Biogeosciences Discuss.,
doi:10.5194/bg-2016-529-AC2, 2017

Biogeosciences
Discussions
Open Access

Interactive comment on “Glycerol dialkyl glycerol tetraether variations in the northern Chukchi Sea, Arctic Ocean, during the Holocene” *by* Yu-Hyeon Park et al.

Yu-Hyeon Park et al.

parky@pusan.ac.kr

Received and published: 13 February 2017

Thank you very much for giving us helpful comments. We agree with most of your comment. We will revise our manuscript according to your comments.

Comment: Taking the amplification of global change in Arctic settings into account, there is merit in exploring downcore variability of GDGTs in the proposed setting. The results have been reported in a clear manner. However, the discussion is not complete yet, and needs to be expanded significantly before publication. Rewriting the discussion will be necessary mainly to include a number of studies that have discussed the production and preservation mechanism of GDGTs in marine shelf sediments. At this moment, the authors have based their entire discussion on one manuscript, i.e., Park

Printer-friendly version

Discussion paper

et al. 2014. Although this manuscript is well-chosen, as it discusses the same study area, the authors discuss mechanisms that have been shown before in Arctic shelf seas, but also in shelf seas globally. Also, previous papers that discuss the problematic T-dependence of iGDGTs in Arctic shelf seas have been ignored. I have included several references to important studies below. I suggest major revisions before this manuscript can be accepted for publication. A number of technical remarks are also made below.

Reply: Thanks for your recognizing the significance of our study. We will expand discussion based on many other references and improve our manuscript.

Remarks: L 77-104. This part of 'material and methods' should be expanded and placed within the introduction. The introduction contains almost no information of the production and environmental drivers on iGDGT and brGDGTs.

Reply: All right. This part will move to introduction.

L 78. Specify here that this concerns brGDGTs from marine sediments, specifically river fan sediments.

Reply: You are right. We will specify the setting, "river influenced sediments".

L. 92. Rephrase and replace 'Measured' by 'determined the concentration'.

Reply: We will replace to "determined the concentration".

L. 114. The description of core 01AGC and 08JPC as shelf cores does not mention the very different distance to the continent, or differences in the source of OM delivered to the surface sediments at these sites from previous studies.

Reply: Distance from each of two cores to continental is very different. The distance from location of 01AGC to continent (ca. 500 km) is much far than those of 08JPC (ca. 50 km). We will include this description.

L. 177. Here, the internal standard has to be mentioned. Also, was the concentration standardized against TOC? Or against g sediment? This has to be made clear.

Reply: We used C46 GTGT internal standard. The concentration is expressed as basis of gram of sediment. We will add this explanation.

L. 102. A proxy for soil OM contribution in river fan sediments (as brGDGTs in more distal marine sediments can be dominantly in-situ produced).

Reply: We will add "in river influenced sediments".

L 191-192. Rephrase, this sentence is not correct.

Reply: we will rephrase to " ....during middle and late Holocene. The same pattern also appeared in...."

L. 194. Replace 'show a variability' with 'change'.

Reply: we will replace to "change".

L 200. Only two cores have values for sediments older than 9ka.

Reply: we will revise to "two cores (01A-GC and 05JPC)".

L 201 and further: it's very useful for the reader if you could refer to the cores as 'shelf cores' and 'slope cores', to refresh the reader memory in-text. Further in the text, I recommend to distinguish between core 01A and core 08JPC, by referring for instance to distant shelf and near shelf. This is much more informative than using only the core names.

Reply: Thanks. We will revise them.

L 203-204. This decrease is not clear to me, based on the Fig. 5.

Reply: Indeed. We delete the phrase.

L 209. All GDGT are 'cyclic' compounds. What you mean to say is the GDGTs that don't contain a cyclopentane moiety. Here you could also mention that the compounds

IIa, IIb, IIc and IIIa, IIIb, IIIc are measured co-eluting with their 6Me counterparts (IIa', IIb', IIc', IIIa', IIIb', IIIc'). Be consistent and refer to Ia, IIa and IIIa as such (and not suddenly as I, II, III).

Reply: Indeed. We will revise the names as you commented.

L 212. With fractional values?

Reply: Yes, it is "fractional" values. We will add it.

L 217. Mention what the concentration was normalized against (g of dry sediment?)

