Peer Review File

Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Palaeogene

Reviewers' Comments:

Reviewer #1:
Remarks to the Author:
The evolution of large river is always a hot topic in earth science and attract huge interest. This study provide the traditional provenance results of the Upper Cretaceous to Paleocene in four basins, eastern Tibet, including petrographic modal counting and heavy mineral analysis for 22 samples and detrital zircon U-Pb ages for 16 samples. The results show the provenance similarities between the samples, the Songpan-Ganzi, Yidun, and Yangtze terranes, and the upper Jinsha, Min and Yalong rivers sand. Based on the provenance interpretation, the authors believed that there was a long-lived continental-scaled river system flowed into the Neo-Tethyan Ocean which generated a low-relief landscape in eastern Tibet. However, the provenance analysis and drainage reconstruction is questionable and thus conclusions in the manuscript are not reasonable. Moreover, there are several similar studies published before which eclipsed the significance of this study, not to mention several big weaknesses. Thus, I have to reject the manuscript for publication in Nature Communications.
Major Comments:
(1) To track the provenance across continents for a specific geological time period (e.g., late Cretaceous to Paleocene), comparisons have to be made amongst such equivalent-aged sedimentary strata. The authors should give the age constraints of the strata as accurate as possible. We all know that the Upper Cretaceous to Paleocene in these studied basins are based on mostly by the ostracods that identified about 40 years ago, which is not robust age constraint. Although the authors provide the youngest single detrital zircon ages (80, 90, 76 Ma for Sample CX-34, CX-25, CX-36, respectively) of the Chuxiong Basin, the single zircon age is insufficient to tell the maximum depositional age (MDA). About the MDA, pls refers to the following paper.
Sharman and Malkowski, 2020. Needles in a haystack: Detrital zircon U-Pb ages and the maximum depositional age of modern global sediment. Earth-Science Reviews 203, 103109.
https://doi.org/10.1016/j.earscirev.2020.103109
Moreover, the samples of the Simao Basin are from the Denghei Formation, which is believed to be deposited during Paleocene to late Eocene. The Upper Cretaceous to Lower Paleocene in the Simao Basin is the Mengyejing Formation which have robust radioactive and magnetostratigraphic constraints (112~>63 Ma, Yan M. et al., 2021, Science China Earth Science; Wang L. et al., 2015, Cretaceous Research). Please use provenance similarities to link continents and restore paleo-drainage systems only when the sedimentary strata are formed within a same time. Without correlation between the coeval strata, it is not convincible that the large-scale river can be flow into Simao and further south.
（2）The depositional environment is essential to reconstruct the paleo-river system. In Supplementary Information Xichang Basin section, for the Xiaoba and Leidashu Formations, Deng et al. (2018, ref. 16) never proposed a meandering river but a fluvial and shallow-lacustrine environment. Also, ref. 25 indicate a lacustrine environment. Together with the occurrence of evaporites, I think the lacustrine environment is more reasonable. In Chuxiong Basin section, I strongly doubt the interpretation of the exorheic lake because the thick evaporites are occurred in the Jiangdihe Formation. Similarly, the Upper Cretaceous Mengyejing Formation developed several hundred meters of evaporites, indicative of an endorheic basin. How to understand the drainage pattern when the large river flow into the Chuxiong and Simao Lake?
(3) Provenance interpretation
a, About the potential sources. I doubt if you correctly plot the age distribution of the Yangtze terrane. Because all 1191 zircon ages in figure 3 and extended data figures 4&6 don't show a prominent age peak of 750-1000 Ma which is believed to be a diagnostic feature of the Yangtze. I tried to find the paper you cited (refs. 14& 30) and found that the zircon ages in these papers are from the Early Paleozoic. So why the age peaks at ca. 150 Ma and 220 Ma occur? I don't know the specific samples of the 1191 ages although you mentioned they were from the pre-late Cretaceous strata. Thus, the relationship between the ages<200 Ma and the Yangtze terrane need to be further proved.
b, line 171-172, We cannot interpret the provenance just based on the age peaks. Why the Lhasa Terrane just simply provide the late Cretaceous zircons to the Chuxiong Basin, not conclude the other age populations? If you plot the Lhasa in the MDS diagram, you will find the Lhasa plot apart from

your samples.
c, In the MDS plot (Figure 5b), clearly, the Min River and Yalong River plot apart from the samples. The age peaks of Yalong River sand are different from the samples and Songpan-Ganzi. Only Upper Jinsha river plot closer to the samples. So how do you explain your drainage reconstruction?
(4) Drainage system
As mentioned above, for the reasons of the depositional environment and provenance interpretation, I disagree with the idea that a large-scale river system that connect the all the so-called exorheic basins although I agree somehow a south-flowing river exists. Specifically, the Chuxiong and Simao are two endorheic basins with several hundred meters of evaporites. Perhaps, the river just stop when it flowed into these two basins. Even if a trans-continental river system flowed to the Neo-Tethyan Ocean, about the river course more evidence should be provided, e.g., provenance correlation to the west Burma? And to Khorat Basin? Cai F.L. et al. (2020, GSAB) indicated that the Upper Cretaceous-Eocene strata are mainly sourced from the western Myanmar Arc with detrital zircon age peaks of 100-60 Ma. This is totally different with the coeval strata of the eastern Tibetan basins in this study. Thus, in the reconstruction map Figure. 4, I strongly doubt the river flowed Simao via Myanmar to the Neo-Tethyan Ocean. Actually, the paleocurrent of the Simao Basin would suggest a connection with the Khorat basin (See Yan M.D., et al., 2021, and references therein). More importantly, large river system in the Late Cretaceous flowed to the Neo-Tethyan Ocean had been proposed by Yan Maodu et al., 2021, Science China Earth Science. So it is not the authors who claimed that they proposed it for the first time. I found that there are several papers proposed a large river system prior to the collision between India and Asia, e.g., Deng et al. (2018, ref. 16); Wang L.C., et al. 2020, Palaeo-3; Yan M., et al., 2021.
(5) Data and methodology
1), Petrography part. The authors should tell the readers what kind of method you use when you do modal analysis. Usually, sedimentary geologists use the Gazzi-Dickinson method (Ingersoll et al., 1984). And all ternary diagrams should cite the original references.
In Extended data Figure 2, you use Lc to represent the carbonatite lithic, however, the Ls was used in the Table S2. Moreover, I strongly doubt the high percentage of carbonatite lithic in the samples, if yes, why don’t you count these into the Lv?
2), For all 22 samples for petrographic and heavy mineral analysis, I suggest the authors give a table that contain the GPS and stratigraphic information (I can’t tell which formation and basin of some samples belong to, e.g., CX-29, CX-01, CX08). In Extended data Figure 1, the authors should locate the heavy mineral samples in the stratigraphic columns of the Xichang, Huili, and Chuxiong basins.
3) Detrital zircon geochronology part. More information should be provided to let the readers examine the reliability of your data. What are the dating results of your age external standards? And how do these results compared to the suggested age values? So you should provide the dating results the standard zircons. In addition, relevant citations should be given regarding external standards. The representative CL images of detrital zircons especially the ones with young ages (<200 Ma in this area) is very important. Because in my opinion, these young zircons are mostly from local source.
4) The method of MDS is not sufficiently explained. (a) How the MDS map was generated? (b) Which metric do you choose when you plot the MDS, e.g., likeness, similarity......?
5) Line 461-464, the K-S test p-value is not recommended to use. Use of the K-S or Kuiper test p-values for quantitative similarity analysis of detrital geochronological data sets is likely to lead to incorrect conclusions (Satkoski et al., 2013, GSA Bulletin; Vermeesch, 2013; Saylor and Sundell, 2016, Geosphere). As noted by Vermeesch (2013), the D or V values provide more robust assessment of the dissimilarity between samples than do p-values. Thus, the D or V values of K-S or Kuiper tests are suggested to use.
Minor Comments:
1, Line 103, Yangtze is believed to be a part of South China block, why did you show a different concept? The same as in Figure 1a.
2, Line 125-128, I can‘t understand why the late Cretaceous-early Paleogene strata could represent the youngest terrestrial clastic deposits? At least the Lower Cretaceous in these basins are terrestrial clastic rocks.
3, Line 131, the same as the previous comment, actually the Upper Cretaceous evaporites developed

in the SW Sichuan, Xichang, Chuxiong, and Simao Basins (Liu Shugen et al., 2019, Journal of Chengdu University of Technology, v.46, No. 1, 1-28; and references therein). Especially, thick evaporites were developed in the Chuxiong and Simao Basin (Liu Chenglin et al., 2018, Ore Geology Reviews).
4, Line 135-137, about the depositional environment interpretation, pls see my comments above.
5, Line 204-205, during K2-E1, the global sea level was gradually rose from ca.80 Ma to 60-50 Ma, and fell since 50 Ma (Miller et al., 2005, Science, 10.1126/science.1116412). Thus, it is not stable. How to understand?
6, Actually, Deng B. et al.(2018, GSAB) published many detrital zircon ages of the Upper Cretaceous-Paleocene in Sichuan, Chuxiong basins. I suggest the authors should compile all published detrital zircon ages together with this study to do provenance analysis.
7, Figure. 1, where is the Sichuan Basin?
8, Supplementary Information line 41-42, very thick evaporites were developed in the Upper Cretaceous Chuxiong and Simao Basin. Thus, it is not an indication of river discharge but an endorheic lake.
9, Extended figure 4a and 4b, all the references are not incorrect. I can’t find any mentioned samples in these cited papers (ref 31, 47, and 25). For the ref. 25, maybe you want to refer to ref. 14 (Chen Y., et al., 2017, EPSL). However, the 272 zircon ages from ref. 14 belong to the Denghei Formation, which was assigned a Paleocene-late Eocene in age based on the fossils. Thus, I doubt whether it is reasonable to correlate to the Simao basin in this manuscript.
The part of Landscape evolution simulation is beyond my expertise, so I can’t give any comments about it.

Reviewer #2:
Remarks to the Author:
Dear Authors and Editor(s),
Thank you for the opportunity to review “Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Palaeogene” by Zhao and co-authors. This study reports provenance data (detrital zircon and sandstone petrography) from Cretaceous-Eocene sedimentary basins that are located along the southeast margin of the Tibetan Plateau. The authors observe that the provenance data is very consistent between basins and back up these observations with statistical methods that are mainly presented in the supplementary figures. To explain these data, the authors argue that there was likely a continent-scale river that connected the basins and drained to the Neo-Tethyan Ocean, which separated India from Eurasia prior to India-Asia collision. The authors note that this interpretation also has relevance to the debate over the low-relief, incised landscapes that are located along the southeast margin of the Tibetan Plateau. This aspect of the research will likely be the most broadly relevant and controversial aspect. Several dynamic models for Tibetan Plateau growth invoke different mechanisms for formation of the low-relief landscapes. This, coupled with the inherent interestingness of ancient, continental-scale rivers, make this research exciting and broadly relevant.
As I am an expert on detrital zircon geochronology and the tectonics of the Tibetan Plateau, I chose to focus on these aspects for my review. In my opinion, the provenance data are robust and well supported by statistical methods. I would encourage the authors to include some more discussion of their comparison between source terrane signatures and basin signatures. A key point that must be proven is that the source areas are distinguishable based on their detrital zircon age spectra whereas all the sampled basins are not, indicating mixing by a large river system. The supplementary figures were very helpful to convince me of the validity of the argument, yet they are sparsely discussed or referenced in the main text.
The authors also present a landscape evolution model that they link with their thermal modeling results. They claim that it illustrates the plausibility of the continent-scale river along the southeastern Tibetan Plateau because faster exhumation rates would be expected in the upstream reaches of the drainages. This seems plausible to me, but the authors should also emphasize that the Lhasa Terrane hosted an active, Cordilleran orogenic system at the time of deposition of the samples. It has

previously been likened to the modern Andes. It should not be implied that fluvial drainage networks were solely responsible for the cooling of samples further toward the interior of the Tibetan Plateau, as the tectonic activity in this region undoubtedly affected these results.
Respectfully,
Dr. Andrew Laskowski
Assistant Professor
Department of Earth Sciences
Montana State University

Reviewer #3:
Remarks to the Author:
Comments to 'Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Paleogene' By Zhao et al.

Based on new petro-stratigraphy, heavy-mineral analysis, and detrital zircon U-Pb studies on late Cretaceous–early Paleogene sediments from the east margin of Tibet, Zhao et al. proposed a novel continental-scale river flowing southwestward to the Neo-Tethyan Ocean along the eastern Tibet in the late Cretaceous–early Paleogene, and used this river to explain the formation of low-relief in the east margin of Tibet. The topic of this study is of broad interest for geologists, and the proposed model is significant different with previous models, which seems valuable for publication in Nature Communications. However, there are some important unclears in the MS, which should be addressed before acceptance.

1. The authors claimed that the sediments they studied are Late Cretaceous-Early Paleogene, but did not provide solid evidences to support, although I noticed in the Supplementary Information they have shown some fossils evidence, but these are not enough. Some recent studies have shown that the age of sediments in this area is significant older than the traditional fossils suggested, e.g., Gourbet et al. (2017). Therefore, if the ages of these sediments are wrong, the river story could be changed.

2. The authors argued that the K2–E1 sediments were not deposited in spatially separated endorheic basins based on the lack of coarse-grained sediments and thick evaporites. However, these are not strong evidence to preclude this possibility. It is very likely that these sediments were derived locally by recycling from surrounding older rocks. For example, the Nangqian and Gonjo basins in eastern Tibet show similar lithologies as the K2-E1 sediments by the authors, but studies have shown that the two basins were sourced locally (Horton et al., 2002). The authors also suggested that the low-relief could not be severed as physiographic barriers for endorheic basins. However, endorheic basin can be formed in any landscape with local structures, e.g., normal fault.

3. The evidence of lack K2-E1 terrestrial deposits in the South China Sea to against a paleo-river flowed to the Proto-Pacific Ocean is not the truth. For example, as shown in the Fig. 3 of Clift et al., 2006 EPSL, the Paleogene sediments are very thick in the South China Sea. So the authors have to find strong evidences to support why a continental-scale river was flowed to the Neo-Tethyan Ocean rather than the Proto-Pacific Ocean.

4. The biggest problem of this MS is the Fig. 4, which is inconsistent with the tectonic background. The authors refer the reference of Muller et al. (2016) for the late Cretaceous paleogeography reconstruction, but the map shown in Fig. 4a is inconsistent with geological evidence: The Indochina and Sibumasu, which locate southwest of the South China Block, have amalgamated to South China in Late Triassic, and the Lhasa has collided with Qiangtang in the early Cretaceous, so it is impossible that a Neo-Tethyan Ocean was still existed between South China and Indochina as shown in Fig. 4a. Therefore, if a southwest flow river to the Neo-Tethyan Ocean existed in the Late Cretaceous, more evidence from the Indochina and Sibumasu terranes must be shown, currently the westernmost basin

shown in the MS is the Simao Basin, which cannot preclude the possibility that the river flowed to the South China Sea as previous model suggested.

Response letter

In this letter we will provide our detailed response (in blue text) to the comments of the editor and the three reviewers (in black) and explain all changes performed on the manuscript.

Response to the point raised by the Associate Editor

Regarding the "additional evidence that supports the proposed route (of the palaeo-river) to the Neo-Tethyan Ocean", we would like to mention four lines of additional evidence:

Reply:

(1) In the Simao–Khorat Basins (located south of the Chuxiong Basin in Indochina; Fig. 1), published data sets on sedimentology proxies, biomarkers, element geochemistry, and isotopic geochemistry consistently indicate that late Cretaceous to early Paleogene evaporites are mainly of marine origin, which has generally been interpreted as the result of a transgression of the Tethys ocean (e.g., Hite and Japakasetr, 1979; El Tabakh et al., 1999; Zhang et al., 2013; Liu et al., 2018; Qin et al., 2020; Wang et al., 2021). This suggests that the Simao–Khorat Basins was very close to the ocean at that time. Moreover, these basins pertain to Tethyan Tectonic Domain during the late Cretaceous–early Paleogene (Yan et al., 2021; Liu et al., 2018). Thus, a more reasonable interpretation is that the proposed continental-scale river system discharged into the Neo-Tethyan Ocean. We have supplemented this evidence on **lines 219–226** of the revised manuscript.

