Response to the interactive comment on "Pollen-based quantitative land-cover reconstruction for northern Asia covering the last 40 ka"

Anonymous Referee #1

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.

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.

Our response: **We agree with the comment about Figure 1. In the new version, we have separated it into two figures: one presents the vegetation and permafrost background of the study region, together with the locations of the site-groups as the new Figure 1; and the second presents the locations of the pollen records (with IDs of pollen sites) and has been moved into the appendices as Appendix 1. The old "Appendix 1" (information for all pollen records) is now Appendix 2.**

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.

Our response: **We revised the presentation of the results. We now present the changes of the site-groups that belong to one cluster as a separate figure. However, the results could not be further summarized. The REVEALS approach is suitable to reconstruct the *regional* land-cover change assuming that these regions have similar temporal change patterns. Previous reconstructions in Europe were at a 1° × 1° spatial scale (c. 100 km × 100 km; Trondman et al., 2016). In Northern Asia, there are not enough available pollen data, thus we had to consider the available data at a larger spatial scale. We feel that summarizing the results into only 5 groups would be an over-simplification.**

The PFT dataset for the 34 site-groups covering the period between 12 and 1 ka is a three-way dataset. We calculated the distance set for the 34 site-groups using the tsclust function and then performed a simple hclust analysis. The hclust analysis cannot produce cluster means for each cluster; hence we cannot present a general pattern for the five clusters. In the new version, we present the cluster diagram as Appendix 5.

Regarding the turnover calculation (DCCA), we cannot perform the DCCA for the entire time span because most site-groups do not cover the entire time span and DCCA cannot deal with missing values. Hence, we can only perform the cluster and turnover analyses for the

Holocene.

In the new version, we present the reconstructed results for each cluster one by one, replacing the old Figure 2 (which presented reconstructed results by site-group). The new Figure 2 includes 5 sub-figures, separated by modern vegetation: A) warm temperate forest margin zone, B) cool-temperate mixed forest, C) dark taiga forest, D) light taiga forest, and E) the tundra-taiga ecotone, which is consistent with the "Results" part.

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 brokenstick 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.

Our response: **We conclude "minor with slight changes in PFTs" during the period between 12 and 1 cal ka BP because the turnover is *low* throughout the period *and* because some of the primary change does not pass the broken-stick test during the constrained hierarchical clustering. Most of the insignificant or significant primary changes occur in the early Holocene, hence we conclude that the most important vegetation changes occur in the early Holocene. In the new version, we define what turnover and the timing of the primary change mean.**

Lines 202-208

"Constrained hierarchical clustering (using chclust function in rioja package version 0.9-15.1;

Juggins, 2018) was used to determine the timing of primary vegetation changes (i.e. the first split) in each site-group. A change was considered to be significant when the split passed the broken-stick test. The amount of PFT compositional change (turnover) through time during the period between 12 and 1 ka was estimated by detrended canonical correspondence analysis (DCCA) for each site-group (ter Braak, 1986) using CANOCO 4.5 (ter Braak and Šmilauer, 2002).”

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

Our response: **we have revised the conclusion in the new version.**

Lines 558-570

“Regional vegetation based on pollen data has been estimated using the REVEALS model for northern Asia during the last 40 ka. Relatively closed land cover was replaced by open landscapes in northern Asia during the transition from MIS 3 to the last glacial maximum. Abundances of woody components increase again from the last deglaciation or early Holocene. Pollen-based REVEALS estimates of plant abundances should be a more reliable reflection of the vegetation as pollen may overestimate turnover and indicates that the vegetation was quite stable during the Holocene as only slight changes in the abundances of PFTs were recorded rather than mass expansion of new PFTs. From comparisons of our results with other data we infer that climate change is likely the primary driving factor for vegetation changes on a glacial-interglacial scale. However, the extension of evergreen conifer trees since ca. 8–7 ka throughout Siberia could reflect vegetation-climate disequilibrium at a long-term scale caused by the interaction of climate, vegetation, fire, and permafrost.”

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.

***Our response*: We used the PFT dataset to calculate turnover because both the REVEALS model and our manuscript focus on the changes in PFTs rather than taxa. We have revised the phrase "in the abundance of major taxa rather than by invasions of new taxa" and replaced it by "in the abundance of PFTs rather than by invasions of new PFTs", In addition, we have added a discussion about why there is relatively low turnover in North Asia compared to Europe.**

Lines 24-28

"Reconstructed regional plant-functional type (PFT) components for each site-group are generally consistent with modern vegetation in that vegetation changes within the regions are characterized by minor changes in the abundance of PFTs rather than by invasions of new PFTs, particularly during the Holocene."

Lines 428-432

"The fewer parameters used in the turnover calculations for northern Asia (PFTs) compared to Europe (pollen taxa) is a potential reason for the lower turnover obtained in this study. In addition, the PPE-based transformation from pollen percentages to plant abundances may reduce the strength of vegetation changes (Wang and Herzschuh, 2011)."

2, The selection of PPE values is critical for this study. The relevance in the present study of using PPEs 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 PPE 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 PPEs that have been used by the authors, e.g. Chenopodiaceae, *Artemisia* and Compositae PPEs in Li et al. (Frontiers in Plant Science 2018). Different species within the Chenopodiaceae family might result in different PPEs, although we

do not know how much this play a role in PPE calculation. All of these issues need to be taken into account in the discussion section, specifically for lines 428-452.

