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Climate
of the Past
Discussions

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.

B. Beckmann (Referee)

bbeckman@uni-bremen.de

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I. General comment:

The paper submitted by Holland et al. addresses the abundance and carbon isotopic signature of lipid biomarkers recovered from the high-latitude Lake El'gygytgyn (covering the past ca. 60ka). Their main research questions relate to evidence for anoxia in the water column during the glacial intervals and identification of OM sources driving the bulk sediment isotopic signature. Other than expected and in contrast to previous

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studies, the authors find no specific isotopic or biomarker evidence for water column anoxia. Based on their biomarker and compound-specific isotope analyses, they identify a mixture of terrestrial and aquatic sources contributing to the organic matter during the investigated interval. This well-written paper addresses questions that are within the scope of Climate of the Past. Scientific approach and the applied methods are good and suitable while the presentation is overall clear. Figures are well prepared and cover the presented information adequately – I especially enjoyed the clearly arranged table with GC specifications.

II. Specific comments:

I would appreciate some information in the introduction, why sediments from MIS 1-3 were investigated (considering that the core is longer), why are they so important? Additionally it would be helpful to be introduced to the term “LLGM” in the introduction as well. At the moment, LLGM is first explained in section 4, but the term is used earlier. My suggestion is to move p. 4636 lines 5-10 to some adequate place in the introduction.

If I understand the authors correctly in section 2.4, they used the internal standard C36 n-alkane (added before extraction) for quantification of the n-alkanes and the same compound, added as external standard for quantification of the FAMEs and TMS-esters of n-alcohols. Quantification of compounds based on internal standards is critical in my opinion, because some material is always lost during extraction and separation of fractions. Comparison of concentrations between different compound classes is hampered by mixing quantification via external and internal standards. Was any external standard used to evaluate the performance of GC and GC-irmMS? Are δ13C measurements of different compound classes single measurements or replicates,? If these are replicate measurements, please give mean values and standard deviation for all samples. The authors should address mean value, standard deviation and accuracy of the n-alkane standard more carefully. Please, check the reported concentrations of compounds. All tables give μg component / g TLE, the text describes ng / g TLE. How much information can be drawn when relating concentrations to g TLE? The total amount of total lipid extract depends on several variables (TOC content, amount of extractable organic matter, amount of extracted sediment). To be able to compare the reported values to results from other studies the authors should either use amount / g sed or amount /g TOC. Would it be possible to calculate accumulation rates of the different compounds? This could make identification of changes in terrigenous vs. lacustrine / bacterial OM easier.

Currently, the term “LLGM” is introduced in the discussion to differentiate the region of Lake El’gygytgyn from other Arctic regions. I would look forward to a part where these regional phenomena are put into a broader perspective, e.g., in comparison with other Arctic sites. This could be a good addition to the conclusions.

As it is, I find the section 4 (pages 4636 and 4637) somewhat misleading. After re-reading the paper some times, I suppose it is meant as general introduction to the discussion. Initially, I was under the impression that some major points are only touched marginally, stated as facts or not discussed at all. I would suggest a re-structuring of this section, maybe in the same order as the following (subsequent) sections are arranged. Maybe even re-state specific questions that will be answered in the next sub-chapters.

I would suggest to re-structure chapter 4.2 a bit by starting with a short overview which compounds indicate terrigenous input and which are of other (aquatic or bacterial) origin. Then discuss the individual compound classes and close with a short summarizing paragraph. Maybe even divide the section further into sub-sections (it is currently almost 4 pages long) or considering some moderate shortening.

Section 4.4 is not really related to the goals of the manuscript (evidence for water column anoxia and identification of sources of organic matter). Currently and oversimplified, the authors state that there is GDGT data giving ambiguous results, temperatures during the LLGM were cooler, as expected, and that a modern calibration in the water-

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shed is needed. Based on figure 5 (there is no supplementary material on GDGT data to compare), I would say that there is at least one cold spell at 50ka, not related to the LLGM. I suggest to delete this section.

Section 4.5 includes some repetition within 20 lines (p. 4645, lines 25 – 29; p. 4646, lines 8 -10). Please consider rewriting and shortening of this part, it contains valuable information. Later on (p. 4646, lines 15 to end of section), I cannot really follow the authors when they discuss the “somewhat surprising contribution to the depleted bulk δ13C signal” as they have already noted before that there is some contribution of other that terrestrial sources to the OM pool. This could be presented in a more concise way.