(2) A new compilation of detrital zircon ages from the studied basins and three other basins farther south (Simao, Muang Xai, and Khorat) are very similar and provide additional support for a through-going sediment transport system (see new Fig. S8 in Supplementary Information) (Carter et al., 1999; Wang et al., 2014; Wang et al., 2017; Chen et al., 2017; Wang et al., 2020; this study). Specifically, late Cretaceous samples from the Muang Xai and Khorat basins show strikingly consistent Precambrian peaks at 2400–2600 Ma, 1900–1600 Ma, and 900–600 Ma (Fig. S8), strongly suggesting the Songpan-Ganzi and Upper Yangtze terranes as main source areas (revised manuscript, **lines 212–216**).

(3) In the current depositional area of the Red River (the Yinggehai-Song Hong Basin of the South China Sea), many boreholes have revealed that the Cenozoic deposits at the bottom of borehole are not older than late Eocene (~37 Ma) (see Figure 4 in papers of Clift et al., 2006 (GRL) and Clift et al., 2008; Lei et al., 2011; Wang et al., 2019). Regionally, the Proto-South China Sea during the late Cretaceous to early Cenozoic period is characterized by a series of deep, rapidly-subsiding small-scale rift basins under back-arc extension (see review of Morley et al., 2012). For example, in the Pearl River Basin in the eastern South China Sea, the upper Cretaceous strata are characterized by a dominance of zircons with ages clustering around the late Jurassic–Cretaceous, which has been interpreted to be from nearby continental arcs (see Figure 6 in Shao et al., 2017, and Figure 10 in He et al., 2020). These observations clearly imply that there was no large-scale drainage system that linked eastern Tibet with the proto-South China Sea prior to late Eocene time (revised manuscript, **lines 216–219**).

(4) Abundant low-relief landscape patches are preserved on both sides of the present-day Ailaoshan-Red River shear zone (e.g., Schoenbohm et al., 2004; Clark et al., 2005; Fig.1; see Figure 1 of Wang et al., 2017) suggesting that there was a regional low-relief surface from the Tibetan hinterland to the sea, which partly supports our interpretation that a paleo-river flowed southwards into the Neo-Tethyan Ocean before the fault system

became active ~35 Ma ago (e.g., Scharer et al., 1994; Gilley et al., 2003). In other words, if there was a paleo-Red river connecting the Tibetan hinterland with the Proto-South China Sea before Miocene surface uplift in eastern Tibet (as proposed by Clift et al., 2006, GRL), its course must have been established after the formation of the regional low-relief surface. We have included this evidence on **lines 250–254** of the revised manuscript.

(5) Recently, Cai et al. (2020) and Zhang et al. (2021) have clearly shown that the Upper Cretaceous–Early Eocene deposits in the Sibumasu–Burmese region are of proximal origin (see Figure 10 in Cai et al., 2020, and Figure 13b in Zhang et al., 2021) based on detrital zircon data, thus excluding the possibility that there was a west-flowing continental-scale river system to the Sibumasu–Burmese region during the late Cretaceous–early Palaeogene.

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Response to common comments from Reviewer #1 and Reviewer #3

(1) Reviewer #1: To track the provenance across continents for a specific geological time period (e.g., late Cretaceous to Paleocene), comparisons have to be made amongst such equivalent-aged sedimentary strata. The authors should give the age constraints of the strata as accurate as possible. We all know that the Upper Cretaceous to Paleocene in these studied basins are based on mostly by the ostracods that identified about 40 years ago, which is not robust age constraint. Although the authors provide the youngest single detrital zircon ages (80, 90, 76 Ma for Sample CX-34, CX-25, CX-36, respectively) of the Chuxiong Basin, the single zircon age is insufficient to tell the maximum depositional age (MDA).

Reviewer #3: The authors claimed that the sediments they studied are Late Cretaceous-Early Paleogene, but did not provide solid evidences to support, although I noticed in the Supplementary Information they have shown some fossils evidence, but these are not enough. Some recent studies have shown that the age of sediments in this area is significant older than the traditional fossils suggested, e.g., Gourbet et al. (2017). Therefore, if the ages of these sediments are wrong, the river story could be changed.

Reply: As the reviewer#1 points out correctly, the maximum depositional age (MDA) is insufficient to constrain depositional ages with certainty. However, although the youngest single-grain ages from our samples are not a robust indicator of the true depositional age, they are consistent with the late Cretaceous–early Cenozoic biostratigraphic age of these deposits. More importantly, younger (i.e. late Eocene) zircons are completely lacking in our samples. Given that late Eocene plutons are common across southeastern Tibet (e.g., Lu et al., 2012; Deng et al., 2014); the absence of late Eocene zircon age implies that the studied continental red-beds are older than late Eocene.

We argue that the fossil assemblages provide reasonable information on the depositional age of our studied sedimentary sections and similar deposits that occur throughout eastern Tibet. The characteristic ostracods, charophyta, and few lamellibranchia are very common in late Cretaceous–early Paleocene strata from other areas of China as shown by recent reviews of Xi et al. (2019) and Wang et al. (2019), which provide further support for our age scheme. We wish to add that a recent magnetostratigraphy study shows that the Guankou and Mingshan Formations of the Shiyang section in the southwestern Sichuan Basin ranges in age from ~84 to ~43 Ma (Shen et al., 2018, Master thesis). Also, the Mengyejing Formation (which roughly correlates with the Guankou, Xiaoba, and Jiangdihe formation of this study) of the Jiangcheng section in the Simao Basin has been dated at ~112–63 Ma (Yan et al., 2021), largely in agreement with the palaeontological results. We added this information to the **Supplementary information**.

The Fig. R1 below summarizes the existing age data, which support our interpretation of a late Cretaceous to early Palaeocene age of the studied sediments.

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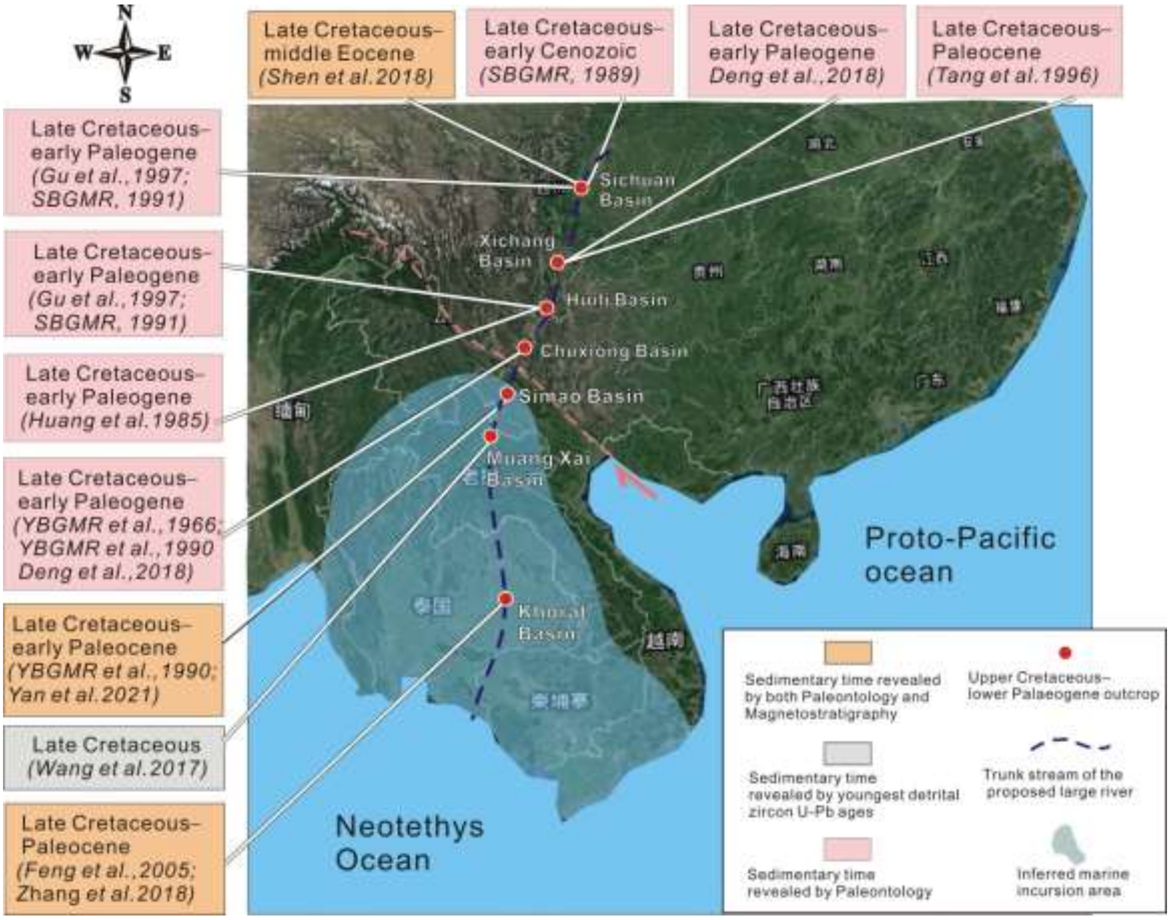

Figure R1. Summary of previously published sedimentary ages for upper Cretaceous to lower Palaeocene deposits in eastern Tibet. The early Cenozoic paleogeography is from Royden et al. (2008). Base map is from Google Earth.

(2) Reviewer #1: Even if a trans-continental river system flowed to the Neo-Tethyan Ocean, about the river course more evidence should be provided, e.g., provenance correlation to the west Burma? And to Khorat Basin? Cai F.L. et al. (2020, GSAB) indicated that the Upper Cretaceous-Eocene strata are mainly sourced from the western Myanmar Arc with detrital zircon age peaks of 100-60 Ma. This is totally different with the coeval strata of the eastern Tibetan basins in this study. Thus, in the reconstruction map Figure. 4, I strongly doubt the river flowed Simao via Myanmar to the Neo-Tethyan Ocean. Actually, the paleocurrent of the Simao Basin would suggest a connection with the Khorat basin (See Yan M.D., et al., 2021, and references therein).

Reviewer #3: The evidence of lack K2-E1 terrestrial deposits in the South China Sea to against a paleo-river flowed to the Proto-Pacific Ocean is not the truth. For example, as shown in the Fig. 3 of Clift et al., 2006 EPSL, the Paleogene sediments are very thick in the South China Sea. So the authors have to find strong evidences to support why a continental-scale river was flowed to the Neo-Tethyan Ocean rather than the Proto-Pacific Ocean.

Reviewer #3: If a southwest flow river to the Neo-Tethyan Ocean existed in the Late Cretaceous, more evidence from the Indochina and Sibumasu terranes must be shown, currently the westernmost basin shown in the manuscript is the Simao Basin, which cannot preclude the possibility that the river flowed to the South China Sea as previous model suggested.

Reply: Please see our response to the comment by the Associate Editor above (**numbers 1–5; Lines 8–80** in this letter). Also, please note that although Clift et al. (2006, EPSL) interpreted sedimentary deposits in the South China Sea to be Palaeogene in age (see Figure 3 of Clift et al., 2006, EPSL), borehole data have subsequently revealed that these terrestrial deposits are not older than **late Eocene** (~37 Ma) (as shown the Figure 4 in Clift et al., 2006 (GRL) and Clift et al., 2008, and Figure 2 in Wang et al., 2019).

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Response to the comments of the three reviewers

Reviewer #1

The evolution of large river is always a hot topic in earth science and attract huge interest. This study provide the traditional provenance results of the Upper Cretaceous to Paleocene in four basins, eastern Tibet, including petrographic modal counting and heavy mineral analysis for 22 samples and detrital zircon U-Pb ages for 16 samples. The results show the provenance similarities between the samples, the Songpan-Ganzi, Yidun, and Yangtze terranes, and the upper Jinsha, Min and Yalong rivers sand. Based on the provenance interpretation, the authors believed that there was a long-lived continental-scaled river system flowed into the Neo-Tethyan Ocean which generated a low-relief landscape in eastern Tibet. However, the provenance analysis and drainage reconstruction is questionable and thus conclusions in the manuscript are not reasonable. Moreover, there are several similar studies published before which eclipsed the significance of this study, not to mention several big weaknesses. Thus, I have to reject the manuscript for publication in *Nature Communications*.

Reply: As acknowledged by the reviewer, the paleo-drainage evolution and low-relief landscape formation in eastern Tibet have been debated for a long time. We acknowledge that we are not the first to propose a large-scale south-flowing river system prior to the India-Asia collision. However, the most significant finding/highlight of our manuscript is to link the development of this long-lived paleo-river system to the landscape evolution and the formation of the low-relief landscape in present-day eastern Tibet before Cenozoic uplift and plateau growth. Therefore, we argue that our study is of great significance and warrants publication in *Nature Communications*.

We have carefully considered the comments of reviewer #1. Below, we respond in detail to his/her major comments and argue that our interpretation is justified and robust.

>Major Comments

(1) The samples of the Simao Basin are from the Denghei Formation, which is believed to be deposited during Paleocene to late Eocene. The Upper Cretaceous to Lower Paleocene in the Simao Basin is the Mengyejing Formation which have robust radioactive and magnetostratigraphic constraints (112~>63 Ma, Yan M. et al., 2021, Science China Earth Science; Wang L. et al., 2015, Cretaceous Research). Please use provenance similarities to link continents and restore paleo-drainage systems only when the sedimentary strata are formed within a same time. Without correlation between the coeval strata, it is not convincible that the large-scale river can be flow into Simao and further south.

Reply: Although the Denghei Formation of the Simao Basin was assigned a Palaeocene–late Eocene age by Chen et al. (2017), their age scheme is based on palaeontology only. Chen et al. (2017) state that "*Paleocene to Eocene ostracods are present in the Denghei Fm., including Pinnocypris, Limnocythere, Ilypcypris, Cyprinotus, together with typical Paleocene charophytes of Gyrogona, Obtusochara, Peckichara"*). Since almost all of the above ostracods also occur in the lower Paleogene strata presented in our study (see Supplementary information) and because the magnetostratigraphic age of the Mengyejing Formation is 112–>63 Ma (Yan et al., 2021), we argue that the Mengyejing Formation and the overlying Denghei Formation form a continuous sedimentary succession (which is older than late Eocene). Furthermore, the stratigraphic units studied by us can be correlated well with the Mengyejing Formation and Denghei Formations. For example, they all formed as red beds during a long period with dry climate (Regional Geology of Sichuan Province, 1991; Regional Geology of Yunnan Province, 1990) and were likely deposited during the same time interval. In the revised manuscript, we have added previously reported detrital zircon data from the late Cretaceous Mengyejing Formation in the Simao basin for comprehensive provenance analysis (for details see **lines 212–216**, Fig. 3, and Figs. S4–6 in the revised manuscript).

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(2) The depositional environment is essential to reconstruct the paleo-river system. In Supplementary Information Xichang Basin section, for the Xiaoba and Leidashu Formations, Deng et al. (2018, ref. 16) never proposed a meandering river but a fluvial and shallow-lacustrine environment. Also, ref. 25 indicates a lacustrine environment. Together with the occurrence of evaporites, I think the lacustrine environment is more reasonable.

Reply: We have double-checked Deng et al. (2018, GSAB) and disagree with reviewer #1. Deng et al. (2018) did not suggest a meandering river environment, but proposed a fluvial environment for the Xiaoba and Leidashu Formations (page 9, lines 1–15). The lithological assemblage of the Xiaoba and Leidashu Formations in the Xichang Basin is characterized by alternating reddish sandstone, siltstone, and mudstone. Specifically, meter-thick sandstone beds with sharp erosional bases (Fig. R2) are most likely the result of lateral fluvial erosion and deposition (cf. Miall, 1996).

