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Climate of the Past Discussions

Interactive comment on "Vegetation history of Central Chukotka deduced from permafrost paleoenvironmental records of the El'gygytgyn Impact Crater" *by* A. A. Andreev et al.

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Dear Editor,

All reviewer comments, notes and suggestions have been used to improve the manuscript. The manuscript was also checked by a native English speaker. Please, see below my replies to the comments.

Yours sincerely

Andrei Andreev

To Anonymous Referee #1

The General Comments and comments, notes and suggestions in the supplement have been used to improve the manuscript. In one case (comment to line 24, p. 1422) is there any pollen data from GS-37? I would assume the pollen would have shrub taxa. There are no information that sample was also analyzed for pollen. Concerning the first general comment about core LZ-1024 published by Matrosova (2009). Fortunately these pollen data are now partly published by Melles at al 2012 in Science, and, therefore, more easy to see. Missing references for the comparison of potential MIS 7 record with PG-1351 pollen data (Lozhkin et al 2007) are now in the text.

Concerning the second general comment about local and long distance Larix pollen. It is also clarified now.

To M. Edwards, Referee #2

All comments have been used to improve a quality of the manuscript. However, I would like to reply to some comments.

First, Larix is not renowned for over-representation or long-distance transport, whereas Alnus and Pinus are. Some references to this would be useful. Also some information on the current distribution of these taxa and clearer information about their past records would be useful. Perhaps a map?

This is really important questions. I was trying to do my best to follow the recommendation. These questions are rather well discussed in the cited Matrosova et al., 2004; Matrosova, 2006, 2009; Glushkova et al., 2009 and are not in a main focus of our study. However, I completely agree about the importance of the surface pollen samples. Surely, that Larix is never over-represented in the pollen spectra. However, its single grains may be present in the surface pollen spectra, even far away from their distribution limits. One of the authors, namely E. Morozova, in August 2011 collected 20 samples under the local plant communities in the lake crater. Single larch pollen grains have been found in some of these samples. After finishing the analysis we plan to publish these data paying a special attention to pollen representation and current

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distribution of the taxa.

Second, it is recognized that the climate implications of the Lake E core are compromised by the long-distance pollen. But the terrestrial records have their own problems of interpretation. In some places, differences between terrestrial records are attributed in the manuscript to local variation in vegetation (presumably due to different site conditions and processes). In contrast, P2 shows an interesting Younger-Dryas type fluctuation in the pollen, whereas P1 does not. Here the P2 record is interpreted as a major climate change, but no possibility of site differences is considered. This is especially important to think about as the highest woody taxa values come from organic-poor pre-Holocene sediments (itself rather interesting and could be the subject of more discussion).

I agree that terrestrial permafrost records have their own problems of interpretation. But I disagree that Younger-Dryas type fluctuation we can see only in P2 record. All three terrestrial records show the similar patterns, but with the different amplitudes. Moreover, LZ-1024 showing the similar that Younger-Dryas type fluctuation is recently published now in Science by Melles at al., 2012. That reference is in the manuscript now. I also have tried to clarify things concerning differences between terrestrial records.

Finally, the 14C dates are not bad at all for a landscape subject to permafrost processes of slope movement. There are, however, major hiatuses, and it would be good to begin the interpretation by linking the dates and any information from the sediments themselves to possible slope processes that might confound the record (hopefully dismissing this possibilityâËŸAËĞTbut it needs to be considered).

Permafrost processes are really a trigger of slope movements and resulted in dramatic changes in sedimentation rates including hiatuses. However, it is very difficult to trace the influence of such processes on formation of past pollen assemblages. The possible influence of Late Quaternary climate to sedimentation in the studied cores is discussed in the special papers (Schwamborn et al., 2006, 2008, this volume). Generally, as we

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can see in all pollen records (including the not-directly-permafrost-influenced lacustrine record, LZ-1024 recently published now in Science by Melles at al., 2012), all pollen record show the similar fluctuations, and, therefore we may suggest that permafrost processes did not much influence the pollen assemblages in the area.

