

***Interactive comment on “A biomarker record of
Lake El’gygytgyn, far east Russian Arctic:
investigating sources of organic matter and
carbon cycling during marine isotope stages 1–3”
by A. R. Holland et al.***

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[Responses are in brackets]

The manuscript describes a biomarker record from Lake E in Siberia, spanning the last 60 kyr. The authors find that the biomarker record does not change abruptly as the bulk geochemical indicators do, indicating both subtlety and complexity in the system that needs further study to understand. They also do not find indicators of anoxia, which was expected during the perennially ice-covered intervals. Overall, I find

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the MS well written and suitable for Climate of the Past. The authors are careful not to over-interpret the data, and do a good job of explaining the potential influences on their biomarker proxies, particularly the GDGT record. I find the significance good, and the presentation and quality to be excellent.

I suggest they take a look at the significant subunits regression for branched GDGT temperature reconstructions used in Loomis et al., EPSL 2012, it may be useful in their study. [We added the stepwise forward selection calibration of Loomis et al. (2012) to figure 5. However, we note that without having modern soil, water column filter samples or surface sediments from Lake El’gygytgyn, it remains unclear which of the available MBT/CBT calibrations, if any, is the most appropriate to apply.]

Also, I think they have undersold the idea of use of CO₂ from recycled OM as the cause of the C isotope depletion. This is something that we know happens, and this seems to me to be the perfect system for it. I don’t know how “perennial” the ice cover is, but in this ultra-oligotrophic system, recycling is likely to happen during periods of ice cover, and any short period of ice free time would allow mixing of deep water nutrients and CO₂ into surface waters, stimulating aquatic production in the short term. Furthermore, given the oligotrophic nature of the system, methanogenesis, which surely happens in deeper sediments currently, is simply not likely to be a major factor on the isotopic systematics. There are many lakes that have active methane cycles that are not influenced isotopically (such as the African lakes that Castaneda worked on). I do think it is better to err on the side of caution as the authors have done, but this seems to me to be the most likely scenario. [Current conditions of ice cover were clarified in the 2nd paragraph of the introduction. We agree that the likely cause of the carbon isotope depletion is recycling of OM. This is now explicitly stated at the end of section 4.7.1.]

Little things: You should define acronyms like “IRSL” dating and “GDGTs”, the first time used. [These acronyms are now defined the first time they are used.]

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Did you mean to use “BAMEs” on page 4634 line 19 and the legend of figure 3? I think that should be FAMES, right? Oh, I see on page 4645 where you defined it as bacterial FAMES. Please define earlier. [This acronym is now defined earlier, as suggested.]

Is it allowed to cite in prep manuscripts such as D’Anjou et al 2012 in this journal? [Yes. This question was posed to the editor of climate of the past to confirm that references published in the same volume are cited correctly. We cite several manuscripts that are published in Climate of the Past Discussions, as is this one, to be included in the special issue on Lake El’gygytgyn. All of these references have been updated to reflect current versions of these papers.]

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