

Interactive comment on “Climate in continental interior Asia during the longest interglacial of the past 500 000 years: the new MIS 11 records from Lake Baikal, SE Siberia” by A. A. Prokopenko et al.

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GENERAL COMMENTS

This manuscript presents new parallel high-resolution records of productivity proxies and pollen for the MIS 11 interval from the most recent Lake Baikal drill core (BDP-99). It also includes a sediment mineralogy study from another Lake Baikal drill core from which the well known long Siberian diatom and BioSi content records are issued. The paper tackles the classical questions of the exceptional duration and warmth amplitude of the MIS 11 interglacial period and its analogy with the Holocene. This study also demonstrates thanks to the new pollen record that the diatom and BioSi records are

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actually recording the MIS 11 interglacial conditions. In addition, it provides the first pollen record giving unique information on the vegetation in Siberia during MIS 11.

The paper is well written and I particularly appreciated the author's effort in making the paper clear and explicit even for non specialists. This study greatly contributes to improve our knowledge on the climate of past periods that are potentially analog to the current interglacial.

This paper is certainly suitable for publication in the CP special issue “Marine Isotope Stage 11”. The authors would have however to consider the following comments and suggestions.

SPECIFIC COMMENTS

1) The Eurasian palynological records (section 2)

I understand that section 2 is devoted to highlight the uniqueness of the Lake Baikal records among the continental records. However, I can't help to note that the state of the art on the Eurasian palynological records must include the Iberian margin pollen records among the southern European ones (Tzedakis et al., 2009, CPD this special volume, Desprat et al., 2005, 2007). They are as well “Long continuous palynological records of several interglacials in a single record at a single site” (p.1955) of southern Europe and offer a direct land-sea correlation.

Which Italian pollen sequence are you referring to p.1955? In my knowledge, the long Italian pollen sequence Valle di Castiglione (Follieri et al., 1988) mentioned in Tzedakis et al. 1997 does not include MIS11.

2) Clarity on the Baikal records and sites (Sections 3 to 5)

The high number of Lake Baikal sites, proxy analyzed in each core and previous publications makes a few points difficult to follow for a reader who is not used with the Baikal works.

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- Is the sediment mineralogy record from the core BDP-96-2 new? It is not clearly mentioned in section 3.

- “previous studies touching on or specifically dedicated to the MIS11 proxy responses dealt with BioSi content and/or diatoms” (p.1955, l.26&27) must be more explicit: which drill cores? What about CHN analyses and biogenic silica d18O shown later? Are the papers cited representing all the works done on the Lake Baikal MIS11 sections?

- In the figure captions, I suggest adding the references to each proxy record previously published to help distinguishing the new data from the old ones.

- For example p. 1956, l.22 or p.1958 l.25, which drill core is implied there since they are all Baikal records?

- In the method section, you could indicate from which drill cores each proxy analysis had been performed.

3) Methodology (section4)

Please, be more specific on the pollen analysis methodology: in adding a summary of the pollen extraction protocol, and more importantly, in indicating the mounting sample media, microscope type and magnification of the objectives used for pollen identification, total number of pollen taxa identified and a description of the percentage calculations.

After Granoszewski et al. (2005) and Demske et al. (2005), pollen grains of the species *Pinus sibirica* and *Pinus pumila* cannot be separated and are included in only one pollen type (*Pinus sibirica*-type). Since the taxa *P. pumila* apparently gives important paleoecological information, could you be more specific about which identification criteria (with references if any) allowed you to distinguish this pine pollen taxa from the others?

Could you indicate if the pollen, diatoms, CHN and stable isotope analyses were performed on the same sample set or if the time resolution is similar for each proxy analy-

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sis?

Please, define acronyms TOC, TN, IR.

4) Millennial-scale variability (section 10)

Although the millennial-scale climatic variability within the Baikal MIS11 interglacial is apparently not one of the main paper objectives, I think that section 10 can be clearer, more precise and go a little deeper into the discussion without waiting for the pollen-based quantitative climatic reconstructions to do so. My suggestions do not necessarily imply to greatly extend this section.

Firstly, this section can be made clearer in pointing out specifically the millennial changes (nature and approximate timing) detected in the BDP-96 and BDP-99 records, in correlating them and in developing a discussion based on these events. The discussion on the millennial-scale variability from BDP-99 is somewhat confusing because the figures does not always show a correspondence between pollen zones and diatom units, and it is not clearly expressed if to each diatom change correspond a vegetation change and inversely (and if not, why), and what is the nature of the inferred climate changes. Why does not the discussion specifically refer to the 3 short intervals of cooler climate inferred from the pollen record? Are they likely concurrent with the 3 small biogenic silica d18O decreases from BDP-96 or to particular productivity proxy response? An additional figure showing a correlation of the BDP-99 and BDP-96 data and/or the addition of pollen zone boundary age in Fig.6 would also help (see following comment). In addition, since the paragraph of section 8 (p. 1965 from l.18) shows that the anomalous excursions in TOC, C/N and d13Corg at 400 ka are possibly related to an abrupt event, it might be suitable to include this paragraph or make a reminder of it in section 10.

