

Response to M.-J. Gaillard (Referee 1)

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.

We are grateful to reviewer for high evaluation of our work and valuable suggestions helping to improve our paper. Basically, we accepted all technical comments and language corrections suggested by reviewer. Here, I would like to mention that the language of the manuscript was additionally proof-read by the expert nominate by the journal on demand of the Editor prior the publication in CPD. For this check additional money have been asked and paid to the Publisher. I am not a native speaker and can not judge the quality of the work. But if the multiple mistakes mentioned by reviewer are real, this means that the language editing was not done properly.

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 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.

We acknowledge reviewer for raising this issue and address it in the revised manuscript. “Biomization” method was developed (Prentice et al., 1996) in order to produce global vegetation maps for the last glacial maximum and middle Holocene time-slices – a major task of the BIOME 6000 Project (e.g. Prentice and Webb, 1998), and to use these results in the data-model comparison experiments, for example, generated by PIMIP (e.g. Joussaume and Taylor, 1995). Since then, the method became rather popular and has been applied to the fossil pollen records from different regions and results are published elsewhere. The main use of the biomization approach was limited to the aims of BIOME 6000 Project, i.e. on getting information on the dominant natural vegetation types. However, this does not mean that the method has no further potential. Mentioned by reviewer work of J. Guiot and co-authors is only one example of using quantitative biome/PFT score values in paleoenvironmental studies. Other studies (i.e. Peyron et al., 1998; Guiot et al., 1999; Williams, 2002; Herzschuh et al., 2004; Gotanda et al., 2008; Rudaya et al., 2009; Müller et al., 2010 etc.) demonstrate broader range of using quantitative results of biome/PFT score calculations. Examining all biome affinity scores (and not only deriving the dominant biome name) along the core or pollen record for more objective interpretation of reconstruction results has been presented in different papers published elsewhere (i.e. Tarasov et al., 1997; Herzschuh et al., 2004; Marchant et al., 2002, 2006; Rudaya et al., 2009 etc.). Some of these articles were reviewed and recommended for publication by C. Prentice, who should know limitations of the method.

2) The method of biomization is indeed “numerical” (i.e. quantitative) approach. However, reviewer is right telling that the method allows mainly qualitative interpretations. I say “mainly” because change from one dominant biome to another can be quantified in terms of change in the respective bioclimatic limits controlling growth of representative taxa/PFTs (see Prentice et al., 1996). However, as score calculation for all potentially present biomes is one of the key steps in the reconstruction procedure (Step 6 in Prentice et al., 1996), it is logic that the results of calculation have right to be presented in the result section, as any other result. We can not agree with Reviewer’s request to delete all subfigures “a” in Figures 3-6 presenting biome affinity scores derived from the El’gygytyn pollen dataset placed on an age scale.

3) The method of biomization (and associated problems of interpretation) is not much different from the method of pollen analysis. Pollen percentages of individual taxa are quantitative values, though relationship between pollen percentage of Taxon X and its participation in the vegetation cover is never one to one. Moreover, this relationship does not remain constant from one sample to another in the pollen record. Nevertheless, nobody argues against presenting pollen percentages curves and their interpretation in terms of qualitative changes in vegetation cover. The use of arboreal pollen sum as a qualitative measure of wood cover changes is a common approach in the literature. Why should the pollen-based results of biomization be treated differently from results of pollen analysis? Nowhere in the text we wrote that biome score values represent percentage of given vegetation type in the regional vegetation (neither that “affinity scores are equivalent to probabilities or proportion of the area covered by individual biome types in the region). To avoid the misunderstanding we deliberately deleted such words as “quantitative” and “semi-quantitative” or replaced them with “qualitative” in the revised manuscript. Several earlier publications (including those contributing to the BIOME6000 Project) already demonstrated importance of “looking beyond the dominant biome”. It is obvious that

if the distance between the dominant biome score (for example, temperate deciduous forest) and the next one (for example, steppe) is negligible, the interpretation of the past vegetation in the region will be different from the situation when the distance between the two scores will be large (i.e. forest steppe/woodland in one case and forested landscape in another case). As for every other method, obstacles in the interpretations are possible, but this does not influence basic concept.

4) The fact that biome score calculations can be presented as part of the results does not automatically mean that these results are essential for further qualitative or semi-quantitative interpretations. The figure from Tarasov et al. (2013) presented below provides good arguments in favor of using biome scores (Fig. 7B) for discussion of regional changes in vegetation.

