the paper is on the appraisal of the MBT/CBT indices to reconstruct MAT, and thus on the distributions of brGDGTs. In this sense, we prefer not to discuss the iGDGT data here. , see also comments to reviewer 1

4) I would usually prefer to see TOC reported via EA analysis rather than LOI, as the former is generally more accurate.

*LOI is one of the methods commonly used to quantify organic carbon contents in soils. Even though the EA is more accurate, the amount of sample analyzed used in an EA is very small. Thus even with 3 replicates one measures a very small portion of the soil sample and this must be thoroughly ground to ensure homogeneity of the material. The LOI instead is done in much bigger samples and thus a higher portion of the sample can be analyzed. Soil structure is much more complex than sediments and even homogenized samples will still present certain level of heterogeneity. By measuring larger samples we expected to average out some of that microheterogenity and thus compensate for the lower accuracy of the technique.

5) The role of acidobacteria as a potential source for br GDGTs is overemphasised, given that this is currently only a hypothesis with a small amount of supporting data, and a lot of circumstantial evidence (e.g. the ubiquity of br GDGTs in soils of all types and in lakes) against. Given that the authors conclude that acidobacteria are not likely sources in Iberian soils, I think a more balanced discussion of this on p 9052 would be more consistent. The authors do acknowledge the other possible sources, so this is mainly a question of rephrasing the paragraph.

*We have changed the phrase into: None the less brGDGTs have been found in a range of environments regardless of the origin or redox state (see Schouten et al. 2013). So far brGDGT were identified in only two aerobic Acidobacteria species suggesting that they are synthesized by different bacterial communities (e.g. anaerobic and aerobic; Sinninghe-Damsté et al., 2011). This was confirmed by our data as de-

spite covering a pH range from 4.8 to 8.7, we did not observe an increase in brGDGTs with lower pH contradicting earlier findings (e.g Peterse et al., 2010; Sinnghe-Damsté et al., 2011; Yang et al., 2011).

6) The key statistics need to be more clearly reported and indicated in the text. For example in section 3.2, the authors report that CBT and measured pH have a linear relationship with a similar slope to the global calibration, but they don't report the strength of this correlation (just having it on the graph is not sufficient). At a minimum, r2 and p values should be cited in the text when discussing the relationships. I have used section 3.2 as an example, but this applies throughout the results and discussion. This is crucial as the authors are making claims such as 'significantly correlated' 'strong relationship' 'weak relationship' etc - these claims need to be supported in each and every case by the statistics.

*We have added the requested R2 and P values.

7) I am not convinced that plotting the study data on top of the Peterse global calibration data adds much to the presentation of the figures and in some cases makes them harder to read. I would like the see the figures redrawn without the Peterse and Weijers data (it would be sufficient to plot their regression lines to show up the differences between those data sets and this), and instead presenting more of the interesting aspects of this study. For example, the authors have split their soils into different soil classes, and note that two of these have higher br GDGT abundances, but that there is no correlation between soil type and proxies. If the authors were to divide their soils on the graphs with different symbols representing the different soil types this would be effectively presented in a form the reader can visually assess.

*We wanted to show not only the calibration line but also the spread of the data from the Peterse et al. 2012 dataset as we feel this greatly reinforces our results and conclusions. Data from earlier studies are shown in the background and in grey so we feel that they are not disruptive to visualize the new Spanish dataset.

As for separating the data we think this will overcomplicate the figures. As we pointed out in the text: The sample set includes a wide range of soil types, belonging to 5 orders and 14 groups, covering a wide range of parent materials, and climatic and geographic conditions. Thus we would have 14 symbols for a 23 sample dataset, although no correlation between soil type and MBT/CBT was found. .

8) In section 3.3, discussing Fig 3.d, the authors state that their MAT(est) residual values have a non-random and bi-directional distribution similar to, but more extreme than that seen in Peterse et al. 2012. However, looking at Fig 3.d, this simply does not seem to be the case - the Peterse data show the relationship described (temperature underestimated below 10 C and overestimated over 10 C), but the data from this study show no obvious strong bias either way. Firstly, according to table 1, the samples all have MAT(im) of 10 C or more. So how can this data be showing anything about the behaviour of samples with a MAT(im) of below 10 C? Secondly, on the graph there is considerable overlap between the MAT(im) of the underestimated and overestimated

*This has been modified as it all steamed from a plotting error. Both the figure and the paragraph have been modified.

Interactive comment on Biogeosciences Discuss., 10, 9043, 2013.