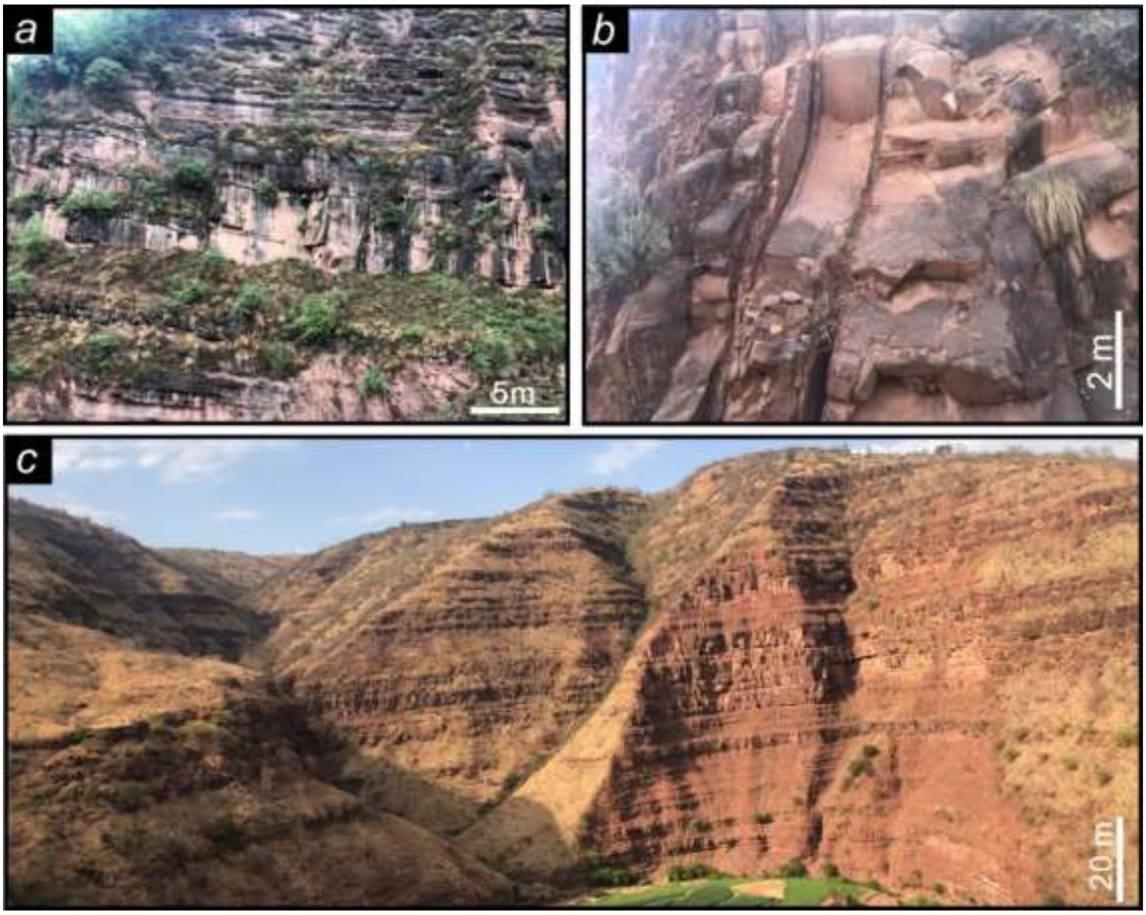

Figure R2. Typical fluvial sandstone bodies in Upper Cretaceous–lower Paleogene sedimentary rocks of the Xichang Basin (a), Huili Basin (b), and Chuxiong Basin (c). All photographs taken by Xudong Zhao.

Apart from lacustrine environments, siltstones and mudstones are also common in modern continental-scale river systems, especially in extensive and low-gradient floodplains of anastomosing or meandering rivers that provide accommodation space for fine-grained sediments (as shown the Figure 3b in Ashworth et al., 2012). Moreover, the present-day largest anastomosing rivers in the world often develop fine-grained sediments, with lakes and wetlands between levee-flanked channel branches and stable alluvial islands that divide flow up to bankfull (Knighton and Nanson, 1993; Abbado et al., 2005). In other words, the presence of fine-grained sediments in big river systems (as shown the Figure 3b in Ashworth et al., 2012) should not be taken as evidence for an overall lacustrine environment. Hence, we argue that our interpretation of a low-energy, muddy anastomosing or meandering river is reasonable for the Xichang Basin.

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(2 continued): In Chuxiong Basin section, I strongly doubt the interpretation of the exorheic lake because the thick evaporites are occurred in the Jiangdihe Formation. Similarly, the Upper Cretaceous Mengyejing Formation developed several hundred meters of evaporites, indicative of an endorheic basin. How to understand the drainage pattern when the large river flow into the Chuxiong and Simao Lake?

Reply: With respect to this comment, we note that *"Evaporite minerals are deposited within fluvial sub-strates by the evaporation of groundwaters and on the surface of playa mudflats during the evaporation of sheet floods. Thin beds, laminae, nodules, and individual crystals (or crystal casts) of evaporite, particularly gypsum and halite,* ***are very common in the deposits of arid fluvial systems****, especially in the distal regions where the braidplain or terminal fan merges imperceptibly into a playa lake or arid tidal flat (Smoot 1983; Glennie 1987; Mertz and Hubert 1990)."* cited from Miall (1996, p. 441). This sentence from Miall (1996) indicates that the presence of evaporites does not necessarily require an internal drainage pattern. During our field investigations in the Chuxiong Basin, we did not observe continuous and/or thick pure evaporites in the Jiangdihe Formation. The formation mechanism of small-scale evaporite rhythms is that "*inflowing runoff 'freshens' the brine body and this, together with cooler air temperatures, causes either cessation of evaporite precipitation or precipitation of a less undersaturated phase—the runoff also brings in the suspended clastic sediment"* (Leeder, 2011). Thus, to deny the existence of a through-going fluvial system on the basis of local evaporites is unsound, especially when considering the warm climate conditions during the Late Cretaceous to early Cenozoic.

From the published literature it may indeed appear that several hundred meters of evaporites occur in the Upper Cretaceous Mengyejing Formation of the Simao basin (e.g., Wang et al., 2020; Yan et al., 2021). However, many studies have demonstrated that these late Cretaceous–early Paleogene evaporites in the Simao basin (and the Khorat basin farther south) are mainly of **marine origin** and formed during incursions of the Tethys ocean (e.g., Hite and Japakasetr, 1979; El Tabakh et al, 1999; Zhang et al., 2013; Liu et al., 2018; Wang et al., 2020; Qin et al., 2020). As a consequence, the presence of these evaporites cannot be used as an argument against a fluvial system that drained into the Neo-Tethyan Ocean. We have clarified this issue in the revised **supplementary information (lines 203–214)**.

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(3a) About the potential sources. I doubt if you correctly plot the age distribution of the Yangtze terrane. Because all 1191 zircon ages in figure 3 and extended data figures 4&6 don't show a prominent age peak of 750-1000 Ma which is believed to be a diagnostic feature of the Yangtze. I tried to find the paper you cited (refs. 14& 30) and found that the zircon ages in these papers are from the Early Paleozoic. So why the age peaks at ca. 150 Ma and 220 Ma occur? I don't know the specific samples of the 1191 ages although you mentioned they were from the pre-late Cretaceous strata. Thus, the relationship between the ages<200 Ma and the Yangtze terrane need to be further proved.

Reply: We are sorry for the misunderstanding about the use of this name for the source area. In the submitted manuscript, the term "Yangtze terrane" was meant to be the present-day Sichuan Basin, to avoid the confusion between "Sedimentary Basin" and "Source Region". The 1191 zircon ages are all from the pre-late Cretaceous basement in the Sichuan Basin (Li et al., 2018). The Jurassic–early Cretaceous zircons (peak at ca. ~150 Ma) in the pre-late Cretaceous basement in the Sichuan Basin were likely derived from the southern margin of the North China Block (e.g., the Qinling Belt, Dabie Belt) (Li et al., 2018). Regarding the zircon age peak at ~220 Ma, there are several potential source areas, including the Songpan–Ganzi and Yidun terranes to the west, the Qinling Belt to the north, and the Western Jiangnan orogen to the east (Li et al., 2018).

It is likely that the source area with a prominent age peak of 750–1000 Ma mentioned by the reviewer reflects the western South China block (see Fig. S4), where a variety of Neoproterozoic strata/rocks along the western margin yield age peaks at 700–900 Ma (e.g., Li et al., 2003; Sun et al., 2003). In the revised manuscript, we now use "Upper Yangtze terrane" (cf. Huang et al., 2021), instead of “Yangtze terrane”, to refer to the generalized source region for the late Cretaceous–early Palaeogene deposits in the “Sichuan Basin”. For the locations of the Upper Yangtze terrane and the western South China, please refer to Fig. 1a. Corrections have also been made in **line 103** of the text.

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(3b) Line 171-172, we cannot interpret the provenance just based on the age peaks. Why the Lhasa Terrane just simply provides the late Cretaceous zircons to the Chuxiong Basin, not conclude the other age populations? If you plot the Lhasa in the MDS diagram, you will find the Lhasa plot apart from your samples.

Reply: As discussed in the submitted manuscript, the Lhasa terrane was not a dominant source area for the late Cretaceous–early Palaeogene strata, but possibly provided some Cretaceous zircon component to these basins, because the provenance signal of the Lhasa terrane is characterized by a single Cretaceous age-peak (see Figure 12 in Yan et al., 2021). We interpret the sediment transport system from the Lhasa terrane as a small tributary of the proposed south-flowing river system. This explains why the Lhasa source would plot apart from our samples in the MDS diagram. Please note that we do not plot the data from the Lhasa terrane in the MDS diagram (Fig. S6).

Except for the Chuxiong Basin, the late Cretaceous-early Paleogene strata in the Simao Basin also contain appreciable Cretaceous zircons (Yan et al., 2021), which further supports the existence of a river system connected to the eastern Lhasa terrane at that time. Consistent with provenance evidence, previous thermochronologic studies have indicated that there was most likely an externally drained river system from the Lhasa terrane to the ocean during the late Cretaceous–early Palaeogene (Hetzel et al., 2011; Haider et al., 2013). Please note that we cannot rule out the possibility that the externally drained river systems originating in the Lhasa terrane were independent from our proposed continental-scale palaeo-drainage system (see Fig. R3). We have explained this issue on **lines 186–191** of the revised manuscript.

Figure R3. Plate reconstruction of East Asia during the latest Cretaceous showing an alternative drainage model with several river systems draining the Lhasa or/and Qiangtang terranes to the Neo-Tethys (based on Hetzel et al., 2011; Haider et al., 2013; Gourbet et al., 2016). The map is generated by Xudong Zhao using open-access GPlates software (accessed through https://www.gplates.org/).

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(3c) In the MDS plot (Figure 5b), clearly, the Min River and Yalong River plot apart from the samples. The age peaks of Yalong River sand are different from the samples and Songpan-Ganzi. Only Upper Jinsha river plot closer to the samples. So how do you explain your drainage reconstruction?

Reply: The reviewer brings up an important point. For exploring this issue, we collected more detrital zircon data of the Yalong River from the literatures (Yang et al., 2012; He et al., 2013), and found that the detrital zircon age components of the Yalong River indeed differ from our late Cretaceous–early Palaeogene samples, because the Yalong River displays (1) a higher abundance of 600–1000 Ma zircon grains derived from the western margin of the South China block, and (2) shows a secondary age peak at 250–200 Ma that likely indicates a contribution from the Yidun Terrane (Fig. S4b). However, the sand sample from the Minjiang River that drains the Songpan–Ganzi region, plots closer to our K_2–E_1 samples in the MDS plot, supporting that the Triassic flysch of the Songpan–Ganzi terrane is the primary source for these K_2–E_1 strata. We have updated the revised manuscript to clarify this issue in **lines 170–172** of the revised manuscript and in Figs. S4 and S6.

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(4) As mentioned above, for the reasons of the depositional environment and provenance interpretation, I disagree with the idea that a large-scale river system that connect the all the so-called exorheic basins although I agree somehow a south-flowing river exists. Specifically, the Chuxiong and Simao are two endorheic basins with several hundred meters of evaporites. Perhaps, the river just stop when it flowed into these two basins.

Reply: We have explained this issue above in our response to the reviewer´s major point 2.

(4 continued) More importantly, large river system in the Late Cretaceous flowed to the Neo-Tethyan Ocean had been proposed by Yan Maodu et al., 2021, Science China Earth Science. So it is not the authors who claimed that they proposed it for the first time. I found that there are several papers proposed a large river system prior to the collision between India and Asia, e.g., Deng et al. (2018, ref. 16); Wang L.C., et al. 2020, Palaeo-3; Yan M., et al., 2021.

Reply: We acknowledge that we are not the first to propose a large-scale south-flowing river system prior to the India and Asia collision. Our work does lend new support to these previous assertions. Moreover, the most significant and novel finding of our study is to link the development and extent of this long-lived paleo-river system to the formation of the low-relief landscape in eastern Tibet before Cenozoic uplift and plateau growth.

(5) Data and methodology

(5-1) Petrography part. The authors should tell the readers what kind of method you use when you do modal analysis. Usually, sedimentary geologists use the Gazzi-Dickinson method (Ingersoll et al., 1984). And all ternary diagrams should cite the original references. >In Extended data Figure 2, you use Lc to represent the carbonatite lithic, however, the Ls was used in the Table S2. Moreover, I strongly doubt the high percentage of carbonatite lithic in the samples, if yes, why don't you count these into the Lv?

Reply: We appreciate the advice from the reviewer. We added the original references (Dickinson et al., 1983; Ingersoll et al., 1984) to the revised manuscript. We have also made the abbreviations for the different lithic components consistent throughout the revised manuscript: Ls is terrestrial sedimentary lithic, Lc refers to carbonate lithic, and Lv is volcanic lithic (the latter should not be combined with the carbonate lithic).

References

Dickinson, W.R., Beard, S.L., Brakenridge, G.R., Erjavec, J.L., Ferguson, R.C., Inman, K.F., Knepp, R.A., Lindberg, F.A. & Ryberg, P.T. Provenance of North American Phanerozoic sandstones in relation to tectonic setting. *Geol. Soc. Am. Bull.* **94**, 222–235 (1983).

Ingersoll, R. V., Bullard, T. F., Ford, R. L., Grimm, J. P., Pickle, J. D. & Sares, S. W. The effect of grain size on detrital modes: A test of the Gazzi-Dickinson point-counting method. *J Sediment Petrol.* **54**, 103–116 (1984).

(5-2) For all 22 samples for petrographic and heavy mineral analysis, I suggest the authors give a table that contains the GPS and stratigraphic information (I can't tell which formation and basin of some samples belong to, e.g., CX-29, CX-01, CX08). In Extended data Figure 1, the authors should locate the heavy mineral samples in the stratigraphic columns of the Xichang, Huili, and Chuxiong basins.

Reply: As requested by reviewer #1, we added a table (Table S3) that contains the GPS and stratigraphic information. In Fig. S1, all detrital zircon samples from the Xichang, Huili, and Chuxiong basins were also analyzed for heavy minerals, so locations of heavy mineral and detrital zircon samples from the Xichang, Huili, and Chuxiong overlap (please see legend of Fig. S1, where red stars refers to samples used for heavy mineral and detrital zircon analysis).

(5-3) Detrital zircon geochronology part. More information should be provided to let the readers examine the reliability of your data. What are the dating results of your age external standards? And how do these results compared to the suggested age values? So you should provide the dating results the standard zircons. In addition, relevant citations should be given regarding external standards. The representative CL images of detrital zircons especially the ones with young ages (<200 Ma in this area) is very important. Because in my opinion, these young zircons are mostly from local source.

Reply: To determine fractionation factors and correct for instrumental drift, two standards (91500 and GJ-1) were analyzed every 10 grains; element content was determined by NIST610 as external standard. We have revised this part in the manuscript (lines 298–304). All dating results of the standard zircons are available and the zircon standards are described in Yuan et al. (2004). We think that an extensive method description is not required, because detrital zircon geochronology is a standard tool and detailed information on methodology, age standards, and analytical procedures are available in the cited references (Andersen, 2002; Yuan et al., 2004).

As zircon grains of different age were indistinguishable based solely on CL images, we refrain from showing CL images. Ages <200 Ma most likely reflect recycled grains from pre-late Cretaceous strata in the Sichuan Basin (Upper Yangtze terrane), as suggested by petrographic and heavy mineral data.

References

Andersen, T. Correction of common lead in U-Pb analyses that do not report 204Pb. *Chem. Geol.* **192**, 59–79 (2002).

Yuan, H. L., Gao, S., Liu, X. M., Li, H. M., Günther, D. & Wu, F. Y. Accurate U-Pb age and trace element determinations of zircon by laser ablation inductively coupled plasma mass spectrometry. *Geostand. Geoanal. Res*. **28**, 335–370 (2004).

(5-4) The method of MDS is not sufficiently explained. (a) How the MDS map was generated? (b) Which metric do you choose when you plot the MDS, e.g., likeness, similarity......?

Reply: We added a method description on how we generated the MDS plot. Note that the MDS method used (cf. Vermeesch. 2013) is based on 'dissimilarities' between samples.

Reference: Vermeesch, P. Multi-sample comparison of detrital age distributions. *Chem. Geol.* **341**, 140–146 (2013).

(5-5) Line 461-464, the K-S test p-value is not recommended to use. Use of the K-S or Kuiper test p-values for quantitative similarity analysis of detrital geochronological data sets is likely to lead to incorrect conclusions (Satkoski et al., 2013, GSA Bulletin; Vermeesch, 2013; Saylor and Sundell, 2016, Geosphere). As noted by Vermeesch (2013), the D or V values provide more robust assessment of the dissimilarity between samples than do p-values. Thus, the D or V values of K-S or Kuiper tests are suggested to use.