As it is, I find that section 5 (conclusions) is mostly a summary of what has been said before but in large parts not a conclusion. It is roughly two pages long – too long in my opinion. My suggestion is to maybe start the conclusion with the paragraph on p. 4649, lines 24 to the end to underline the importance of this study to the community.

Section 2.2 – Page 4629 – Lines 6-10: Concerning the chronology: Judging from Figure 2a, I would say that the very last part of LZ1029 is too low in comparison to the increase in TOC visible in PG1351. If there is a good reason for this, maybe it become obvious when comparing the δ13Cbulk records of both cores?

Section 2.2 – Page – 4629 – Lines 14-15: For the data discussed in this paper it seems to be not relevant how long cores LZ1024 and PG1351 are and how much time the respective cores cover.

Section 3.1 – Page 4632 – Lines 24-25: Please give probability of correlation coefficients, are they statistically significant?

Section 3.2.1 – Page – 4633 – Line 18: Please give δ13C value of sample at 38 kyr.

Section 3.2.3 – Page 4634 – Lines 26&27: Judging from figure 2, I would rather say that the increase in total n-alcohols starts after 38 ka, in agreement with the fatty acids.

Section 3.2.3 – Page 4635 – Lines 1-3: Based on the data in the supplementary material C21 n-alkanol is fourth in abundance (out of five compounds) and therefore not one of the most abundant compounds. Please be more precise, every compound except the n-C26 alkanol is most abundant compound in one sample or the other.

Section 3.2.4 – Page 4635 – Lines 18-25: If all other original data is summarized in the supplementary material, why not the GDGTs? Please include in supplement.

Section 4.1 – Page 4638 – Lines 13-18: Is there any explanation why the environmental change in and around the lake is not captured in bulk % TOC and C/N records? If pollen records indicate almost exclusively herbaceous plants, this could explain the relative high abundance of the n-C31 alkane.

Section 4.2 – Page 4639 – Lines 17-19: Please be more specific, what kind of sites? Give references to support your statement.

Section 4.2 – Page 4630 – Lines 6-9: This record is not the first one to demonstrate that care should be taken when interpreting long-chain FAMEs in sediments. Please cite other work here as has been done in lines 1&2 on this page.

Section 4.2 – Page 4640 – Lines 14&15: What could be the possible aquatic source organism for n-C25 alkanes?

Section 4.2 – Page 4641 – Lines 3-6: Confusing to me, why C23 and C25-alkanes are referred to as showing trends in terrestrial sources, especially when it was indicated before that C25-alkanes can have an additional aquatic source.

III. Technical comments

Abstract – Page 4626 – Lines 7&13: Spelling of Lake El'gygytgyn

Section 1 – Page 4627 – Lines 7&8: position of Lake El'gygytgyn is 67°30'N and 172°05'E (see also in Figure 1; Layer, 2000, Meteorites and Planetary Sciences, v. 35, p. 591-599)

Section 1 – Page 4628 – Line 10: Investigated sediments span the past 63 kyr, not 90

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kyr.

Section 2.4 – Page 4630 – Line 10: Please add the mixing ratio of DCM:isopropyl alcohol.

Section 2.4 – Page 4630 – Line 24: . . . were identified on a Hewlett Packard (HP) 6890 series. . .

Section 3.2.2 – Page 4634 – Line 19: . . . were only obtained on one sample from within. . .

Section 4.4 – Page – Line 18: Please complete Lake El'gygytgyn.

Please use either “kyr” or “ka”, but do not mix both writings.

You have to decide whether you sort references in the text in alphabetical or temporal order, but do not mix both styles in the text.

Decide whether you want to use n-alkanols or n-alcohols (e.g., see section 3.2.3)

I could not find the following references in the text: Hayes (1993), Layer (2000)

Figure captions:

Fig. 1: Location is 67°30'N, 172°05'E

Fig. 2: Give reference for TOC content in PG1351 (and add letter a after “relative to TOC"). Please explain the orange box. Was it drawn based on the age model?

Fig. 4: What is the green line in comparison to the green dots?

Interactive comment on Clim. Past Discuss., 8, 4625, 2012.