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Interactive comment on "Provenance of branched GDGTs in the Tagus River drainage basin and its outflow in the Atlantic Ocean over the Holocene" *by* Lisa Warden et al.

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We appreciate the positive assessment of the referee and thank him for the positive criticism of our manuscript. Below we explain how we will respond to these comments.

1) More information on the nature of the sediments used in this study would be useful. The conclusions reached are often dependent on the nature of the sedimentary material, yet there is little in the text or supplementary information to convey this information. I would suggest a supplementary table of sedimentary properties and/or a core description or core photograph and comments on the uniformity or bioturbation of the samples.

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Discussion paper

We see how this additional information on the nature of the sediments for the cores from this study could be helpful. In the supplemental of the revised manuscript we will include this additional information such as core photographs and core descriptions when possible and a reference to the core description in the case that it has already been previously published.

2) Line 573 talks about the correlation between BIT and 13C, and claims a moderate negative correlation. I feel that this correlation may be overstated. While it would be expected that 13C and BIT show similar trends, their relationship is not necessarily linear (see for example Sparkes et al., 2015, Biogeosciences). The offshore core samples all have very low BIT values, yet have outliers with very negative 13C values. The terrestrial core and soil/sediment/SPM samples have significantly higher BIT values, but overlapping 13C values. If the terrestrial samples are looked at on their own they have very poor/no correlation between the two measurements - within the terrestrial samples, $r2 = 0.02$. Within the offshore samples (mudbelt + canyon head + canyon) $r2 = 0.1$, and so what the graph is actually showing is that terrestrial and marine are different, but an r2 value is not showing much about the nature of that difference. I do not feel that this correlation plot adds any value to the argument, and if it is included for reference/information then perhaps the authors should comment on why the two measurements, which should correlate quite well, are actually a poor match.

We agree with the reviewer that there is a poor correlation between the BIT index and the δ13C values for the individual datasets. Therefore, in the revised manuscript we will remove that figure and the corresponding sentence description in the text since as the reviewer pointed out and we agree that it does not add much value to our arguments.

3) Line 586 assumes that soil-derived brGDGTs from across the catchment are delivered through the river to the marine sediments. This is quite an assumption, since there is the potential for both deposition and degradation in the river and coastal systems, even before the river was dammed in the 20th Century. There is also the question of connectivity between the river and canyon. River outflows contain freshwater, which

is buoyant above salty seawater. Unless the sediment load in the river is particularly high, the sediment will disperse as a plume above the seawater rather than running down the canyon itself. Therefore the majority of the brGDGTs delivered from the river will be spread over the shelf rather than following the canyon down to 4000m. Do the authors know the phase/grainsize in which brGDGTs are travelling? Are they associated with larger grains, that would deposit quickly beside the river mouth, or fine grains that will disperse across the ocean? In the conclusion the authors state that most terrestrial material is not making it out the the ocean, and therefore palaeotemperature reconstructions will be difficult with datasets such as this one. I agree.

The referee questions whether soil-derived brGDGTs from across the catchment are delivered through the river to the marine sediments and rightly so. However, we do not see the problem here because this was actually one of our main research questions that was tested in this study (see introduction). Previous studies that have been performed on this system have shown that little terrestrial material makes it down the canyon from the river (e.g. Jouanneau et al., 1998; de Stigter et al., 2011; Vis et al., in press) and most of the terrestrial material is being trapped inside the estuary even though the outflow from the Tagus River does form a plume that dominates the continental shelf (Jouanneau et al., 1998). Our results for the brGDGTs in Holocene sediments confirm this. For this study we did not look at grainsize of the particulate matter by which the brGDGTs are traveling from the continent to the open ocean. Although that would be an interesting topic of study, it is not one we pursued since as stated in the study, even if the terrestrial material were making it out to the canyon sediments, in-situ production in the river and marine systems is complicating the use of brGDGTs for MAT estimates in this region as is the fact that brGDGTs cannot be used for accurate MAT estimates even on the soils from the region. In marine sediments where brGDGTs would reflect the distribution of the soils in the watershed of the river the dependence of particle size on brGDGT distribution would certainly be an interesting line of research.

4) Line 517 concludes that in-situ production is a problem for all sample sets. This could have profound implications for all brGDGT based proxies. The authors should expand on this point.

We agree with the referee that this has profound implications for the brGDGT based proxies in this system as well as others where it has been shown that in-situ production is an issue. We believe, however, we have made this clear as we mention it several times throughout the manuscript;

Lines 556-561 However, even though the sum of the brGDGTs are lower in the marine sediment than in the Tagus River Floodplain sediments, the amount of brGDGTs in all four sediment cores are higher than in the Tagus soils (~6.8±6.5 μg gOC-1) indicating the origin of the brGDGTs in the sediment cores are not all soil derived and pointing instead to riverine in-situ production as well as possibly in aquatic sediments (Fig.3b).

Lines 626-628 Additionally, we confirm the findings of Zell et al. (2014; 2015) that in-situ production of brGDGTs is occurring in the river and marine systems of the Tagus River basin and go on to show that there are indications that it occurred in the past as well.

Lines 650-653 Because of these unique features in this region, perhaps the development of a local calibration could assuage difficulties in using brGDGTs as a paleoclimate proxy for soils in the Tagus River basin. This would not, however, solve the issue of in-situ produced brGDGTs overwhelming the amount of soil derived brGDGTs in aquatic sediments.

Minor comments / typographical errors:

Line 232 – column Line 288 – Section numbering error Line 344 – Figure 4c should be 5c Line 360 – BIT is given as 0.1 0.0. Would increasing the significant figures be helpful here? Line 478 – Do the authors have p and n values for this correlation? Not a p value but n value, it is for the entire data set so n=109. Table 3 sample Lisbon

Canyon Head 1cm has a 13C value of 22.9, should be -22.9

We thank the reviewer for pointing these out and these minor things will all be fixed in the revised version.

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