

Interactive comment on “Detailed insight into Arctic climatic variability during MIS 11 at Lake El’gygytgyn, NE Russia” by H. Vogel et al.

Anonymous Referee #1

Received and published: 31 January 2013

General comments

1. The discussion paper of Vogel et al. provides a multi-proxy study of climate and environmental change during MIS 11c based on a core from Lake El’gygytgyn, NE Russia. The core archive from Lake El’gygytgyn is beyond any doubt of unique quality in the Quaternary since it offers a continuous environmental archive from the high northern latitudes of the last 2.8 Ma (as documented by magnetostratigraphy). An overview of this great archive has been published (Melles et al. 2012, *Science*). Here the authors focus on the interglacial associated with MIS 11c, which is considered to be an exceptional interglacial since (i) the orbital climate forcing resembles that of the Holocene and near future, and (ii) the interglacial conditions in MIS 11c were most likely exceptionally long and warm; a finding that may hold implications for the prediction of

C3247

future climate change.

2a. Most of the records used in this study (i.e. BSI, TOC, TOC/TN, Ti, Mn/Fe) are of great resolution (decadal-to-centennial-scale), however, the relation to climate is partly not visible (i.e. in case of the Mn/Fe-record) or remains hardly constrained (i.e. TOC, TOC/TN). This is also reflected in the manuscript text, as the records seems to offer a substantial degree of freedom in interpretation. Speculations occur at several occasions in the text (see line-by-line comments). I suggest to reduce the speculative sections in the manuscript.

2b. The BSI-record seems to be a useful climate proxy since BSI-values are a measure for the diatom frustules frequency which is strongly influenced by duration of ice cover at Lake El’gygytgyn, and hence by climate. Thereby, the BSI-record from El’gygytgyn reflects the MIS 11c signal known from globally integrating ice-volume proxies (L&R 04 stack, or relative sea level), Antarctica, European pollen records and Lake Baikal. I suggest to make stronger use of the BSI-record.

2c. The record which offers the most detailed and direct insight into climate change, i.e. the pollen record, is unluckily much too low in resolution (20 samples for a 35-ka-long interval) to draw robust conclusions. In addition, if the pollen percentage data are meant to reflect open (tundra) versus closed vegetation (forest-tundra or taiga), then the 100 % reference sum should include representatives of open vegetation such as Poaceae; whereas Spores should not be included in the reference sum. I strongly recommend to recalculate the percentages based on a reasonable reference sum; and plot the pollen data in a more informative way (for details see specific comments).

3. The authors conclude that the warmest and wettest phase of the interglacial associated with MIS 11c was between 418–415.5 ka. In specific the authors argue “Coinciding peaks in MTWM, PANN, spruce pollen, and TOC (Fig. 3b, g, i, k), together with the occurrence of OM-rich lenses in the sediment record between 418–415.5 ka (Fig. 2), indicate the warmest and wettest phase at Lake El’gygytgyn during MIS 11.”

C3248

This would imply that the warmest and wettest phase of the interglacial associated with MIS 11c was in the early part of the interglacial, which is in contrast to what we know from that interglacial, i.e. the climate optimum in the upper part. Closer inspection of the El'gygytgyn data shows that the data do not support a warmest and wettest phase between 418–415.5 ka since: (i) the highest values of spruce MTWM, PANN, and the TOC data (which integrate the occurrence of OM) are at around 401, and not between 418–415.5 ka (ii) the BSI record which seems to be a useful climate proxy, shows highest values in the upper part of the interglacial, as known from globally distributed sites. Thereby, the conclusion that the warmest and wettest phase in El'gygytgyn was in the early part of the interglacial is not substantiated by data.

4. The same applies to the conclusion about a precipitation anomaly at 401 ka that is (according to the authors) “associated with a significant increase in soil erosion”. The data do not support that since: (i) the pollen sample at this depth indicates highest Picea percentages together with lowest values of taxa indicative for open vegetation. Hence the pollen data indicate that the vegetation cover at 401 ka was dense; a finding that does not support soil erosion. (ii) The PANN values at 401 are not significantly higher than between 418 and 415 ka (iii) The authors use the peaks in the TOC and TOC/TN records at 401 ka to argue for a precipitation anomaly. However, these peaks indicate an increased input of OM into the lake and as the peaks correlate with peaks of Spruce, it rather seems like that the TOC and TOC/TN peaks reflect the input of litter from tree populations surrounding the lake.

5. The authors conclude that full and remarkable stable interglacial conditions persisted for ca. 27 kyrs between ca. 425 and 398 ka BP. Given that the interval in question is not constrained by an independent chronology but tuned to the orbital target the conclusion seems not too robust. Apart from chronological issues the reader may raise the question why Picea percentages are reduced between 415 and 405 ka BP (this may indicate a muted cooling within the interglacial). Since percentages of total trees and shrubs do not decline within this interval, it would be good to learn which

C3249

tree or shrub taxa gained during that interval (In the specific comment – for page 6320 lines 15 to 30 – I suggest steps that will help to get from pollen data better information on vegetation change). The answer to this question could tell us how stable the climate conditions were in the high northern latitudes during MIS 11c.

6. Figure 4 puts the Lake El'gygytgyn record in large/global context which is desirable. However, the question emerges whether the records are placed on chronologies that allow such a comparison. Especially, the records in figure 4e and 4f (which do not comprise the base of MIS 11c) seem to be placed on a chronology that allows no comparison with the other records in figure 4. I suggest to skip these two records and include an SST proxy record from ODP Site 980 in the N-Atlantic (McManus et al. 1999). Still, the question remains how comparable are the chronologies.

