Clim. Past Discuss., 5, S222–S224, 2009
www.clim-past-discuss.net/5/S222/2009/

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.

D. Peteet (Referee)

peteet@ldeo.columbia.edu

Received and published: 12 May 2009

Tarasov Review

Overall, the paper enlarges our view of the vegetation and climate in a little known region of the world, near Lake Baikal. The author provides a quantitative estimate of temperature and precipitation derived from pollen-based biome reconstructions. The method is objective, and utilizes a sediment core from Lake Kotokel. I belive the paper should be published. However, there are a few problems which would be beneficial for

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the authors to address in the paper. 1) The lake core is very poorly dated, with only 3 dates. They should be added to the pollen diagram so that the reader can see the chronology upon which the age model is constructed. 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. 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? 4) Pollen data do indicate the Younger Dryas is a distinctive pollen zone, and yet the 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? 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. In North America, the rise in Alnus is linked to deeper snows (see Peteet et al., l993 as well as Mayle et al. 1993?). This topic merits further discussion. Where is the modern analogue for high Alnus in Siberia? 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. 7) While the results are very quantitative, it does not mean that the interpretation is 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. 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. 9) Many minor grammatical changes can

CPD

5, S222–S224, 2009

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

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