

Interactive comment on “GDGT distributions in the East Siberian Sea: implications for organic carbon export, burial and degradation” by R. B. Sparkes et al.

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Reviewer's comment 1) The constitution of the end-members used for the model should be better discussed. Especially the pooling for different OC sources for e.g. the fluvial delivery. The authors indicate that permafrost/yedoma/ice complexes have low GDGT concentrations, though rivers discharge GDGTs-rich material. Then what does this material comprise of? Are these GDGTs soil (other than yedoma?) derived, or produced in the river? Should/can we assume that soil and river-derived GDGTs have similar properties/show equal behavior upon discharge? After all, a recent study showed that fluvially discharged OC (in the form of lignin phenols) comprises multiple sources that

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that are transported following a variety of mechanisms (Feng et al., 2013 PNAS). How does this work in the system studied here?

Authors' response: We thank the reviewer for identifying this area of uncertainty. Fluvial endmember values, including those for GDGTs, were defined using surface sediment samples closest to the GRAR mouths. It is assumed that these samples represent an integrated signal from the river catchments, delivering mainly active layer soil material. In the case of brGDGTs, some of these will also be produced in situ in the river, but Peterse et al. (2014) showed that these contributions are minimal in the ESAS region. However, it remains to be seen how these two contributions behave off-shore. This is subject requires further research but for the present study it has been assumed that GDGTs produced in-river behave similarly to those from the active layer soil.

Authors' changes in the MS: The following lines have been inserted in the revised MS in line with the response above: -In the discussion, page 16: 'fluvial endmember values were defined using surface sediment samples closest to the GRAR mouths. It is assumed that these samples represent an integrated signal from the river catchments, delivering mainly active layer soil material and, in the case of brGDGTs in situ river production.' And page 16 as well: 'Endmember values were defined using ice complex samples, since these represent the majority of the sediment eroded from the East Siberian coastline (Schirrmeister et al., 2011)'. -In the Appendix, page 20: 'this material is primarily sourced from soil erosion with minor contributions from in situ river production (Peterse et al., 2014)' And page 21: 'Specifically, it is not currently possible to model the individual degradation rates of multiple sources of OC such as soil-sourced versus river-produced GDGTs, with any certainty.'

Reviewer's comment 2) According to methods, both IPL-derived as well as CL GDGTs have been analyzed, although only CL (or CL+IPL?) concentrations are reported and discussed. Do IPLs and CLs show different trends in distributions/BIT? How does the percentage IPL behave? How does the information from the IPL data influence the model in/output? Are there (specific) sites with (increased) in situ production? Also,

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the amount of carryover of br and isoGDGTs is not necessary equal due to differences in polarity between these two classes. How is this in your samples?

Authors' response: We understand the misunderstanding but although both the CL and IPL fractions were isolated only the core lipids were analysed in this study. To avoid confusion all references to IPLs have been removed. Given that IPL-derived GDGTs were not analysed questions related to IPL derived GDGTs such as sites with (increased) in situ production are outside the scope of present study. This also means that we have not investigated the carry-over from the CL into IPL fraction of our samples. However, as already clearly stated in the MS (section 2.1.1) previous studies have shown that the loss of CL GDGTs is insignificant (Weijers et al., 2011).

Authors' changes in the MS: The section dealing with CL fractionation (page 8) now reads:

"Core lipid (CL) fractions were isolated using silica column chromatography with 4 mL hexane:ethyl acetate (1:1, v/v) as the eluent. 0.2 μ g of a synthetic C46 GDGT standard was added to the CLs, which were dried under N₂, re-dissolved in Hexane : Isopropanol (99 : 1 v=v) and filtered through a 0.45 μ m PTFE filter. GDGTs analysis was carried out on the CLs via..."

Reviewer's minor comments: A) Please specify in the introduction that you are primarily addressing the organic carbon cycle in this paper, rather than the global carbon cycle as a whole.

Authors' response and changes in the MS: We agree and have made the required clarification in the first line of the introduction.

B) p. 643, line3: I think this should be Weijers et al., 2007 EPSL instead of Weijers et al., 2006.

Authors' response and changes in the MS: Actually, neither reference is required here and therefore the Weijers et al reference has been removed.