1410-23 – better expressed now.

1411-3 et seq. – clarified.

1412-1 et seq – it's not really permafrost per se, but rather permafrost-affected sediments, and we need to know what type of sediments. This perennially frozen sediments and active layer varies between ca 40 and 60 cm. So, we probably may use permafrost sediments in the text. One author is a specialist in that field, but we do not want to discuss that in the pollen paper. Moreover, the detailed descriptions of the studied sediments are already published (Schwamborn et al., 2006, 2008, and this volume).

1412-17- changed

1412-22 please clarify what you mean by "loose" sediment The detailed descriptions of the studied sediments including the used term are in Schwamborn et al., this volume.

1413-5 clarified. 1413-12 clarified. 1413-18 added 1416-3 et seq. Given the core sediment is interpreted as derived from various forms of mass wasting transport, and that the sediment accumulation rate is extremely uneven, I think it would be useful to discuss the reliability of the profile as regards possible hiatuses or redeposition of material out of temporal order. Where are the last 3000 years? Is this the functional radiocarbon age of modern soils (due to slow decomposition and/or frost heave)? If so, what is the "real" age of the underlying sediments? For example, between 240 and 270 cm is an apparent age gap of about 3000 years.

I agree that the sediment accumulation rate is extremely uneven. However, it is very difficult to discuss much about the reliability of the studied profiles taking in consideration

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revealed and possible hiatuses, re-deposition of organic material and lack of organic in the core sediments. Nevertheless, we tried our best to explain our results taking in consideration the uneven accumulation rate, avoiding the possible misinterpretations and/or overestimations of the obtained results. Concerning the functional radiocarbon age of the soils. Unfortunately, we cannot say about the "real" age of the underlying sediments. Based on our experience we believe that our radiocarbon results rather well the age of the sediments. But we are trying to avoid misinterpretations and do not make any age/depth models for the core sequences and prefer to use more common terms like early and late Holocene to estimate ages of the revealed environmental changes. Concerning the missing last 3000 years. It is a common when sedimentation or sediments were removed because of following erosion. Such processes are common in the Arctic.

1416-25. Do the surface samples show Larix as a long-distance pollen grain at the site today? I have generally understood Larix pollen to be poorly distributed and likely to reflect local production. Why do you assume it is long-distance transport? Perhaps such an assumption should be left to the Discussion, especially as in P2 you report macrofossils.

Yes, the surface samples contain single Larix pollen grains, which are certainly must be treated as long-distance pollen at the site today. We changed the text to make it clearer.

1420-23. The text is changed to make it clearer.

1421-19 It would be better to present the pollen diagrams with calibrated ages as that's what you use to discuss them in the text. You might point out that the date of 11,160 14C is in the middle of zone I at P1 and you have no dating control between that and the cluster of dates 10,000-9000 higher up. The upper part of zone I may be younger than 13,000 cal yr BP. The dates are not showing properly on Figure 4.

We think that presentation of calibrated ages in the pollen diagrams will make the di-

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agrams less readable. So, it is better to present of calibrated ages in the table and discuss them in the text in order to make clear how they are reliable etc. Yes, unfortunately, we have no dating control date between 11,160 in the middle of zone I of P1 and the cluster of dates 10,000-9000 higher up. Unfortunately, there are no organic to date these sediments attributed to the Younger Dryas. However, based on all pollen records (including lacustrine LZ-1024) showing the similar trends we believe that the upper part of zone I is not younger than 13,000 cal yr BP. The dates on Figure 4 are shown properly now.

