

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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A) This manuscript, entitled, “Aridification signatures from middle-late Eocene pollen indicate widespread drying across the Tibetan Plateau after 40 Ma” by authors Yuan et al., presents a detailed and well-written new palynological study worth of publication in *Climate of the Past*. The new work on the RZ section from the Nangqian Basin may become a valuable contribution to the understanding of the climatic and tectonic histories of Tibet.

We would like to thank the reviewer for their positive evaluation of our manuscript, and for their insightful comments which have helped to improve the work.

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B) First, the authors should do a better job disclosing, both in the text as well as figures, where in the stratigraphic sections and to which zone each of the 21 productive samples belongs. For example, this should be clear for zone II, which the authors interpret as MECO: are these interpretations based on a single sample? Such bold regional or global claims should be substantiated not only by robust evidence within the section but also corroborating evidence published elsewhere. I suggest the authors not only plot their samples on their stratigraphic sections (e.g. Figs 2, 3 and 4) but also discuss the statistical limitations of their samples (Zone II has only 2 samples; Zone III has 3).

We agree and will expand on this in the revised manuscript: based on comments from both reviewers we have decided to adopt a more cautious approach to our age assignment due to the limited number of samples, and will assign an age range (late Lutetian-Bartonian) to our section rather than a specific age (i.e., 42 Ma; 40 Ma/MECO; 38 Ma as in Fig. 5) to each of the pollen zones. We are confident that the palynological character of the assemblages combined with the K-Ar ages and zircon U-Pb age data (discussed in Section 5.1: Age assignment) is sufficient evidence for assigning this age range. There are three different lines of evidence that support this age assignment (lines 296-325). 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

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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 here on statistical limitations of the samples. These are already plotted on Fig. 2 and we will also plot them on Fig. 4; to do this on Fig. 3 is challenging because of the reduced interval of time the studied section encompasses compared to other sections across the TP. Expanding the figure to allow 21 samples to be plotted on Section 4 (this study) would render the figure too large for publication.

C) Further, I think the manuscript could benefit from additional discussion and a new figure similar to figure 3 that compares the palynological record presented here with non-palynological data such as stable isotope data from the region.

Unfortunately, we did not obtain stable isotope data during our study and generating a new figure on this spanning the TP is outside the scope of this study, but our record can be compared in the text with previous studies presenting these data from the Nangqian Basin, e.g., Li et al., 2019. Carbonate stable and clumped isotopic evidence for late Eocene moderate to high elevation of the east-central Tibetan Plateau and its geodynamic implications. *GSA Bulletin*, 131(5-6), 831-844.

D) Second, there are ample opportunities to help this manuscript reach a broader audience. As a non-palynologist familiar with paleoclimate, I repeatedly found myself searching for the significance of some of the findings or the implications of a particular species abundance. This is particularly true for the paleoclimate discussion sections. For example: 1) Line 65: Explain the I-AM more.

We agree and will expand on this in the revised manuscript.

2) Figure 1: These index maps aren't particularly useful. Perhaps something that is more (paleo)geographical or a vegetation map would help with the paleoclimate

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reconstructions to come?

We agree and will replace Fig. 1A with an Eocene palaeogeography of the area with locations of the different basins marked. Fig. 1B will be a current vegetation distribution map of the TP to allow for comparison with the reconstructed Eocene vegetation presented later.

3) Figure 2: The ecological groups (e.g. Pteridophytes) could be better annotated for non-specialists, NLR should be explained, and N/E ratios could be labeled desert/semi-desert and steppe-desert.

We agree and will clarify these points in the revised manuscript.

4) Figure 3: Index map could be greatly improved and this study could be highlighted with a different marker. The plant functional types listed here aren't being consistently used throughout the paper (e.g. "temperate broad-leaved forest" etc in figure 2). These should be consistent throughout.

As above, we will replace the index map, as well as highlight this study and standardise terminology of the plant functional types.

5) Figure 4: These taxa should be explained, especially as you go on to stress the importance of Ef/Ed ratios later.

We agree and will expand on this in the revised manuscript.

6) Background on MECO should be developed earlier.

We agree and will amend this in the revised manuscript.

7) PFTs should be developed earlier and consistent throughout the text.

We agree and will amend this in the revised manuscript.

8) More explanation is needed as to why you favor N/E over Ef/Ed.

We agree and will expand on this in the revised manuscript.

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9) Age constraints should include Ma throughout in addition to just stratigraphic stages e.g. line 423.

We agree and will amend this in the revised manuscript.

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