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Climate of the Past Discussions

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 is an ambitious manuscript which seeks to bring together old and new proxy data for the interglacial stage during MIS 11 in two Lake Baikal records. In this respect the paper is within the scope of CP, and new data are presented for an important time-slice in the Earth's recent history.

The proxies considered in the manuscript include (i) BioSi, (ii) diatom composition, (iii) δ18Odiatom composition of diatom silica, (iv) organic geochemistry (%TOC, %TN, C/N ratio, δ13C), (v) pollen and (vi) bulk mineralogy. Some of these proxies (diatom

composition, organic geochemistry) have been analysed for two sites with contrasting sedimentation histories (BDP-96; BDP-99). Others have been analysed for BDP-96 only (BioSi, δ18Odiatom) or for BDP-99 only (pollen, bulk mineralogy).

The main findings of the manuscript confirm that the interglacial period during MIS 11 in Lake Baikal was long, with pollen evidence indicating a warm, moist climate lasting at least between c. 424- c. 396 ka BP. However, the new BDP-99 record did not recover the early stages of MIS 11, so new insights into the important MIS 12/11 transition period are not possible.

I recommend that the objectives for the manuscript be revised. The first objective is to discuss the duration and orbital signature of MIS 11 interglacial. But the orbital signature has largely been described before in previous BDP publications. And as the new BDP-99 record is missing the start of MIS 11, conclusions about its duration are limited to a certain extent. The second objective is broader, and seeks to discuss the climatic signature of MIS 11 interglacial by comparing lacustrine proxy records and sediment mineralogy with the first detailed MIS 11 palynological record. However, the discussion of the pollen record is actually quite qualitative. Moreover, there is no real in-depth discussion of past climate variability through comparing pollen with mineralogy evidence.

From the organization of the manuscript, at times it is not clear which data have been published before, and which data are published for the first time. This could be made more explicit. Nevertheless, this manuscript is a welcome addition to the MIS 11 CP special issue.

Specific comments:

Section 2: P1955: lines 16-18: maybe also highlight that in recent years ICDP programmes have facilitated other long lacustrine records to be retrieved, including Malawi and Bosumtwi. And of course there is also Tule Lake in western USA which has a continuous diatom record spanning c. 3 million years.

Section 3: P1957: lines 4-6: need to specify here that this is the first time that proxies for weathering have been undertaken on an MIS 11 sequence in Baikal, as these proxies have been looked at in previous Baikal studies. State here that these data are newly published. The authors could also briefly comment on the data on e.g. grain size characteristics during MIS 11 from BDP-98 presented by Ochiai & Kashiwaya (2005).

Section 4: P1957: lines 10-13: would it be possible to construct pollen zones using numerical techniques such as constrained cluster analysis? Such zones can then numerically be tested for significance (e.g. using broken stick), and used as a robust basis for interpretation. Furthermore, numerical zonation of the BDP ought to greatly enhance comparison with future sequences as they are being carried out. Numerical zonation of all proxies might further help interpretation of whether changes in e.g. pollen and diatoms are coeval etc. Other numerical techniques such as ordination would also bring out the major trends in the pollen data, which can greatly aid interpretation.

Section 5: Very good detail is provided with regard to how the BDP cores were originally dated, together with recent developments (e.g. Prokopenko et al. 2006). However, the construction of the age model for BDP-99 is rather brief, although I agree that trying to orbitally tune the BDP-99 record might not be the way forward.

In this age model section, the authors should make it absolutely clear that the age model for BDP-99 has been developed from identifying major, identical diatom changes with BDP-96 and using these as tie-points. Reference to Fig 2 here is not actually very helpful, as I at first took it to read that the BDP-96 biogenic silica was compared with the BDP-99 diatom relative abundance record, which is obviously very different. So I would recommend that some of the diatom detail provided in Section 7 relevant to construction of the age model be moved to section 5.

Section 8: P1964: lines 9-16: I agree that the C/N records are counter-intuitive, given the depositional settings of the two core sites. This is very interesting, and needs more work that is probably beyond the scope of this current manuscript.

P1964: lines 20-23: it is true that at the MIS 12/11 boundary there is a small lag in increase in BioSi compared to TOC and TN. But the diatom increase is comparable – why might BioSi and diatom records differ?

P1964: lines 23-25: does BioSi really lag TOC max by as much as several ka only? TOC max seems to be at 417 ka, whereas BioSi is much later at SP19 c. 404 ka BP.

P1965: lines 18-19: this excursion also occurs at the same time as a decline in δ18O diatom values, which Mackay et al. (2008) also interpret as slowing down of the THC in the North Atlantic.

Section 9: Overall, a good descriptive treatment of the pollen data.

P1970: lines 1-3: However, if the pollen is the “centrepiece” to the manuscript, then it is rather a shame that much of the interpretation and quantitative reconstructions are being reserved for another publication.

Section 10: It is intriguing to see different proxies contained in Baikal sediments during MIS 11 apparently all responding to regional cooling but via different mechanisms. But maybe this section could come at the end of the manuscript, taking into account all the proxies, including the mineralogy record. Millennial-scale variability is of course a key question in comparing Quaternary interglacials, no more so than e.g. MIS 5e and MIS 11.

Section 11: P1970: in comparison to the other proxies analysed by the BDP team, the resolution of the mineralogy proxies is much lower than the other proxies being considered. Some consideration needs to be given how this will impact on conclusions able to be drawn between the proxies analysed.

P1972: the authors invite a considerable about of comparison between Holocene datasets (diatoms, pollen, mineralogy) and MIS 11 all from different papers. This does not make the reading of this manuscript straightforward, as to verify what is being said one must consult the original articles. Instead it would be easier to show the Holocene

and MIS 11 data together where relevant. For example, on P1972, lines 16-17 we are invited to consider visual similarities between BioSi and illite profiles during the Holocene and MIS 11. The reader should at the very least be directed to the relevant Fig(s) in Solotchina et al. 2009.

Technical corrections

P1953: Line 4: Tighten up the sentence starting “Instrumental and even historical records. . .”; the start of the Holocene (c. 11.7 ka yrs BP) started well after the end of the last ice age.

P1953: Line 19: replace “. . .3-4 orbital precession. . .” with “. . .4-5 orbital precession. . ..”

P1955: line 24: Fig. 1. Should be Fig. 2.

P1956: line 1: Mackay et al. (2008) focussed really on oxygen isotope analysis of diatom silica rather than diatoms per se.

P1956: line 22: given that the MIS 11 BDP-99 record is incomplete, can the authors really discuss the duration of the interglacial?

P1960: line 2: I would not use the word ‘remarkable’ here – the duration of the IG can be explained and modelled

P1964: lines 28-29: not only biomarker studies, but pigment studies in general (Fietz et al. 2007), e.g. Chlorophyta from Chlb

P1965: lines 1-2: are the Baikal Holocene dates given accurate, given the problems that BDP had in using 14C to date Holocene sediments.

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