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Discussions

Interactive comment on "Predominance of hexamethylated 6-methyl branched glycerol dialkyl glycerol tetraethers in the Mariana Trench: Source and environmental implication" *by* Wenjie Xiao et al.

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Interactive comment on "Predominance of hexamethylated 6-methyl branched glycerol dialkyl glycerol tetraethers in the Mariana Trench: Source and environmental implication" by Wenjie Xiao et al. Anonymous Referee #2 Received and published: 30 December 2019

The manuscript by Xiao et al. describes the distribution of branched tetraether lipids in Mariana Trench sediments. Very few studies to date have investigated the organic

geochemistry of deep-sea trench sediments and even fewer have studied the distribution of tetraether lipids. The study presented by Xiao et al. thus is a novel contribution to the field. Specifically, the authors show that the distribution of branched tetraethers is unique when compared to previously studied environments. I have two major criticisms, or suggestions, that the authors should consider in preparing a revised version.

1) The uniqueness of the study site is both a strength and a weakness of the presented work. It is a strength, as the remote setting may allow distinguishing marine in situ production from a terrestrial origin of brGDGTs that muddles interpretation of shelf sediments. However, it is a weakness because it is unclear how comparable the site is to continental shelf sediments. This is regardless of whether brGDGTs originate from sediments or from the water column, since factors such as nutrients, particle loading, bacterial community composition, oceanographic parameters (oxygenation, salinity, currents etc.) will vary between the shelf and trench sediments and between shelf water column and the pelagic water column above the trench. These points are particularly important since it remains unresolved whether brGDGT production in the ocean originates from the water column or sediments, or both, and which bacterial clades synthesize brGDGTs. The authors should address these caveats in their manuscript. A good place to discuss these issues would be between lines 374-400. This discussion should then be reflected in the revised abstract.

Response: This is a good suggestion. Although we have mentioned this point, we did not explain it systematically. So, in the revised manuscript, we accepted the reviewer's suggestion and added this content in session 4.3. We added a paragraph as "The unique feature in the composition of brGDGTs in the Mariana Trench has significant implications on the brGDGTs-derived proxies. As the remote setting from the landmass, the Mariana Trench provides an opportunity to distinguish marine in situ production from a terrestrial origin of brGDGTs that muddles interpretation of shelf sediments. However, at the current stage, it is unclear how similar and different in brGDGTs-producing microbes as well as their response behaviors to ambient environments be-

tween the Mariana Trench and continental shelf sediments. In addition, the weight contribution to brGDGTs from sediments or water column remains elusive. Since factors such as nutrients, particle loading, bacterial community composition, oceanographic parameters (e.g., oxygenation, salinity, currents) vary significantly between the shelf and trench, the organisms to biosynthesize brGDGTs are likely different between two marine settings. Therefore, it should be caution to apply the MBT/CBT and BIT proxies in the open ocean." We also updated our abstract in the revised manuscript.

2) I fundamentally disagree with the use of soil calibrations for reconstructing seawater, or porewater, pH and temperature (lines 360-372). There is no evidence supporting the applicability of these calibrations. Therefore, the brGDGT-based pH reconstructions cannot be used as evidence for in situ production. Specifically, it is currently unclear if the clades of bacteria producing brGDGTs in soils are similar to those in the ocean, particularly because the perceived adaptation of branched GDGT distributions to environmental parameters has previously been suggested to be a community effect.

Response: We accept reviewer's suggestion. It is true that the brGDGT-based pH and MAT reconstructions were established based on terrestrial samples (soil and peat). Thus, MBT, CBT and modified parameters cannot be used as evidence for in situ production. So, we have deleted relevant results and discussion. We also deleted the figure 7 and figure 9b, c, and d that are related to global calibration of brGDGTs-proxies and temperature/pH. Please see lines 15, 137-140, 275-279, 374-380 and 423-427 as well as our response to the reviewer 1.

3) I think that the number of figures in the main text could be reduced in order to streamline the manuscript. I suggest moving figures 3-5 to the supplement.

Response: Since both reviewers mentioned too many figures, and unconvincing calibration of brGDGTs-proxies in ocean, we removed figure 7, 9b, c, and d in the revised manuscript. However, we kept the figs 3-5 because reviewer 1 think the most novelty of our work is the first report on the absence of 5-methyl brGDGTs and strong pre-

dominance of 6-methyl brGDGTs in marine sediments. We agree with the reviewer 1's statement. In order to determine the source of brGDGTs, the information on bulk geochemical parameters of total organic matter and molecular composition of brGDGTs is greatly needed. Taken together, we still keep the figs. 3-5 in the revised manuscript, but deleted figs. 7 and 9 that are about global calibration of brGDGTs-derived proxies with temperature and pH. The detailed explanation for removal of these figures can be found our response to the reviewer 1's comment.

4) Finally, I recommend the authors carefully examine their manuscript to fix multiple typos and grammar.

Response: Thank you for pointing out this issue. All authors checked the grammars carefully and made the corrections if necessary.

Additional comments: 1) All figures: To ensure accessibility, please use color blind-friendly colors, e.g., do not use red and green in the same figure.

Response: Thank you for the suggestion. We have changed the color of figures.

2) Line 8-24: Please correct grammar issues throughout the abstract

Response: We checked the grammars carefully and made the corrections in abstract. Please see the revised manuscript for the details.

3) Line 16: Please specify that δ13C values are for OC

Response: We have specified δ13C to δ13COC.

4) Line 28-30: Please consider finding more appropriate citations. Sinninghe Damste 2000 and Weijers et al. are neither the first to report on iGDGTs/brGDGTs nor are these the most comprehensive papers.

Response: We have replaced previous citations with Schouten et al., 2013, which is the most comprehensive and cited papers about GDGTs.

5) Line 120-122: Without response factors of GDGTs relative to the C46 standard, concentrations cannot be determined. Please report concentrations as response units or peak areas normalized to OC.

Response: This is a good comment. In the revised manuscript, we added the sentences as “Since all brGDGT isomers were assumed to have an identical response factors on the instrument, our analytical method is better regarded as semi-quantification.” (Line 289-290). However, it is common to report the concentration of GDGTs in gram per dry weight sediments or OC even without consideration of response factor in,literatures. So we still keep the current format for the concentration.

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