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Interactive comment on "Patterns in recent and Holocene pollen influxes across Europe; the Pollen Monitoring Programme Database as a tool for vegetation reconstruction" *by* Vojtěch Abraham et al.

Anonymous Referee #1

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The manuscript presents a new and valuable dataset that is made publicly available on a well-established database (NeotomaDB) that adheres to the World Data System and FAIR principles.

The dataset consists of pollen counts from traps located in various European regions (and associated metadata). It is potentially extremely useful to explore pollen-vegetation relationships, as has already been shown in a number of previous, regionally-focussed, studies. In comparison to these prior studies, the present manuscript explores this modern-pollen dataset at a scale that spans across much

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wider geographical (latitudinal and altitudinal) gradients.

The manuscript focuses on pollen abundances of 14 pollen taxa (12 trees and shrubs genera, 2 herb families) and explores the relationships between the sum of the pollen-accumulation rates (PAR) of the 14 taxa ("total PAR") and selected environmental parameters (mean annual temperature, annual precipitation, forest cover). It also explores the relationship between tree PAR and forest cover, and the relationship between pollen deposition as a function of increasing distance to the nearest range boundary of the parent plant species. A long-distance transport threshold (LDT) is obtained that may be used to infer range-size changes based on fossil PAR values. Further, a comparison between modern and fossil PAR is presented that shows PAR-inferred population-size changes for selected taxa through time at different sites located across the latitudinal gradient.

The manuscript is at times very confusing. For instance, as far as I was able to understand, the results indicate that total PAR is strongly related with forest biomass within a 10km radius around the traps (Table S4). The text instead reports that forest cover explains 72% of the variance of total PAR. Further, Figure 3 shows how total PAR is related to latitude (besides, this relationship seems to be strongly determined by one datapoint), but does not show how total PAR are related to forest cover. Instead, the Figure shows how tree PAR is related to forest cover.

In my opinion, one of the main weaknesses of the manuscript is that the Introduction does not convey which knowledge gaps and hypotheses are being addressed. The Introduction is strongly disconnected from the Abstract, the Results, the Discussion, and the Conclusions. It dwells on how and why the Pollen Monitoring Programme was established and lists prior, regional, studies. Some concepts that are mentioned both in the Abstract and the Discussion (CO2 fertilization, land use) are not found in the Introduction. Another concept (the importance of comparisons between modern and fossil pollen data, and of past and modern plant distributions and abundances) is only marginally mentioned in the Introduction, although it is important in the Discussion.

Moreover, links to Figures seem to be wrong in some places of the text, methods could be described better, tables and their captions sometimes are incomplete, and the description of the results is confusing and in some places contradictory. Further, the paragraph 3.4 “Taxa specific linkage. . .” seems wordy and confusing, and could be substantially shortened (besides, I was surprised when I noticed that the taxa specific linkage is placed as supplementary material for some of the taxa. Maybe it was mentioned earlier in the Mat & Methods section?). Some of the conclusions do not seem to be supported by the data.

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General comments:

-> Abstract:

- the collection of [. . .] is important. . .

- statement “This dataset shows that climate parameters [. . .] determine pollen productivity” is not supported by the data, which shows that forest cover explains a much larger share of the variance in “total PAR”.

- the statement “A signal of regional forest cover can be detected [. . .], while local tree cover seems more important” suggests that forest cover is substantially different from tree cover. I might have missed this difference when reading the text and suggest to better point this difference out.

- the statement “PAR values up to (smaller than?) 30 grains [. . .] in fossil records should therefore be interpreted as long distance transport” should be nuanced further. I suppose this refers to PAR values of the pollen taxa that were explored in this study. There are very likely some plant species whose pollen is less well dispersed or that simply produce less pollen (e.g. Larix, insect-pollinated plants). Moreover, it seems to me that the threshold value represents the PAR at 200 km from the distribution limit (as of Figure 4b). Does this mean that you (arbitrarily) consider any distance beyond 200

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km as a “long distance”? If so, please state this in a clearer way in the text.

- the statement “Comparisons to fossil data from the same areas show comparable values” is unclear. What is meant exactly with the term “the same area”? Figures 6-13 show that the geographical distance between similar modern and fossil PAR values is often quite high.

