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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.

Anonymous Referee #1

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General comments

In this study, the authors analysed branched GDGTs in 23 soil samples collected across the Iberian Peninsula covering a wide range of precipitation. They showed that the methylation degree of branched GDGTs (MBT') was correlated with precipitation rather than with air temperature. This suggests that soil moisture can have an influence on the distribution of branched GDGTs in dry environments.

This paper deals with a subject of topical interest and confirms the fact that temperature and pH are not the only environmental parameters controlling the distribution of branched GDGTs. Nevertheless, several issues need to be solved before the

manuscript can be published.

1) The discussion section needs to be improved and is not sufficient in the present form. It is mainly based on the paper by Peterse et al. (2012) who recently published an extended soil calibration between MBT'/CBT and temperature and showed that the MBT' did not correlate with temperature in arid regions. Nevertheless, several papers already discussed the effect of soil moisture on branched GDGT distribution, for example Loomis et al. (OG, 2011) in soils, Huguet et al. (OG, 2010) in peat and more recently Dirghangi et al. (OG, 2013) who studied the distribution of branched GDGTs in soils from arid and wet environments collected along two environmental transects in the USA. The conclusions of the latter paper are very similar to the present one, i.e. MBT/CBT-derived temperatures depend on precipitation amount. The authors have to strengthen the discussion part by using these other papers. The investigation of the impact of humidity of branched GDGT distribution is not something totally new.

2) Were isoprenoid GDGTs also analysed? It would be worth showing the corresponding data and to look if some of the isoprenoid GDGTs are more abundant than some others in dry soils. Dirghangi et al. (2013) notably observed that crenarchaeol and its regioisomer (i.e. Thaumarchaeota) were very abundant in dry soils.

3) The authors found a correlation between the aridity index and the MBT'. Nevertheless, I am not sure the aridity index is very convenient, since mean annual precipitation and mean annual potential evapotranspiration data have to be available. The authors should comment on this. In any case, the validity of the correlation between the MBT' and the aridity has to be confirmed by analysing branced GDGTs in a large number of soils.

Detailed comments

Page 9046, lines 5-10. The authors have to specify in this part of the introduction that isoprenoid GDGTs are produced by Archaea and branched GDGTs by still unknown bacteria (even though Acidobacteria could be one potential source for branched

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GDGTs, as recently suggested by Sinninghe Damsté et al. (2011)).

Page 9046, lines 10-11. I disagree with this sentence, which has to be modified. Several studies clearly showed that branched GDGTs were also produced in situ in aquatic environments. Therefore, in lakes, branched GDGTs can originate from surrounding soils but can also be produced in the water colum and/or sediment.

Pages 9046, lines 13-15. This sentence is unclear and should be rephrased. I would simply say that MBT is correlated to air temperature and pH and that CBT mainly depends on soil pH.

Page 9047, lines 4-7. This sentence has to be clarified. The errors of the calibrations developed by Weijers et al. (2007) and Peterse et al. (2012) are similar: 5.0 °C for the original calibration by Weijers et al., 4.8 °C for the extended calibration by Peterse et al. Nevertheless, the temperature estimates based on the extended calibration seem to be generally more consistent with recorded temperatures.

Page 9047, line 15. The authors do not need to refer to the review paper by Schouten et al. (2013), since they already quote a large number of papers.

Page 9047, lines 16-24. As specified above, several other papers aldreay discussed the fact that other environmental parameters than temperature and pH could have an effect on branched GDGT distribution. This could be briefly mentioned in the introduction.

Page 9048, lines 7-9. When were the soil samples collected? Please also refer to Fig. 1a at the end of the sentence.

Page 9048, line 14. What is the UNEP? I do not think it is useful to provide the values of the aridity index in two tables.

Page 9049, section 2.2. I am wondering why the TOC contents were estimated by the loss on ignition technique and not by using an elemental analyser.

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Page 9049, section 2.3. Why do the authors use the abbreviation “GR” for the internal standard? Is the internal standard the same as the one described in the publication by Huguet et al. (2006)? Please specify how many extractions were carried out. Why did the authors use silica rather than alumina (as in most of the papers for the separation of core lipid GDGTs) for lipid separation?

Page 9051, lines 9-11. Were the residuals also calculated for the CBT?

Page 9051, lines 14-24. The concentrations of branched GDGTs reported by the authors are very low and are usually higher in soils (several μg/gTOC). Please compare your concentrations to those reported in soils in other papers. In tables 1 and 2, the samples should be listed in the same order in order to improve the readability of the paper.