Reply: Thank you for this helpful comment. As suggested, we replaced the K-S test p-value by the *D* values of K-S and the *V* values of the Kuiper tests (please see improved Table S1).

(6) Minor Comments:

(6-1) Line 103, Yangtze is believed to be a part of South China block, why did you show a different concept? The same as in Figure 1a.

Reply: We have explained this issue above in our response to the reviewer´s major point 3a.

(6-2) Line 125-128, I can't understand why the late Cretaceous-early Paleogene strata could represent the youngest terrestrial clastic deposits? At least the Lower Cretaceous in these basins are terrestrial clastic rocks.

Reply: This appears to be a misunderstanding by the reviewer. Apart from very limited Quaternary sediments (i.e. the Xigeda Formation), the late Cretaceous–early Paleogene strata are the youngest terrestrial clastic deposits at the eastern margin of Tibet. Of course, there are also older sedimentary strata of Lower Cretaceous age (but these are not the focus of our study).

(6-3) Line 131, the same as the previous comment, actually the Upper Cretaceous evaporates developed in the SW Sichuan, Xichang, Chuxiong, and Simao Basins (Liu Shugen et al., 2019, Journal of Chengdu University of Technology, v.46, No. 1, 1-28; and references therein). Especially, thick evaporites were developed in the Chuxiong and Simao Basin (Liu Chenglin et al., 2018, Ore Geology Reviews).

Reply: We have explained this issue in our response to the previous comments above. Here, we only add that Liu Chenglin et al. (2018, Ore Geology Reviews) did not propose that thick evaporites occur in the Chuxiong basin, which is consistent with our field investigations.

Reference:

Liu, C.L., Wang, L.C., Yan, M.D., Zhao, Y.J., Cao, Y.T., Fang, X.M., Shen, L.J., Wu, C.H., Lv, F.L. & Ding, T. The Mesozoic-Cenozoic tectonic settings, paleogeography and evaoritic sedimentation of Tethyan blocks within China: implications for potash formation. *Ore Geol. Rev.* **102**, 406–425 (2018).

(6-4) Line 135-137, about the depositional environment interpretation, pls see my comments above.

Reply: We have already addressed this issue in our response to the major comment 2 of reviewer #1 above.

(6-5) Line 204-205, during K2-E1, the global sea level was gradually rose from ca. 80 Ma to 60-50 Ma, and fell since 50 Ma (Miller et al., 2005, Science, 10.1126/science.1116412). Thus, it is not stable. How to understand?

Reply: We agree that sea level was not stable in a rigorous sense. Nevertheless, Figs. 2 and 3 in Miller et al. (2005) indicate that the global sea level rose very slowly (but did not change significantly between ~92 Ma and ~55 Ma (as shown the Figure 3 in Miller et al., 2005). In the revised manuscript, we explain that the sea level was slowly rising (**line 233**). Note that the slowly rising sea level, coupled with an arid climate, could well be responsible for the marine incursions to the Simao to Khorat basins and the formation of the marine evaporites there.

References

Miller, K. G., Kominz, M. A., Browning, J. V., Wright J. D., Mountain, G. S., Katz, M. E, Sugarman, P. J., Cramer B. S., Christie-Blick, N. & Pekar S. F. The Phanerozoic Record of Global Sea-Level Change. *Science* **310**, 1293–1298 (2005).

(6-6) Actually, Deng B. et al. (2018, GSAB) published many detrital zircon ages of the Upper Cretaceous-Paleocene in Sichuan, Chuxiong basins. I suggest the authors should compile all published detrital zircon ages together with this study to do provenance analysis.

Reply: The error calculation of the detrital zircon data of Deng B. et al. (2018) and ours are different. Moreover, zircon age spectra from Deng et al. (2018) are largely similar to our data, thus compiling more detrital zircon ages would not change the conclusions of our study.

References

Deng, B., Chew, D., Jiang, L., Mark, C., Cogne, N., Wang, Z. J. & Liu, S. G. Heavy mineral analysis and detrital U-Pb ages of the intracontinental Palaeo-Yangtze basin: Implications for a transcontinental source-to-sink system during Late Cretaceous time. *Geol. Soc. Am. Bull.* **130,** 2087–2109 (2018).

(6-7) Figure. 1, where is the Sichuan Basin?

Reply: We added the term "Sichuan Basin" (also called Upper Yangtze terrane as explained above) in Fig. 1.

(6-8) Supplementary Information line 41-42, very thick evaporites were developed in the Upper Cretaceous Chuxiong and Simao Basin. Thus, it is not an indication of river discharge but an endorheic lake.

Reply: We have addressed this issue in detail in our response above.

(6-9) Extended figure 4a and 4b, all the references are not incorrect. I can’t find any mentioned samples in these cited papers (ref 31, 47, and 25). For the ref. 25, maybe you want to refer to ref. 14 (Chen Y., et al., 2017, EPSL).

Reply: We are sorry that the numbers of the cited reference were incorrect. In the revised manuscript, the reference numbering has been corrected.

(6-9 continued) However, the 272 zircon ages from ref. 14 belong to the Denghei Formation, which was assigned a Paleocene-late Eocene in age based on the fossils. Thus, I doubt whether it is reasonable to correlate to the Simao basin in this manuscript.

Reply: We have addressed this issue in our response to the second part of main point 1 above.

Reviewer #2 (Andrew Laskowski)

This study reports provenance data (detrital zircon and sandstone petrography) from Cretaceous- Eocene sedimentary basins that are located along the southeast margin of the Tibetan Plateau. The authors observe that the provenance data is very consistent between basins and back up these observations with statistical methods that are mainly presented in the supplementary figures. To explain these data, the authors argue that there was likely a continent-scale river that connected the basins and drained to the Neo-Tethyan Ocean, which separated India from Eurasia prior to India-Asia collision. The authors note that this interpretation also has relevance to the debate over the low-relief, incised landscapes that are located along the southeast margin of the Tibetan Plateau. This aspect of the research will likely be the most broadly relevant and controversial aspect. Several dynamic models for Tibetan Plateau growth invoke different mechanisms for formation of the low-relief landscapes. This, coupled with the inherent interestingness of ancient, continental-scale rivers, make this research exciting and broadly relevant.

As I am an expert on detrital zircon geochronology and the tectonics of the Tibetan Plateau, I chose to focus on these aspects for my review. In my opinion, the provenance data are robust and well supported by statistical methods. I would encourage the authors to include some more discussion of their comparison between source terrane signatures and basin signatures. A key point that must be proven is that the source areas are distinguishable based on their detrital zircon age spectra whereas all the sampled basins are not, indicating mixing by a large river system. The supplementary figures were very helpful to convince me of the validity of the argument, yet they are sparsely discussed or referenced in the main text.

Reply: We sincerely thank Prof. Andrew Laskowski for his positive and constructive comments. We applied multiple statistical methods to the zircon U-Pb age distributions including probability density function plots, multidimensional scaling, DZStats, and DZMix modeling. These different methods yielded consistent results.

As requested, we first emphasize the application of multiple methods and their consistent results at the beginning of the provenance analysis section (**lines 155–158**). More importantly, we also added more comparison and discussion details for each statistical method in the revised manuscript (please see **lines 162–169**).

The authors also present a landscape evolution model that they link with their thermal modeling results. They claim that it illustrates the plausibility of the continent-scale river along the southeastern Tibetan Plateau because faster exhumation rates would be expected in the upstream reaches of the drainages. This seems plausible to me, but the authors should also emphasize that the Lhasa Terrane hosted an active, Cordilleran orogenic system at the time of deposition of the samples. It has previously been likened to the modern Andes. It should not be implied that fluvial drainage networks were solely responsible for the cooling of samples further toward the interior of the Tibetan Plateau, as the tectonic activity in this region undoubtedly affected these results.

Reply: We agree with the comment on the tectonically active Lhasa terrane during K_2–E_1 and mention this issue in the revised manuscript (**lines 186–191**). Previous studies revealed widespread uplift in the Lhasa area during the late Cretaceous to early Cenozoic (e.g., Rohrmann et al., 2012; Kapp and DeCelles, 2019), and argued that there was an externally drained river system that connected the Lhasa terrane to the Neo-Tethys Ocean (Hetzel et al., 2011; Haider et al., 2013). Thus, a combination of surface uplift due to crustal shortening and fluvial erosion likely caused more rapid cooling in the Tibetan hinterland as revealed by thermochronologic data (see Fig. 1). These observations indicate that the spatial trend in exhumation rates from central to eastern Tibet created a gently topographic slope in the eastern part of our envisaged paleo-river system.

References

Haider, V.L., Dunkl, I., Eynatten, H., Von Ding, L., Frei, D. & Zhang, L. Cretaceous to Cenozoic evolution of the northern Lhasa Terrane and the Early Paleogene development of peneplains at Nam Co, Tibetan Plateau. *J. Asian Earth Sci.* **70–71**, 79–98 (2013).

Hetzel, R., Dunkl, I., Haider, V., Strobl, M., von Eynatten, H., Ding, L. & Frei, D. Peneplain formation in southern Tibet predates the India-Asia collision and plateau uplift. *Geology* **39**, 983–986 (2011).

Kapp, P. & DeCelles, P.G. Mesozoic–Cenozoic geological evolution of the Himalayan-Tibetan orogen and working tectonic hypotheses. *American Journal of Science*. **319**, 159–254 (2019).

Rohrmann, A., Kapp, P., Carrapa, B., Reiners, P. W., Guynn, J., Ding, L. & Heizler, M. Thermochronologic evidence for plateau formation in central Tibet by 45 Ma. *Geology* **40**,187–190 (2012).

Reviewer #3

Comments to 'Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Paleogene' By Zhao et al. Based on new petro-stratigraphy, heavy-mineral analysis, and detrital zircon U-Pb studies on late Cretaceous–early Paleogene sediments from the east margin of Tibet, Zhao et al. proposed a novel continental-scale river flowing southwestward to the Neo-Tethyan Ocean along the eastern Tibet in the late Cretaceous–early Paleogene, and used this river to explain the formation of low-relief in the east margin of Tibet. The topic of this study is of broad interest for geologists, and the proposed model is significant different with previous models, which seems valuable for publication in *Nature Communications*. However, there are some important unclears in the manuscript, which should be addressed before acceptance.

(1) The authors argued that the K2–E1 sediments were not deposited in spatially separated endorheic basins based on the lack of coarse-grained sediments and thick evaporites. However, these are not strong evidence to preclude this possibility. It is very likely that these sediments were derived locally by recycling from surrounding older rocks. For example, the Nangqian and Gonjo basins in eastern Tibet show similar lithologies as the K2-E1 sediments by the

authors, but studies have shown that the two basins were sourced locally (Horton et al., 2002). The authors also suggested that the low-relief could not be severed as physiographic barriers for endorheic basins. However, endorheic basin can be formed in any landscape with local structures, e.g., normal fault.

Reply: The sedimentary facies in the Xichang, Huili, Chuxiong, and Simao basins are dominated by fluvial, lacustrine, and floodplain, lacking proximal facies (e.g., alluvial-fan) and do not resemble the basins in the Nangqian-Yushu region argued to be internally drained. Horton et al. (2002) suggested an internal drainage for basins in the Nangqian-Yushu region of east-central Tibet based on centrally directed paleocurrents, dominantly lacustrine depositional conditions, and a lack of single lithostratigraphic units that can be correlated regionally among the basins. More importantly, preserved proximal facies (i.e., alluvial-fan) were limited to basin margins, and fine-grained lacustrine deposition was mainly developed in the present-day basin interior. Such distinct lateral facies evolution, together with growth strata along the basin margins, indicates that the basins developed as distinct, isolated features with dimensions approximately similar to their present-day outcrop areas.

If the K_2–E_1 sediments in this study had formed in endorheic basins, analogous to the Nangqian basin, thick gravel or gravelly sandstone deposits or growth strata (near basin-controlling faults) would be expected; however, such sedimentary deposits were not identified by us.

From the perspective of provenance data, if the K_2–E_1 sediments were derived locally by recycling from surrounding older rocks, we would expect the widespread Neoproterozoic metamorphic rocks surrounding these basins (at the western margin of South China Block) (e.g., Li et al., 2005) to provide a major source of detrital material and a unique prominent peak at 600–900 Ma (similar to the provenance signal of the current Anning river, which drains the western margin of the South China block; see Figure 2 in Yang et al., 2020). However, our data clearly show that zircon age populations from all studied basins fall mainly into five different groups of 200–300 Ma, 390–480 Ma, 700–900 Ma, 1700–2000 Ma, and 2300–2600 Ma. Furthermore, a slow long-term exhumation of the areas around these basins from late Cretaceous to early Palaeogene is clearly documented by our compilation of thermochronological data (Fig. 1b), indicating there was no significant local erosion occurring around these basins at that time. In summary, we favor that all studied basins along eastern margin of the Tibetan Plateau were characterized by externally drained rivers from late Cretaceous to early Paleogene. We added this discussion to the revised manuscript (**lines 197–207**).

Although we agree with reviewer #3 that endorheic basins can be formed in any landscape with local structures, the combination of the axial distribution of the southwestern Sichuan, Xichang, Huili, and Chuiong basins along the foredeep depozone between eastern Tibet and South China (Fig. 1) and their provenance interpretations, fits well with the model of **Lateral tributary-dominated trans-continental river system** (e.g., Ganges, Mississippi, Paraná) proposed by Ashworth et al. (2012) (as shown the Figure 2 in Ashworth et al., 2012). *"Big rivers may extend into sedimentary basins, but they also cross them (e.g. Danube, Yangtze), and have longitudinally-extensive depositional zones"* (Ashworth et al., 2012).

The identification of ancient big rivers would follow three elements based on the suggestion from Miall. (2006), including (1) Prediction from plate-tectonic setting, (2) Analysis of the scale of depositional elements, and (3) Study of sedimentary provenance. If it is a case, a combination of long-term tectonic stability, several meters to tens of meters thick channel beds, and common provenance signals presented our study, leads us to more strongly believe the existence of a large-scale river system.

References

Ashworth, P. J. & Lewin, J. How do big rivers come to be different? *Earth Sci Rev.* **114**, 84–107 (2012).

Horton, B. K., Yin, A., Spurlin, M. S., Zhou, J. Y. & Wang, J. H. Paleocene-Eocene syncontractional sedimentation in narrow, lacustrine-dominated basins of east-central Tibet. *Geol Soc Am Bull.* **114**, 771–786 (2002).

Li, Z. X., Li, X. H., Kinny, P. D., Wang, J., Zhang, S., & Zhou, H. Geochronology of Neoproterozoic syn-rift magmatism in the Yangtze Craton, South China and correlations with other continents: evidence for a mantle superplume that broke up Rodinia. *Precambrian Res*. **122**, 85–109 (2005).

Miall, A. D. How do we identify big rivers? And how big is big? *Sediment Geol.* **186**, 39–50 (2006).

Yang, R., Suhail, H. A., Gourbet, L., Willett, S. D., Fellin, M. G., Lin, X. B., Gong, J. F., Wei, X. C., Maden, C., Jiao, R. H. & Chen, H. L. Early Pleistocene drainage pattern changes in Eastern Tibet: Constraints from provenance analysis, thermochronometry, and numerical modeling. *Earth Planet. Sci. Lett.* **531**, 115955 (2020).

(2) The biggest problem of this manuscript is the Fig. 4, which is inconsistent with the tectonic background. The authors refer the reference of Muller et al. (2016) for the late Cretaceous paleogeography reconstruction, but the map shown in Fig. 4a is inconsistent with geological evidence: The Indochina and Sibumasu, which locate southwest of the South China Block, have amalgamated to South China in Late Triassic, and the Lhasa has collided with Qiangtang in the early Cretaceous, so it is impossible that a Neo-Tethyan Ocean was still existed between South China and Indochina as shown in Fig. 4a.

Reply: We agree with this comment regarding the late Cretaceous paleogeography pattern and acknowledge our mistake. We have now corrected Fig. 4 by integrating the Indochina and Sibumasu terranes with the South China Block, and by showing the collided/amalgamated Lhasa and Qiangtang terranes north of the Neotethys ocean.

Reviewers' Comments:

Reviewer #1:
Remarks to the Author:
The authors had a positive feedback to most reviews. However, I still see the insufficient interpretation or ignored reviews that I and editor gave.

