Clim. Past Discuss., 9, C2088–C2099, 2013
www.clim-past-discuss.net/9/C2088/2013/

Climate of the Past Discussions
Open Access

Interactive comment on "The biome reconstruction approach as a tool for interpretation of past vegetation and climate changes: application to modern and fossil pollen data from Lake El'gygytgyn, Far East Russian Arctic" *by* P. E. Tarasov et al.

M.-J. Gaillard (Referee)

marie-jose.gaillard-lemdahl@lnu.se

Received and published: 23 September 2013

General Comment

The paper by Tarasov et al. presents the pollen-based biome reconstruction using the pollen record from Lake El'gygytgyn (East Russian Arctic) that covers the last ca. 3580 ky BP. This is an extraordinary record already used for qualitative vegetation and quantitative climate reconstructions that were published earlier. The biome reconstruction

offers new insights on the dominant biomes through time and the timing of transitions between periods characterized by different dominant biomes, and provides a great opportunity to evaluate the capacity of climate models to simulate climate characteristics over these periods in a correct way. The paper is a very important and unique contribution to the understanding of past arctic climate change and interactions with the environment, and therefore fully relevant for publication in Climate of the Past. The paper is generally well organized and written, although there are some lengthy parts that could be shortened as well as some language oddities (see specific comments and technical corrections below). The figures are carefully prepared.

In terms of the interpretation of the pollen-based biome reconstruction, I have only one major concern. The authors do not use the method only to reconstruct the "dominant" biome (or the biome with highest affinity with the pollen assemblage), as is usually done, but also use it as a method to "quantify" (in relative terms) the abundance of several biomes from the same pollen assemblage, i.e. use the affinity scores of biomes (or group of biomes) as a measure of their relative abundance in the region of the pollen site: the lowest the score, the less abundant the biome and the highest the score the more abundant the biome. My understanding of the biomization method tells me that this is not a sound/reasonable way to interpret its results. The method allows identifying THE biome (as defined by PFTs or groups of plant taxa) that has the highest "affinity" with the studied pollen assemblage. The biomes that have lower affinities with that same pollen assemblage cannot be reconstructed. The biomization method is qualitative; it uses typological assignment of pre-determined biomes to pollen assemblages based on affinity scores. Guiot and Goeury have used affinity scores of all the biomes (i.e. not only the assigned biome, but all others as well) to optimize the power of pollen-based biome identification using a neural network. It is a way to use the information from pollen assemblages as much as possible. However, it is not reasonable to use affinity scores to "quantify" different biomes in such a way that, for example, the results are used to evaluate forested vs. unforested areas (or biomes) in the region. Affinity scores are not equivalent to probabilities or proportion of the area covered by

CPD
9, C2088–C2099, 2013

individual biome types in the region. In theory, the method does not allow to make such interpretations. One of the interesting aspects of the biomization method is that it uses the concept of "individualistic behavior" of plants into pollen-vegetation relationship in a crude way. Prentice uses the concept that plant communities do not move, but rather individual species move when climate changes and a combination of plant species is defined as a vegetation unit (or biome.) One of the limitations is that the approach is still typological; the method assumes pre-defined biomes, and PFT-based affinity scores among the biomes are compared to assign which biome is likely reflected by the pollen assemblages used. Thus, it is basically qualitative, it is NOT a quantitative method. Therefore, the text should be corrected accordingly, i.e. all interpretations in quantitative terms of the non-dominant biomes should be deleted. It is what I suggest in my comments below. It also implies that all subfigures a and c in Figures 3-6 and subfigure b in Figures 7-8 should be deleted. Pollen % of major trees and herbs that are discussed in the paper along with the biome reconstruction would be more useful to add to these figures. Use the results from biomization for what it is GOOD for, don't make them tell what they cannot tell!

I define the revisions needed as "minor" as they do not imply new analyses and major changes in the organization of the paper, although the concern on interpretation is serious and the revisions require that new figures are prepared.

