

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

**Anonymous Referee #2**

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### 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 of revisions would substantially improve the paper, and should be addressed prior to publication.

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### 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.

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.

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.

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

5) The role of acidobacteria as a potential source for br GDGTs is over-emphasised, 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.

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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,  $r^2$  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.

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 to 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.

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

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residuals. There is also a typo in this section where both the temperatures above and below 10 C are described as over-estimated.

9) In tables 1 and 2 it would be useful if the samples were listed in the same order.

10) The authors refer to a weak inverse correlation between MAT and MAP but don't plot it. As they are basing a conclusion on this, it would be useful to see.

11) The authors seem to be largely ignoring the possible role of pH. They dismiss it as a reason for precipitation affect MBT' because CBT does not correlate with MAP, but do not discuss the fact that their MBT' does correlate with pH, in fact with a better  $r^2$  than CBT against pH, and indeed MBT' vs MAP, which the authors are focussing on. This area needs a much better discussion, with reference to recent literature on soil and lake pH and GDGTs (e.g. Yang et al 2012, Schoon et al 2013 (OG)).

12) Why are only the MAT(est) residuals plotted against AI? Why not plot MBT' itself, given how important the authors are claiming AI is?

13) Vegetation type is mentioned as a possibly important control - was any information on the vegetation at the sample sites collected? If so, how does this relate to the results found?

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