

Response to P. Bartlein (Referee 2)

General comments

This paper describes the application of the "biomization" approach to the long pollen record from Lake El'gygytgyn, and fulfills its objective of describing the vegetation history in a fashion that is more accessible to non-specialists in palynology than the traditional pollen diagram.

We are grateful to reviewer for high evaluation of our work and valuable suggestions helping to improve our paper.

Specific comments

One important contribution that is overlooked in this manuscript arises from the information on the uncertainty in the biomization procedure that is provided by the analysis of the surface samples (i.e. in Fig. 2). The surface samples provide what amounts to replications of the biomization of the present-day pollen assemblage (ignoring and systematic intra-lake variations, which I suspect are small). The variation (standard deviation) of the biome scores therefore could be used to represent the uncertainty of any single down-core biome score (or at least those from interglacial periods). Inspection of the variability of the biome scores in Fig. 2, suggests to me that, for example, the Holocene biome scores of TUND and CLDE are really not distinguishable from one another. This resolves the apparently contradictory result (p. 3460) of high Holocene TUND (tundra) biome scores in the presence of macrofossils of tree taxa. It would be useful to discuss in quantitative terms the present-day variability in biome scores, and perhaps represent the current values on Figs. 3 to 6 as ranges as opposed to point values (i.e. the inverted triangles on the diagrams).

Here reviewer rises a very interesting point and encourages us to go further in quantitative interpretation of the biome scores and the past changes in vegetation. Following this suggestion I calculated standard deviation intervals for the TUND, CLDE and STEP biomes in the set of surface pollen assemblages from the study lake. The 95% ranges (2 standard deviations away from the mean) are added on Figs. 2 to 6.

Chronology. I realize that this paper is part of a collection, but it would be good to include a short paragraph describing the chronology used here, its basis, with specific citations (i.e. page numbers in the appropriate sources).

As suggested, we added a paragraph on the age model to the text (2.2 Pollen data and chronology) and respective references to the reference section (see below)

2.2 Pollen data and chronology

The age model for the ICDP- Site 5011-1 sedimentary composite record was developed in several steps, successively refining the model. Initial tie points for the age model were provided from magnetostratigraphic investigation of the cores from ICDP Site 5011-1 (Haltia and Nowaczyk, 2013; this issue), defining the positions of major geomagnetic reversals back to the early Gauss chron. The age of the El'gygytgyn impact at 3.58 ± 0.04 Ma (Layer, 2000) provided another initial tie point. Additional tie points could be derived from fine-tuning of various climatically controlled sedimentological and geochemical parameters (biogenic silica, total organic carbon (TOC), tree and shrub pollen percentages, grain size analyses, sediment color, Si/Ti ratio obtained from X-ray fluorescence (XRF) scanning, magnetic susceptibility) to both the marine oxygen isotope stack (LR04) of Lisiecki and Raymo (2005) and the Northern hemisphere summer insolation provided by Laskar et al. (2004). For further details see Nowaczyk et al. (2013; this issue)

Added references

Haltia, E.M. and Nowaczyk, N.R.: Magnetostratigraphy of sediments from Lake El'gygytgyn ICDP site 5011-1: paleomagnetic age constraints for the longest continental record from the Arctic. *Clim. Past*, 2013 (this issue).

Laskar, J., Robutel, P., Joutel, F., Gastineau, M. Correia, A.C.M., and Levrard, B.: A long-term numerical solution for the insolation quantities of the Earth. *Astron. Astrophys.*, 428, 261-285, 2004.

There is a lot of material in section 3.7 and in the conclusions that is more the motivation for the study than the results, and this could be usefully condensed and moved up to the introduction.

This has been done in response to the suggestion from reviewer 1

Figures. I found the addition of the oxygen-isotope curves to Fig. 8. extremely useful. (Yes, it's well known that odd-numbered stages are warm, even-numbered stages are cold, cold, etc., but they all differ in degree.) It would be helpful to add appropriate curves to the other figures (e.g. the NGRIP record to Fig. 3). Also, it would be good to adopt a common y-axis tick spacing on Figs. 3-6, to facilitate comparison. Fig. 7 is completely redundant, it would be great if it could be sacrificed in favor of rendering Fig. 8 at a larger size. (Also, Fig. 7 is cited after Fig. 8.) (Incidentally, the LR04 curve on Fig. 8 looks like a poorly digitized version of the curve. The original data are available online, and should be substituted.)

We followed suggestions of reviewer and added appropriate NGRIP curves to the other figures (i.e. to Fig. 3-6) and adopted common y-axis tick spacing on Figs. 3-7, to facilitate comparison. I am also unhappy with how Fig. 8 looks like in the printed version of the manuscript. The hope is that it will become better visible in the final published version, when the A4 page format will be used.

Fig. 7: We agree with reviewer and deleted Fig. 7 from the revised manuscript.

The LR04 curve on figure 8 is based on the original dataset and not digitized. I believe this reviewer comment again came due to a small size of the journal page.

Technical comments

p. 3451, line 17: "long phases"

Done

p. 3451 line 25: Reword –the sentence now says ("The reconstruction: : : is particularly noticeable: : :")

This sentence has been already modified according to suggestion from reviewer 1

p. 3452, line 1: "The biomization results: : :" Already said this (previous page, line 23).

Deleted

p. 3452, line 21: "For the first time: : :" unnecessary.

