

## ***Interactive comment on “A 250 ka oxygen isotope record from diatoms at Lake El’gygytgyn, far east Russian Arctic” by B. Chaplignin et al.***

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Received and published: 2 July 2012

There are few continuous terrestrial records of environmental change, spanning several glacial / interglacial cycles from the far-east Russian Arctic. This makes the isotopic data presented here by Chaplignin et al. timely and exciting. The isotope record certainly provides new insight into past interglacial variability in this region, and demonstrates the strengths of Lake El’gygytgyn as a key site for palaeoclimatic studies.

Overall, interpretation of the data has been robust. However, there are several areas where a more critical stance would strengthen the manuscript even further, and I outline these below.

Introduction

P1171, Line 6: replace “an” with “a”; replace “emerging” with “growing”

P1171, Line 14: Bezrukova did not measure any isotopic values. Also, this sentence does not really make much sense as  $\delta^{18}\text{O}$  values are affected by a number of processes. It may be better to decompile this sentence. For example, we know that the majority of precipitation to the Lake Baikal region comes via the Westerlies during summer months. In the lake itself,  $\delta^{18}\text{O}$  is influenced mainly by inflowing rivers. Furthermore, those with southern catchments have higher  $\delta^{18}\text{O}$  values due to lower proportion of snow-melt (e.g. Afanasjev 1976; Seal and Shanks 1998).

P1171, line 16: delete “long-term”

P1172, Line 2+ The sentence starting “By taking...” Should be altered. The observation that a palaeoclimate signal can be obtained here is “rare” is due to the lack of suitable archives, not so much the proxy.

P1172, Line 17: be explicit here in terms of species-effect on isotope fractionation – but thought not to be important?)

P1173, Line 2: no capital letter needed for lake

## Methods

I like the fact that the authors have adjusted preparation protocols based on the size of the dominant diatom fraction. Given that these diatoms are relatively small, it must have been quite challenging to obtain ‘pure’ samples.

P1175, Line 5: I wonder if Swann and Leng 2009 is the best reference to use here, and wouldn’t Brewer et al. 2008 be more appropriate? Also Chaplignin 2012 should be Chaplignin et al. 2012

P1175, Line 6: perhaps don’t use “cont.” as abbreviation for contamination, as cont. is usually used as an abbreviation for continued.

Otherwise a robust account of contamination is provided.

P1176, Line 14+: which of these bonds, if any, are influenced by, or are a product of diagenetic changes? i.e. what specifically will FTIR be expected to find in this respect.

P1176, Line 23: delete '-1' after 4cm?

P1176, Line 26: provide a reference for the observation that absorbance peaks > 1500 cm<sup>-1</sup> are not related to biogenic silica

## Results

Nice account of contamination assessment and correction methods

P1179, Line 23: why are correlations given as r<sup>2</sup> values (coefficient of determination)? (cf. abstract where r values are quoted)

## Discussion

Section 4.1.1. is nicely argued

P1181, Line 8-10: given quite marked changes in lake levels during e.g. middle Pleistocene, are changes in photic zone small due to bathymetry of the impact crater?

P1181: Wilkie et al. 2012 reference is still in preparation. So either refer to as Wilkie et al. (unpublished data) or better still, include relevant isotope data in this manuscript. For example, it would be helpful to show  $\delta^{18}\text{O}$  vs.  $\delta\text{D}$  plot of data from lake water, inflowing rivers and precipitation in relation to the global meteoric water line. Such a figure would also depict nicely the info given in the latter section of 4.1.2

P1182, Line 21+: Nolan et al. do state “That is, in the modern record, general warming (local or imported) is more important by orders of magnitude than changes in storm tracks in controlling air temperature at Lake El'gygtyn”. But this is during a time of unprecedented anthropogenic global warming. Do models also show this for periods not affected by AGW?

Also, there is evidence that during the last interglacial warm wet climates in northern

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Siberia persisted due to changes in AMOC influencing currents along the coast of northern Siberia (e.g. Velichko 1984).