Specific comments

Below I list all comments that are more substantial than just minor corrections of the language or other details related to the main text and figure captions (see technical corrections below).

1. Title: I would reword the title as follows: A pollen-based biome reconstruction over the last 3580 ky BP in the Far East Russian Arctic – new insights into climate-vegetation relationships at the regional spatial scale.

2. Abstract: (question from a non-expert in Pliocene/Early Pleistocene palaeoecology):

CPD
9, C2088–C2099, 2013

does the abstract really take up all new results/insights? Are the results mentioned new? For instance, is it new that the two interglacials MIS 31 and MIS 11.3 were interpreted as the longest and warmest of the past million years? Or does it confirm something that was not really sure before? Is it new to find that the transition between forested biomes to open vegetation biomes happened step-wise rather than gradually? Isn't this an effect of the biomization, i.e. either this biome, or that biome, therefore can't show something progressive. I do not think you can talk about gradual (if you mean "progressive") changes based on biomization results. You should explain your "thought" in a different way. What do you mean exactly?

3. Abstract: page 3451, lines 25-27 and page 3452, lines 3-7: Change as follows: "The reconstruction indicates that the taxon-rich cool mixed- and cool conifer-forest biomes are mostly characteristic from the time prior to MIS G16, whereas the tundra biome . . ...", and " The reconstructed biomes also suggest that the transition between forested biomes and open vegetation biomes was not gradual but rather occurred in a step-like fashion (or step-wise?) (this should perhaps be deleted or worded in another way, see my comment 2 above). (delete "Thus") The cold- and drought-tolerant steppe biome etc. . .", and " . . .. . . during this generally warm Pliocene interval are characterized by the tundra biome.".

4. Introduction: somewhat weak. . .. It is here I would like to know as a reader what is/are the aim(s) of the study, in this case, the biome reconstruction. It is said later, at several place through the text, what biomization is good for, and why it can add something to the former studies, i.e. the pollen-based climate reconstruction using BMA and the vegetation reconstruction based on pollen %. Say all this in the introduction and formulate your aims, including your model-data comparison in the discussion (it is also one of your aims to use your biome reconstruction to evaluate the model simulations by Kleinen et al., isn't it?).

5. Page 3453, Site setting: what are the size of the lake and the size of its hydrological catchment area? Important for interpretation of the pollen data.

CPD
9, C2088–C2099, 2013

6. Page 3454, line 28: how many cm of top sediment did you collect, ca. how many years?

7. Page 3457, line 17: "However, we retain the use of weighting of Larix percentages. . .. : what do you mean, did you weight Larix % or not? I suppose you didn't. In that case the word "retain" is not relevant. Just write "We did not weight Laris percentages in this study." You may explain why, if there is a specific reason.

8. Page 3457, lines 22-28: confusing wording; reword as follows: "The final biome identification . . ..., i.e. the biome with the highest affinity score . . .. be dominant, and is the biome reconstructed from the pollen assemblage."; and "Prentice et al. (1996) suggested. . ..the problem - often occurring if the taxa/. . .. – by ordering the biomes so that subsets always come first". You should also explain briefly why it solves the problem. Otherwise it is not useful you mention this.

9. Page 3458, lines 11-16: Delete from "However, this missing information . . .. " until ". . ..(e.g.,Fig. 3c)." See my general comment on the interpretation of affinity scores.
10. Page 3459, lines 6-12: Write instead: " The application of biomization on the modern pollen assemblages identifies the tundra biome as having the highest affinity score, which reflects well the regional vegetation type in northern Chukotka." Delete the remaining part of the section, it is not relevant.

11. Page 3459, lines 18-24: delete! It is useless to describe the changes in affinity scores of the other biomes than the one with highest affinity score, because you cannot use this in your interpretation. See my general comment above. Describe instead the changes in biome with highest affinity score, i.e. column 2 in Figure 3.