Reply: The concentration was normalized against g of dry sediment.

L 250. In my opinion it is biased and not correct to explain the observed results in the discussion using only one manuscript (i.e, Park et al., 2014). I urge the authors to expand the number of manuscripts consulted for this discussion. It would be more logical to start the discussion with the novel data presented in this study and to move the references to the conclusion from Park et al. (at L. 250-252 and L. 284-292) further back.

Reply: Thanks for this comment, we will reorganize the order of discussion according to your suggestion. We also add the other reference papers in the discussion.

L 275. Another study in Arctic shelf sediments that concludes a negative effect of sea ice cover on iGDGT production is Sparkes, R.B., DoÄSelver, A., Bischoff, J., Talbot, H.M., Gustafsson, ö., Semiletov, I.P., Dudarev, O.V., van Dongen, B.E., 2015. GDGT distributions on the East Siberian Arctic Shelf: implications for organic carbon export, burial and degradation. Biogeosciences 12, 3753–3768.

Reply: Thanks. We will add this study (Sparkes et al., 2015).

L 275. Does the proposed effect of sea-ice cover fit with the concentration changes in the third core (distant shelf, 1A-GC)? There has been a (more recent?) decline of sea ice at this site as well, but no clear increase in iGDGT distribution. Does this favor the last hypothesis, that the iGDGTs are transported to the core sites by the Bering Strait inflow (whose course may also influence/be influenced by changes in the extent of sea ice cover through time).

Reply: Very recently Stein et al. (2016) report the IP25 and PIP25 indices in the 01A-GC core, showing a maximum of PIP25 around 7-6 ka. The iGDGT peaked around 5-6 ka. We can see the offset again in the case of 01A-GC. We will add the discussion on 01A-GC in the revised manuscript.

L 291, 292. The discussion very quickly links the CBT signature to different source organisms, without explaining the GDGT distribution produced by marine producers, and how this allows to contract with continental material. It is complex that the terrigenous signal is found further offshore than the marine signal, and this has to be explained better to the reader.

Reply: Because this interesting phenomenon was described in our previous paper (Park et al., 2014), we did not explain in detail in this manuscript. Thanks to this comment, we realize that the explanation is necessary and we will add the explanation about this phenomenon in more detail according to the interpretation of Park et al. (2014). We suppose that more organic matter supply enhances the production of branched GDGTs in relatively shallow marine environments (Yamamoto et al., in press, Quaternary International, http://dx.doi.org/10.1016/j.quaint.2016.06.024)

L 288-292. There are a number of studies that have shown the in-situ production of cyclopentane-containing brGDGTs in Arctic sediments (i.e. De Jonge, C., Stadnitskaia, A., Cherkashov, G., Sinninghe Damsté, J.S., 2016. Branched glycerol dialkyl glycerol tetraethers and crenarchaeol record post-glacial sea level rise and shift in source of terrigenous brGDGTs in the Kara Sea (Arctic Ocean). Organic Geochemistry 92, 42–54; Peterse, F., Kim, J.-H., Schouten, S., Kristensen, D.K., Koç, N., Sinninghe Damsté,J.S., 2009. Constraints on the application of the MBT/CBT palaeothermometer at high latitude environments (Svalbard, Norway). Organic Geochemistry 40, 692–699) andglobaly (Sinninghe Damsté, J.S., 2016. Spatial heterogeneity of sources of branched tetraethers in shelf systems: The geochemistry of tetraethers in the Berau River delta (Kalimantan, Indonesia). Geochimica et Cosmochimica Acta 186, 13–31 and references herein). This list is non-exhaustive, and ALL relevant studies have to be read and referenced to in a valid discussion. The discussion also refers to a separate mechanism, the preferred conservation of soil OM compared to marine OM, which has fi been discussed in De Jonge, C., Stadnitskaia, A., Hopmans, E.C., Cherkashov, G., Fedotov, A., Streletskaya, I.D., Vasiliev, A.A., Sinninghe Damsté, J.S., 2015. Drastic changes in the distribution of branched tetraether lipids in suspended matter and sediments from the Yenisei River and Kara Sea (Siberia): Implications for the use of brGDGT-based proxies in coastal marine sediments. Geochimica et Cosmochimica Acta 165, 200–225. The discussion also lacks a part where the absolute CBT values are compared to those of other studies in Arctic sediments, comparing a typical terrigenous and marine signal, and evaluating whether the observed distribution fits an Arctic signal, or whether a significant is derived from the Bering Strait inflow (for instance Park et al., 2014, references above, Hanna, A.J.M., Shanahan, T.M., Allison, M.A., 2016. Distribution of branched GDGTs in surface sediments from the Colville River, Alaska: Implications for the MBTâAËŸ š/CBT paleothermometer in Arctic marine sediments. J. Geophys. Res. Biogeosci. 121, 2015JG003266).