Deleted

p. 3455, line 13: "age model" This should be elaborated a bit, simply to make this manuscript a little more autonomous.

Done as suggested.

p. 3456, line 6: “further testing of the paleodata” I don’t understand. How are paleodata “tested”?

Corrected: should be “testing of the method”

p. 3456-3457: This is a key paragraph that basically describes “tuning” of the biomization approach for the local conditions, and that tuning (which is ok to do) should be acknowledged. Later on, the “objectivity” (p. 3471, line 20+) the method is asserted, but the procedure as described here is not “straight out of the box”.

Recommended note has been inserted to the text (page 3455, line 27). The sentence is now “The method initially was tested using global-scale modern pollen datasets (e.g., Prentice et al., 1996, 2000), but further modifications of the biome–taxon matrixes (i.e. “tuning” of the biomization approach for the regional conditions) were developed for large parts of Siberia and Alaska (e.g., Tarasov et al., 1998; Edwards et al., 2000)”.

We replaced “an objectively generated record of vegetation change” by “a pollen-derived record of vegetation change” at page 3471 (line 20).

p. 3457, line 17: “We retain the use of weighting of Larix percentages: : :” Does this mean you did weight the Larix percentages, or not?

No, we didn’t. This sentence has been clarified.

p. 3458, line 15: “distance” Do you mean simply “difference” or is some kind of multivariate distance being calculated?

“Difference” is correct, we corrected the text.

p. 3458, line 21: “Despite existing variations: : :” Please elaborate. (I’m not sure what the argument is here.)

This sentence has been shortened and clarified.

p. 3459, line 13: This would be a good place to discuss the variability of the biome scores.

We added necessary information to the text by the end of chapter 3.1 (page 3459)

p. 3459, line 24: “Younger Dryas” is an adjective, not a noun, so say “Younger Dryas chronozone” or something.

We wrote “Younger Dryas chronozone”

p. 3460, line 5: This paragraph sounds like a recapitulation of internal arguments among coauthors. I think that the issue (as a read it) is that the biome scores point to tundra, while there is ample evidence for trees. I think this could be resolved by acknowledging the uncertainty in the biome scores/biomization procedure revealed by Fig. 2.

The following sentence has been added to the text by the end of the chapter 3.2 (page 3461).

“Inspection of the biome score variability in Fig. 2a suggests that the scores of tundra (95% range =15.95-19.41, mean = 17.68) and cold deciduous forest (95% range = 13.86-16.52, mean = 15.19) in the modern pollen spectra are not easily distinguishable from one another. This result of biomization

helps to resolve the apparently contradictory result of high Holocene TUND (tundra) biome scores in the presence of macrofossils of tree taxa.”

p. 3461, line 8: “increase” relative to what?

We revised this sentence adding “above present-day level”

p. 3461, line 19: “oscillation” Later (p. 3462, line 16) you refer to this as “YD-like”, so maybe use the same terminology here (and also cite some of the abrupt-reversals during-deglaciation literature (e.g. Martrat et al. (2007, Science); Cheng et al. (2009, Science)).

The mentioned sentence has been edited and the two references have been added to the reference list.

Cheng, H., Edwards, R. L., Broecker, W. S., Denton, G. H., Kong, X., Wang, Y., Zhang, R., and Wang, X.: Ice age terminations, *Science*, 326, 248-252, 2009.

Martrat, B., Grimalt, J. O., Shackleton, N. J., de Abreu, L., Hutterli, M. A., and Stocker, T. F.: Four climate cycles of recurring deep and surface water destabilizations on the Iberian Margin, *Science*, 317, 502-507, 2007.

p. 3465, line 23: Fig. 7 is completely contained in Fig. 8, and Fig. 8 already has been cited, and so I think Fig. 7 is redundant.

We agree with reviewer: Fig. 7 has been taken from the manuscript and replaced by the current Fig. 8.

p. 3467, lines 6-16: It should be acknowledged that the model does not attempt to simulate the distribution of individual taxa, particularly those with weird adaptations to cold or snow.

The suggested sentence has been added to the text (page 3467).

p. 3469: Much of this paragraph, and the third one on the Conclusions describe the motivation for writing this manuscript, and this is material better suited for the introduction.

As suggested a part of the text has been replaced to the introduction chapter. The 3rd paragraph in the conclusion chapter has been deleted.

p. 3471, paragraph beginning on line 23: This paragraph reads like boiler-plate from a proposal, and isn't really a “conclusion”.

Bearing in mind that advantages of biomization approach are still underestimated (i.e. comments from referee 1), we vote for keeping this part of the text in the conclusion section.

p. 3473, line 3: “Biome reconstructions do not rely on modern reference datasets.” In theory, yes, but in practice, whenever the method is modified (e.g. p. 3456) they do.

This sentence has been modified accordingly

p. 3492 (Fig. 3). Unless I missed it, Lisieck and Raymo (2005) don't explicitly list MIS boundaries; (they do list magnetic reversals and terminations). What was the actual source?

I downloaded the file from

Ages of MIS boundaries. - Lorraine Lisiecki

www.lorraine-lisiecki.com/LR04_MISboundaries.txt

MIS LR04 Boundary Age(ka) 1/2 14 2/3 29 3/4 57 4/5 71 5.1(peak) 82 5.2(peak) 87 5.3(peak) 96 5.4(peak) 109 5.5(peak) 123 5/6 130 6/7 191 7/8 243 8/9 300 ...