12. Page 3461, line 8: "indicate a 2 to 4 °C increase in MTWM" which is how many degrees warmer compared to present?

13. Page 3461, lines 15-28: delete all parts describing fluctuations in affinity scores of the non-dominant biomes, i.e. " The CLD affinity scores show. . .. (Fig. 3a)". Delete

CPD
9, C2088–C2099, 2013

"Although" after (Fig. 4d). Delete "relatively high TUND . . .. 123kyr BP".

14. Page 3462, lines 1-7: delete "Although the biome scores. . ... the Holocene," "Starting from . . .. transition."

15. Page 3462, line 19: delete "and with the results of the quantitative. . ..." and replace by "and the biome reconstruction (CLDE biome 128-115 kyr BP) (Fig. 4)".

16. Page 3463, 27-28: "As expected from both the quantitative biome (this study, Fig. 8a and b) . . .." What do you mean with "as expected" in this context?; Delete "quantitative"; Do you really want to refer to Figure 8? Shouldn't it be Figure 5? I do not understand. . ..

17. Page 3464, line 18: delete "the relatively high . . .. and explains"; it should be instead "is in line with the reconstructed re-establishment of the tundra biome in the region . . .. . .".

18. Page 3464, lines 23-28: delete "The onset of the interglacial occurs . . .... when STEP biome scores. . ...Holocene levels. At this time. . ..although". Replace by "TUND is the reconstructed biome until 1086 kyr BP (Fig. 6b)." Delete "Extremely low STEP scores . . .. (Fig. 6d). Replace by "The TAIG biome is found from 1074 to 1062 kyr BP (Fig 6b), or during the middle. . .. . . (Fig. 6d). Delete "A turn to colder . . .. by the end of MIS 31. However". Continue with: " TUND replaces TAIG . . .. Vegetation type at 1062 kyr BP, marking. . .... (Fig. 6d)".

19. Page 3465, line 14-15: " indicates that the long-distance transport. . .. . .. ACCELERATED during the transitional intervals along with a decreased pollen production. . ...". I do not understand "accelerated" in this context. And I do not understand either why long-distance transport would increase just at these three levels. . .., what do they have in common?

20. Page 3465, line 28: Delete "and landscape openness (Fig. 7b)".

21. Page 3466, line 22: Delete "and the great . . .... (Fig. 7b) in the regional vegetation.".

CPD
9, C2088–C2099, 2013

22. Page 3466, line 28: Replace “quantitative reconstructions of past vegetation” by “reconstruction of past biomes (Fig. 8)”.

23. Page 3467, line 25: Delete “quantitative”.

24. Page 3468, line 10: Delete “quantitative” and replace “validating” by “evaluating”.

25. Chapter 3.7: “Qualitative or quantitative interpretations. . .. . . particularly for reconstructing past climate and vegetation”. This should be said from the beginning of the interpretation of results!! Perhaps even in the introduction!

26. Chapter 3.7, lines 23 and following: it is indeed interesting to compare the biome reconstruction with the BMA climate reconstruction, but. . .... one should not forget that they both are based on pollen data, and in this case on the same pollen record!.... They are not independent and, therefore, cannot evaluate each-other!.... This should be pointed out in the text. The comparison with the benthic delta 18 records is more convincing!

27. Page 3469, lines 4-5: “provides only semi-quantitative and indirect climate information.” Delete “semi-quantitative”, it is also qualitative in this case! And Delete “However it is “closer” to the actual vegetation and don’t suffer. . ... no-analog problem . . .. etc. . ..”. I know what you mean by “biomization being closer to vegetation” but it is not clear! Explain better (see my general comment above on why biomization is an intelligent way to consider the individual behaviour of individual taxa (one of Colin’s many brilliant ideas!)). This should be explained earlier in the text, in the introduction or methods.

28. Page 3470, line 16: what do you mean by “and a reduction in vegetation cover for . . .”? How can a model simulate the fraction of vegetation cover? Did you mean “forest cover”?

