General

This manuscript describes the brGDGT distributions in 23 soil samples from the Iberian Peninisula, a relatively arid region. This is a relatively small dataset compared to previous publications in this field. It also has been established before that brGDGT distributions in soils from arid regions respond differently to environmental parameters such as soil pH and MAT (Peterse et al., 2012; Dirghangi et al., Org. Geochem. 59, 49-60, 2013, not cited) but the authors push this a little forward by showing that the deviation of calculated MAT (using the global soil dataset) is negatively correlated with mean annual precipitation (MAP) and the aridity index (AI) and this aspect makes the dataset of interest. However, there are many issues with this manuscript that need to be fixed (see specific comments below). The title claims that water availability is the only control on brGDGT distributions, whereas the authors clearly show (although not surprisingly) that pH clearly effect their distribution (Fig. 1a) and the title should thus be changed. Furthermore, it is often not exactly clear what the authors have done (and why) and the use of statistics is not always straightforward. One also wonders why the authors have not done multivariate analysis to try to correlate the brGDGTs relative abundances in their sample set with envrironmental parameters suchs as pH, MAT, MAP and AI.

Specific comments:

P 9051, line 15. “original”? Not clear what is meant here.

P 9051, line 14. The concentrations of the brGDGTs are normalized on TOC but TOC was not measured. Instead TOC was “estimated” using a loss of ignition technique that has been tested for lake sediments but not for soils (Heiri et al., 2001). Since the authors claim that brGDGT concentrations are partially dependant on TOC abundance (p9052, line 3), MAP and AI (Fig. 4) they should just measure the TOC content of the 23 soil samples studies (not a big effort) or provide a calibration set for which they show that the LOI values of their soils shows a good correlation to the TOC content. Furthermore, the reported summed concentrations of brGDGTs (1-45 ng g^{-1} TOC) seem to be three order of magnitude lower than those reported in the literature (typically in the µg g^{-1} TOC range; e.g. Weijers et al., 2007; Kim et al. Limnol. Oceanogr. 55, 2010, 507–518; Yang et al., 2012; note that this is 2011 in ref list).

P 9052, line 13. Why is this sample considered to be an outlier? Its concentration is higher than that of the others but still low compared to literature data (see previous comment). There is no basis to consider it an outlier and it should be used in all regression analyses (e.g. Figs. 4a and 4b).

P 9052, line 5-16. These sentences should be carefully rephrased. Weijers et al. (2006) showed by determining the stereoconfiguration of the glycerol units that brGDGTs are most likely of bacterial origin. Based on their higher abundance in the catotelm of peat bogs they were speculated to be derived from anaerobic bacteria (Weijers et al., 2006). Subsequently, the application of molecular ecological tools showed that *Acidobacteria* (Weijers et al., Geomicrobiol. J. 26, 402-414, 2009) were omnipresent in these peats and were, thus, a potential source organisms of the brGDGTs. This was subsequently confirmed by the study of Sinninghe Damste et al. (2011) who identified brGDGT Ia in two species of *Acidobacteria* and detected the presence of the building block *iso* diabolic acid in all of the analyzed subdivision 1 and 3 *Acidobacterial* species. As concluded by Sinninghe Damste et al. (2011) this does not exclude that other bacteria are also producing brGDGTs. The authors conclude based on an absence of a relationship of brGDGT concentration (?) with pH (which was reported by

Weijers et al., 2007 (not cited) and Kim et al. 2010 (not cited), but not by Peterse et al. (2012) and Sinninghe Damsté et al. (2011)) that Acidobacteria are “not necessarily the main source of brGDGTs”. It remains entirely unclear where they base this rather vague conclusion on.

P 9052, line 26. One can only conclude that they are below the level of detection not they are not present.

P 9053, line 7. How can one say that “many CBT values in the Spanish dataset underestimate the pH”? I assume the authors use the Weijers et al. (2007) or Peterse et al. (2012) calibration to calculate “pH_{est}”? The way to go would be to show that the CBT-pH relationship for the Spanish soil dataset (Fig. 2a) is statistically different from the global soil correlation. Judging from Fig. 2a and knowing the substantial scatter in the global soil dataset I doubt this. If this cannot be demonstrated, there is no basis for an underestimation of the pH in the dataset (“CBT bias”) and the data of the Spanish soil sample set just follows the global soil dataset (which contains an order of magnitude more samples).

P 9053, line 16. This heading is completely misleading. One cannot calculate temperature from MBT’. The authors provide equation (5), from Peterse et al. (2012), that clearly contains MBT’ and CBT. This is because both pH and temperature effect the degree of methylation of brGDGTs in soils (see Weijers et al., 2007 for an extended discussion). I guess that the authors have used equation (5) to calculate MAT_{est}; if not they should explain clearly explain this. Assuming that they used equation (5) section 3.3 has to be rewritten completely (i.e. phrases like “an MBT’ value of X.XX relating to a MAT_{est} of Y.Y°C” should be avoided).

P 9054, lines 2-3. This is a really confusing sentence. First of all, I guess a correlation between MBT’ and MAT_{im} (not MAT_{est}) is meant. Secondly, I would basically conclude that there is no correlation between MBT’ and MAT_{im} (it is just a scatter plot and P is much higher than for all other correlations presented). Consequently, the sentences following this section (lines 6-11) should be skipped.

P 9054, lines 12-22. Again (like for pH_{est}) it should be explained how MAT_{est} values were obtained (probably through the use of the global soil correlation). The following sentences focus on just one of the 23 samples studied. I feel that the text should concentrate on general trends, not on individual cases. The authors also mention that they tried to perform a regional calibration but do not present any data. This paragraph ends with a very vague statement (first part of sentence) and some kind of general conclusion which should be at the end of the paper (and should be specified; other environments?).

P 9054, lines 23-29. This is an important section for the manuscript but it is not clear. The experimental section mentions that $MAT_{res} = MAT_{im} – MAT_{est}$. The clear observation from the data is that for the studied soil sample set $MAT_{im} > MAT_{est}$, except for one sample with a MAT_{est} of ca. 22°C (missing from Table 1?). However, when MAT_{res} is plotted in Fig. 3d both positive and negative values are observed. This is in contrast with Figs. 4c and 4d where MAT_{res} is always positive. Clearly, this issue should be solved. This issue comes back in line 4 of page 9055 (over or underestimated?).

P 9055, lines 13-14. Poor use of statistics. Here the correlation between MAP_{im} and MAT_{im} is called weak, whereas R^2 is substantially higher that for the relationship between between MBT’ and MAT_{im}.

P 9055, line 21. I don’t think one needs the insert. In fact, one can argue that plotting the data from the global soil data set does not really help to get the message across.

P 9055, line 26 - P 9056, line 3. See earlier comments on concentration: the reported concentrations of brGDGTs are very low in comparison with other soils and one wonders if this relates to the deviation from the global soil dataset.

P 9055, lines 15-20. I believe one cannot make any conclusion based on soil type. The sample set (23) is too small and the number of type of soils (14) is too high for that.

P 9055, lines 21-23. I have read this sentence numerous times but I really don’t understand the argument.

P 9057 Figure 4 really forms the message of this manuscript, i.e. the deviation of calculated MAT (using the global soil dataset) is negatively correlated with MAP and AI. This message would, however, be much stronger if the authors could demonstrate a direct relationship between brGDGT distribution and rainfall and aridity. In the way they present it now it heavily relies on the calculation of MAT using a correlation that they claim does not work (at least not for their region). If rainfall and aridity exert such a strong control on brGDGT distribution as claimed by the authors, it should be possible to reveal this in an independent way.