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C) Yedoma and its properties could be better introduced. From the current ms it seems that permafrost/ice complex material similar to yedoma, which is obviously not the case. Also, is there any explanation for the relatively low GDGT concentrations in yedoma? What does this mean for the source of the (br)GDGTs discharged by the Arctic Rivers? Similarly, is all material derived from coastal erosion yedoma (and thus has low GDGT concentrations)? Please clarify this in the ms. Also p 651, l21: do you refer to yedoma with these ice complexes?

Authors' response: We thank the reviewer for raising this point. The distinction between Yedoma and Ice Complexes is a complex one, since many publications have used the term interchangeably. The term "Yedoma" has, for instance originally been used to refer to small hills in the region. A review paper by Schirrmeister et al (2011), states that ice complexes and yedoma are used interchangeably to describe late Pleistocene fine grained sediments. These are thought to primarily be sourced from wind-blown loess. There is a distinction between these deposits and permafrosted soils and peats. It is the permafrosted soils and the active layer soils that we believe are a major source of GDGTs. Since Yedoma was formed by incorporation of sediment into an ice matrix, rather than the freezing/thawing of active layer soils, it is likely that the microbial community within the yedoma is significantly different to the permafrost soils. This is likely to affect the GDGT distribution and concentration.

Authors' changes in the MS: To avoid (further) confusion and to make a clear distinction with (permafrosted) soils, we have replaced "yedoma" with "ice complexes" in all places, including figures 3 and S2 and p651, line 121. We have included an introductory sentence in the methods describing the nature of ice complexes/yedoma, page 6: "this region is also the site of severe coastal erosion of terrestrial ice complexes (ice, dust and carbon rich deposits also known as "Yedoma"; Schirrmeister et al 2011), which has been estimated to deliver 44 +/-10 Mt C y-1 to the East Siberian Shelf (Vonk et al 2012). Ice complexes are a relatively organic-rich mixture of permafrost and sediment of Pleistocene age that exist in metres-thick layers underlying large amounts of

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Eastern Siberia (Schirrmeister et al 2011, Peterse et al 2014).”

We also included a sentence in the appendix addressing the concentration of GDGTs in the ice complexes, page 20: “Coastal erosion mostly impacts ice complex material (Vonk et al 2012), formed in a different manner if compared to permafrost soil. Considering the large differences between GDGT and other bacterial biomarker concentrations in these complexes if compared to the sediments collected near river mouths (DoÅ§rul Selver et al 2015) it is likely that these complexes support different microbial communities. This indicates that ice complexes are a major source of OC but not of brGDGTs.”

D) P653, what does the fluvial contribution of 13% sediment and 72% GDGTs mean for the source of the GDGTs (more specific than fluvial)? And the SOC?

Authors’ response: In accordance with the model inputs, which were based on measurements reported in this study and Peterse et al. (2014), brGDGTs are much more concentrated in river-delivered sedimentary material than in ice complex deposits. With the available data we cannot take this argument further to invoke a significant role for in-river production. The general conclusion to draw is that brGDGTs are enriched per gram sediment and per gram OC in river sediment compared to material sourced by coastal erosion.

E) P653, l25: how much greater was the role of coastal erosion in Vonk et al? Are there any plausible explanations for the discrepancy? Which one is more realistic?

Authors’ response and changes in the MS: Vonk et al report 57% of the OC being sourced from ice complexes. The discrepancy is likely due to the general nature of the model, which has not been tuned to the geography of the area, and uncertainties in both our modelling approach and Vonk’s endmember-based calculations. We are not in the position to comment on which estimate is more accurate. This has now been made more explicit in the manuscript on page 17.

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F) P653, L26: how long is there between delivery and sampling? What time-scales, and thus degradation rates are we looking at?

Authors’ response and changes in the MS: The model assumes that the degradation occurs in the watercolumn and that sampling occurs shortly after deposition. This is in principal the case since only the top few cm of the sediment were sampled, and as such should be relatively young. The estimated sediment age has been included in the manuscript.

Page 17 now reads: “(this study only considers surface sediments, so the sampled material should at most be only a few years old. Subsequent diagenesis is ignored, but likely to be substantial (Arndt et al 2013)).”

G) How applicable is the model to other regions where SOC is mainly derived from rivers and not much coastal erosion takes place? Is it possible to upscale?

