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Interactive comment on "Aridification signatures from middle–late Eocene pollen indicate widespread drying across the Tibetan Plateau after 40 Ma" *by* Qin Yuan et al.

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1) The authors discussed the aridification on the Tibetan Plateau (TP) during the middle to late Eocene based on a palynological study from Nangqian Basin in northeastern TP. This work provides fundamental and important data for the evolution of plant diversity as well as paleoenvironmental change on the plateau.

We would like to thank the reviewer for their time in reviewing our manuscript, and for their insightful comments which have helped to improve the work.

2) Firstly, the authors need to clarify the position of Nangqian Basin on the TP, it seems

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Discussion paper

that their statement is not consistent throughout the MS. In lines 50-51, it is “The uplifting, large-scale thrusting and striking of the TP caused several Paleogene intracontinental basins to form within the northern TP, including the Nangqian Basin”; but in lines 80-81, it is “The location of the Nangqian Basin on the east-central part of the TP”.

We agree and will ensure this is made consistent in the revised manuscript.

3) There is few evidence to indicate that the aridification in central Asia related to this northeastern part of the TP, actually, they belong to two different tectonic units. Therefore, it is beyond the scope of this study to use palynological evidence from northeastern TP to discuss the aridification of central Asia.

While uplift of the TP has traditionally been invoked to explain the onset of Asian aridification, retreat of the proto-Paratethys Sea in the Eocene has now also been shown as a major factor (Kaya et al., 2019). This sea extended from the Mediterranean Tethys to the Tarim Basin in western China, and through moisture transport via the westerlies, constituted a major moisture source to the Central Asian interior (Bougeois, 2014; Bougeois et al., 2018; Caves et al., 2015) despite its eastern extent being thousands of kilometres (roughly equidistant) from both the Xining and Nangqian basins. Both Northern Tibet (Xining Basin) and Central Asia (east-central Tibet: Nangqian Basin) have received moisture dominantly via the westerlies, which have maintained a semi-arid to arid climate in Central Asia since the early Eocene (Caves Rugenstein & Chamberlain, 2018). Therefore, we argue that aridification in both parts of Tibet is indeed related to a single, large-scale atmospheric transport system operating over this part of the TP during the Eocene, which justifies our comparison of palynological records.

An additional reason we use NE Tibet for more detailed comparison and age correlation is that this is one of the few sections on the TP that is both time-extended (Paleocene-Oligocene; Dupont-Nivet et al., 2008, 2008; Hoorn et al., 2012; Bosboom et al., 2014) and has good independent age control. This allows to observe long-term trends in palynomorph variation through time, so that correlations between different sections

can be based on real vegetation changes instead of possible short-term fluctuations that would not be detected in less time-extensive sections. We do caution in Lines 334-335 that further investigations should be made in Nangqian and other parts of the Tibetan Plateau using independent age control to corroborate this finding. However, we support our results with biostratigraphic correlation to multiple other assemblages from different parts of the TP (illustrated in Figs. 1 & 3) in order to detect large-scale patterns of vegetation change across the plateau through the Paleogene. While there are obviously regional controls on plant distributions and abundance on different parts of the plateau, there are also broad similarities that can be used for correlation between all of the sections, which was previously recognised by Wang et al., 1990a, 1990b, Sun & Wang, 2005, and others. We now also include the adjacent basins of Gonjo and Markam for comparison as suggested in a later comment.

4) The authors need to use quantitative method (such as the pollen/spore percentage to evaluate if they might be in-situ or not) to discuss paleoelevation/ paleoclimate in Nangqian Basin with palynologial data, because the downslope transport of pollen/spores from taxa living on high elevations could disturb their paleoenvironmental signals.