In summary, since the study is based on a archive of unique quality in the Quaternary and the BSI record seems to be a useful proxy that provides information on climate variability in the high northern latitudes during an exceptional interglacial I think the study should be published provided the authors properly address the comments given above.

Specific line-by-line comments

abstract page 6310 line 5 be more precise. The paper is dealing with Substage 11c I suggest to replace “MIS 11” by “MIS 11c” in the entire paper including headline

line 10 how do the authors know that the forests are “dark”? please replace “dark coniferous forest” by an appropriate term like “forest-tundra”

line 12 persisted for ca. 27 kyrs between see general comments: not constrained by an independent chronology

line 14: “teleconnections” a typical bass word

line 15 A peak warm period between ca. 418–415.5 ka and a precipitation anomaly at ca. 401 ka see general comments; not substantiated by data

C3250

page 6311

line 18 "Numerous" or several?

line 23 McManus et al 1999 do not say that, it was Loutre and Berger 2000, 2003 who pointed that out first (actually, the first time at the INQUA 1999 meeting). McManus et al. 2003 in Droxler (ed) Geophys. Monogr 137; and Tzedakis 2010 CP are appropriate references

6312 line 23 skip "typical"

6313 line 3 please indicate whether the core have been spliced together within the interval considered in this study

6317 line 2 Is the Mn/Fe record really that helpful?

6318 line 26 Some Mn/Fe peaks are high but no generally high values are visible

6319 line 25 the term "available" does not indicate clearly which data shown in this paper are new and which are already presented in Melles et al 2012.

6320 line 8 "Correspondingly," I suggest to phrase: "Pollen-based climate reconstructions suggest, that . . ."

6320 lines 15 to 30 "The lagged response of in-lake productivity in comparison to the gradual establishment of forest-tundra in the vicinity of the lake may be explained by . . ." See general comment #2a The lines 15-30 are an example for a substantial degree of freedom in interpretation and speculation which is actually not needed. Have a look on the BSI values: the increase shows no lagged response in comparison to the increase of Spruce.

The summary pollen record in fig. 3f might be misleading as the green shading sums up trees and shrubs. In order to get better information on vegetation change I suggest:
- remove Spores from the 100% sum - sum up the percentages of herbs and grasses and make one shading that represents open vegetation - sum up the percentages of

C3251

real trees and make one shading that represents closed vegetation - sum up the percentages of shrubs, make one shading for them and plot that in between the shading for open and closed vegetation These steps will help to get clear information from the pollen data and sort out equivocality in the comparison with the other records.

6321 line 21 ff." See general comments: it would be helpful to learn why Picea percentages are reduced between 415 and 405 ka. Is Picea replaced by shrubs or by other trees (e.g. Alnus)? If the authors decided to perform the steps suggest above this would help to sort out how stable the interglacial was in the high northern latitudes

6322 line 13ff "Coinciding peaks in MTWM, PANN, spruce pollen, and TOC (Fig. 3b, g, i, k), together with the occurrence of OM-rich lenses in the sediment 15 record between 418–415.5 ka (Fig. 2), indicate the warmest and wettest phase at Lake El'gygytgyn during MIS 11." See general comment # 3

6322 line 22 to 6323 line 5 See general comment #2a these line are contain lots of speculation

6323 line 9 to 23 again lots of speculation

6324 line 10 dark coniferous forests ???

6325 line 1 "In contrast, marine sea surface temperature (SST) records from the Atlantic . . ." Skip "marine"

6325 line 4 to 20. " Lots of speculation!!! . . . to explain contrasting patterns The simple explanation for the contrasting pattern is rather that the chronology of the records in Fig. 4 e and 4f is not comparable (different tuning?) to the rest of the records in fig 4

6326 line 4 ff "The indicated coeval timing of the B/A-like event at lakes El'gygytgyn and Baikal with the ACR-like event (Fig. 4) . . ." There is no evidence for a "indicated coeval timing" I agree it is tempting to see that but it is again . . .speculation . . .

6326 line 10 "The warm period between 418–415.5 ka, suggested as being the

C3252

warmest period during MIS 11 at Lake El'gygytgyn, See general comments:

6326 line 20 ff "...suggest the peak warm period at Lake El'gygytgyn between 418–415.5 ka to be a regional phenomenon. Possible explanations for this peak warm period could be..." until 6327 line 14 "remains elusive." Again lots of speculation which are not needed as the El'gygytgyn data do not support peak warm between 418 and 415. (see general comments)

6327 line 15 "...interpreted as being a result of enhanced summer precipitation at Lake El'gygytgyn can probably be regarded as a very restricted regional anomaly. until 6328 line 2 Again lots of speculation which are not needed as the El'gygytgyn data do not support a precipitation anomaly at 401 ka (see general comments)

Fig. 1 explain Lz1024 in the caption

Fig. 2 I suggest to label the depth scale in even units 18.00, 18.10, etc

Fig.3 I suggest to enlarge this figure, maybe rotate it by 90° To show confidence interval for precipitation estimates would be important Consider suggestions to rearrange the the pollen record in panel f

Fig. 4 Skip records in panel e and f (chronology is not comparable) and test how the OPD 980 record plots in this figure

Interactive comment on Clim. Past Discuss., 8, 6309, 2012.

C3253