29. Page 3471, lines 1-5: change as follows: “Correlations between the reconstructed. . .... were observed by Brigham-Grette et al. (2013) and are also found with the pollen-based biome reconstruction presented here (Fig. 8). The similarities

CPD

9, C2088–C2099, 2013

between reconstructions underscore. . ..

30. Conclusions, general comment: explain better what is unique in your record, what are the new insights, i.e. results and interpretations that were not known earlier and/or not expected.

31. Page 3471, line 18-27 + page 3472, lines 1-19: much too long and partly not relevant. I do not agree that biome reconstructions are understandable for a larger community of geoscientists than pollen percentages as the method is not particularly easy to understand and, therefore the results easily misunderstood. The only relevant/interesting reason to use pollen-based biome reconstructions rather than pollen % is that the methods classifies past pollen assemblages into the world's modern biomes for comparison with simulated biomes, for example.In other words, it uses a typological assignment of pre-determined biomes to pollen assemblages that is similar to the biomes simulated by dynamical vegetation models for instance.

Change the text as follows: " The pollen-based biome reconstruction from Lake El'. . ..provides a record of climate-driven vegetation change that can be more easily compared with qualitative or quantitative proxy-based climate reconstructions or model results (both from vegetation and climate models). [Here you can explain why it is so, in case you have not done it earlier, either in the introduction – where I would personally already tell the advantages of pollen-based biome reconstructions – or in the Method part]. Data-model comparison is beneficial because it i) allows . . .. . ., ii) facilitates the selection of . . ..., and iii) helps improve our understanding of the earth-system development at the global and regional scales.

32. Page 3472, line 20: the current study provides time series of the pollen-based reconstructed biomes between 3580 and 2200 kyr BP [use the same units throughout the paper!] and through four glacial-interstadial cycles within the last 1090 ky BP.

33. Page 3472, line 29 " The middle Pleistocene, in particular,. . .." Until page 3473, line 2: ". . ... MIS 11.3 interglacials". I do not understand what "(i.e. 22 and 25 kyr)"

CPD
9, C2088–C2099, 2013

refers to, and therefore do not understand the whole sentence. . ..

34. Page 3473, lines 3-5: “Biome reconstruction . . ... analog method”. I do not agree that biome reconstruction does not rely on modern reference datasets. It does not rely on transfer functions established from modern pollen-vegetation datasets, this is correct. But it does rely on the modern climatically defined biomes, i.e. on modern plant functional types. So, as all reconstructions of the past from palaeoecological data, it is dependent on modern references. Also, the POLLEN-BASED reconstructed biomes are not an INDEPENDENT confirmation of the POLLEN-BASED reconstructed climate using BMA. They both depend on the same proxy data, i.e. the POLLEN RECORD!

Technical corrections

1. Check the use of “which”. For instance on Page 3451, line 22: “and the alternation of cold and warm marine isotope stages, which reflect changes. . ...” should be: “isotope stages that reflect changes. . ..”. It would be “which” if it was the fact that “there is good correspondence etc. . ..” that was meant to be reflecting the changes in the global ice volume and sea level, WHICH is not the case!. Similarly: “The biome reconstruction demonstrates changes in the regional vegetation which suggest”, should be “that suggest . . .. . .”. This grammatical mistake is recurrent through the entire text.

2. Page 3453, lines 7-14: Change as follows: “ This paper presents the pollen-based biome reconstruction using the published late Pliocene and Quaternary pollen record from Lake El’. . .. and the “biomization” mewthod first introduced by Prentice et al. (1996). The results are discussed in terms of the regional . . ... and the bioclimatic variables controlling vegetation dynamics. The biome reconstruction is also compared with the published quantitative climate reconstruction using the BMA approach and other palaeoclimate reconstructions based on other proxies than pollen data.

3. Page 3454, lines 13-14: “Snow cover is up to 0.3-0.5 m, a depth sufficient to support growth. . ..”. I do not understand. . ... Do you mean: “Snow cover . . . 0.5 m, which still allows growth etc. . .”?

