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Climate of the Past Discussions

Interactive comment on "Late Glacial and Holocene changes in vegetation cover and climate in southern Siberia derived from a 15 kyr long pollen record from Lake Kotokel" *by* P. E. Tarasov et al.

P. E. Tarasov et al.

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On behalf of all co-authors I would like to express our gratitude to the reviewers for the comprehensive reviews, which allowed us improving the text. Our responses to all raised questions and comments are summarized below

Pavel Tarasov

Reply to D. Peteet (Referee #2)

1) The radiocarbon dates should be added to the pollen diagram so that the reader can see the chronology upon which the age model is constructed.

The details concerning the radiocarbon dates (including sample depths) are provided in the section 3.1 Coring, core lithology and age determination. The portrait page format used in the journal makes it difficult to add dates in the pollen diagram without making it difficult to read. Therefore, we used arrows to indicate positions of the radiocarbon dates in the pollen diagram (Fig. 2) and added the dates in the figure caption.

2) The authors do not describe the lithology or loss-on-ignition from the core, which would be helpful in assessing the shifts in climate. Are these available? They should be visible in the diagram.

The core lithology is described in the text (section 3.1), but the LOI analysis has not been performed on our samples. Therefore, this information is not included in the manuscript.

3) Why does the lake begin recording at 15k? The authors do not address this point, and it is

important;does it agree with evidence of regional deglaciation from other lakes and glacial studies?

All reconstructions presented in our study are based on the KTK1 pollen record representing the more organic (and easily accessible for coring) part of the Lake Kotokel sediment, which was accumulated during the late glacial and Holocene interval. This interval is relatively well represented by the pollen records from Siberia (see for example Müller et al. 2009 published in Climate of the Past and references therein), and is roughly synchronous with the late glacial climate amelioration recorded elsewhere. However, the longer cores were obtained suggesting that Lake Kotokel existed at least during the past 50 kyr. The coarse-resolution analysis of the longer core is presented by Shichi et al. (QI, available online). We are now working on high-resolution pollen study of the longer core.

4) Pollen data do indicate the Younger Dryas is a distinctive pollen zone, and yet the

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authors show a very large shift in the YD only in the last figure of the paper. Why does the YD not merit its own zone in Fig. 2?

This is, indeed, an interesting question. Conventionally, we used CONISS statistical package assembled with TILIA in order to identify boundaries of the major pollen zones in the KTK1 pollen record. The pollen zones suggested by CONISS are shown in figure 2. According to this statistical exercise the YD interval only deserves to be the uppermost sub-zone of the KTK1-4 pollen zone. However, already in the figure 3 results of the biome reconstruction show biome/vegetation zone, which is roughly corresponding to the YD. Such phenomenon was previously mentioned and discussed (see Tarasov et al. 2006 in Palaeo3). In our interpretation we rely more on the biome reconstruction results, which take into account plant ecology and not only statistical similarity between pollen assemblages composed of many pollen taxa. The climate and woody cover reconstructions (Fig. 4) are in line with the biome reconstruction results, suggesting that "absence of the YD in the Baikal region suggested by CONISS" is an artifact of the statistical approach and that biome reconstruction can be recommended for zonation of the pollen diagrams.

5) In examining the shifts the authors attribute to YD, the rise of *Alnus* is quite visible. Yet the authors do not say why *Alnus* would have increased, and describe the increase as indicative of colder, drier conditions. Globally, *Alnus* is usually indicative of wetter areas, so this is anomalous. Perhaps the *Alnus* is indicative of disturbance instead, which is also possible, but not necessarily climatically linked. This topic merits further discussion. Where is the modern analogue for high Alnus in Siberia?

The *Alnus* problematic was discussed in details in relation to the Last Interglacial (MIS5e) pollen record from Lake Baikal (Tarasov et al. 2005 Climate Dynamics). Here we put a citation from this paper: "A plot of modern *A. fruticosa* pollen percentages in the Eurasian surface pollen spectra against modern climatic variables shows that high percentages of this taxon, attributed to the arctic-alpine shrub PFT (Prentice et al. 1992), appear under a very severe climate in the tundra of northern and eastern Siberia

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(Fig. 5a). In Fig. 5c *Alnus* pollen in surface spectra located south of the dashed line in forest and forest-steppe vegetation zones, originated from tree forms of alder (e.g. *Alnus glutinosa, A. incana*), while large values of *Alnus* pollen found north of this line (e.g. in forest-tundra and tundra) were mainly produced by its shrub forms (e.g. *A. fruticosa*)." In order to avoid repetitions and to keep the current paper short we only add a reference to the earlier publication in the revised version of the manuscript as following "The modern analogue method and geographical distribution of the modern analogues with *Alnus fruticosa* pollen in Siberia were discussed in details by Tarasov et al. (2005)". We do not see any contradiction with the reviewer's comment that the peak of alder pollen can be an indicator of the improved wetter conditions. In our record it occurs during the AL interstadial and reflects climate amelioration during the generally colder/dryer than present late glacial period (see Fig. 3 and Fig. 4).

