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Climate of the Past Discussions

Interactive comment on "Pollen-based quantitative land-cover reconstruction for northern Asia during the last 40 ka" *by* Xianyong Cao et al.

Anonymous Referee #1

Received and published: 26 November 2018

This paper focuses on past land-cover changes based on pollen data. This study considers the REVEALS model to quantify plant abundances (27 taxa) and related PFTs at the spatial scale of northern Asia and a temporal scale covering the last 40 ka. It is a great paper providing substantial information for the scientific community. One of the major interests of this work is the application of the REVEALS model at such spatial scale as it has been done over the last years for Europe. This work is therefore a good contribution for environmental and climate sciences by providing additional quantitative land-cover reconstructions at the continental scale of the Northern Hemisphere, and this is particularly critical for climate modelling. I would highly recommend this manuscript for publication in Climate of The Past. I do have comments and suggestions hereafter.

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Discussion paper

Major comments

1- I strongly recommend to revise the presentation of the results (core text) and figures 2 and 3. The authors use 42 groups for the REVEALS reconstructions and this makes difficult to observe major trends in vegetation changes, at least as it is presented here. To improve this, I have the following suggestions. First, I would only situate the pollen archives in figure 1; this figure has already too much information. Second, I suggest to group pollen archives into meaningful data such as vegetation zones, however to be objective I recommend to use the results from the cluster analysis, i.e 5 cluster groups. The 42 groups can be shown in an appendix when in the core text only the PFT results for the 5 cluster groups would presented. This would mean only one figure 2 rather than the duplication of figure 2. This would make easy the reading of the result sections and more useful/meaningful the figures. I further recommend to show the results for the turnover (entire time period and 5 cluster groups); this can be a new figure 3. This means that the result section should be partially revised, and the results of the cluster analysis should be shown in a specific graph; there are no graphs about the results of the cluster analysis in the present version of the manuscript.

2- Through the manuscript, the authors wrote that vegetation changes in northern Asia within the Holocene are “minor with slight changes in PFTs” (e.g. lines 212-213). This conclusion is based on the fact that the turnover is high during the early-Holocene and the numerical analysis based on constrained hierarchical clustering and the broken-stick model provide a timing of the primary change mostly during the early-Holocene. However, changes in PFT abundances can be high (e.g. G7 shows around 20 % fewer abundance of PFT VII between 2 ka and 1 ka). I would suggest to clearly define what the turnover and the timing of the primary change really mean. I am wondering if the identification of a primary change necessary implies that no other changes of similar importance can occurred more recently.

3- The conclusion is too short and do not show the potential of the present study.

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Minor comments

1- Why the turnover has been calculated from PFTs (see lines 203-206) rather than pollen types? This might explain differences between the turnover results in Europe and this study. Lower turnover in the present study than the ones in Europe might be related to the use of less variables here (PFTs) than in Europe (pollen types). The discussion lines 411-417 need to be revised by considering this issue.

2- The selection of RPP values is critical for this study. The relevance in the present study of using RPPs that have been obtained in Europe or other environmental and ecological conditions in Asia needs to be discussed in more details. Furthermore, more than the 20 RPP studies that the authors refer to have been published, and if they are taken into account they would increase a lot the uncertainties related to the choice of the specific RPPs that have been used by the authors, e.g. Chenopodiaceae, Artemisia and Compositae RPPs in Li et al. (Frontiers in Plant Science 2018). Different species within the Chenopodiaceae family might result in different RPPs, although we do not know how much this play a role in RPP calculation. All of these issues need to be taken into account in the discussion section, specifically for lines 428-452.

3- The choice of PFT VII (steppe and forb tundra) might be misleading. Artemisia pollen type is included in PFT VI (arid-tolerant shrub and herb), however Artemisia is an important component of steppe vegetation. Furthermore, tundra vegetation is located north of the study region (vegetation zone A) when steppe are located more south (vegetation zone D), this considers the vegetation zones that the authors provide. It would probably be more relevant to relate steppe to Artemisia and therefore PFT VI. This would not affect the results. 4- Lines 148-151. Why the authors have selected the specific value of 100 m for all bogs?

5- Lines 193-195. This linear interpolation when it corresponds to a large time gap of missing time windows might be a source of uncertainties, and it would be good to further discussed this.

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6- Lines 372-376. I suggest to be more specific about how the DNA information supports the results.

7- Lines 377-380. This sentence is not clear.

8- The authors should be consistent and use the term RPP through the manuscript; the term PPE can be found in several paragraphs.

9- I would suggest to add a global map in a corner of figure 1 to show the location of the study area. It is too much information and the reader can be “spatially lost”.

10- There is here no discussion about land-use changes for the last millennia. I would be interested in how land-use can be discussed based on these PFTs; I expect that the land-use should have some influences on forest covers at some points (e.g. late-Holocene primary vegetation changes).

11- Can the authors give the REVEALS standard errors in an appendix to get an idea about how reliable the reconstructions are?

12- I suggest to move Table 3 to Appendix.

13- Lines 464-466. I suggest to give more information about what “riverine” really means here (erosions, water-runoff, temporal lake etc...) and how these processes might affect the results.

14- I would add to the discussion a short sentence about the assumed constant RPP values over the last 40ka.

15- Line 473. I would not use the term “observed” but rather “past”. This to avoid a potential confusion with modern vegetation that can really be “observed”.

16- Line 189. I think the “The end of moisture increase” is confusing or there is a mistake here.

17- The authors might add some climate information via a new summary figure. This

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could be informative and useful to follow the discussion section.

18- Lines 504-505. I disagree with this conclusion. Vegetation–climate relationship can be ”linear” and no strong effect observed at short time scales (e.g. few decades). It might just be a matter of time scales, i.e. long-term responses of vegetation.

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