CPD

9, C2088–C2099, 2013

4. Page 3454, line 19:" . . . graminoid (Poaceae and Cyperaceae). . . ". Isn't it simpler to write ". . .... grasses and sedges. . ...".

5. Page 3455, line 15 and lines 18-22: Change as follows: "Pollen-based biome reconstructions using the method of Prentice et al. (1996) provides new insights into past vegetation dynamics and facilitates data-model comparison. The latter is possible thanks to the use of the same concepts of biome definition in the pollen-based biome reconstructions and the climate-based biome simulations (Prentice et al., 1996). ".

6. Page 3456, lines 9, 21, 26: replace "which" by "that", see comment 1 above.

7. Page 3456, line 14: ". . .employs the taxa-to-biome attribution etc. . .".

8. Page 3457, line 3: ". . . using the standard equation. . .."

9. Page 3457, line 11: ". . .(Overpeck et al., 1985)", delete "cf.".

10. Page 3457, line 13: " . . ...(1996) in order to minimize. . ..".

11. Page 3458, line 1: " In order to distinguish the cold from the warm steppe biomes, the presence. . ..".

12. Page 3458, lines 5-6: Change to: ". . .tree and shrubs pollen taxa that can be identified to a lower taxonomic level than herbs (full details etc. . ..

13. Page 3458, lines 19-20: Change to: "43 pollen assemblages from the surface sediments of Lake El' . . ... indicate that only . . .. . .

14. Page 3458, line 22: replace "samples" by "assemblages".

15. Page 3458, line 25: replace "spectra" by "assemblages".

16. Page 3459, line 18: replace "spectra" by "assemblages".

17. Page 3459, line 25: Delete "However"; and line 27: delete "biome results are" and write "The Holocene is characterized by . . ...".

CPD

9, C2088–C2099, 2013

18. Page 3460, line 8: Change to “. . ..life forms that may also represent different biomes.”.

19. Page 3461, line 3: “. . .data suggest that the study. . ..”.

20. Page 3461, lines 26-28: Change to: “All the evidence suggest that this interval. . ..optimum with the highest temperatures and precipitations, and the most extensive. . ..within the region.”.

21. Page 3462, lines11-12: “. . ... Alnus that, when combined with . . ..., constitute. . ...”.

22. Page 3462, lines 13-15: Delete “ The MIS 5.5. . . .. reveal” Start instead with “ The percentages of herbaceous taxa are substantially lower than those recorded in the Holocene assemblages.” And continue with “These characteristics suggest that the MIS 5.5 climate . . .. regional vegetation (Lozhkin and Anderson, 2013).” . Delete “The differences in the paleovegetation” (lines 14-15) and “during MIS 5.5. (line 17).

23. Page 3463, line 5: replace “Yar, which is located” by “Yar that is located. . ..”.

24. Page 3463, line 13: replace “during” by “over”.

25. Page 3464, line 2: replace “spectra” by “pollen assemblages”.

26. Page 3464, line 3: “unambiguously defined”, do you mean “identified”?

27. Page 3464, line 9: replace “spectra” by “assemblages”.

28. Page 3466, line 24: Delete “Palynological analysis of” and start with “The lower 216 m . . .. provide the most complete. . ...”.

29. Page 3467, lines 5-6: “pollen record that influences the reconstruction . . ..mixed forest as Quercus belongs to the temperate etc. . ...”.

30. Page 3469, line 18: “climate-model simulations”.

31. Page 3470, line 12: replace “for 416” by “at 416”.

CPD

9, C2088–C2099, 2013

32. Page 3470, line 13: replace “LPJ” by “CLIMBER2-LPJ”.

33. Page 3470, line 16: replace “for 400” by “at 400”.

34. Figure captions: Do not forget to explain the abbreviations STEP, TUND, TAIG, CLDE, COCO, COMIX, although the three first ones are obvious I agree.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/9/C2088/2013/cpd-9-C2088-2013-supplement.pdf

---

Interactive comment on Clim. Past Discuss., 9, 3449, 2013.