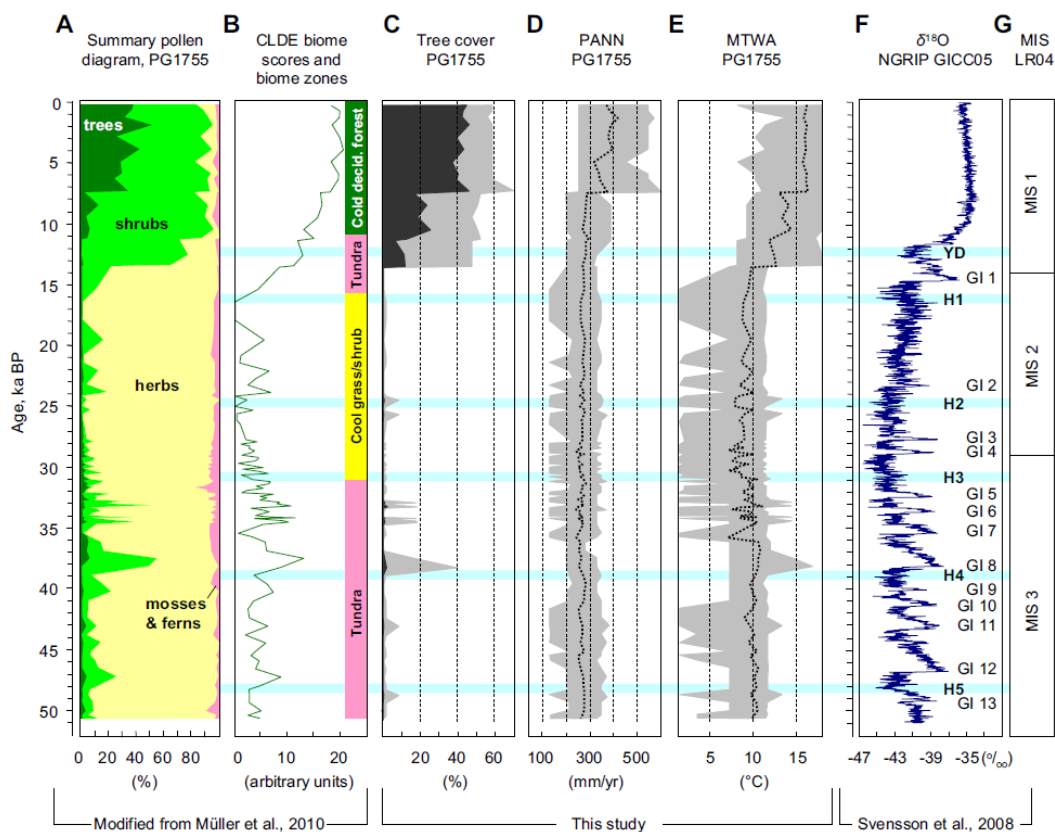


Fig. 7. (A) Summary pollen diagram (percentages are based on total sum of pollen and spores identified at each level) and (B) cold deciduous biome scores and biome zones derived from the PG1755 core record (based on Müller et al., 2010) compared with the pollen-based reconstructions of (C) tree cover, (D) annual precipitation, and (E) mean temperature of the warmest month (this study, dashed lines (D–E) indicate most probable values and grey bands (C–E) show range of the best modern analogues for each reconstructed variable), along with (F) the NorthGRIP $\delta^{18}\text{O}$ profile and the Greenland Interstadials (GI) 1 to 13 (after Svensson et al., 2008) and (G) the marine isotope stages (MIS) stratigraphy (after Lisiecki and Raymo, 2005). Blue bars indicate approximate positions of the Younger Dryas (YD) and Heinrich events H1 to H5 (after Tierney et al., 2008). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Correspondence between the woody taxa pollen % and biome score plates (Fig. 7A and Fig. 7B) is obvious, as well as generally good correlation with non-pollen climatic archives (Fig. 7G).

