

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.

Anonymous Referee #1

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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. However, the authors fail to prove their conclusions with their data and explanations. Firstly, the authors need to clarify the position of Nangqian Basin on the TP, it seems 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”. Moreover, there is few evidence

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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. Secondly, 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 palynological data, because the downslope transport of pollen/spores from taxa living on high elevations could disturb their paleoenvironmental signals. Meanwhile, the authors should compare their results with recent studies from adjacent basins including Gonjo Basin and Markam Basin. Thirdly, 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. My another concern is the vegetation types concluded by the authors. 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’. Other minor suggestions: 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. Meanwhile, 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. The authors did not demonstrate on SEM method they used for taxonomic identification; moreover, they did not tell why only few pollen/spore morphotypes were observed by SEM as shown in Plate III. Figure 1: The southeastern marginal part of Qiangtang Terrane should be much narrower than shown. Moreover, the authors need to uniform the format of cited references: 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.