Page 9052, lines 1-3. Please specify the average LOI % value at the end of the first sentence. Please refer to Fig. 4a, which indeed suggests that branched GDGT abundance is correlated with MAP.

Page 9052, lines 9-12. The authors should also discuss the fact that branched GDGTs were detected in all the samples analysed until now, whatever the origin (soils, peat, lakes, coastal environments. . . . These compounds are ubiquitous and it is therefore unlikely that the branched source microorganisms are the same in all the ecosystems. In any case, Sinninghe Damsté et al. (2011) only detected minor traces of one GDGT (Ia) in two Acidobacteria species, which suggest that these microorganisms are very likely only one potential source for branched GDGTs among others.

Page 9052, lines 18-20. Please refer to other papers who suggested the heterotrophic lifestyle of branched GDGT source microorganisms, e.g. Oppermann et al., 2010; Huguet et al., 2012; Ayari et al., 2013.

Page 9052, line 25. Branched GDGT Ic is also present in minor amounts.

Page 9053, lines 12-14. Which additional soil parameters not taken into account in this

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study could affect the CBT? Please specify.

Page 9053, lines 17-18. When were the air temperatures measured in the field? The day when the samples were collected?

Page 9053, lines 19-21. The MBT' vary from 0.08 (and not 0.09) to 0.59. Did the authors try to reconstruct the temperatures using the original calibration by Weijers et al. (2007)? What would be the difference with the temperatures estimated using the extended calibration by Peterse et al. (2012)? Do the temperatures reconstructed using the original calibration fit better with the instrumental data?

Page 9054, lines 3-6. This sentence has to rephrased: "MBT' and MATim show a weak but negative correlation (. . .) in contrast to the positive correlation between MBT and MAT observed by Weijers et al. and Peterse et al.". The authors state that the correlation between the MBT' and MATim for their sample set is significant, but did they perform any statistical test? If yes, please provide the p-value. If no, the correlation cannot be considered as significant.

Page 9054, lines 7-10. I disagree with this sentence. The problem is not that the temperature range over the authors' dataset is just above the MBT/CBT uncertainty (5 °C), but that there is no clear correlation between the MBT' and MATim. Indeed, the R2 is only 0.21, which is not sufficient to see any clear correlation.

Page 9054, lines 20-22. The present paper indicates that the environmental parameters controlling the distribution of branched GDGTs have to be investigated in other parts of the world and that the key parameters are not necessarily only temperature and pH.

Page 9054, lines 23-28. This paragraph is unclear and should be rewritten. According to section 2.3., the residuals were calculated by subtracting the estimated MAT (MATest) from the instrumentally measured MAT (MATim). Since MATim is systematically higher than MATest, then the residuals should be positive for all samples. Neverthe-

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less, they are negative for roughly half of the samples (Fig. 3d). This point should be clarified. According to the present version of Fig. 3d, MATest values are underestimated below 10 °C and overestimated for the temperatures above. This should also be clarified.

Page 9055, lines 1-2. There are other studies suggesting that changes in hydrologic moisture regime may have an impact of branched GDGT distribution, as suggested above. This should be taken into account into a revised manuscript.

Page 9055, lines 4-6. According to Fig. 2f, the slope of the correlation between MAPim and MBT' for the data from this study should be positive. Nevertheless, according to the equation presented in Fig. 2f, the slope is negative (-0.0003 x MAPim). This point needs to be clarified.

Page 9056, lines 21-23. The link between the two parts of this sentence is not very clear. In any case, the correlation between branched GDGT abundance and MAPim (R2 = 0.68; Fig. 4a) is only slightly higher than the one between branched GDGT abindance and MAPim (R2 = 0.60; Fig. 4c). This should be specified in the text.

Page 9057, lines 3-6. Please provide the data showing that AI can explain 50 % of the variance in MBT' index.

Page 9057, lines 6-8. The authors conclude that soil moisture availability, rather than precipitation, is the main factor controlling the MBT' index. This is logical, since branched GDGT source microorganisms live in soils.

Page 9057, lines 12-20. The authors should specify that other soils have to be analysed in order to confirm the relationship between the AI index and the MBT'. Other studies should also confirm the AI value under which MBT/CBT-derived temperatures can be biased by the low soil moisture content. These two points should be mentioned in the conclusions. The authors state that hydrological conditions should be evaluated through paleohydrological proxies before using the MBT/CBT for paleotemperature re-

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construction. Which paleohydrological proxies? Please give more details.

Page 9058, line 1. Even though the MBT' index was shown to be directly coupled with MAP, the authors should specify that this index is (indirectly) correlated with soil moisture. This is this parameter which is going to directly impact the distribution of branched GDGT-producing bacteria in soils.

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