

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

Anonymous Referee #1

Received and published: 27 January 2015

Sparkes et al have analyzed GDGT distributions in the East Siberian Sea to study the contribution of fluviially discharged soil organic carbon to marine sediments. They find a rapid reduction in river influence with increasing distance from the coast, and are able to model this observation, resulting in an estimate for the contribution of coastal erosion-derived OC and mineral matter to the East Siberian Sea sediments. The paper is well written, and the conclusions are in principle supported with sufficient data. However, I do have a few issues that may further improve the manuscript. Page and line numbers are based on the printer friendly version.

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 au-

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

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? Are there (specific) sites with (increased) in situ production? How does the information from the IPL data influence the model in/output? Also, the amount of carry over of br and isoGDGTs is not necessary equal due to differences in polarity between these two classes. How is this in your samples?

Minor comments: - 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.

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

- 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?

-P653, what does the fluvial contribution of 13% sediment and 72% GDGTs mean for

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the source of the GDGTs (more specific than fluvial)? And the SOC?

-P653, I25: 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?

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

- 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?

Interactive comment on Biogeosciences Discuss., 12, 637, 2015.

BGD

12, C32–C34, 2015

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