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Interactive comment on "Assessing branched tetraether lipids as tracers of soil organic carbon transport through the Carminowe Creek catchment (southwest England)" *by* Jingjing Guo et al.

Anonymous Referee #3

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Review of "Assessing branched tetraether lipids as tracers of soil organic carbon transport through the Carminowe catchment (southwest England)" by J. Guo. Biogeosciences Discussions, 2020.

The authors of this paper aimed to use brGDGTs as soil OC tracers in a small catchment located in southwest England and compared the concentration and distribution of these lipids in soils under different land use, riverbed and lake sediments. They showed that the relative abundance of brGDGTs does not significantly differ between soils under different land use and that brGDGTs in the riverbed and land sediments

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are mainly produced in situ (in the water column and/or sediment). Therefore, they cannot be used as soil OC tracers in this specific catchment. The analysis of brGDGTs and isoGDGTs along a lacustrine sediment core covering the last 100 yrs additionally showed that the distribution of these lipids (the degree of cyclisation of brGDGTs and the ratio of isoGDGT-0 vs. crenarchaeol) is roughly consistent with eutrophication changes over this period of time.

This study is of interest, as it is comprehensive and one of the few comparing extensively 5- and 6-methyl brGDGT distribution in soils under different land use, river and lake sediments. The paper is well-written and easy to read, and to my mind deserves publication in Biogeosciences after some revisions. The authors should sometimes be more moderate in their assertions and should avoid overinterpreting the data.

The following comments should help in improving the manuscript: Line 13: Here, the authors mention the fact some tracers are required to quantify the fluxes of soil OC. Nevertheless, brGDGTs would be more qualitative than quantitative tracers. Therefore, this sentence should be modified.

Lines 52-53: Here, I would directly say that brGDGTs are ubiquitous lipids, present in terrestrial and aquatic environments, and thus not necessarily specific soil tracers.

Line 54-55: This sentence should be rephrased, as only some of the brGDGT producers may belong to the phylum Acidobacteria. As brGDGTs were detected in various settings, it seems unlikelly that they are produced by the same microorganisms everywhere.

Lines 77-93: It should be clearly mentioned somewhere that BIT index can be largely biased by in situ production of brGDGTs in aquatic settings (which was not taken into account in the initial hypothesis by Hopmans et al. 2004) and therefore should be applied with caution in coastal and lacustrine settings.

Lines 82-90: These two studies are restrictive and specific. Other examples of studies

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dealing with brGDGT in situ production should be mentioned here (Miller et al., 2018, Climate of the Past; Loomis et al., 2014, GCA; Buckles et al., 2014; Biogeosciences etc.). Please also mention that in situ production of more cyclized but also more methylated brGDGTs is generally observed in aquatic vs. terrestrial settings.

Lines 100-102: In order to trace soil OC with brGDGTs, these lipids should be mainly derived from soils, with only reduced in situ production. Such an assumption should be clearly specified.

Lines 160: Were some samples analysed in replicates?

Lines 172-181: IsoGDGT-0 concentrations are only reported for the lacustrine sediments. What about the soils and the riverine sediments?

Line 227: principal component analysis instead of principle component analysis

Line 236: In Fig. 4b, a lot of samples are outside the circles (the 3 groups of soils) and do not overlap. This should be acknowledged.

Line 251: Regarding the turnover of brGDGTs in soils, please also refer to the publication by Huguet et al. (2017, GCA), with turnover times between 8 and 41 years in the same range as Weijers et al. (2010).

Lines 269-270: please specify the 2 transects along which large spatial variations in BIT are observed. T1 and T2 ? All the discussion about spatial variations in BIT and soil moisture remains very speculative. How can you explain that these variations occur only along 2 transects? What about the other transects? Are they any in situ measurements of soil moisture available to strengthen the argumentation? Or measurements in the lab (after having dried the soil samples)?

Line 277: similarly, please specify the 4 transects along which large spatial variations in IR index are observed.

Lines 281: Is the relationship between the relative abundance of 6-methyl brGDGTs

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and pH given for all the soils of the catchments or only those of the 4 transects previously mentioned?

Lines 281-283: similarly, please specify to which soils correspond the different pH values (those of the 4 transects, the total dataset etc).

Lines 321-322: In addition to Congo, brGDGTs are also mainly derived from soils in other large riverine systems such as the Amazon (Kim et al., 2012, GCA) or Rhône river (Kim et al., 2015, Frontiers in Earth Science).

Line 326: why whould brGDGTs would be degraded more rapidaly in soils than in aquatic settings? This sentence should be removed as it appears too speculative.

Line 348-349: as said above, the identity of the brGDGT producers remains elusive in soils as well.

Lines 352-358: I do not see the interest of this part of this discussion on the ecological niches of brGDGTs producers in Loe Pool as it is totally speculative and has no direct link with the main aim of the paper (using brGDGTs as soil OC tracers).

Lines 359-389: in this section about local environmental changes, what about reconstruction of past temperature/pH variations with brGDGT-based indices? It would be complementary to the discussion about the lake eutrophication.

Lines 385-387: The authors should also mention the in situ production of isoGDGTs in deep lacustrine sediments, as it could bias the signal recorded in the sediments.

Lines 395-397 : I would rephrase this sentence. There is no direct evidence that soil moisture exerts a control on brGDGT distribution here and the variations in BIT were observed along 2 transects only.

Line 401: Please replace “replaced” by “mixed”, as the soil brGDGT signal is not replaced by the aquatic brGDGT signal, the two signals are mixed in the sediment.

Lines 407-410: please be more moderate here, as the interpretation based on

brGDGTs is purely qualitative and complementary to previous data. I would rather say that the trends derived from GDGT data are roughly consistent with the historical record of lake eutrophication.

Lines 411: this sentence should be modified, as in the case of the Carminowe Creek catchment, this study clearly showed that brGDGTs do not record land management change and that in situ production dominates in the riverine system.

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