Secondly, it would be interesting to briefly deal with the still open questions “Is millennial scale climatic variability a common feature of the MIS 11 interglacial? Is there abrupt reorganizations of the ocean-atmosphere systems during this period?” I don’t ask for a

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long distance comparison with European and North Atlantic records showing millennial oceanic and terrestrial events (e.g. Poli et al., 2000; de Abreu et al., 2005; Desprat et al., 2005) which is difficult and too far from the paper objectives. However, it could be mentioned in a few words that the millennial scale climatic variability in Siberia is possibly not an isolated phenomenon and has eventually relationships with the North Atlantic and European changes through atmospheric teleconnections.

5) MIS 11-Holocene comparison (section 11) – A new figure displaying the BDP-96 and BDP-99 records versus age and the comparison with the Holocene records

Since determining which part of MIS 11 (if any) can be regarded as an analog of the Holocene is one of the study objectives, the paper would gain from a supplementary figure supporting the MIS11-Holocene comparison. In the current paper version, it is sometimes hard to figure the relationships between MIS 11 changes of sediment mineralogy from BDP-96 (shown in age) and vegetation from BDP-99 (only shown in depth) as discussed in section 11. This figure would a) synthesize the data from BDP-96 and BDP-99 versus age (main mineralogical components or indexes, main forest taxa, BioSi content and $\delta^{18}O$ for the MIS 11, b) show the Holocene records (if possible) and c) the orbital parameters.

In addition, it would be interesting to extend the discussion of the Holocene and MIS 11 analogy on a) the comparison of the vegetation formation changes which is surprisingly more extensively developed in the conclusions than in this section, and b) to the orbital configuration – climate relationship (cf. precession – high latitude seasonality) during the potential analog MIS 11 part and the Holocene.

6) Various

p.1953, l.22 & 23: Be more explicit on “the analogous principal boundary conditions” (ice volume, CO₂. . .) and which orbital parameters (eccentricity, precession, obliquity) you are describing.

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Paragraphs p.1956, l.15 to 21 and p. 1966, l.4 to 10 both explain that quantitative climatic reconstructions can be achieved from the present MIS11 pollen record and that this work is in progress. In my sense, although it is interesting to know, these paragraphs appear too long and redundant since the quantifications are not part of the current paper.

p.1957, first line: Define what you mean by “full-scale interglacial” in this section.

p.1959: What do you want to demonstrate in showing “that with no specific adjustment, the alignment of the LR04 stack and Baikal BioSi is quite remarkable”? Both curves are indicating different variables which are possibly not varying in phase at the glacial-interglacial scale.

p.1967: PZ 2 is said to be “characterized by the elevated abundance of *Abies* pollen”. However, in Fig. 5 we can see that *Abies* percentages do not rise at the beginning of the zone but just before the level 417 ka (may be at 420 ka). Could you draw another pollen zone or a fourth PZ2 subzone between ~ 6410 and 6360? The low *Abies* and high *Pinus sylvestris* abundances reveal a different forest composition than later in PZ2a. This would permit to clearly distinguish the important prolonged interval of “dark conifer optimum”.

Finally, the conclusions would gain in clarity and strength to be more synthetic and clearly organized around the three main objectives mentioned in section 3.

TECHNICAL CORRECTIONS

Please, add the references of the papers showing:

- p.1956, l.9&10: that “*Abies* and *Picea* pollen are not easily transported by wind and *Larix* pollen poorly preserved in sediments”

- p.1956, l. 11 to 15: the discontinuous sequences (or some examples) to which you compare the palynological signature of the Lake Baikal records

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- p.1965, l.24: the d13C record from BDP-93 site
- p. 1967, l.4: the “sub-recent pollen spectra” you are referring to

Typing errors:

- p.1955, l.24: “Fig.2” instead of “Fig.1”
- p. 1956, l.8: “are among the most representative among the Baikal sites”, is that correct?
- P.1957, l.19: “analyses”
- p.1966, l.25: “coniferous taiga” looks like a pleonasm to me
- p.1959 (and Fig. 1 & 3): the benthic d18O stack from Lisiecki and Raymo (2005) is originally named “LR04” stack
- p.1960, l.24: “the Baikal records”
- p. 1967, l.3: “seasonality”
- p. 1971, l. 17: “be regarded as”

Figures:

- Fig. 2: The site named BDP-96 is named elsewhere BDP-96-2, any specific reason? Add on the map the location of the Academician Ridge area and Selenga River mentioned in section 5.
- Fig. 3: The green dashed arrow is probably not well located. “LDAZ 18” ends at 422 ka in this figure while it ends at 417ka in the following figures.
- Fig. 5 and caption: What means the green arrow with “forest”above? And the pink arrow within the *Pinus sylvestris* curve? Please specify if the diagram represents the main pollen taxa of the spectra or the whole pollen assemblage and the age of each pollen zone boundary on the side of the diagram.

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- Fig.6: “Illitization” in yellow is hard to read. What means the grey arrows and curve close to the illite percentages?

Interactive comment on Clim. Past Discuss., 5, 1951, 2009.

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