Therefore, is the evidence really that robust that conclusions from Nolan et al. 2012 “suggest that these weather patterns have been relatively stable with time and are likely representative of this and other interglacial periods”. I note that this paper is still undergoing the review process, so I'd be interested in seeing more evidence for such a claim.

But otherwise a robust consideration of the potential controls on  $\delta^{18}\text{O}$  is given

Page 1184, Line 10: “Apart from this study” is out of place. Omit.

Page 1184, Line 11: Give the average Holocene resolution

Section 4.2.1: Were both isotope studies done on the same material?

That the two records have significantly different isotope values especially for the Holocene period is important. The authors here do go through potential reasons, and each are dismissed. Nevertheless, there is an issue about reproducibility within any one site (lake) that should therefore be emphasised more. Furthermore, what other potential sources of error not discussed here have the authors considered? What are the main types of clays found in Lake E's sediments. Could these be important?

Section 4.2.2: in Fig 7, need to state that the shaded parts of the stratigraphy indicate interglacial periods. However, the shaded area for MIS3 is different for Figs 7 and 8 because the shaded area in Fig 7 is too broad.

Also, in fig 7, swap  $\text{TiO}_2$  and BSi, so that BSi can be better compared with the isotope record. In the discussion, discrepancies with respect to e.g. correlations (P1186, Lines 3-8) are given in terms of age periods (e.g. LGM). What do the data look like plotted on an age scale? What is the correlation between  $\text{TiO}_2$  and mag susc – I assume that one would expect these to show quite a high correlation.

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P1185, Line 25-25: Colman ref is for BSi in Lake Baikal, and really only for interglacial periods. Given that Baikal is such a unique water body perhaps the authors could provide a bit of more detailed consideration of BSi in lakes over glacial – interglacial periods. For example, bottom waters of Baikal are oxygenated, and so different from anoxic bottom waters of Lake E.

P1185, Line 14-16: Colman et al. attribute increasing BSi in Baikal to increasing summer temperatures during interglacials. Moreover, I would be surprised if there was not a direct link between TiO<sub>2</sub> and precipitation, especially if a major source of TiO<sub>2</sub> comes from fluvial input (as stated in previously). So do the authors really think that increases in BSi are subject to “delayed reactions”? I’m not sure I understand how this would be manifested in a lake ecosystem. The authors suggest that  $\delta^{18}\text{O}$  peaks “often” occur earlier than e.g. TiO<sub>2</sub> minima – how often? The majority of the time?

Section 4.3: P1186, Line 24: the NGRIP curve covers interglacial and glacial periods. The authors should state early on what LR04, NGRIP and EPICA data shown are representative of.

P1187: statistical evaluation has been undertaken between the isotope data and the climate proxy data. Were such evaluations done for the correlations between isotope data and BSi, TiO<sub>2</sub> and mag susc? How do the authors arrive at the conclusion that “a clear precipitation driven climate signal is preserved in the d18O record from diatoms. . .”?

P1187, Line 15+: how exactly does the correlation with obliquity support the proposed age model? Does it matter that the age model was tuned to insolation?

P1181, Line 24: the  $\delta^{18}\text{O}$  record is assumed to be one of temperature, but on P1187,  $\delta^{18}\text{O}$  preserved a clear precipitation signal – can both drivers be determined from one proxy?

But this section does show that the proxy has great palaeoclimate potential

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Section 4.5 should ideally come before the palaeoclimate interpretation – i.e. demonstrate that there is little diagenetic effect, then following interpretations can be done with confidence.

Figures:

Age scales between the Figures Fig 4b and the rest need to be the same units

References used in review:

Afanasjev AN. 1976. The Water Resources and Water Balance of the Lake Baikal Basin [in Russian]. Nauka, Novosibirsk

Seal RR, Shanks WC. 1998. Oxygen and hydrogen isotope systematics of Lake Baikal, Siberia: implications for palaeoclimate studies. *Limnology & Oceanography* 43 : 1251-1261

Velichko, A.A., 1984. Late Pleistocene spatial paleoclimatic reconstructions. In: Late Quaternary Environments of the Soviet Union. Ed: A.A. Velichko, University of Minnesota Press, Minneapolis. Pp 261-285

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