Climate
of the Past

Open Access

Discussions

Interactive comment on "The biome reconstruction approach as a tool for interpretation of past vegetation and climate changes: application to modern and fossil pollen data from Lake El'gygytgyn, Far East Russian Arctic" by P. E. Tarasov et al.

P. Bartlein (Referee)

bartlein@uoregon.edu

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.

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 43 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).

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).

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.

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.)

Technical comments

p. 3451, line 17: "long phases"

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

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

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

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

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

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".

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

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

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

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

scores.

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

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.

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

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)).

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.

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.

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.

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

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.

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?

C2286