

Interactive comment on “High-latitude environmental change during MIS 8–12: biogeochemical evidence from Lake El’gygytgyn, Far East Russia” by R. M. D’Anjou et al.

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D’Anjou et al. present an overview of preliminary biomarker results from the relatively recently-collected ICDP core from Lake El’gygytgyn (Lake E). This is a nicely and carefully written paper and the interpretations of the biomarkers are appropriately conservative, especially given outstanding uncertainties regarding, for example, the brGDGT temperature proxy. I’ll second Phil Meyers’ suggestion that organic carbon concentrations, if available, would be useful to have a look at and normalize the biomarker concentrations to. If TOC has not been analyzed, another way to isolate the effect of organic carbon concentration on the biomarker concentrations would be to normalize

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the biomarker concentrations to each other; e.g., look at the concentration of diols / sum(concentrations of all biomarkers measured). Looking at the biomarker variability in this manner may help clarify whether the variations are due to production or preservation.

I have a few other comments and suggestions as well:

1) Concerning the interpretation of the MBT/CBT temperature signal as a summer temperature signal: While this could make logical sense given the fact that ice covers the lake for much of the year (although, not knowing the ecology of the producers, this is still speculation) I don’t personally see a similarity between the insolation curve and MBT/CBT in Fig. 4 (Fig. 4e and Fig. 4f); in fact, it seems to me that they are weakly correlated (for example, MBT is high near 420-430 ka but insolation is low; same is true near 340 ka). Therefore, I don’t think the comparison with the insolation curve really supports the summer temperature signal argument as is stated. I am not sure how much age model uncertainty could affect a mismatch; perhaps some discussion of that is warranted. It might also be useful to have a look at annual insolation rather than seasonal for comparison.

2) Concerning the diols and specifically the diol index. The authors state on p. 4762 that the diol index, when calculated, looks similar to MBT/CBT. This would be useful to see in a figure. In addition, they speculate that the record of the C30 alkyl diol alone could “corroborate” the brGDGT temperatures. I’m not really clear on what is meant here. Is it that the concentration of this compound alone could be useful as a temperature proxy? How would that make sense from a mechanistic point of view? More generally speaking, it would be useful at this point in the text to discuss the mechanistic and interpretation of the diol index proxy as it is new and readers are on the whole not going to be familiar with it. My understanding is that the inferred relationship between the diol index and temperature is completely empirical; e.g., has no basis in known membrane lipid adaptations of species thought to make diols. I think it would be appropriate here to interrogate the diol proxy a little bit further and discuss

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its potential applicability/non-applicability to a lake system like Lake E.

Minor comments:

-p. 4752, top: Might be appropriate here to add an additional sentence noting that Acidobacteria are suspected source of brGDGTs and some strains do seem to produce one of the brGDGTs (brGDGT-I; Sinninghe Damste et al., 2011, Appl. Environ. Microbiol.

-With respects to the different MBT/CBT calibrations: it would be useful to list the calibration equations used (perhaps in Table 1) just so that readers not familiar with them can see the differences in the equations.

Fig. 3: I think the reference should be Tierney et al. 2010 (GCA) not Tierney et al. 2009. Also, is this the MBT/CBT-based calibration or the fractional abundances calibration?

-Jessica Tierney

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