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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.

Anonymous Referee #3

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Summary

This paper discusses a low-resolution biomarker record (14 samples) from one of the multiple cores taken in a large remote ancient Arctic lake in an impact crater with an objective of "investigating sources of organic matter and carbon cycling". The main motivation for this research was that "the degree to which variations in aquatic productivity, water column anoxia, or methanogenesis may have impacted the sediment record is not well understood" (p. 4627). The authors put more emphasis on what kind of processes may have impacted their particular sediment record than on what one

may learn about regional climate from the studied proxies.

There appears to be rather little on 'climates of the past' in this current manuscript - the scope of the current version of the paper appears more relevant for journals such as J. Paleolimnology or Organic Geochemistry. Beyond the Introduction, the narrative drifts further and further away from climate issues, the age model is reduced to the most basic stratigraphy of A-B-C intervals and climate is forgotten by the time one gets to the Conclusions and "outstanding questions".

The dominant factor responsible for the studied biochemical signals appears to be ice cover: the leading hypothesis is that under the conditions of permanent ("perennial", "interannual") ice cover during the last glacial the lake water/atmosphere gas exchange was greatly limited, causing dramatic oxygen depletion in the lake. This hypothesis is not original as it derives from earlier observations: oxygen depletion was suggested based on in sediment texture (lamination), magnetic properties (dissolution of magnetite = low magnetic susceptibility MSUS) and high total organic carbon content (TOC) in bulk sediment suggestive of less degradation in the oxygen-depleted water column. The authors of the current contribution were looking for compound-specific signals of anoxia in order to test this earlier hypothesis (p. 4645) but did not really succeed. The discussion therefore revolves around the prior evidence (MSUS, d13C and TOC) and invokes production (delivery) and preservation as alternative mechanisms responsible for producing a TOC peak, which is recognized as the L[ocal]LGM interval.

Two significant components which could potentially make the study stronger are unfortunately 'outsourced' to other papers in prep.: (1) diatom record which is expected to constrain the primary production component of the lake's carbon cycle is presented elsewhere (Snyder et al., in prep.), and (2) the age model, which is essential to make a linkage to climates of the past, is also supposedly presented elsewhere (Murdoch et al., in prep; see below for comments on Age Model). A reader may only assume that these papers, when(if) published could strengthen the arguments in the current manuscript.

The manuscript is not acceptable in its current form, it needs to be thoroughly reworked and supplied with a better description of climate, age model, description of the components of present-day carbon cycle in the Arctic to provide a better context for understanding the significance of a rather sparse low-resolution data set presented here. Below I suggest several areas requiring attention and list specific comments.

1. Relationship of proxy signals to past climate

The focus of the work is on the LGM but there is little description on what the LGM climate may have been in the study area. “The most recent cold period” referred to in conclusions is actually ‘the most recent high-TOC and low magnetic susceptibility interval’ - no evidence is shown to support the contention that this interval was in fact cold or “coldest”.

Nowhere in the manuscript is there any mention of possible changes in the water budget of the lake during glacial/interglacial cycles and LGM in particular. Has the lake level remained unchanged in the past 60 kyr? Is there any evidence from lithology of the studied core and/or other cores? What kind of effect on the studied proxies may one expect in case of climate-driven changes in lake level (and lake volume)?

The stated relationship of regional proxy records to past global climate changes is not consistent throughout the manuscript. First the authors state that MSUS, d13C and TOC "are closely tied to regional climate variables" (p. 4627) but provide no evidence for this; then they admit that their glacial intervals are in fact "interpreted" (p. 4628) and that "regional cooling ... may or may not have been entirely synchronous with glacial activity" elsewhere (p. 4636). Suddenly, on p. 4637 they state to the contrary that MSUS and TOC variations "are synchronous with MIS boundaries” (no evidence is shown for that either). If the main shift is not “synchronous”, Conclusion 1 (p. 4648, line 16) is irrelevant.

The sense of confusion reflected in these contradictory statements on synchroniety (or lack of thereof) naturally follows from the little effort the authors have spent in dis-

cussing and presenting the age model. Reading the manuscript, one could feel how eager they are to get away from the undesirable age model section to the subjects they are most comfortable with. The problem is that without the age model the Discussion lacks clarity and it is of limited interest to the readership of Climate of the Past.

2. Age model

Tie points should be shown in respective Figures even if the age model is discussed in detail elsewhere. How many AMS/IRSL dates? If there are none in this particular core, the position of absolute age tie points relative to proxy signals in other cores should be shown for reference. How was "magnetic susceptibility tuned to insolation"? (tie points should be shown in Figures). What is the main assumption underlying the tuning of the magnetite dissolution signal (MSUS) to insolation? Is there any phase lag associated with tuning? Why would dissolution of magnetite in an Arctic lake be “synchronous” with marine oxygen isotope stages? If low insolation is the predictor of persistent/permanent ice cover (dissolution), why is there a strong magnetic susceptibility signal of dissolution (lack of oxygen) in the early Holocene at ca. 9 kyr BP, when insolation is high (Fig. 4)? Why is this Holocene signal similar in amplitude to the magnetic susceptibility signal of the Local LGM when insolation is low?