***Our response*: We agree with the reviewer. The quality of the PPEs is most relevant for the reliability of reconstruction. However, hitherto only 20 PPE records from Eurasia have been published. We applied a consistent approach to calculate the PPEs from the published values. i.e. we used all PPE records of different species within one family or genus in the calculation of the mean PPE for the family or genus. Furthermore, we argue "the regional differences in the PPE for each taxon are small compared to the large between-taxa differences ". We have added some further discussion about the reliability of PPEs in the new version.**

Lines 154-155

"We included these PPE values for various species in the mean PPE calculation for their family or genus."

Lines 451-456

"The available PPEs were estimated from various environmental and ecological settings, which might cause regional differences in each PPE. Also, PPEs of different species within one family or genus were included in our mean-PPE calculation for the family or genus, ignoring the inter-species differences. Both these aspects can cause uncertainty in the mean PPE to some extent."

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.

***Our response*: *Artemisia* pollen at high abundance is found in arid central Asia, while quite low abundances are found in northern Asia (Siberia). As mentioned by the reviewer, *Artemisia* is an important component in arid steppe community. However, in order to separate the tundra forb and the steppe forb, we had to separate the arid-tolerant forb and the wet-favouring forb. The name for PFT VII was misleading and so we have changed it to "grassland and tundra forb" in Table 2.**

4- Lines 148-151. Why the authors have selected the specific value of 100 m for all bogs?

***Our response*: We had performed test-runs that showed that the different bog radii (i.e. 5 m, 10 m, 20 m, 50 m, 100 m, 200 m and 500 m) did not significantly affect the REVEALS estimates, hence a standard radius of 100 m was set for all bogs. We have added the explanation into the new version and a figure as Appendix 3.**

Lines 148-151

"A test-run showed that using different bog radii (i.e. 5 m, 10 m, 20 m, 50 m, 100 m, 200 m and 500 m) did not significantly affect the REVEALS estimates (Appendix 3), hence a standard (moderate size) radius of 100 m was set 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.

***Our response*: We agree with the reviewer and have added some discussion.**

Lines 485-488

"In addition, the linear interpolation of pollen abundances for time windows with few pollen data

might be another source of uncertainty, particularly for the late Pleistocene and its broad time windows (Table 1).”

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

***Our response*: Agree and added.**

Lines 386-394

“During the late Pleistocene (40, 25, 21, 14 ka), steppe PFT abundance was high in central Yakutia and north-eastern Siberia (e.g. G25, G36, G37, G39, G40, G41), which may reflect the expansion of tundra-steppe, consistent with results from ancient sediment DNA which reveal abundant forb species during the period between 46 and 12.5 ka on the Taymyr Peninsula (Jørgensen et al., 2012). The tundra-steppe was replaced by light taiga in southern Siberia and by tundra in northern Siberia at the beginning of Holocene or the last deglaciation, which is consistent with ancient DNA results (forbs-dominated steppe-tundra; Willerslev et al., 2014).”

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

***Our response*: Agree and improved.**

Lines 395-397

“During the Holocene, reconstructed land cover for each site-group is generally consistent with their modern vegetation. The slight vegetation changes are represented by changes in PFT abundances rather than by changes in PFT presence/absence.”

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

***Our response*: Agree and modified.**

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".

***Our response*: Agree and added.**

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).

***Our response*: Yes, land cover could be modified by humans during the late Holocene. However, in our study area, human impact on vegetation is not very clear, because the regional land cover was reconstructed from a multi-record combination. Hence, we cannot discuss the land-use history.**

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

***Our response*: We will upload the reconstruction datasets including standard errors to Pangaea after this manuscript is accepted. In the new version, we select three site-groups to illustrate the reasonable standard errors of reconstruction as Appendix 8.**

Lines 439-441

"We consider the REVEALS-based regional vegetation-cover estimations in this study as generally reliable with reasonable standard errors (Appendix 8) thanks to the thorough selection of records with high quality pollen data and reliable chronologies."

12- I suggest to move Table 3 to Appendix.

***Our response*: Agree and done.**

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.

***Our response*: Agree. We have replaced "riverine" by "water-runoff".**

Lines 488-491

"Finally, pollen signals from certain sites and during certain periods may be of water-runoff origin rather than aerial origin violating the assumption of the REVEALS-model that pollen is transported by wind."

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

***Our response*: Agree and done.**

Lines 138-140

"The REVEALS model assumes the PPEs of pollen taxa are constant variables over the target period, and requires parameter inputs including sediment basin radius (m), fall speed of pollen grain (FS, m/s), and PPE with standard error (SE; Sugita, 2007)."

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".

***Our response*: Agree. We had replaced "observed" by "pollen-based reconstructed".**

Lines 493-496

"On a glacial-interglacial scale, pollen-based reconstructed land-cover changes in northern Asia are generally consistent with the global climate signal (e.g. sea-surface temperature: Pailler and Bard, 2002; ice-core: Andersen et al., 2004; solar insolation: Laskar et al., 2004; cave deposits: Cheng et al., 2016; Appendix 9)."

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

***Our response*: Agree. We have deleted “The end of”.**

17- The authors might add some climate information via a new summary figure. This could be informative and useful to follow the discussion section.

***Our response*: Agree. We have prepared a figure as Appendix 9.**

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.

Our response: Agree and modified.

Lines 568-570

“However, the extension of evergreen conifer trees since ca. 8–7 ka throughout Siberia could reflect vegetation-climate disequilibrium at a long-term scale caused by the interaction of climate, vegetation, fire, and permafrost.”