The authors give four lines of additional evidence to support the river route to the Neo-Tethys Ocean. However, unfortunately, some evidence was incorrectly used by the authors. Firstly, the marine evaporites in the Simao-Khorat Basins reach a consensus, but discharge model in the cited paper is totally misunderstood by the authors. Recent publications (in cited papers) proposed that seawater incursions were from the Meso-Tethys Ocean or proto-Paratethys Sea which came from northern part of the Simao-Khorat Basins, not from the southern Neo-Tethys Ocean. Moreover, as the authors stated that the Khorat Basin was the last stop to the Neo-Tethyan Ocean, it is absolutely impossible to form such a salt giant in the Khorat Plateau Basins in an open water mass. The Khorat Plateau Basins cover an area of ca. 247,000 km2 with evaporite thickness up to 1000 m (Hite and Japakasetr, 1979, Economic Geology). Another evidence to deny the river court is that all publications and geological observations tell us the evaporite-bearing Maha Sarakham Formation in the Khorat Basin was lacustrine environment. So when we considered the direction of marine incursion and salt giant, it would be reasonable why the Simao-Khorat are endorheic lakes.
Secondly, in Figure S8 (Supplementary Figure 8), the authors cited the data of Carter and Moss (1999, Geology) and show the age distribution of the Khorat Basin from so-called late Late Cretaceous and early Paleogene. However, I checked the paper of Carter and Moss (1999) and found the youngest strata in their study is the Early Cretaceous Khok Khruat Formation. There was totally no any data of the late Cretaceous to early Paleocene.
Based on these two evidence, the river court don’t convince me.

(1) Age constraint
The age and depositional environment cannot convince me in the revised manuscript and that is what I am concerned about. Why the age is so important and I repeatedly stress it? K2-E1 is a large time range from 100 Ma to 56 Ma and you mentioned that it was a long-term tectonic stability. In this region, at least two tectonic events identified by many geologists: 1, the mid-Cretaceous (ca. 100 Ma) tectonic event (e.g., Lovatt-Smith et al., 1996) caused by collision between Qiangtang and Lhasa; 2, the India-Asia collision at ca. 65 Ma. The first event caused unconformity in the studied basins. It’s hard to imagine the existence of such a long-term tectonic stable environment.

Of course, it’s hard to do the geochronology work in thick red bed basins. But in my opinion, the charophyta and ostracod fossils would give wrong age constraints compared with the U-Pb ages. This is true in the Jianchuan Basin and Simao Basin (See Gourbet et al., 2017; Yan et al., 2021). So age issue is the first priority, or it would be another story.

About the MDA, the reason that you got so few youngest zircons is inadequate zircon tests. Most samples for the U-Pb analysis in the study is n=100/150. The ‘large-n’ datasets (e.g.,> > 300 analyses per sample; Pullen et al., 2014; Daniels et al., 2018; Sundell et al., 2019a, 2019b) in order to increase the probability of analyzing young grains should be used.

(2) Depositional environment
The author’s reply is actually not convinced me. We all know that the meandering river system typically consists of lag and sand bar deposits in the bottom and flood plain deposits showing a upward finning sequence. However, from your supplementary field photo Figure R6a and stratigraphic column Supplementary Figure 1, I cannot tell these features in the Xichang Basin. Moreover, I don’t think a meandering environment that consist of a thickness of about 4,000 m of sandstone and claystones (Supplementary Figure 1) in the Xichang Basin.

For the Chuxiong Basin, I stated that "thick evaporite are occurred in the Jiangdihe Formation ". Yes, thin layers or nodules or crystals of evaporite can be formed in the arid fluvial environment. Of course, we cannot see thick evaporites in the field in the Chuxiong Basin, even in the Simao Basin and Khorat Basin since that the evaporite is easily dissolved under the tropical monsoon climate in these basins. Previous publication showed that several medium-scale salt mine of the Jiangdihe Formation were found (ref. 77). I am very curious about what the authors stated in the Supplementary Note line 211-2113 "Thus, the presence of thin evaporites is in contradiction with the existence of a low-gradient and continental-scale river system". So, what is exactly the authors' opinion?

The authors acknowledged that all stratigraphic and sedimentary work are from previous publication (in supplementary note). For the section in the Xichang Basin, the stratigraphic column (Supplementary Figure 1) is totally copied from Deng et al. (2018). Deng et al. (2018) state that "the basal member of the Xiaoba formation having 1400 m in thick changes from a fluvial environment at the base to a shallow-lacustrine facies at the top". "The second member of the Xiaoba Formation is composed of ~100 m of lacustrine red sandstone, calcareous siltstone, calcareous mudstone, and limestone (mainly at the top of the section), interbedded with gray-purple silt¬stone and calcareous lenses". The third member is over 700 m thick and is primarily composed of calcareous siltstone and calcareous mudstone, interbedded with gray-purple quartz siltstone and gypsum layers, and represents a fluvial and shallow-lacustrine facies. The Paleogene Lei¬dashu Formation is over 1300 m thick changes from a fluvial environment at the base to a shallow-lacustrine facies at the top. Therefore, the authors did not do sedimentary work, but they denied the lacustrine interpretation of Deng et al. (2018) even if they copied Deng's section.

Similarly, the authors copy the stratigraphic column of Deng's section in the Chuxiong Basin. However, they wrongly place the basal conglomerates of the Matoushan Formation in Deng's paper as the Jiangdihe Formation in Supplementary Figure 1. Deng et al. (2018) stated that "The Jiangdihe Formation represents a fluvial and shallow-lacustrine facies and The Zhaojiadian Formation fines upwards and repre¬sents a shallow-to-marginal lacustrine facies". However, the authors proposed fluvial+lake and floodplain+fluvial environment for the Jiangidhe and Zhaojiadian formation, respectively, without any sedimentary work.

Therefore, I easily found that the authors just simply copied two sections in the Xichang and Chuxiong basins of Deng et al. (2018). Based on my personal opinion and authors' cited publication (ref 16, Deng et al., 2018), I do prefer a fluvial and lacustrine environment during that time. It also indicates that the sedimentary environment fluctuate between fluvial and lacustrine and is not stable in such a long time range as the authors' claimed.

As for the Simao and Khorat basins, no matter the evaporite is marine or non-marine, it is impossible to form such a salt giant in an open environment (Please see Warren, 2016, Evaporites-A Geological Compendium). The authors claimed in the response letter line 417 to 422 that marine incursion evaporites cannot be used as an argument against a fluvial system that drained into the Neo-Tethyan Ocean. I guess the authors argued the seawater may be intruded from the Neo-Tethyan Ocean (from south to north) and thus claimed like that. However, marine incursions were not from the Neo-Tethyan Ocean, but rather from the Meso-Tethys Ocean or proto-Paratethys Sea as the cited publications (in Response letter line14 to 15) argued. Therefore, the authors used the marine incursion evaporite to support the existence of fluvial system is unreasonable and untenable. Moreover, the lithological and sedimentary features is indicative of a typical saline lake by many publications (Hite and Japakasetr, 1979; El Tabakh et al., 1999; Zhang et al., 2013; Liu et al., 2018; Qin et al., 2020; Wang et al., 2021).

As such, the Xichang, Chuxiong, Simao, and Khorat Basins during the so called late Cretaceous to Early Paleogene is mostly lacustrine. And Simao and Khorat saline lakes were not exorheic.

(3) Provenance analysis

I suggest you put the newly added data of Simao and Khorat in the MDS plot in Supplementary Figure 6. And it is obvious that the Indochina plot so far away from your samples, indicating there was no provenance connection between them. So why do the authors' believe the fluvial system can flowed to the Indochina?

(4) Landscape and large river system
Such a long and large river system existed in the eastern Tibetan Plateau maybe need a higher elevation and large erosion, and thus will results in enough clastic materials to transport into the Neo-Tethys Ocean. Together with the authors' claimed that there were no proximal sources, I have three concerns. (1) How can a low-relief landscape with slow exhumation and erosion (slow cooling) can supply so many clastic materials to the Neo-Tethys Ocean in such a long distance (at least 3000 km)? (2) If the river do flow into the Neo-Tethys Ocean, do the authors have any evidence of sedimentary sequence in delta and offshore Thailand?

(5) Sections and samples
Authors provide the GPS of all samples, however I found sample distance in the Dujiangyan section is nearly up to 30 km from Google Earth. That is absolutely not the sampling action in the same section because the Dujiangyan section only have ca. 800m in thickness. The same as the Leshan section, two sample distance is up to 21 km. So it is absolutely questionable of the sampling.
The authors acknowledged that all stratigraphic and sedimentary work in the Xichang and Chuxiong basins are from previous publication. And as I mentioned above, the stratigraphic columns in these two basins are totally copied from Deng et al. (2018).
So I doubt that the authors' sampling is consistent with the section description.
Therefore, from the authors' sampling and stratigraphic work, I have to trace back to the age of the section again. All age constraints from this study is the fossils which is done about 40 years ago. Previous fossil results were from regional mapping at scale of 1:200,000 or type section. The studied sections are obviously not the type section, so previous results should not be applied directly. How can the regional fossils be used in whole basin?

(6) Standard zircons
I commented the dating results of the Standard zircons, but I cannot see correct or appropriate response. Yes, as the authors replied, the detrital zircon dating is a usual way. But for each test, your 91500 and GJ-1 zircons should generate their ages. What I asked is you should provide all the ages of 91500 and GJ-1 during your experiment, so that I can determine your deviation between the test values and recommended ages. And then I will have an idea about the reliability of all ages of the samples. So you cannot use the dating results in Yuan et al.(2004) to tell me that the ages in your study is reliable.

Reviewer #2:
Remarks to the Author:
Greetings,

I reviewed the revision submitted by Huiping Zhang and co-authors. All of my comments were sufficiently addressed. I support publication of this manuscript.

Andrew Laskowski

Reviewer #3:
Remarks to the Author:
Dear Editor and Authors,

Thanks for the detailed response to my previous comments. After reading the manuscript, I thought there are still a few problems that need to be addressed before publication.

First, the authors presnet a few pictures to prove a continental scale river through the basins in SE Tibet, but I suscept this. As shown in the pictures, the lanminted and fine grained sandstones and mudstones are better to be interpreated in lacustrain environment, as suggested by the Reviewer 1. If the sediments were transported by a large river, we should observe some cross beddings, which are scarce in these basins.

I am also disagree with the statement that the Late Cretaceous-Paleogeen sediments in Simao Basin is still marine facies. The sediments have been terrestrial since late Jurassic.

Second, I am not expert to modeling, so cannot judge the relibility of the modeling results to support a low relief at sea level before Eocene. But I am curious even there is a continental scale river flow to the Neo-Tethys Ocean, why must be a low relief existed?

Third, in recent studies, Clift et al. (2020) and Zheng et al. (2021) shown that, the Gonjo, Jianchuan, Yuanjiang, and the Northern Vietnam basins have similarprovenance in the early Cenozoic, which supporting a southward-flowing river from eastern Tibet to the South China Sea. This is contrast with the figure as suggested in this manuscript. I am wandering how the authors reconcile this inconsistence.

Reviewer #4:

Review of "*Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Palaeogene*"

I am assessing this paper on the basis of its integrated regional data, its innovative interpretations, and the potential impact of those interpretations on our understanding of some large-scale geomorphic anomalies along the eastern and SE margin of the Tibetan Plateau. This approach likely stands in contrast to some other reviewers who are more familiar with details of the local geologic-stratigraphic-petrographic-chronologic data sets that this submission exploits and builds upon. The key scientific question that this paper addresses is the origin of the widespread, high-altitude, low-relief surfaces that characterize much of the SE Tibetan Plateau: a region where local relief at present is typically quite high, with deeply incised river gorges, and rock-uplift rates are rapid in a global context. The presence of these long-lived, low-relief surfaces at rather high altitudes has long piqued our curiosity: why are they there; how did they form; what are modern analogues of their formative sequence; what data can be used to test various hypotheses?

The authors argue that, despite some coeval tectonic uplift, a long-lived, ~north-to-south river system in eastern Tibet during Late Cretaceous to Paleogene times created abundant, low-relief surfaces during a time of relative stability (or in the face of ongoing, but slow rock uplift) prior to the Indo-Aisan collision and the main Himalayan orogeny. The authors support this scenario by comparing different data sets from these terranes: contrasts in cooling histories from the proposed river corridor (versus the bounding terranes); contrasts in detrital mineral compositional abundances and U/Pb zircon cooling ages within the "drainage corridor" versus outside of it; mixing models that optimize inputs from diverse source areas in order to "match" the observed age abundances; etc.

The cooling histories of the compiled thermochronological records (Figure 1b) make a rather persuasive case that slow Late Cretaceous to Early Tertiary cooling in Songpan-Garzi and Yidun terranes contrasts markedly with the regions of significantly more rapid Late Cretaceous-Early Tertiary cooling to the east and west of these terranes. Hence, while considerable rock uplift, erosion, and bedrock cooling was going on to the east and west. during Late Cretaceous to Paleogene times, this north-south corridor in eastern Tibet appears quite stable. To support their interpretation of an integrated fluvial system draining southward to the Neotethyan ocean, the authors combine paleocurrent analysis with detrital mineralogy and detrital zircon U/Pb cooling ages to show a noteworthy consistency among dated sampling sites spanning ~600 km from north to south along the proposed fluvial corridor. For me, the match between (i) the detrital zircon ages from the Songpan-Ganzi and Yidun terranes (proposed source areas in the north) with (ii) the suite of consistent detrital zircon ages from depositional basins spanning 600-750 km from north to south provides critical support to their hypothesized drainage basin geometry and the proposed timing of its existence as an integrated depositional system. To me, this spatial-temporal consistency is a key factor supporting the interpretation offered by these authors.

I suspect that, for some readers, examination of the extensive supplementary data will be needed to convince them of the validity of the authors' hypotheses. I find both (1) their multi-dimensional scaling plots of detrital data sets and potential source areas and (2) their modeled relative contributions from potential source areas quite persuasive.

I note that previous reviews brought up many specific issues and questions, commonly related to the characteristics of a given source area and an alternative interpretation. I am not qualified to judge the merits (or validity) of these objections. But, I did find that this contribution's authors gave quite convincing justifications for their choices and interpretations.

Overall, this provocative, innovative synthesis and interpretation provides a potential resolution to a long-standing problems related to how there low-relief, high-elevation surfaces in SE and Eastern Tibet developed. I believe it is worthwhile to get this data set and interpretation "out there" for the interested audience to contemplate and to try to test with new data or re-analysis. I also think that the "problem" that this paper addresses is a long-standing and puzzling one: a problem that has come into clearer focus in recent decades as (i) high-resolution digital topography has become available of even remote or restricted areas (thereby enabling clear topographic syntheses, comparisons across regions, and identification of "anomalies") and (ii) as high-resolution, low-cost, and high-throughput analytical techniques have enable thousands of analyses to be made and synthesized, and (iii) as improved and diversified numerical modeling approaches have enabled more rigorous evaluation of hypotheses. This contribution from the edge of Tibet exploits all of these technologies in a creative synthesis that is sure to inspire (and provoke) further research focused on the evolution of large-scale dynamic orogens.

Note to editors/authors: I show my "linguistic/grammatical/clarification" suggestions below in red text.
58 Note that the Yangtze River is not identified/labelled in any figure that I could find!
99 "comprise" means "to be composed of" So eastern Tibet comprises these provinces, not the other way around.
119 with sustained ~~topography~~ topographic relief
120 **SUCH r**egional differences in erosion/exhumation rates could be explained by a
127 along the foredeep depozone (i.e., SW Sichuan, Xichang, Huili, and Chuxiong basin**s: Fig. 1a**)
139-41 ~~Several meters to tens of meters~~ "Thick, cross-stratified sandstone beds ~~with cross-stratification~~ represent channel deposits of southward-flowing, low-**energy** ("energy" or "gradient"?) rivers ~~and associated floodplains, and/or exorheic lakes~~ I don't think that crevasse splays or lake deposits should be cited as indicative of overall paleocurrent directions for large river systems, especially for thick sandstone deposits. What indicates that these rivers are "low-energy"? Are there complete channel cross sections and longitudinal sections to enable you to deduce "energy" versus gradient or simply associated grain size?
148 "Leshan section" in the Sichuan Basin, (given that the Leshan section is not identified in the figure.
164 "genuine"?? Does this mean "statistically significant"?
173-5 "The consistent provenance signal from the different basins **requires** the existence of a continuous fluvial system during the late Cretaceous–early Palaeogene." Does it truly "require"? That may well be the most likely scenario, but it doesn't "require" this scenario, in my opinion.