In the beginning, quite precise ages are cited (e.g., 43-17 kyr BP) producing an impression of a tight chronology, but then the discussion drops into a very basic stratigraphy (A-B-C intervals). The boundary between intervals C and B appears rather arbitrary: one could argue it is better placed at 36 kyr BP or so, not at 43 kyr BP.

3. Modern processes, values and carbon cycling in the Arctic - ??

The discussion of the present-day conditions and carbon cycle, isotope ratios of organic matter sources today and the significance of proxy signals of the transition from LLGM to the present-day Holocene interglacial is missing entirely. The lack of effort in addressing these questions does not help improving the potential impact of the described findings. Several times unspecified “other lakes” are mentioned in Discussion.

What are these lakes? Where are they located? How many “other lakes” did they compare their records to? How many of these “other lakes” are located in the Arctic in similar geographic setting? There must have been studies of similar processes and similar biomarkers in Canadian Arctic in relation to ongoing changes in the permafrost, there must be at least some published lake values for concentrations and isotope ratios. . . Why not review at least some factual supportive evidence to strengthen the Discussion?

4. Production, delivery and/or preservation of organic matter - Conclusions

This triangle (production, delivery and/or preservation) forms the framework of the Discussion: which of the mechanisms appears to be dominant in their lake record during the LLGM interval with negative d13C and high TOC. These are the typical mechanisms potentially controlling bulk content and isotope composition of organic matter in lake sediments. The authors imply that different explanations are possible and at the top of p.4637 they challenge a prior idea of increased terrestrial input playing a key role. However, they seem to confirm this same idea on page 4640 (lines 27-28) where they conclude that "LLGM corresponds to the time of maximum terrestrial OM delivery to the sediments". In Conclusions, they may want to make it clear that they tested and confirmed the scenario proposed in previous studies.

“Increased productivity [they probably mean production] of aquatic OM” is inferred during LLGM (p.4643, top). This conclusion is further supported by the reference to unpublished observations of "unexpectedly high biogenic silica values" from diatoms during LLGM (p. 4637, lines 9-10 ; - see also a comment below). In their Conclusion 2, however, they favor preservation over production and state that "higher aquatic productivity cannot be discounted" (p. 4649, line 2). There is a notable disconnect between what is stated in the Discussion and in Conclusions (‘increased, unexpectedly high but can be discounted . . .or maybe not’) - the authors would need to sort this out.

In section 4.6 the authors propose the use of nitrogen stable isotope ratios "to test

the original interpretation” suggesting highly anoxic conditions during LLGM interval (p. 4647). The test appears straightforward: one would expect d15N enrichment as a result of denitrification under anoxic conditions (lines 14-15). The test comes in negative: the authors find depletion instead of enrichment. Yet their conclusion is quite puzzling: "results neither support nor preclude the existence of a significant anoxic portion of the water column". What use is the “test” if it provides no answer? If it does not it should not be presented as the test, just as another proxy. . .

"Isotope mass balance may place bounds on the contribution. . ." (p. 4649) - isotope mass balance calculations are not shown, there was not even an attempt to constrain potential end-member d13C from literature. There is no place for this statement in Conclusions, at best in the Discussion

The remaining "several outstanding questions" (p. 4649) appear to be of little interest to the readership of Climate of the Past as being very local and very specific

5. Specific comments:

Starting from p. 4636 (line 27) “One interpretation” is described, but further along in the text no ‘other interpretation’ is given, there is just this “One”. Apparently these are leftovers from previous versions of the text which need to be cleaned up

p. 4637, lines 9-10 "unexpectedly high biogenic silica values" - how high and why “unexpected”? How much did authors expect, why and how much more silica is actually observed as compared to what they expected to see? Without specifics this phrase does not help the argument

p. 4640, lines 6-9 "caution must be applied when assigning sources of . . . FAMEs in sediments" - is this the first time ever this has been demonstrated in a lake? Have these compounds always been interpreted in “other lakes” as being purely terrestrial and now, for the first time ever, the results reported in the manuscript suggests that approach used formerly is no longer valid? This finding is implied to be significant as it

is featured in Conclusions, so the authors need to explain the context better

p. 4648, lines 1-14 - it is not clear what exactly did the authors do to “look to the iron cycle”. This entire paragraph is not supported by any data in this study. Some references are provided following by the "more research is needed" statement. This paragraph can safely removed to save space

Figures: the orange frame approximately marking LLGM interval in Figures 2 through 5 appears misaligned with major peaks of TOC and MSUS, the alignment seems to be with bulk d13C record (Fig. 3). For instance, Fig. 4 makes it look like LLGM in the MSUS profile is at 19-25 kyr BP, not at 20-26 kyr BP as currently shown. The explanation needs to be provided for what exactly this orange frame stands for.

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