- L11: “may be hard to find” seems colloquial. Replace with “do not occur, were not found in this dataset”?

- the last sentence could be replaced with a DOI link (or a NeotomaExplorer link) to the dataset in Neotoma. . .the link could be added on L5 after “1981 to 2017”.

-> It is striking to see that you explored the relationship between the sum of the pollen-accumulation rates (PAR) of the 14 taxa (“total PAR”) and forest cover (text on P8 L24-30 and Table S4). On which grounds would one expect a relationship between the PAR of herbs (Poaceae and of Cyperaceae) and forest cover?

-> A regression model for tree PAR vs forest cover is presented. The regression is based on selected tree PAR values for 3% wide forest-cover bins (Figure 3b). The regression model suggests that an 80% forest cover within 10 km radius results in tree PAR values > 3200 (Conclusions, P23 L 15). I might miss an important point, but it seems to me that the deduction is not supported by the data presented in Figure 3b. The Figure shows that values greater than 3200 tree PAR can be found even for 0% forest cover. It seems to me, instead, that tree PAR are > 20,000 for forest cover >20% (though strikingly the two sites with highest forest cover show rather low tree PAR values).

-> the manuscript shows decreasing pollen deposition as a function of increasing distance to the nearest distribution limit of the parent plant species (Figure 4). Based on this evidence, a long-distance transport threshold (LDT) for a distance of 200 km beyond the distribution limit is calculated. The thresholds could be used to infer range-

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size changes based on fossil PAR values, the manuscript reports.

- While these are interesting results and a potentially useful approach, some critical discussion of this may be useful. The distribution limits were extracted from GIS shapefiles published by Caudullo et al. (2017), which are publicly available on the figshare website with associated DataCite link (Caudullo, Giovanni; Welk, Erik; San-Miguel-Ayanz, Jesús (2017): Chorological maps and data for the main European woody species. figshare. Collection. https://doi.org/10.6084/m9.figshare.c.2918528.v5). In the original manuscript where Caudullo et al. present the maps, they specifically mention that "Since the maps aim at representing the species general chorology at continental scale, providing a synthetic overview of distribution range, the mapped boundaries should not be considered as precise and sharp limits where the species is definitely present or absent, particularly at local level. Indeed, the first version of this dataset was created for the European Atlas of Forest Tree Species [16] to concisely outline the distribution ranges of described species, complementing information on the species biology and ecology. Errors and imprecision are partly inevitable, due to various causes, such as the quality of the original source, the geo-referencing procedure, the interpretation and the comparison of the sources in the same area and finally due to the limited precision of the manual digitalization process of the range borders (Fig. 1)."

- It is therefore highly questionable as to whether the distances to the distribution limits measured by Abraham et al. truly represent the actual distances to the species distribution limits. Thus, the precision and accuracy of the LDT values may be strongly overestimated and misleading. Instead of using one single distribution limit, a range of distribution limits may better represent the uncertainty of the mapped limits. Question is therefore: what LDT values would be obtained if the distribution limits of Caudullo et al. (2017) had an uncertainty? Say ca. +100km or even +200km?

- Moreover, it would be useful to show the complete data in the plots of figure 4, including the PAR values within the distribution range (thus extend the x-axes of the plots to include negative x-axis values). In theory at least, these PAR values should be greater

than PAR values around the distribution limit and beyond the limits.

-> The comparison between modern and fossil PARs is interesting (paragraphs 3.3 and 3.4).

- Paragraph 3.3. should be deeply revised and could be shortened. It could focus more on PAR-inferred presence/absence based on LDT limits that were presented previously (Figure 5), and on the identification of the closest modern counterparts of populations sizes and forest cover. Currently, some statements are descriptive and their relevance could be made clearer (for instance, on P13 L31 “Modern and fossil values agree for the sites in central Sweden at PARs between 1900-5600 grains. . .”). Some phrases could be removed (e.g. P13 L27 “As discussed in the main manuscript, ”), other ones are unclear (e.g. P13 L25 “ignoring traps from the Caucasus and Turkey”), and several statements should be supported with references to the literature (e.g. P16 L12 “Picea abies is planted in many European regions outside its natural distribution”, or “Fagus pollen occurs at fossil sites that were assumed to have never been within the distribution of the tree”), to name few examples.