367 showing main tectonic units "**(red text)**" (in reference to what represents these units in the figure) and major river systems (except for the Yangtze! **Add a label for this river!**)

370 low-relief plateau areas24,31–34,36,58. New suggested text: **Hexagons indicate sites with Cretaceous-Tertiary cooling histories (shown in 1B)** Dashed

Comments on Figures

Figure 1a. Nowhere is the Yangtse River labeled on this figure. Its name should be clearly identified. Note that in the figure caption, no description is given of the light blue vertical band from 40-50 Ma in Fig 1b. What is that? I presume it's a proposed "boundary" between rapid L Cret-Paleocene cooling versus post-50-Ma rapid cooling. Why not add blue or red labels for rapid cooling pre-50 Ma and post-40 Ma, respectively? I also note that the chosen level of transparency of some of the "yellow" low-relief surfaces makes these surfaces appear to be a different color (more orange than yellow, where superimposed above the orange swath), and that there is a strange (inconsistent?) mix of yellow and orange surfaces just above the red "5" in the figure. These issues should all be readily corrected. Could a label be added to the orange region so that it's more self-explanatory? Similarly, a label on the blue-dashed line("hypothesized drainage divide") would make its significance more obvious.

Figure 1b. Add a legend indicating blue lines for rapid cooling prior to 50 Ma, versus red lines for rapid cooling since 50 Ma.

Figure 2. How about helping your readers along with a title box, "Drainage Scenarios" and cryptic summary titles for each scenario's panel?

Figure 3. The rationale is unstated for the red and blue lines for the probability density functions. Please make that clear!

Comments on Supplemental Figures

Supp Figure 3. Illustrative and quite compelling figure!! Spell out the names of the sections (CX, HL, etc) for each group of samples.

Response letter

In this letter we will provide our detailed response (in blue text) to the comments of the editor and the four reviewers (in black) and explain all changes performed on the manuscript.

Response to the comments of the four reviewers

Reviewer #1

The authors had a positive feedback to most reviews. However, I still see the insufficient interpretation or ignored reviews that I and editor gave.

(1) The authors give four lines of additional evidence to support the river route to the Neo-Tethys Ocean. However, unfortunately, some evidence was incorrectly used by the authors. Firstly, the marine evaporites in the Simao-Khorat Basins reach a consensus, but discharge model in the cited paper is totally misunderstood by the authors. Recent publications (in cited papers) proposed that seawater incursions were from the Meso-Tethys Ocean or proto-Paratethys Sea which came from northern part of the Simao-Khorat Basins, not from the southern Neo-Tethys Ocean.

Reply: As the reviewer#1 points out correctly, the marine origin of Late Cretaceous–early Palaeogene evaporite in the Simao-Khorat Basins has reached a consensus. This clearly indicates that, as we have emphasized in the main text, these areas were close to sea level at that time. Although Wang et al., (2021) has inferred that the seawater likely originated from the proto-Paratethys Sea to the northwest, these authors also proposed an alternative hypothesis that marine incursions were from the Neo-Tethyan Ocean to the southwest (Wang et al., 2015). In addition, the proto-Paratethys Sea-derived model is subject to debate, because it cannot explain the present perception that the Cretaceous marine strata were disrupted between the Qiangtang and the Simao-Khorat Basins (Qin et al., 2020). About the model of "Meso-Tethys Ocean-derived" proposed by Qin et al. (2020), we infer this just reflects ambiguity about the use of this name for "Tethys Ocean" because it is generally believed that the Meso-Tethys Ocean had closed before the late Cretaceous (see Kapp et al., 2019, Li et al., 2019 for reviews; as shown the Figure 38 in Metcalfe, 2021). Overall, we originally showed the existence of marine evaporates in the Simao-Khorat Basins through the two studies (Qin et al., 2020; Wang et al., 2021).

About the source of paleo-seawater, Qu (1997) proposed that paleo-seawater recharged from the south to the north based on the similar stratigraphic ages, sedimentary features, mineral sequences, and solute sources of the evaporites between the Simao and Khorat Basin. Moreover, the thickness of the late Cretaceous evaporate formation in the Khorat Basin is apparently thicker than that in the Simao basin (e.g., Zhang et al., 2018, Yan et al., 2021a, Yan et al., 2021b). These observations, coupled with the reconstructed block-tectonic pattern during the late Cretaceous–early Paleogene (e.g., Boucot et al., 2009; Poblete et al., 2021; Metcalfe, 2021), it is proposed that that the seawater originated from the Neo-Tethys Ocean to the south or southwest during sea intrusion episodes (Qu, 1997; El Tabakh et al., 1999; Wang et al., 2015; Rattana et al., 2021). We have clarified and improved our discussion of this issue in the revised manuscript (see lines 233–243).

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(2) Moreover, as the authors stated that the Khorat Basin was the last stop to the Neo-Tethyan Ocean, it is absolutely impossible to form such a salt giant in the Khorat Plateau Basins in an open water mass. The Khorat Plateau Basins cover an area of ca. 247,000 km^2 with evaporite thickness up to 1000 m (Hite and Japakasetr, 1979, Economic Geology). Another evidence to deny the river court is that all publications and geological observations tell us the evaporite-bearing Maha Sarakham Formation in the Khorat Basin was lacustrine environment. So when we considered the direction of marine incursion and salt giant, it would be reasonable why the Simao-Khorat are endorheic lakes.

Reply: We respectfully disagree with the referee on this as they appear to have misinterpreted Hite and Japakasetr (1979). Hite and Japakasetr, (1979) definitely stated that "*The potash deposits of the Khorat Plateau are in the Maha Sarakham Formation of Cretaceous age. This formation is present only on the Khorat Plateau. North, in the Sakon Nakhon Basin, the formation extends over an area of about 21,000 km^2. South, in the Khorat Basin, the formation covers a slightly larger area of about 36,000 km^2. The maximum thickness of **the formation** in either basin is unknown, but it could exceed 1,000 m*". This sentence just shows that the thickness of the Maha Sarakham Formation is up to 1000 m, and does not mention the thickness of evaporate deposits. In fact, the evaporite thickness from the late Cretaceous–early Paleogene Maha Sarakham Formation in most areas of the Khorat Plateau is only tens to hundred of meters according to a collection of bore documents (see Hite and Japakasetr, 1979; Rattana et al., 2021, in press).

In terms of the formation mechanism of evaporites in the Khorat Plateau Basins, in addition to hydrological conditions, the coupling of regional aridity and global high-temperature events, along with eustatic change would have further promoted the formation of potash salts (Liu et al., 2018; Yan et al., 2021). According to previous studies, if this sea is located in a low-latitude setting and especially if it straddles the belt of high-pressure dry air, evaporation can result in the formation of a major evaporite deposits in continental margin/shelf settigs (e.g., narrow seas, tidal flats, lagoons) (Reynolds and Johnson, 2019; Miall, 2016; as shown the figure below in p. 240 of Reynolds and Johnson, 2019). Thus, it is likely that major salt-bearing sub-basins / depressions in the Khorat Plateau represent several lagoons or tidal flats during low sea level episodes, and the large-scale river system may flow to the sea near these tidal flats or lagoons (resembling landscape referred to the figure in p. 76-77 of Reynolds and Johnson, 2019; modified Fig. 4b).

We have never denied the occurrence of lacustrine environments. Rather, several lines of evidence below clearly corroborate a co-existence of fluvial and/or delta-plain environments in the Khorat basins during the late Cretaceous–early Paleogene: (i) we note that “*The non-marine red beds interbedded with the evaporites are **fluvial or alluvial deposits** and include displacive anhydrite nodules and beds and displacive halite in cubic forms*”; and “*Siliciclastics from the Maha Sarakham Formation in the Khorat Basin are composed of alternating cross-bedded siltstones and massive mudstones of **fluvial origin** suggesting fluvial deposition in the basin province*” cited from El Tabakh et al., (1999, p. 54, 59). The two sentences from El Tabakh et al., (1999) support the existence of a fluvial depositional environment for upper Cretaceous–lower Paleogene strata in the Khorat basins, even though lacustrine environments may be more extensive. Similarly, the late Cretaceous Maha Sarakham Formation in northeastern Thailand consists of red to reddish-brown, fine-grained, laminated and small-scale cross-bedded sandstones interbedded with siltstones and mudstones with disseminated salts and gypsum (Meesook, 2000), which was interpreted as a meandering river system, even if evaporitic conditions also prevailed (Meesook, 2000). (ii) Sedimentary structures in mudstones such as mudcracks, chaotic textures, root structures and burrows suggest a subaerial setting (El Tabakh et al., 1999). (iii) A widespread and dominantly south-directed paleocurrent from the alternating thin mudstone and sandstone beds of the late Cretaceous units in the Khorat basins provides additional physical evidence corroborating the existence of a south-flowing sediment routing system (Heggemann et al., 1992; Singsoupho et al., 2015). (iv) Isotope measurement results of potash deposits suggest that a terrestrial input or riverine water had entered into the Khorat Plateau when the potash deposits were precipitating (El Tabakh et al., 1999; Zhang et al., 2013), which likely has a close relationship with the influence of fluvial influx (Zhang et al., 2013). Of course, our interpretation of a hydrologic connection between the Khorat Basin and the Neo-Tethyan Ocean does not preclude a more complex drainage configuration, involving the assembly of multiple channels along a near south-to-north major route. More detailed provenance analyses of Upper Cretaceous samples from the Khorat basin may further test this hypothesis to constrain a more complete drainage configuration of the proposed large river. We have included part of this evidence in the revised manuscript (lines 228–233).

From the perspective of provenance data, the late Cretaceous–early Paleogene deposits in the Simao Basin and Muang Xai Basin to the south have widely been proposed to be mainly from the Songpan-Ganzi Yidun, and Qiangtang terranes, rather than proximal source areas (e.g., Indosinian terrane) (Wang et al., 2014; Wang et al., 2017; Chen et al., 2017; Yan et al., 2021). In other words, if the Simao and Muang Xai Basins were endorheic basins during the late Cretaceous–early Paleogene as suggested by reviewer #1, K_2–E_1 sediments in the two basins must be derived from surrounding older rocks, and we would expect the widespread Triassic metamorphic rocks surrounding these basins to provide a major source of detrital material and a unique prominent peak at 200–250 Ma (Fig. S4). However, previously published detrital zircon data from late Cretaceous–early Paleogene samples of the two basins show strikingly consistent peaks at 200–300 Ma, 390–480 Ma, 700–900 Ma, 1700–2000 Ma, and 2300–2600 Ma (Wang et al., 2014; Wang et al., 2017; Chen et al., 2017; Yan et al., 2021; Fig. S8). This strongly suggests the Simao and Muang Xai Basins were not endorheic basins in the late Cretaceous–early Paleogene, but were connected to a transcontinental drainage system that linked central–eastern Tibet with the two basins (Wang et al., 2014; Wang et al., 2017; Chen et al., 2017; Yan et al., 2021). Although no detrital zircon data from K_2–E_1 sediments in the Khorat basin have been reported to date, a dominantly south-directed paleocurrent from the sandstone beds in the late Cretaceous Maha Sarakham Formation in the Khorat Basin provides additional physical evidence for a south-flowing sediment transport pathway (Heggemann et al., 1992; Singsoupho et al., 2015).

Based on the collective weight of stratigraphic and sedimentologic observations described above, we contend that the deposits in the Khorat Basin do not preclude our hypothesis that a continental-scale river system discharged into the the Neo-Tethyan Ocean near what is now the Khorat Basin (see Figure 4), even though the major paleo-channels cannot be traced due to post-depositional modification and later erosion.

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(3) Secondly, in Figure S8 (Supplementary Figure 8), the authors cited the data of Carter and Moss (1999, Geology) and show the age distribution of the Khorat Basin from so-called late Late Cretaceous and early Paleogene. However, I checked the paper of Carter and Moss (1999) and found the youngest strata in their study is the Early Cretaceous Khok Khruat Formation. There was totally no any data of the late Cretaceous to early Paleocene. Based on these two evidence, the river court don’t convince me.

Reply: We thank the reviewer for catching this oversight regarding the age of the Khorat Group (Carter and Moss, 1999). We have removed this from our compilation of detrital zircon in a modified Fig. S8. Fortunately, this does not impact our interpretations.

(4) The age and depositional environment cannot convince me in the revised manuscript and that is what I am concerned about. Why the age is so important and I repeatedly stress it? K2-E1 is a large time range from 100 Ma to 56 Ma and you mentioned that it was a long-term tectonic stability. In this region, at least two tectonic events identified by many geologists: 1, the mid-Cretaceous (ca. 100 Ma) tectonic event (e.g., Lovatt-Smith et al., 1996) caused by collision between Qiangtang and Lhasa; 2, the India-Asia collision at ca. 65 Ma. The first event caused unconformity in the studied basins. It’s hard to imagine the existence of such a long-term tectonic stable environment.

Reply: We must emphasize that it has been widely appreciated that the final collision between the Qiangtang and Lhasa terranes occurred in the Early Cretaceous (~130–120 Ma) (e.g., Kapp et al., 2005; Li et al., 2019; also see Figure 7(G), 7 (H) in Kapp et al., 2019). Moreover, Cretaceous shortening, basin deformation, and magmatism activities mainly occurred in the central Tibetan plateau due to northward underthrusting of the Lhasa terrane beneath the Qiangtang terrane along the Bangong suture during low-angle subduction of Neo-Tethys oceanic lithosphere (e.g., Kapp et al., 2005, 2019; Rohrmann et al., 2012; Li et al., 2019). This region is well to the west of our study area. With respect to eastern Tibet, there is no clear evidence for an obvious erosion surface between the early and late Cretaceous horizons in the studied basins (~100 Ma), and contacts are likely conformable. More importantly, as reviewer #1 acknowledged, we proposed a prolonged tectonic stability in eastern Tibet from late Cretaceous to early Paleogene (＜100 Ma–50 Ma). Thus, even if there was a transient tectonic event in the mid-Cretaceous (＞100 Ma), this would be fully compatible with our interpretation and conclusions.

Although discussion still persists, a growing number studies have consistently agreed that the initial timing of the India-Asia collision (or collision of India with an intra-Tethyan island arc) is between ~60 Ma and 50 Ma (e.g., Tapponnier et al., 2001; Najman et al., 2010; DeCelles et al., 2014; Martin et al., 2020; An et al., 2021; Yuan et al., 2021). But please note that the convergence rate began to rapidly decrease after the onset of the India-Asia collision (Copley et al., 2010; van Hinsbergen et al., 2011), thus the main crustal thickening of a large part of Tibet occurred later (e.g., Tapponnier et al., 2001; Cao et al., 2020). For example, the strong south-north compression in the central TP and the continuous north-ward subduction of the Indian lithosphere mainly occurred between 50 and 34 Ma (Kapp et al, 2019; Lin et al., 2020; Wang et al., 2008; Wang et al., 2014). Back to our study, the large-scale tectonic deformation of eastern Tibet occurred during the post-collision of India with Asia. Based on low-temperature thermochronology (e.g., Zhang et al., 2016; Wang et al., 2012), paleoaltimetry (e.g., Hoke et al., 2014; Xiong et al., 2020), and recent paleomagnetism studies (Tong et al., 2017; Zhang et al., 2020), significant tectonic deformation and uplift on the eastern margin of the Tibetan plateau and the western margin of the South China block have widely been acknowledged to occur later than the middle Eocene. In addition, the widespread magmatic activities at ~40–30 Ma in eastern Tibet also suggest a diachronous uplift history for the Tibetan plateau (Chung et al., 1998). Thus, the initial India-Asia collision at ca. 60–50 Ma did not immediately affect the region of eastern Tibet studied here.

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(5) Of course, it's hard to do the geochronology work in thick red bed basins. But in my opinion, the charophyta and ostracod fossils would give wrong age constraints compared with the U-Pb ages. This is true in the Jianchuan Basin and Simao Basin (See Gourbet et al., 2017; Yan et al., 2021). So age issue is the first priority, or it would be another story.

Reply: The Jianchuan Basin is a middle-late Eocene intracontinental basin in eastern Tibet, whose basin property, depositional age, lithologic and facies features, and subsidence mechanism are totally different from those of the basins we study (Gourbet et al., 2017; Zheng et al., 2020; Feng et al., 2021). More significantly, please note that a dispute regarding age scheme of the Cenozoic sedimentary sequences in the Jianchuan Basin stems from absolute ages of the Shuanghe, Jianchuan, and Jiuziyan Formations. Previously estimated ages of the three formations were mainly based on plant fossils, bivalves, gastropods, and nannofossils, rather than charophyte, or ostracod. Instead, ages of the underlying Paleocene Yunlong and Eocene Baoxiangsi Formations in the Jianchuan Basin were initially estimated by characteristic charophyte, and ostracod assemblages, which is well in agreement with recent magnetostratigraphic and radiochronologic dating results (Fang et al., 2021). Thus, taking an example of the Jianchuan Basin to question the age scheme presented this study is untenable.

