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Interactive Comment

Interactive comment on "Distributions of glycerol dialkyl glycerol tetraethers in surface soils of Qinghai–Tibetan Plateau: implications of GDGT-based proxies in cold and dry regions" *by* S. Ding et al.

Anonymous Referee #3

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This study investigates the distribution of GDGTs in 33 soils on the Qinghai–Tibetan Plateau (QTP). Correlation with environmental parameters show strong correlations of BIT and CBT with soil pH and weaker correlations of MBT with MAP and MAT. The data of the QTP are then added to previously reported GDGT data from Chinese soils to arrive at a regional calibration of soil GDGT with MAT.

The paper reports useful data for the community working with branched GDGT but I can not really see what novel insights have been obtained by this work. One of the main conclusions from this work is that BIT and CBT show a strong correlation with

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soil pH. This has been observed in many other studies, some of them cited here (but not all, see below), and thus comes as no surprise. The authors also find a weaker relationship of MBT/CBT with MAT and with MAP, again as also observed in many other studies. The regional calibration was achieved by adding their 33 data points to the 164 from other study, with the far majority coming from Yang 2014. This is not a real significant expansion of the current (Chinese) data set and it is also not clear to me what new information has been obtained by this exercise. A final complication is that comparisons between calibrations are not statistically tested to see if differences observed are significant or not.

Another issue is the framing of this study. From the introduction it is stated that the QTP is an important climate-sensitive area for which several climate archives exist. Regarding organic proxies, and in particular GDGTs, this seems mainly related to lake archives on QTP. Hence, for validating past climate records I would expect a study of lake sediments of which there have been several previously done at QTP (cited in the introduction), so a relatively well studied topic. However, the authors investigated the controls on GDGT distributions in soils rather than lakes. Since many studies have now shown that lakes often have a large in situ overprint, it is unclear how this study is going to help interpret lake climate archives on QTP. In other words, how can the soil correlations obtained be applied to climate archives on QTP ? This is not coming back in the discussion nor in the conclusions. So the framework does not make sense to me.

A final important issue is that these are soils from a fairly arid region for which it is known that MBT-CBT applications can be difficult. This is acknowledged by the authors and they cite several papers which have observed this problem. However, this problem has partly been resolved by De Jonge et al., 2014 (GCA), i.e. arid soils are problematic because of the interfering abundance of 6'-methyl brGDGTs. These GDGTs coelute with the regular 5'-methyl brGDGTs under the LC conditions used by the authors and therefore cannot be quantified separately. I think for this study site it is essential to quantify the impact of 6’-methyl brGDGTs as it can sometimes strongly improve correlations between branched GDGT and environmental parameters (see also recent paper by Yang et al in press with Organic Geochemistry).

In summary, I find it difficult to recommend this manuscript for Biogeosciences as it provides no real new insights, adds little to regional (Chinese) calibrations and ignores the issue of co-eluting 6’methyl branched GDGTs which potentially can explain part of the patterns observed.

Other minor issues.

p. 483, l.16. Peterse et al 2012 is about GDGTs, not the methods mentioned here. Replace with references where these caveats are discussed.

p.483, l. 17. ‘In addition’ sounds strange as the sentence discusses a positive application rather than a caveat.

p. 483, l.26 . . . .lakes, although they. . .

p. 484, l. 7. According to their chemical structure. . .

p. 484, l. 10-15. A more nuanced BIT distribution can be found in Schouten et al. 2013, OG where endmembers are different than based on older literature.

p. 485. Can all these calibration equations not simply be summarized in a Table ?

p. 485. l.8. It is impossible to say if proxies are successful, what you can say is that they have been applied. Furthermore, the lake situation is quite different from that of soils, i.e. different calibrations are used because of in situ production (e.g. Tierney 2010, Pearson 2011, etc.). This is different from a ‘regional calibration’,i.e. the MBT/CBT is not used but rather transfer functions of individual GDGT concentrations. This should be made clear in the introduction right here, and not just briefly at p. 486.

p. 485, l. 20. Blyth and Schouten was not using soils but stalagmites.

p. 487, l. 7. In the Suppl. Table the range in MAT is much wider than simply 0.1 C. Importantly, how many weather stations are on the QTP and how were the temperature corrections for adiabatic laps rates done ?

p. 489. l. 10. I see there has been no correction for the difference in ionization efficiency of the internal standard (a C46 GTGT) compared to GDGTs. This can be quite variable and substantial (see Huguet et al., 2006). Hence, you do not have real absolute concentrations but estimated based on the (likely incorrect) assumption that the internal standard has the same response factor as GDGTs. Please note this caveat.

p. 490, l. 15. Please refer to the recent compilation of Schouten et al 2013 which showed that the average BIT value is 0.90+/- 0.14.

p. 490. l. 20. I do not know of any recent studies who use the BIT in lakes to trace soil OM because of the now well-known large in situ production in lakes. Hence this 'warning' is unnecessary.

p. 490. L. 2. Cite also Kim et al. 2010 L&O who found lower BIT values at pH>7 in French soils. Your Fig. 3a looks exactly like their Fig. 6.

p. 492. I wonder in how far the RDA results are affected by the limited number of soils and also the specific range in pH, temperature and MAP of QTP. Compared to the different global data sets the range in temperature is relatively small (<10 C). In particular, the global calibrations shows at MAT<15 C a pronounced heterogeneity. Can it therefore not be expected that MAP plays a more important role than MAT ?

p. 493. l. 3. Please perform a statistical test to see if the slopes are significantly different between the different calibrations.

p. 494, l. 15. I think the smaller RSME of your QTP calibration compared to the global calibration is artificial. This is because the temperature range of your data set is much smaller than that of the global calibration (ca. 10 C versus 30 C). So, an error of 2.4 C on a range of 10 C is worse than that of 5 C RSME on a scale of 30 C. This should be discussed more fairly.

p. 495. l. 26. Perhaps a better way to test this is to see statistically if the correlations for QTP are significantly different from the global correlation or the Yang et al. calibration. Note also that interlaboratory differences between labs may play a big role in the added scatter with the Yang et al 2014 data set mostly coming from a single lab.

p. 496, l. 8-17. This part is not clear to me. First I wonder if the slightly better correlation is really statistically significant or just because the temperature range seems larger for this season. I also do not understand on what data the conclusion is based that "more variation in brGDGT compositions occurs in winter". And I also do not see the connection with soil respiration as stated in the next sentence. This should be clarified.

Table 1. Considering the uncertainty in the quantification of branched GDGTs (see previous comment), the numbers should not be reported with a decimal point.

Interactive comment on Biogeosciences Discuss., 12, 481, 2015.