Palynological assemblages generally reflect the regional vegetation, except in particular environmental settings such as coal swamps in which the autochthonous palynomorph content can be up to 100%. Therefore, it is expected that the assemblage will not only record vegetation that was present at the site itself but also the wider area, and this is beneficial as it reflects regional climate instead of conditions that could be locally controlled. By the middle-late Eocene on the TP, it becomes clear that palynological assemblages reflect a vertical zonation of vegetation, and the existence of surrounding higher elevations (e.g., Hoorn et al., 2012; Wu et al., 2018). Accordingly, many significant works on the palaeoelevation and palaeoclimates on the TP (e.g., Song et al. 2010; Hoorn et al., 2012; Miao et al., 2013; Sun et al. 2007, 2008; Miao at el., 2016; Wu et al. 2018) have used palynology without applying quantitative methods to deter-

mine whether all the palynomorphs were deposited at site, because it is already known this is not the case. In recognition of the fact that the assemblage reflects the regional vegetation, we do not use palynology to calculate precise climatic parameters such as mean annual temperature or mean annual precipitation. In Section 5.3: Elevational implications, we discuss in detail the difficulties of calculating palaeoelevation based solely on palynology, including the issue of arboreally transported pollen. Because of this factor, we did not use conifers or other taxa prone to longer-distance transport (e.g., Alnus, Betula) to estimate palaeoclimatic conditions, in order to be cautious about allowing taxa from a potentially significantly different elevation to influence the paleoenvironmental analysis. In our section the percentage of spores is high relative to other TP basins (Miao at el., 2016), suggesting a significant proportion of deposition at site, and we will insert discussion on this into the revised manuscript.

5) The authors should compare their results with recent studies from adjacent basins including Gonjo Basin and Markam Basin.

We agree and will include comparisons to these basins in the revised manuscript; regarding pollen data from the Gonjo Basin, at present we are only aware of two publications: 1) "BGMRX, 1993. Regional Geology of Xizang (Tibet) Autonomous Region. Geol. Mem., vol.1. Geological Publishing House, Beijing" which contains a short mention of some species from the tops of the Gongjue Formation and Lawula Group but unfortunately no percentage data or information about the Ranmugou Formation; and 2) "Studnicki-Gizbert, C., Burchfiel, B.C., Li, Z. and Chen, Z., 2008. Early Tertiary Gonjo basin, eastern Tibet: Sedimentary and structural record of the early history of India-Asia collision. Geosphere, 4(4), 713-735" which reports only 3 very poorly preserved palynological samples, making it difficult to correlate with our section. If the reviewer could let us know of any further references that exist with more information on pollen content from Gonjo Basin, this would be appreciated.

6) I do not think that the geological age could be well constrained by palynological evidence such as Ephedra, which has quite rich fossil record throughout the Cenozoic.

The age of our section has been determined using a variety of different constraints, including K-Ar ages, zircon U-Pb age data, and biostratigraphic means. Firstly, emplacement ages from shoshonitic lavas and felsic and porphyry intrusions that are either interbedded with, or unconformably overlie, the lacustrine to alluvial Nangqian strata were used to determine a minimum age of ca. 37-38 Ma for the RZ section. This is congruent with palynological evidence for the overall age of the sampled strata. Next, biostratigraphic correlation between assemblage from the RZ section and other parts of the Tibetan Plateau (Fig. 3) provides a refinement of the age to middle-late Eocene. This is discussed in detail in Section 5.1: Age assignment in the main manuscript. The relative abundances of Ephedripites (Ephedripites) and Ephedripites (Distachyapites) are further proposed to constrain the age, as at some point during the Paleogene Ephedripites (Distachyapites) became more abundant than Ephedripites (Ephedripites), which is common in the Cretaceous (Han et al., 2016; Bolinder et al., 2016). In NE Tibet this change has been determined to be from ca. 39 Ma onwards, but we agree that this may not have occurred across the TP simultaneously. Expanded discussion on this will be included in the revised manuscript to justify use of this approach. Furthermore, we agree that it is challenging to determine a precise age from palynology, and hence we will adopt a more cautious approach by revising our final assignment to an age range (late Lutetian-Bartonian) for our section rather than a specific age (i.e., 42 Ma; 40 Ma/MECO; 38 Ma as in Fig. 5) for each of the pollen zones.

7) How could the authors suggest a tropical forest in Zone II with data from few taxa? There should be much higher plant diversity and more thermophilic species in the assemblage if it is a real 'tropical forest'.