The Mengyejing Formation in the Simao Basin has been dated at ~112–63 Ma using magnetostratigraphic method (**Yan et al., 2021**), largely in agreement with the previously reported palaeontological results (i.e., Ostracod, Charophyte fossils, and Sporopollen) (as shown the Figure 10 of Yan et al., 2021). Similarly, the Guankou and Mingshan Formations of the Shiyang section in the southwestern Sichuan Basin has been dated at ~84–43 Ma using the magnetostratigraphic method (Shen et al., 2018, Master thesis), which is also largely consistent with the biostratigraphic age of these deposits. Taken together, we argue that the charophyta and ostracod assemblages provide reasonable information on the depositional age of our studied sedimentary sections, especially given that most geochronology methods are impracticable for dating these deposits, as acknowledged by reviewer #1.

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(6) About the MDA, the reason that you got so few youngest zircons is inadequate zircon tests. Most samples for the U-Pb analysis in the study is n=100/150. The ‘large-n’ datasets (e.g., > > 300 analyses per sample; Pullen et al., 2014; Daniels et al., 2018; Sundell et al., 2019a, 2019b) in order to increase the probability of analyzing young grains should be used.

Reply: As we mentioned in our Response letter from the first round of reviews, younger (i.e. late Cretaceous–early Paleogene) zircons are completely lacking in our samples from the Sichuan, Xichang, and Huili Basins. Thus, the maximum depositional age (MDA) is insufficient to constrain depositional ages with certainty for these basins. Although the youngest single-grain ages in samples from the Chuxiong Basin are too few to be a robust indicator of the true depositional age, they are consistent with the late Cretaceous–early Cenozoic biostratigraphic age of these deposits. Detrital zircon “large-n” (n ≈ 1000) results from samples are generally used to assess dissimilarities amongst their age spectra during provenance analysis (Ibañez-Mejia et al., 2018), but samples from our study consistently show similarities (five age peaks, see Figs. 3 and S4). Moreover, we note that only one young zircon grain was observed from individual samples in the Chuxiong Basin. Thus, even if “large-n” is applied, the MDA calculated from several young zircons cannot reflect the exact age of each sampled horizon because of the unknown lag time between erosion and sedimentation (especially given that Cretaceous zircons likely experienced long-distance transport from the Lhasa terrane). Given this, we contend that the number of samples in our study area, in fact, sufficient to support the conclusions

References:

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(7) The author’s reply is actually not convinced me. We all know that the meandering river system typically consists of lag and sand bar deposits in the bottom and flood plain deposits showing an upward finning sequence. However, from your supplementary field photo Figure R6a and stratigraphic column Supplementary Figure 1, I cannot tell these features in the Xichang Basin. Moreover, I don’t think a meandering environment that consist of a thickness of about 4,000 m of sandstone and claystones (Supplementary Figure 1) in the Xichang Basin. The authors acknowledged that all stratigraphic and sedimentary work are from previous publication (in supplementary note). For the section in the Xichang Basin, the stratigraphic column (Supplementary Figure 1) is totally copied from Deng et al. (2018). Deng et al. (2018) state that “the basal member of the Xiaoba formation having 1400 m in thick changes from a fluvial environment at the base to a shallow-lacustrine facies at the top”. “The second member of the Xiaoba Formation is composed of ~100 m of lacustrine red sandstone, calcareous siltstone, calcareous mudstone, and limestone (mainly at the top of the section), interbedded with gray purple siltstone and calcareous lenses”. The third member is over 700 m thick and is primarily composed of calcareous siltstone and calcareous mudstone, interbedded with gray-purple quartz siltstone and gypsum layers, and represents a fluvial and

shallow-lacustrine facies. The Paleogene Leidashu Formation is over 1300 m thick changes from a fluvial environment at the base to a shallow-lacustrine facies at the top. Therefore, the authors did not do sedimentary work, but they denied the lacustrine interpretation of Deng et al. (2018) even if they copied Deng's section.

Reply: As acknowledged by the reviewer #1, fluvial environments do exist in the late Cretaceous Xiaoba Formation and the early Paleogene Leidashu Formation, even if lacustrine facies is also common. Meanwhile, we aim to emphasize the occurrence of fluvial environment in these deposits, thus lacustrine/lake interpretation was clearly not expressed in the earlier Supplementary Note. This has been clarified in the revised manuscript, and please noted that we have never denied the lacustrine interpretation of Deng et al. (2018), the existence of (exorheic) lake environment was mentioned in Supplementary Figure 1 and main text of the earlier manuscript.

More importantly, the sedimentary facies results in Deng et al., (2018) were also taken from previous province-scale or regional-scale geology reports, and they only provided a typical section (i.e., Mishi section) in the Xichang Basin. In the submitted Supplementary Note, we collected more sedimentological descriptions and depositional environment interpretations from more detailed geological mapping surveys of scale 1:50000 (Sichuan Bureau of Geology and Mineral Resources, 1990), and more recent geological mapping surveys of scale 1:250000 (Sichuan Geological Survey Institute, 2013). This literature clearly indicates that the late Cretaceous Xiaoba Formation and early Paleogene Leidashu Formation are dominated by alternating fluvial and lacustrine environments, based on detailed stratigraphic and sedimentological analysis (see revised Supplementary Note for details). Typical fluvial-lacustrine sequences of the Xiaoba Formation and Leidashu Formation in the Xichang Basin were presented in geological mapping survey report of scale 1:250000 (see Figures 1-56 and 1-57 in the Sichuan Geological Survey Institute, 2013). Overall, the fluvial-lacustrine facies association in the basin interpreted from the literature most likely records a hydrologically open lake or exorheic lake according to Carroll and Bohacs, (1999) and Bohacs et al., (2000).

The identification of ancient large-scale drainage systems should be fully related to the understanding of the depositional environments associated with modern large rivers. According to a review for the world's continental-scale rivers by Ashworth et al. (2012), the different combinations of sedimentation types lead to floodplain morphologies for big rivers that can be classified into four main types, and Type 1 is a lacustrine-dominated river system (as shown Figure 8A in Ashworth et al., 2012). The present-day largest anastomosing or meandering rivers in the world often develop lakes between trunk streams and their major tributaries (Maill et al., 1996; Ashworth et al., 2012). In other words, lake environments are a common component of large river systems (Ashworth et al., 2012), especially in extensive and low-gradient catchment areas, as in the Dongting Lake of the middle Yangtze (Chen et al., 2007).

References:

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(8) For the Chuxiong Basin, I stated that “thick evaporite are occurred in the Jiangdihe Formation “. Yes, thin layers or nodules or crystals of evaporite can be formed in the arid fluvial environment. Of course, we cannot see thick evaporites in the field in the Chuxiong Basin, even in the Simao Basin and Khorat Basin since that the evaporite is easily dissolved under the tropical monsoon climate in these basins. Previous publication showed that several medium-scale salt mine of the Jiangdihe Formation were found (ref. 77). I am very curious about what the authors stated in the Supplementary Note line 211-2113 “Thus, the presence of thin evaporites is in contradiction with the existence of a low-gradient and continental-scale river system”. So, what is exactly the authors’ opinion?

Similarly, the authors copy the stratigraphic column of Deng’s section in the Chuxiong Basin. However, they wrongly place the basal conglomerates of the Matoushan Formation in Deng’s paper as the Jiangdihe Formation in Supplementary Figure 1. Deng et al. (2018) stated that “The Jiangdihe Formation represents a fluvial and shallow-lacustrine facies and the Zhaojiadian Formation fines upwards and represents a shallow-to-marginal lacustrine facies”. However, the authors proposed fluvial+lake and floodplain+fluvial environment for the Jiangidhe and Zhaojiadian formation, respectively, without any sedimentary work.

Reply: From the published literature (ref.[77] (i.e., Yunnan Bureau of Geology and Mineral Resources, 1965)) it may indeed appear that several sets of salt-bearing sequences occur in the Jiangdihe Formation. However, these sporadic salt-bearing sequences are still dominated by terrigenous clastic materials (e.g., 50% – 70% of siltstone and glutenite; ref.[77]), implying the lacustrine-dominated depositional systems were not persistently stable. In addition, deposition of the salt-rich calcareous mudstone facies only developed in the middle-upper part of the Jiangdihe Formation in local areas, which may have resulted from local lowlands (e.g., mire, pond, and oxbow lakes), or short-term climatic fluctuations. Please note that the current drainage basins of the world’s largest rivers also contain some small lakes (e.g., Amazon, Congo, Orinoco, and Mississippi), providing modern examples consistent with our idea.

More significantly, all Late Cretaceous–early Palaeogene stratigraphic sections in the Chuxiong Basin did not record prolonged lacustrine deposition. From a sedimentological perspective at the basin-scale, all literature consistently suggests that fluvial and shallow lacustrine depositional environments were predominant during deposition of the Upper Cretaceous Matoushan and Jiangdihe Formations (Yunnan Bureau of Geology and Mineral Resources, 1965, 1966, 1996; Xue et al., 2019). For example, a thick wedge-shaped sandstone body, known as “Fangjiahe wedge-sandstone body”, was found in the lower part of the Jiangdihe Formation (Yunnan Bureau of Geology and Mineral Resources, 1996). This sandstone body is characterized by gray purple and dark purple thick lithic quartz sandstone interbedded with purplish red siltstone and mudstone, which are laterally continuous over scales of hundreds of meters, and longitudinally spread about 4.3 km. Single bed thickness is commonly >2 m, and sandstones show large-scale cross stratification, consistent with deposition by a large-scale fluvial system. For the early Paleogene Zhaojiadian Formation, Deng et al., (2018) interpreted this unit in the Yijiu section as dominated by a shallow-to-marginal lacustrine system, but approximately synchronous lateral facies changes may exist across the basin. The Zhaojiadian Formation in the Guatang-Sanzhi section (near the Yijiu section) clearly contains fluvial deposits (Yunnan Bureau of Geology and Mineral Resources, 1965), which shows typical fluvial sedimentary features including: (i) purplish thickly bedded red sandstone interbedded with siltstone and mudstone beds; (ii) individual sandstone beds with basal granule lags and multi-scale cross stratifications, which probably resulted from bar migration and channel migration and filling processes; (iii) Mud cracks, calcareous nodules, and bioturbation structures are common in finer-grained deposits, likely representing floodplain or overbank depositional environments.

Biofacies records in the Chuxiong Basin provide further evidence. Here the fossil record is dominated by ostracodes, freshwater lamellibranchia, fish species, and plant fossils which are typical of the paleontological assemblages of many other hydrologically open lakes (Carroll and Bohacs, 1999; see Figure 1 in Bohacs et al., 2000). Taken together, these findings strongly suggest that the Chuxiong Basin was not a protracted endorheic basin, but instead a hydrologically open basin over the time span of accumulation of depositional sequences during the late Cretaceous–early Paleogene. Such overfilled lake setting has been believed to be very closely related to perennial river systems (Carroll and Bohacs, 1999; Bohacs et al., 2000). We have added these significant supplements to the revised manuscript and Supplementary Note. Finally, errors in placement of the boundary between the Matoushan and Jiangdihe Formations in Supplementary Figure 1, and inappropriate sedimentary environment assemblages of Deng et al., (2018) have been corrected.

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Yunnan Bureau of Geology and Mineral Resources. Geological Map of the Chuxiong Sheet (scale 1:50000) (Geological Publishing House Press, 1996).

Yunnan Bureau of Geology and Mineral Resources. Geological Map of the Dayao Sheet (scale 1:200000) (Geological Publishing House Press, 1965).

(9) Therefore, I easily found that the authors just simply copied two sections in the Xichang and Chuxiong basins of Deng et al. (2018). Based on my personal opinion and authors' cited publication (ref 16, Deng et al., 2018), I do prefer a fluvial and lacustrine environment during that time. It also indicates that the sedimentary environment fluctuate between fluvial and lacustrine and is not stable in such a long time range as the authors' claimed.

Reply: We agree the reviewer that fluvial and lacustrine sedimentary environments were dominant in the studied basins during the late Cretaceous–early Palaeogene. As mentioned in points 6 and 7 above, such fluvial-lacustrine facies associations together with characteristic freshwater fossil assemblages demonstrates that these late Cretaceous–early Palaeogene basins are best interpreted as typical overfilled lake basins or hydrologically open lakes based on Carroll and Bohacs (1999) and Bohacs et al. (2000). This suggests the influx rate of water + sediment fill generally exceeds potential accommodation for these studied basins, which would allow a development of through-going river systems (Carroll and Bohacs, 1999).

Although the two typical stratigraphic columns of the Xichang and Chuxiong basins in Fig. S1 were cited from Deng et al., (2018), we have included more detailed sedimentological information from a basin-scale perspective in the revised manuscript (lines 138–150) and Supplementary Note. All evidence and/or observations do not support the interpretation of endorheic lakes for late Cretaceous–early Palaeogene basins envisaged by the reviewer (See revised Supplementary Note for more details).

We have emphasized a prolonged period of regional tectonic stability, rather than the stability of sedimentary environments. Temporal and spatial shifts of sedimentary environments between fluvial and lacustrine are very common in large-scale drainage systems (e.g., the Yinchuan rift basin through which the Yellow River flows; see Figure 5 in Ma et al., 2021), possibly due to channel migration, climate fluctuations, differential subsidence/compaction and local tectonic perturbation. Especially in a large-scale drainage system that includes several overflow lakes as interpreted in this study, shifts between fluvial and lacustrine conditions would be frequent.

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(10) As for the Simao and Khorat basins, no matter the evaporite is marine or non-marine, it is impossible to form such a salt giant in an open environment (Please see Warren, 2016, Evaporites-A Geological Compendium). The authors claimed in the response letter line 417 to 422 that marine incursion evaporites cannot be used as an argument against a fluvial system that drained into the Neo-Tethyan Ocean. I guess the authors argued the seawater may be intruded from the Neo-Tethyan Ocean (from south to north) and thus claimed like that. However, marine incursions were not from the Neo-Tethyan Ocean, but rather from the Meso-Tethys Ocean or proto-Paratethys Sea as the cited publications (in Response letter line14 to 15) argued. Therefore, the authors used the marine incursion evaporite to support the existence of fluvial system is unreasonable and untenable. Moreover, the lithological and sedimentary features is indicative of a typical saline lake by many publications (Hite and Japakasetr, 1979; El Tabakh et al., 1999; Zhang et al., 2013; Liu et al., 2018; Qin et al., 2020; Wang et al., 2021). As such, the Xichang, Chuxiong, Simao, and Khorat Basins during the so

called late Cretaceous to Early Paleogene are mostly lacustrine. And Simao and Khorat saline lakes were not exorheic.

Reply: We have explained these issues in our response to the reviewer´s major points (1) and (2) above. Here we need to add that we have never considered the use of the marine incursion evaporite to directly support the existence of a large-scale fluvial system that presented this study. Instead, Late Cretaceous–early Palaeogene sea transgression in the Simao–Khorat Basins strongly suggests these areas were very close to the ocean and at a relatively low elevation (near sea level) at that time, which allows the more reasonable interpretation – advocated in our paper – that the proposed continental-scale river system discharged southward into the Neo-Tethyan Ocean (see Fig. 4 for an illustration of our inferred paleogeographic position).

(11) Provenance analysis

I suggest you put the newly added data of Simao and Khorat in the MDS plot in Supplementary Figure 6. And it is obvious that the Indochina plot so far away from your samples, indicating there was no provenance connection between them. So why do the authors' believe the fluvial system can flowed to the Indochina?

Reply: Thanks, we had put the newly added data of the Simao basin in the MDS plot in the last submitted version. The Simao Basin and Muang Xai Basin are located on the Indochina terrane. Detrital zircon data from late Cretaceous–early Paleogene samples of the two basins show strikingly consistent peaks at 200–300 Ma, 390–480 Ma, 700–900 Ma, 1700–2000 Ma, and 2300–2600 Ma (Wang et al., 2014; Wang et al., 2017; Chen et al., 2017; Yan et al., 2021; Fig. S8), which has been interpreted to be sourced mainly from the Songpan-Ganzi, Yidun, and Qiangtang terranes, rather than proximal source area (i.e., Indosinian terrane) (Wang et al., 2014; Wang et al., 2017; Chen et al., 2017; Yan et al., 2021). In other words, if the Simao and Muang Xai Basins were endorheic basins during the late Cretaceous–early Paleogene, sediments in the two basins must be derived from surrounding older rocks in the Indosinian terrane, and we would expect the widespread Triassic metamorphic rocks surrounding these basins to provide a major source of detrital material and a unique prominent peak at 200–250 Ma. But this is not the case. This is why we have believed there was a large-scale paleohydraulic system connected the current Tibetan Plateau and the Simao-Muang Xai basins during the late Cretaceous–early Paleogene, as discussed in the main text (lines 224–227).