5) However, generally good correlation between trees/shrub pollen % and the scores of the cold deciduous forest (the dominant among the forest biomes) presented in Fig. 7A and B does not provide an unambiguous answer to the reviewer’s comment “Pollen % of major trees and herbs that are discussed in the paper along with the biome reconstruction would be more useful to show in these figures (than biome scores). In order to check what is better reflect woody vegetation cover (i.e. the landscape openness) in the study region, we compared % values of trees and shrubs and biome-score-based landscape openness calculated from the modern surface pollen spectra in northern Asia with the satellite-based values of woody cover around the modern pollen-sampling sites (calculations are from Tarasov et al., 2007 and Williams et al., 2011). The arboreal pollen %, biome-derived landscape openness and satellite data from two macro-regions of Eurasia, namely “central Eurasia” (ca. 40-70°N, 30-120°E) and “Russian Far East” (47-63°N, 132-152°E) have been compared. The results of these tests clearly demonstrate stronger correlation between satellite-derived (actual) woody cover and biome-score-derived openness (calculated as in current manuscript) in both regions ($R_1=0.76$ and $R_2=0.88$) and substantially weaker

correlation between satellite-derived woody cover and woody pollen % ($R_1=0.55$ and $R_2=0.56$). The reason for that are partly explained by Prentice et al. (1996) and mentioned by the reviewer, as biome score calculation takes into account the ecology of pollen-producing plants and does not entirely rely upon a statistical similarity between pollen assemblages composed of numerous taxa representing ecologically different plant functional types (Bezrukova et al., 2010). The additionally obtained results obtained with the modern datasets from the study region justify methodological approach applied in the current study. Once more we would like to stress that biome scores are used as qualitative indicator of climatic-driven trends in the vegetation development (somewhat similar to sedimentary or oxygen isotope data) rather than quantitative estimates of woody cover or biome-covered area in the study region.

Cited references:

- Bezrukova, E.V., Tarasov, P.E., Solovieva, N., Krivonogov, S.K., Riedel, F., 2010. Last glacial–interglacial vegetation and environmental dynamics in southern Siberia: Chronology, forcing and feedbacks. *Palaeogeography, Palaeoclimatology, Palaeoecology* 296, 185–198.
- Gotanda, K., Nakagawa, T., Tarasov, P.E., Yasuda, Y., 2008. Disturbed vegetation reconstruction using the biomization method from Japanese pollen data: Modern and Late Quaternary samples. *Quaternary International* 184, 56–74.
- Guiot, J., Torre, F., Cheddadi, R., Peyron, O., Tarasov, P., Jolly, D., Kaplan, J.O., 1999. The climate of the Mediterranean Basin and of Eurasia of the Last Glacial Maximum as reconstructed by inverse vegetation modelling and pollen data. *Ecologia Mediterranea* 25(2), 193-204.
- Herzschuh, U., Tarasov, P., Wünnemann, B., Hartmann, K., 2004. Holocene vegetation and climate of the Alashan Plateau, NW China, reconstructed from pollen data. *Palaeogeography, Palaeoclimatology, Palaeoecology* 211 (1-2), 1-17.
- Joussaume, S., Taylor, K.E., 1995. Status of the Paleoclimate Modeling Intercomparison Project (PMIP). Proceedings of the first international AMIP scientific conference. WCRP Report, 425-430.
- Marchant, R., Boom, A., Hooghiemstra, H., 2002. Pollen-based biome reconstructions for the past 450 000 yr from the Funza-2 core, Colombia : comparisons with model-based vegetation reconstructions. *Palaeogeography, Palaeoclimatology, Palaeoecology* 77, 29-45.
- Marchant, R., Berrio, J.C., Behling, H., Boom, A., Hooghiemstra, H., 2006. Colombian dry moist forest transitions in the Llanos Orientales—A comparison of model and pollen-based biome reconstructions. *Palaeogeography, Palaeoclimatology, Palaeoecology* 234 (2006) 28–44.
- Müller, S., Tarasov, P.E., Andreev, A.A., Tütken, T., Gartz, S., Diekmann, B. (2010) Late Quaternary vegetation and environments in the Verkhoyansk Mountains region (NE Asia) reconstructed from a 50-kyr fossil pollen record from Lake Billyakh. *Quaternary Science Reviews* 29, 2071-2086.
- Peyron, O., Guiot, J., Cheddadi, R., Tarasov, P. E., Reille, M., Beaulieu, J. L., de Bottema, S., Andrieu, V., 1998. Climatic reconstruction in Europe for 18,000 yr B.P. from pollen data. *Quaternary Research* 49, 183–196.
- Rudaya, N., Tarasov, P., Dorofeyuk, N., Solovieva, N., Kalugin, I., Andreev, A., Daryin, A., Diekmann, B., Riedel, F., Tserendash, N., Wagner, M., 2009. Holocene environments and climate in the Mongolian Altai reconstructed from the Hoton-Nur pollen and diatom records: a step towards better understanding climate dynamics in Central Asia. *Quaternary Science Reviews* 28, 540-554.
- Tarasov, P.E., Jolly, D., Kaplan, J.O., 1997. A continuous Late Glacial and Holocene record of vegetation changes in Kazakhstan. *Palaeogeography, Palaeoclimatology, Palaeoecology* 136, 281-292.
- Williams, J.W., 2002. Variations in tree cover in North America since the last glacial maximum. *Global and Planetary Change* 35, 1–23.
- Williams, J.W., Tarasov, P., Brewer, S., Notaro, M. (2011) Late Quaternary variations in tree cover at the northern forest-tundra ecotone. *Journal of Geophysical Research* 116, G01017, doi:10.1029/2010JG001458.