1422-25 et seq – I feel slightly uncomfortable that the nature of the sediment changes and discontinuities is not examined independently from the pollen record. There is a tendency here to assume this is the "Allerod" and therefore must be warm and wet and thus cause the sediment and pollen changes observed. There probably is a relationship, but hiatuses or active slope movement could also create artifacts, or truncate sequences, and this is not considered. Authors in Beringia generally do not use "Allerod" to describe late-glacial phases. I agree that the P2 record shows a reversal to less shrubs and more graminoids during a period that includes Younger Dryas time, and I think that you should suggest that this is quite interesting and unusual, and not just assume that the YD oscillation should be present at this site. It is not everywhere in Beringia(see Kokorowski et al 2008). If Matrosova in her papers refers to the YD please report it here for non-Russian readers.

Actually, the nature of the sediment changes and discontinuities were examined independently from the pollen record. For details please see Schwamborn et al., 2006, 2008, this volume. However, it is not easy to see such changes and especially to interpret them unambiguously.

Although some Beringian authors do not use Allerod and/or Younger Dryas to describe late-glacial phases, these terms are also commonly used in publications concerning Beringia or El'gygytgyn Lake. For example it is the most recently used by Melles at al., 2012 in Science. Yes, Matrosova in her papers refers to the YD (e.g. see Matrosova

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2009 p. 28). It is properly cited now.

1424-12 changed. 1426-3 clarified and added. 1426-18 clarified. 1426-25 replaced. 1427-5 added. 1427-14 et seq. Before concluding, if the long-distance vs local presence of shrubs and trees at the site is a key element in your analysis, it might be useful to devote a small section to this in the discussion. I agree that it is a difficult problem in the north, and being able to distinguish local vs regional pollen rain is important in making palaeoenvrionmental reconstructions. So I would increase the prominence of this part of the study (and also ensure that your statements about the different species do not contradict – see Larix above)

The text is now changed in order to avoid the possible contradictions and to clarify the statements.

1428-3 et seq. Earlier, you point out, quite reasonably, that differences in pollen spectra between the sites might reflect differences in local development of the vegetation. Typically, highly local records are not the most reliable climate records. Here you assume that the pattern observed at P2 (it is not clear really that it appears at P1) is now a regional climate record.

Well, I do agree that highly local records are not the most reliable climate records. But our permafrost records are not such highly local records as they show the same pattern (trends) in development of the vegetation, which are very similar to those in the lake record (see Melles at al., 2012)

It would be better to highlight that P2 apparently shows a clear reversal that corresponds to YD time (interesting), although P1 not so clear. This could be a relatively local phenomenon, or it could be a real reflection of the regional signal. It also seems interesting that the shrub values in P2 are higher in the “Allerod” than they are in the early Holocene, even though you emphasize the early Holocene as the key time for climate warming (at least as suggested by other regional records and records of treeline shifts). Why then are the values so high in P2 early in the record? Do you think

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the climate was even warmer then? We believe that P2, P1, 5011-3 as well as lacustrine LZ-1024) apparently shows a clear reversal that corresponds to YD event. We do not think that it could be a relatively local phenomenon, it is apparently a real reflection of the regional signal. I do not think that the shrub values in P2 are higher in the Allerod sediments than in the early Holocene ones. Moreover, you can see that during the early Holocene peak shrub alder pollen are showing values higher than during the Allerod one. Taking in consideration that shrub alder is more thermophilic taxa than dwarf birch or dwarf willows we may assume that the early Holocene climate was warmer than during the Allerod. The detailed climate reconstruction based on lacustrine LZ-1024 pollen records is published now in Melles at al., 2012. We refer to that publication to make our statements clearer.

There is little or no TOC in the earlier (pre-YD) part of the core. What are the likely issues with respect to pollen preservation and/or the growth potential for shrubs on this earlier form of substrate?

Pollen preservation was about the same through the cores, so we can exclude that factor. I assume that the growth potential for shrubs on form of substrate was most likely differ during the Allerod, Dryas, and Holocene however, such things are not possible to estimate basing only on the pollen assemblages.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/8/C740/2012/cpd-8-C740-2012-supplement.pdf

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