Reply: Thank you. All right. We will include the findings of the above papers in the discussion.

The results section introduces an increased amount of brGDGT IIb, but this is not discussed further. The references provided above will help including this observation in the discussion. The MBT values are also not compared with other studies from Arctic shelf seas with/without terrigenous impact. Here, an apparent weakness in this manuscript becomes obvious, as brGDGT IIIa as represented here, includes both brGDGT IIIa', a pH-sensitive compound that is produced in the marine environment, and brGDGT IIIa, that is generally not associated with marine in-situ production (see

De Jonge et al., 2014). This should be mentioned when expanding on the current discussion on MBT values. The authors postulate that the brGDGT signal is possibly dominated by a terrigenous signal in sediment formed under ice-covered conditions. Does the GDGT signature fit with a terrigenous source (i.e., is the reconstructed MAT and pH realistic?)

Reply: Yes, it does. The GDGT signature in the sediments under sea ice fits with a terrigenous source (Park et al., 2014). We did not separate IIIa and IIIa' in this study but recognize its importance. We will touch on the issue in the revised manuscript.

L 323. There have been a number of studies that have concluded that the TEX86 does not correlate with temperature in Arctic regions (for instance Ho, S. L. et al. Appraisal of TEX86 and thermometries in subpolar and polar regions. Geochimica et Cosmochimica Acta 131, 213–226 (2014), Tierney, J. E. Tingley, M. P. A Bayesian, spatially-varying calibration model for the TEX86 proxy. Geochimica et Cosmochimica Acta 127, 83–106 (2014)). These should be mentioned in the conclusions. Perhaps it makes more sense to discuss the iGDGTs separately (i.e., GDGT0 and crenarchaeol), when discussing the observed variability in 'low sea-ice low terrigenous input' strata?

Reply: Please give us more time to consider this issue. We discussed this issue in Park et al. (2014) but we recognize further advance by the above papers. We will discuss it more carefully by citing the papers.

Fig. 2. Indicate in caption and figure what the amount of GDGTs is standardized against (ng/g of what?). All panels in this Fig are are very close reproduction from Park et al. (2014). In my opinion, it should be sufficient to simply refer to the study itself, without including this data in a separate Fig.

Reply: We will delete Figure 2.

Fig. 5. Here, the Fig. caption should be extended to include all the panels. Based on the location in the Chuckchi Sea (near shelf, distant shelf, slope), perhaps it would

make more sense to plot the cores in the order 08JPC, 05JPC, 01A-GC?

Reply: We prefer this way. This is the order from north (top) to south (bottom) according to geographical sense.

Fig. 6 is a fairly complex Fig. that is only referred to once in the manuscript. Perhaps it makes sense to leave it out?

Reply: We prefer to show this figure to indicate the characteristics of branched GDGTs in different sites and ages.

Supp Fig. 1 is identical to a Fig. in Park et al. (2014)??

Reply: The figure is not the same as Fig. in Park et al. (2014).

Technical comments: L 48: From instead of on L 92. Employed instead of empolyed. L 112. Situated instead of sited. L 115. Sampled from instead of raised. L 258. Fig. 5 instead of Fig. 6? L 289. Characterize. L 580: Coastal instead of costal.

Reply: We will correct all the typos you indicated.

L583: Indicate which color corresponds to which glacial boundary extent.

Reply: We will indicate the corresponding colors. Blue indicates 20th century average. Red indicates 2012 minimum.

Fig. 4. Typo: Terrestrial instead of terristrial.

Reply: We will correct it.

Fig. 5. I would rescale the BIT graph for those cores that have only low BIT values (between 0 and 0.5 fi).

Reply: We will rescale the BIT values from 0 to 0.7.

Thank you very much again for all of your comments.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-529, 2016.