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

We changed the title as suggested by reviewer. The new title is “A pollen-based biome reconstruction over the last 3.562 million years in the Far East Russian Arctic – new insights into climate-vegetation relationships at the regional scale”

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

All biome reconstruction results summarized in the abstract are new and not published elsewhere, except in a very short form (and only for the late Pliocene-early Pleistocene part of the record) in the Science article (Brigham et al., 2013). We modified the sentence criticized by reviewer and deleted “which suggest a step-like transition”. The edited sentence is now “The biome reconstruction demonstrates changes in the regional vegetation from generally warmer/wetter environments of the earlier (i.e., Pliocene) interval towards colder/drier environments of the Pleistocene.”

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.”

The text was modified and edited accordingly.

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?)

The introduction section was updated and edited, as suggested by reviewer.

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.

The requested information has been added, i.e. “The lake surface area is about 110 km² and the hydrological catchment area is about 293 km² (Nolan and Brigham-Grette, 2007).” Cited reference was added to the Reference list.

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

The upper 1 cm layer was analyzed that may cover up to one to three hundred years, according to the age models.

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.

Corrected as suggested.

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.

The paragraph has been simplified. Details are published elsewhere and there is no need to repeat them in this paper.

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.

The paragraph has been edited. "Semi-quantitative" is replaced by "qualitative". See also our response to general comment.

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.

Done

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.

Not relevant. See our response to general comment.

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

Clarified

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

Not relevant. See our response to general comment.

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

Not relevant. See our response to general comment.

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)".

Quantitative is changed to qualitative. Otherwise see our response to general comment

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....

The questioned sentence has been deleted and the question is resolved. Quantitative is changed to qualitative.

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".

The sentence has been revised.

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)".

See our response to general comment

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?

The sentence has been clarified. "A discontinuous appearance of COCO during the initial and final stages of the interglacial likely indicates that long-distance transport of the temperate pollen taxa from warmer regions." The part starting with "accelerated" has been deleted as rather subjective.

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

Done

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

Done

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

Done

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

Done

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

Done

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!

As suggested, we replaced the following paragraph “Qualitative or quantitative interpretations of past environments can be challenging, especially during times when modern analogs are weak or nonexistent. However, the expansion of modern datasets and development of more sophisticated models and statistical treatment of the data over the past two decades facilitate the comparison of results from multiple approaches, particularly for reconstructing past climate and vegetation.” To the Introduction chapter (page 3453)

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!

Comparison of qualitative (biomes) and quantitative (temperature and precipitation) results does not imply that one evaluate another. However, it is always beneficial to see whether results obtained using different approaches (for example, indicator species approach with statistical approach) are comparable or not. We do not see any problem here. Moreover, two approaches are principally different even pollen record is the same. The comparison with the benthic delta 18O record is also provided. Note: Similarly, two medical doctors make a diagnosis based on different approaches applied to the same patient (even to the same blood sample). In this case comparison is also welcome for a robust conclusion.

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.

"Semi-quantitative" has been deleted and the sentence has been edited for clarity using suggestion of reviewer.

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"?

We corrected this error. This should be "and a reduction of woody vegetation 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 between reconstructions underscore...."

Done

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.

Done

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 method 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.

We respect reviewer opinion and hope that our work will contribute to better understanding and further use of the biomization approach

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.

The text has been edited as suggested.

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.

The text has been edited as suggested.

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

Corrected

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!

This paragraph has been edited taking into account of reviewer suggestion.

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.

Corrected.

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” method 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.

Done

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...”?

The sentence has been edited. “Snow cover is up to 0.3–0.5 m and is greater than the minimum snow depth of 15 cm required to support growth of perennial cold shrubs (Kaplan et al., 2003).”

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

Done

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). "

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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....

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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.”.

Done

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

Done

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).

Done

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

Done

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

Done

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

Done

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

Edited

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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

Done

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.

Done as suggested