Authors’ response: Although the ESAS system is relatively unique considering its high rates of coastal erosion, the model is extremely portable and should be applicable in other environments. If erosion rates and endmember values are available or can be estimated the model could be applied. This indicates that in principal it would be possible to upscale the model to cover, for instance the whole Arctic region. A sentence to highlight this has been added.

Authors’ changes in the MS Page 17: “The model is based on simple principles and is applicable in other areas if the relevant endmember values (GDGT concentrations, $\delta^{13}\text{CSOC}$) and model parameters (e.g. sedimentary input rates) are known.”

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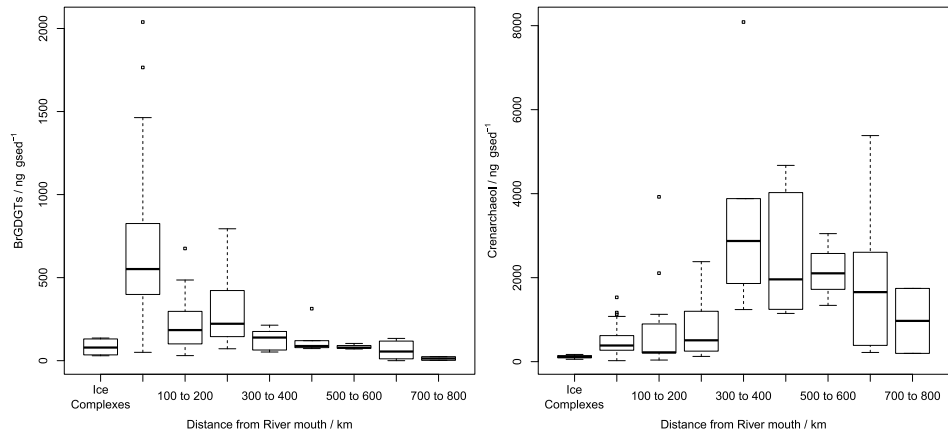


Fig. 1. Figure 3 revised version

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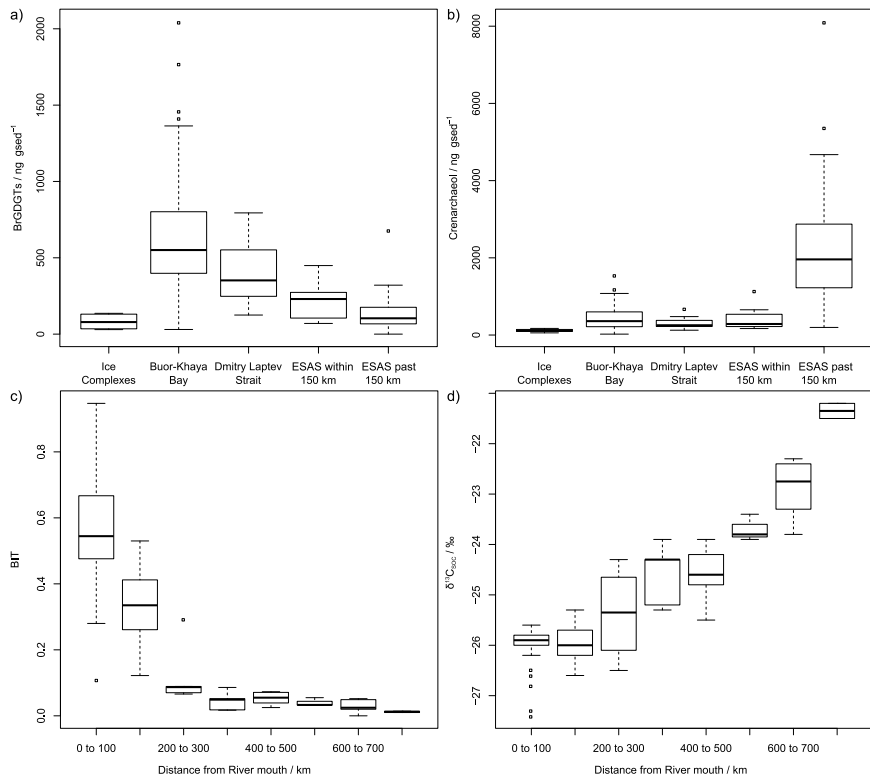


Fig. 2. Figure S2 revised version

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