- Paragraph 3.4 is enlightening. However, the term “analogue” (and “modern analogue”, which is used later in the Discussion) is not approriate. With pollen records, modern analogues are generally referred to pollen assemblages (thus to vegetation composition). Here instead you refer to “comparable, similar, population size of one taxon”. Using the term “modern analogue” without clarifying that you are using it with a different meaning creates confusion, particularly in the Discussion where reference is made to the early-Holocene hazel maximum.

-> There are other proxies that may be useful to determine presence of trees in fossil records (plant macrofossils and stomata). This could be mentioned in the text. Moreover, using fossil sites where such data is available could be useful to actually test the LDT limits, at least for some of the taxa.

-> Further, how do the inferences based on the LDT limits compare with inferences

made previously based on pollen percentages (or on plant macrofossils and stomata)? For instance, in a prior study (Giesecke et al., 2017 in JBiogeogr) a good agreement between the estimates of overall spread (Fig. 5a) based on different pollen percentage abundance classes was found. Some of the fossil sites were actually analysed for pollen, stomata, and plant macrofossils (e.g. Sägistalsee, Bachalpsee), but these results are not mentioned in this manuscript.

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Detailed comments:

- title: influx, or PAR? In the text you use PAR, not influx. Please homogenise terminology.

- Please consistently use italics for latin names.

- P6 L21-22: you extracted forest cover data from all grid cells within 10km radius of the traps. Did you then calculate a mean forest cover value? Please clarify.

P7 L8: why 271 modern samples? The abstract mentions 2742 anual samples.

P7 L10-ff.: am having trouble understanding what has been done, and why. Did you match fossil and modern PAR based on their ranking in the classes? What has been averaged? why should 500-year bins represent periods of long-term vegetation stability (fossil pollen records show fast population doubling times for some taxa)? What distance measure did you use for the cluster analysis (Euclidean distance)?

- P10 L111-12: total PAR is not shown in Figure 5.

- P11 L12: here the conclusion is presented before the data and then the data are presented to support the initially stated conclusion. Please reverse the line of arguments.

- P11 L13: am having trouble to understand why 92 pairs were obtained. please clarify.

- P11 L14: cannot find t-test and p-values on figure 6. Neither were “t-test” and “p-

values" mentioned in the Material and Methods section. Please clarify why and what has been done.

P20 L12: MAT has a very low influence on pollen deposition (see Table S4).

P20 L17-18: Which data/analysis/result shows that biomass cannot explain the latitudinal PAR gradient?

P 20 L31: why cannot the details be discussed here?

P21 L14: statement "modern PAR for Betula and Pinus are not found in fossil samples" contradicts a prior statement (P10 L12 "Maximum PAR in traps are always higher compared to fossil situations, with the exception of Corylus").

P22 L16: the effects are possibly small in relation to the wide environmental gradient.

P22 L34-35: these are interesting notions. Please mention the usefulness of modern PARs (as listed here), and their importance for ecological and biogeographical studies, in the Introduction.

P22 last line: remove "and it is almost surprising that. . .explanatory power". It might be surprising, but is a fact that seems to contradict a prior study (Matthias et al.). Could the gradient length be an important factor here?

-> Figures 6-13: - the a) and b) frames could be merged by using horizontal boxplots (instead of barplots) in a), and adding b) as an overlay;

- font sizes are too small;

-> References: - add: Caudullo, G., Welk, E., San-Miguel-Ayanz, J., 2017. Chorological maps for the main European woody species. Data in Brief 12, 662–666. https://doi.org/10.1016/j.dib.2017.05.007

-> In Table S4:

- caption: what does "alternatively" mean here? Please clarify.

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- Forest biomass, or forest cover (as of P6 L20-21)?

- are the “Adjusted R2” values of the “adjusted PAR” values the ones obtained with the Andersen correction factors (P6 L23-24)? Please clarify.

- please add r2 values for “tree PAR” (not total PAR) vs forest cover.

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