We agree, and recognize that this implication should be better discussed in the revised manuscript. As pointed out by the reviewer, the palynology does not indicate the existence of a real 'tropical forest' during Zone II but only an increase in regional input of some tropical taxa. While this suggests a temporary warming period, it does not mean a complete biome transition from steppe vegetation to forest. In the original manuscript

this was not made clear, and we will amend this. We are however confident that this zone is distinct and represents a change in climate based on three lines of evidence. Firstly, this zone shows a large decrease in steppe-desert pollen which is not observed in the other zones of this section (average 9% steppe-desert pollen in Zone II vs 38% (Zone I) and 32 % (Zone III)), nor later in the Eocene in the Nangqian Basin (Yuan et al., 2017). There is also a spike in the ancestral Ephedra type during Zone II, and this is also not observed elsewhere in this section or that of Yuan et al. (2017). This spike in ancestral Ephedra, together with an increase in warm forest, are only observed over the MECO in the Xining Basin, NE Tibet and not later in the Eocene (Hoorn et al., 2012; Han et al., 2016) or in the middle Eocene (Meijer et al., submitted). Lastly, the tropical forest spike in Zone II of the RZ section is unusual and also not observed elsewhere in this section or elsewhere in Nangqian in the Eocene (Yuan et al., 2017) or the late Paleocene-early Eocene of Nangqian (Barbolini et al. 2018: Barbolini, unpublished data), however we recognise that this spike is only present in one sample, and therefore further investigations should be made in Nangqian and other parts of the Tibetan Plateau to corroborate this finding. We mention this limitation in the Discussion section (lines 318–325). We are also confident that the pollen in Zone II do not represent reworking or contamination, as the palynomorphs from these samples were not degraded or compressed to a greater degree than palynomorphs from the rest of the section, and of a similar colour and appearance. However, we will also include a discussion on statistical limitations of the samples.

8) I suggest the authors to change the title. Why could the authors conclude a 'widespread' drying across the Tibetan Plateau mainly based on palynological study from one site in northeastern part of the plateau? The authors need to clarify it in the title even they have used data already published from different parts of the plateau in the analyses. It is not accurate to use the word 'pollen', which only includes seed plants (angiosperm and gymnosperm). It is 'spore' in ferns, which the authors also observed in the sediment.

We are aware that both spores and pollen are present in the samples as illustrated in Plates I-III, however the use of the word “pollen” in the title refers to the progressive aridification observed from key pollen species in the samples, and therefore its use is accurate in that case. Throughout the main manuscript, we will ensure that when the term “pollen” could also include spores, this shall be changed to “palynological” to avoid ambiguity. We agree the title should be modified and contracted to focus on the present results, i.e. “Aridification signatures from middle-late Eocene pollen indicate a drying climate on the east-central Tibetan Plateau”.

9) The authors did not demonstrate on SEM method they used for taxonomic identification; moreover, they did not tell why only few pollen/spore s morphotypes were observed by SEM as shown in Plate III.

As is standard for palynostratigraphic studies, we used primarily LM (light microscopy) to identify, count, and photograph the pollen and spores present in the samples (e.g., Traverse, A., 2007, Paleopalynology, 2nd ed. Springer, Dordrecht, Appendix: Palynological Laboratory Techniques and p. 53: “light microscopy is the workhorse method for study of palynomorphs, and this will remain the case for the immediate future”). The SEM plate is included primarily to illustrate the appearances of Ephedripites (Ephedripites) and Ephedripites (Distachyapites) under SEM as well as some other key species in different palynozones of the studied section. SEM was not necessary for taxonomic identifications of all of the pollen and spores present, thus duplicate SEM plates showing the same palynomorphs as Plates I and II were not included. This explanation will be added to the Methods section for clarity.

10) Figure 1: The southeastern marginal part of Qiangtang Terrane should be much narrower than shown.

We agree; this will be amended and the Songpan-Ganzi Terrane also marked above.

11) The authors need to uniform the format of cited references: a few references are listed by full author names, and they are not in chronological order (e.g., Line 42); both

‘and’/’&’ (Line 321) occur in cited references.

The references have been double-checked for consistency and errors amended.

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