6) On page 8, top paragraph, the authors note that between 12.7 and 11.5 there is highest scores of tundra biome; but this is not visible to the reader. Where? What is considered tundra biome. This is confusing, since Alnus is not considered tundra and Salix and Artemisia are very low, much lower than between 14,000 and 13,000 yr BP.

We believe that this comment appeared due to misunderstanding. The fig. 3b shows scores for tundra, steppe and taiga biomes derived from KTK1 pollen records using biome reconstruction method described in details in Prentice et al. 1996; Tarasov et al. 1998, 2000; 2005, etc. In all cited publications shrubby alder is attributed to the arctic-alpine plant-functional type and consequently to tundra biome. *Artemisia* is indeed attributed to the steppe and desert herb plant functional types, which are not contributing to the tundra biome scores. The mentioned sentence was modified as following: "Pollen of alder and birch shrubs (both taxa are representative for the tundra and forest-tundra communities in northern Eurasia, e.g. Prentice et al. 1996; Tarasov et al. 1998, 2005) once more become a dominant component of the pollen assemblages between ca. 12.7 and 11.5 kyr BP".

7) While the results are very quantitative, it does not mean that the interpretation is

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better than some qualitative results based upon macrofossils, LOI, etc. as well as pollen. Authors such as H.H. Birks have noted the problems in interpreting pollen which is windblown with macrofossils studies, and she has argued that both are far superior to pollen alone. I would concur, and hope that the authors keep an open mind to multiple possible

interpretations for their data.

We have a great respect to the works of abovementioned specialists, as well as to the reviewer's own contribution to the palaeoenvironmental research in the Arctic. Under no circumstances we aimed to present our results as "superior" above the other records and climate interpretation approaches. Vice versa we always try to combine different approaches in our studies when the material allows (see for example Kienast et al. 2008 in Global and Planetary Change). So far as we know, there are no publications on macrofossil-based post-glacial climate reconstruction from the Baikal region up to date.

8) p. 12, next to last sentence. Is the Holocene change from pollen really in agreement with the O-18 from Greenland? Look at your winter temp. shifts which Greenland does not have.

The Holocene fluctuations in both records are too small and the Holocene time-control and resolution of the KTK1 record are too coarse to discuss minor or short-term climate variations, without risk of getting more critical comments. However, similarities in the general trends are well visible in Fig. 4. Respecting concern of the reviewer, we corrected the sentence by substituting "perfectly" by "roughly".

9) Many minor grammatical changes can be made to improve the manuscript ; these include: p. 3, line 6, change "the recent" to "this" p. 3, line 10 variable...from about 15 ...to present is... p. 3, 5th line up from bottom, ...has a well-pronounced p. 4, line 11 up from bottom..a Livingston piston... p. 6 line 4 ...from the Continent

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We gave the manuscript to our colleague Anne Beck, who carefully checked English grammar and spelling. All corrections suggested by her and by the reviewer are incorporated in the revised version of the manuscript.

10) p. 7, last line of 2nd para...is this pine possibly reworking?

This would be easy to imagine if the under-laying sediment was deposited during the relatively warm interglacial/interstadial, but this record is an opposite case. Therefore, we do not have enough evidences to suggest re-deposition.

Reply to Anonymous Reviewer (Referee #1)

We are grateful to Reviewer 1 for the positive evaluation of our submitted manuscript. All suggested changes are constructive and will be addressed in the revised version. Below we placed our reply to the questions raised by Reviewer.

1. page 1. 1 kyr =1000 cal. yr. BP.

This repetition was done intentionally. The Abstract is often provided independently from the article and thus the short information about the age definition should be present both in the abstract and in the main text.

2. page 2. Unfortunately, pollen concentration was not specially calculated in this study. However a fixed volume of sediment was taken for the pollen extraction. This provides general ideas on how rich in pollen are different levels.

3. pages 4-5. We are aware that the dating of the pollen zones from bottom to the top can be frequently seen. However, we do not completely agree with this practice, which make a difficulty to continue the pollen description further in depth/time without renumbering the whole profile. The downward numbering is rather common in geology, e.g. all MIS zones are numbered in this way.

4. 'The YD stadial' is of course correct. We corrected this error in the text.

5. Comments 5 to 8 concerning literature are taken into account.

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Authors Reply to H. Grobe (Comment)

(1) The comment of H. Grobe touches an important issue of the primary data availability. In our research we are sharing the same principles. It is therefore stated in the manuscript, that primary pollen data as well as numerical reconstruction results will be submitted to the public database after our manuscript is accepted. This is a common practice and I do not see the reason why we should not follow our promises. Worse to be mentioned that all pollen and lake status data generated by P. Tarasov, for example, is publicly available in the EPD and GPD via the WDC.

(2) We are aware about the requirements of DFG and following them with all respect.

(3) Age-depth model absence. This is clearly misunderstanding. The linear regression is used for the age-depth calculation and it is very easy to calculate the age of the every level using the sedimentation rate value given in the text.

Further modifications:

The reference list was updated.

The figure captions for Fig. 2 and Fig. 4 were updated.

We added the reviewer names in the acknowledgement

The complete data/reconstruction dataset is submitted to PANGEA

Interactive comment on Clim. Past Discuss., 5, 127, 2009.