The dominance of upper reaches/headwaters-derived provenance is also commonly observed in the middle and lower reaches of most today's large river systems. For example, southeastern Tibet (the upper reaches of the Yangtze River) has been determined to be the dominant sediment contributor for stored sediments in the middle-lower reaches of the Yangtze River (Zhang et al., 2020); Similarly, the drainage area of the upper Yellow River (i.e., northeastern Tibetan Plateau) has been interpreted as a major sediment source for the Yinchuan Basin (approximately 1500 km northeast of the headwater area) (Wang et al., 2019).

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(12) Landscape and large river system.

Such a long and large river system existed in the eastern Tibetan Plateau maybe need a higher elevation and large erosion, and thus will results in enough clastic materials to transport into the Neo-Tethys Ocean. Together with the authors' claimed that there were no proximal sources, I have three concerns. (1) How can a low-relief landscape with slow exhumation and erosion (slow cooling) can supply so many clastic materials to the Neo-Tethys Ocean in such a long distance (at least 3000 km)?

Reply: We do not argue that a higher elevation is a prerequisite for the establishment of large river systems, because a stable long-wavelength topographic slope is sufficient to sustain the longevity of a continental-scale drainage system. The combination of depositional environments, thermochronological results, landscape modeling, available palaeo-altimetric data, and tectonic setting discussed in this study shows that the proposed large river was low-gradient, low-energy, and long-lived, and also was accompanied by extensive floodplains, wetlands, and exorheic lakes that are similar to modern continental, lowland rivers. This implies this river system was likely dominated by lateral planation over most of the region, rather than vertical incision. Concurrently, a prolonged Late Cretaceous–early Palaeogene tectonic stability in eastern Tibet would not have provided sufficient relief to generate significant amounts of denudation. Last but not least, sediment budget and provenance studies widely demonstrate that the majority of the eroded sediments of large rivers are stored in terrestrial basins (Potter, 1978; Nie et al., 2015) which resemble the Sichuan, Xichang, Huili, Chuiong, Simao basins in our fluvial model. In this case, those pre-existing upland reliefs were progressively smoothed away from the main riverine network as individual negative relief elements were filled with fine-materials over tens of millions of years. Thus, the detrital materials flowing into the Neo-Tethys Ocean may be only a small fraction of the total sediment load; however, more thorough examination of Late Cretaceous–early Palaeogene in the Khorat Basin and the area to the south, especially adjacent sea area, will be required to fully ascertain sediment budget delivered by the proposed transcontinental river during that time period, using offshore drilling and seismic investigations.

With respect to the ~3000 km long axis of the river envisaged by reviewer #1, this appears to be overestimated. Because both the Simao and Khorat Basins were located at paleolatitudes of ~20°–30° N during the late Cretaceous to early Paleocene based on available palaeomagnetic results (e.g., Charusiri et al., 2006; Singsoupho et al., 2014; Zhang et al., 2018; Yan et al., 2021), which means the paleo-position of the Khorat Plateau during the late Cretaceous was located to the northwest of the present position, at least ~750 km apart (Singsoupho et al., 2014).

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(13) If the river do flow into the Neo-Tethys Ocean, do the authors have any evidence of sedimentary sequence in delta and offshore Thailand?

Reply: On the basis of frequent marine incursions and a paleo-elevation of near sea level in the Simao–Khorat areas, detrital zircon provenance results from the Muang Xai and Khorat Basins, a dominant south-directed paleocurrent from the Khorat Basin, and reconstructed paleo-continental configuration of East Asia (see revised manuscript for details), we interpret that the Late Cretaceous–early Palaeogene large river system discharged into the Neo-Tethys Ocean. However, it is very difficult to reconstruct basin paleogeography, and to exactly determine where this river discharged into the Neo-Tethys Ocean at that time, due to significant paleo-position change resulting from extrusion of Indochina, complex block rotation, and intense uplift-erosion since that time. For example, 1000–1250 m of Late Cretaceous deposits once covered the whole of the Khorat Plateau area, and has subsequently been eroded (Booth and Sattayarak, 2011), which explains why remnants of the Late Cretaceous–early Palaeogene Maha Sarakham Formation are mainly exposed inlow-lying areas, particularly in the central part of the Khorat Plateau (Meesook, 2000). Moreover, a latitudinal movement of the Indochina Block of about 5–11° (translation of about 750–1700 km in the southeastward direction along the Red River Fault) and clockwise rotation of 13–18° with respect to the South China Block have occurred since the Mesozoic (Charusiri et al., 2006; Singsoupho et al., 2014).

Nonetheless, we still try to explore this issue from a broader context. In the Borneo area south of the Indochina terrane, there are very thick, late Cretaceous to early Cenozoic deep water sub-marine fan deposits, known as the Rajang Group and Kayan Group. Field observations and sedimentological analyses indicate such very thick deep-water sequence must has been deposited in one of the world's largest ancient submarine fans and transported by a large river system (Galin et al., 2017). Galin et al. (2017) identified the late Cretaceous–early Paleogene Rajang Group as deposits of a sub-marine fan similar in size to some modern river fans like the Amazonas or the Mississippi, remarkably mismatching present-day smaller river systems in Borneo. Detrital zircon results from late Cretaceous–early Paleogene samples have shown that these deposits were likely from SW Borneo, Sibumasu, and Indochina based on a dominance of Mesozoic zircon ages that derived proximally from widespread Mesozoic magmatic arcs (Galin et al., 2017; Breitfeld and Hall, 2018). However, some distal provenance signals were diluted or even overridden by a robust proximal source given that the zircon yield of magmatic rocks is significantly higher than that of sedimentary rocks. We note that late Cretaceous–early Paleogene samples from the giant marine fan distinctly show three Precambrian peaks at 2400–2600 Ma, 2000–1600 Ma, and 1100–800 Ma (Fig. S8; also see Figure 11 in Galin et al., 2017, and Figure 12 in Breitfeld and Hall, 2018), suggesting the Songpan-Ganzi and Upper Yangtze terranes as possible source areas. Moreover, Paleozoic and

Precambrian zircons are usually subrounded to rounded and also indicate recycling from older sediments as important source (Galin et al., 2017). Thus, the Borneo fan could be a potential sink of the large river presented this study, but a further investigation of late Cretaceous–early Paleogene strata from the Khorat to Borneo area is needed to test this hypothesis (revised manuscript, lines 252–256; Fig. S8).

In any case, it is clear from the above review that the Simao–Khorat area was very close to the ocean at that time, and even a marginal marine or sea/land facies has been identified within the Late Cretaceous strata (Wang et al., 2021). Therefore, a more reasonable interpretation is that the proposed continental-scale river system discharged into the Neo-Tethyan Ocean.

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(14) Sections and samples Authors provide the GPS of all samples, however I found sample distance in the Dujiangyan section is nearly up to 30 km from Google Earth. That is absolutely not the sampling action in the same section because the Dujiangyan section only have ca. 800 m in thickness. The same as the Leshan section, two sample distance is up to 21 km. So it is absolutely questionable of the sampling.

The authors acknowledged that all stratigraphic and sedimentary work in the Xichang and Chuxiong basins are from previous publication. And as I mentioned above, the stratigraphic columns in these two basins are totally copied from Deng et al. (2018). So I doubt that the authors' sampling is consistent with the section description. Therefore, from the authors' sampling and stratigraphic work, I have to trace back to the age of the section again. All age constraints from this study is the fossils which is done about 40 years ago. Previous fossil results were from regional mapping at scale of 1:200,000 or type section. The studied sections are obviously not the type section, so previous results should not be applied directly. How can the regional fossils be used in whole basin?

Reply: This has been clarified in the revised text. Unfavorable outcrop conditions of late Cretaceous–early Palaeogene deposits exposed in the studied basins preclude the possibility of dating every sample horizon. Field investigations have revealed that Upper Cretaceous to lower Palaeocene outcrops in the southwestern Sichuan Basin were either partially covered by vegetation or slightly deformed. The observed outcrops are restricted to road cuts, coast and valleys. Thus, sampling locations in the Dujiangyan and Leshan areas were not from a single cross section, but an integrated stratigraphic section of a gently folded anticline or syncline. Stratigraphic horizons of samples were based on mapped geological cross-section, lithofacies associations, depositional systems, strata thickness, and deformation patterns that described from detailed 1: 200000 and 1: 50000 regional-scale geological maps. In order to avoid misunderstanding, we used the Dujiangyan *area* and Leshan *area* instead of the Dujiangyan *section* and Leshan *section* in the revised manuscript.

The section sampled in the Xichang basin in this study is the Mishi Section of Deng et al. (2018), thus the sampling horizons in Fig. S1 are correct. In fact, late Cretaceous–early Palaeogene stratigraphic successions in the Xichang basin have only been exposed in a single syncline including the type section, which is conducive to spatiotemporal correlation of limited sedimentary sequences of late Cretaceous–early Palaeogene. The extent of late Cretaceous–early Palaeogene strata in the Chuxiong Basin is larger than that in Xichang Basin, but it is also exposed in two adjacent synclines based on regional mapping at scale of 1:200000 and geological cross-sections. Moreover, multiple late Cretaceous–early Palaeogene stratigraphic sections including the Yijiu section from Deng et al. (2018) in the Chuxiong Basin show very similar stratigraphic cycles, lithofacies and facies associations, and marked beds, despite slightly varying lateral facies. Combined with well-mapped geological cross-sections, this allows the sampling horizons in different sites to be easily set on corresponding positions of typical sections. We admit that the sampling horizons of this study may not be very exact due to unavoidable uncertainties, but adequate sedimentation interval between samples from one basin suggests the provenance signal has been stable over tens of millions of years. Just as the comment from Reviewer #4, "*the suite of consistent detrital zircon ages from depositional basins spanning 600-750 km from north to south provides critical support to their hypothesized drainage basin geometry and the proposed timing of its existence as an integrated depositional system. To me, this spatial-temporal consistency is a key factor supporting the interpretation offered by these authors.*"

About depositional age constrained by palaeontological results, abundant ostracods and charophyta presented this study were obtained from the whole basin, rather than a type section. Characteristic ostracods and/or charophyta assemblage is also one of justifications of sequence correlation. Also, please note that palaeontological types, formation boundaries, and even sedimentary facies results in Deng et al., (2018) were mainly taken from previous province-scale or regional-scale geology reports. By comparison, we have added more detailed materials on the basis of research of Deng et al., (2018) from smaller-scale geology reports (e.g., Yunnan Bureau of Geology and Mineral Resources, 1996; Sichuan Bureau of Geology and Mineral Resources, 1990)

References:

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(15) Standard zircons

I commented the dating results of the Standard zircons, but I cannot see correct or appropriate response. Yes, as the authors replied, the detrital zircon dating is a usual way. But for each test, your 91500 and GJ-1 zircons should generate their ages. What I asked is you should provide all the ages of 91500 and GJ-1 during your experiment, so that I can determine your deviation between the test values and recommended ages. And then I will have an idea about the reliability of all ages of the samples. So you cannot use the dating results in Yuan et al. (2004) to tell me that the ages in your study is reliable.

Reply: As requested by reviewer #1, we added a Table that contains dating results of the standard zircons (available from https://figshare.com/s/a6f79d6f03b18aec1d44). In our study, the external standard zircons of 91500, GJ-1 and Qinghu yielded ^{206}Pb/^{238}U weighted ages of **1062.5 ± 1 Ma (n = 212), 604.6 ± 2 Ma (n = 137), and 159.9 ± 0.83 Ma (n = 34),** respectively. These are consistent with reference ages of 1063.1 ± 8.1 Ma (91500, Yuan et al., 2004), 599.8 ± 4.5 Ma–610 ± 1.7 Ma (GJ-1, Jackson et al., 2004 and Eelhlou et al., 2006), and 159 ± 0.2 Ma (Qinghu, Li et al. 2013). This indicates our detrital zircon data are credible (revised manuscript, lines 339–343).

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Reviewer #2

I reviewed the revision submitted by Huiping Zhang and co-authors. All of my comments were sufficiently addressed. I support publication of this manuscript.

Andrew Laskowski

Reply: We sincerely appreciate Dr. Andrew Laskowski for accepting our corrections in previous manuscript.

Reviewer #3

Dear Editor and Authors,

Thanks for the detailed response to my previous comments. After reading the manuscript, I thought there are still a few problems that need to be addressed before publication.

(1) First, the authors present a few pictures to prove a continental scale river through the basins in SE Tibet, but I suscept this. As shown in the pictures, the lanminted and fine grained sandstones and mudstones are better to be interpreated in lacustrain environment, as

suggested by the Reviewer 1. If the sediments were transported by a large river, we should observe some cross beddings, which are scarce in these basins.

Reply: Thank you for this comment. Apart from lacustrine facies, late Cretaceous–early Paleogene strata from these studies basins also contain significant fluvial deposits, which show typical fluvial sedimentary features, including various-scale cross stratifications, upward fining sequences, basal granule lags, lenticular or tabular sandstone beds with erosional contacts, and crevasse splay deposits, as widely described by many studies (e.g., Yunnan Bureau of Geology and Mineral Resources, 1965, 1966, 1996; Sichuan Bureau of Geology and Mineral Resources, 1990; Sichuan Geological Survey Institute, 2013; Deng et al., 2018; see revised Supplementary Note for details; Fig. R1). It is noteworthy that the type of sheet and ribbon sandstone interbedded with the mudstone beds has commonly been formed by aggradation of fixed channels and simple fills (Miall, 1996), rather than lacustrine facies due to the prevalence of mud cracks, calcareous nodules, and bioturbation structures within siltstone and mudstone interlayers.

Fig. R1 Representative sedimentary structures of fluvial architecture in studies late Cretaceous–early Palaeogene basins. (a) Lenticular sand body of channel in fluvial deposits (Dujiangyan area). (b) Clast-supported conglomerates and coarse-grained sandstone showing upward fining trend (Dujiangyan area). (c) Reddish sandstone with large-scale tabular-cross bedding (Leshan area). (d) Planar cross-bedded sand overlying suspected rippled cross-bedded sand (Xichang Basin). (e) Rippled cross-bedded and climbing cross-bedded sandstone (Xichang Basin). (f) Floodplain and crevasse splays deposits; fine-grained deposits are floodplain fines, lenticular sandstones are interpreted as crevasse splays (Huili Basin). All photographs taken by Xudong Zhao.

For authenticating the occurrence of fluvial environments, we collected detailed sedimentological descriptions and depositional environment interpretations from more detailed or more recent geological mapping surveys in the submitted Supplementary Note. Again, this literature clearly indicates that late Cretaceous–early Paleogene sediments from these basins were dominated by alternating fluvial and lacustrine environments (e.g., Sichuan Geological Survey Institute, 2013; Sichuan Bureau of Geology and Mineral Resources, 1990; Yunnan Bureau of Geology and Mineral Resources, 1965, 1966, 1996; Xue et al., 2019; see revised Supplementary Note for details). More importantly, based on the identifying criterion of lake types from Carroll and Bohacs (1999) and Bohacs et al. (2000), such fluvial-lacustrine facies association has been ascribed to be typical of the deposits of hydrologically open lakes or exorheic lakes associated with perennial river systems (see Figure 7 in Bohacs et al., 2000).

The biofacies records provide further evidence. In sequences exposed in these late Cretaceous–early Paleogene basins, ostracods, freshwater lamellibranchia, fish species, and plant fossils have been widely found — typical paleontological assemblages of many other hydrologically open lakes (Carroll and Bohacs, 1999; Bohacs et al., 2000; Figure 1 in Bohacs et al., 2000). These findings strongly suggest that these studied basins are best interpreted as hydrologically open basins/overfill basins associated with a large-scale river system during the late Cretaceous–early Paleogene (Carroll and Bohacs, 1999; Bohacs et al., 2000). We have added this clarification and additional documentation to the revised manuscript (lines 146–153) and revised Supplementary Note.

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(2) I am also disagree with the statement that the Late Cretaceous-Paleogeen sediments in Simao Basin is still marine facies. The sediments have been terrestrial since late Jurassic.

Reply: Thanks again, this appears to be a misunderstanding. As mentioned by reviewer, late Cretaceous–early Paleogene red beds in the Simao–Khorat Basins have been consistently interpreted as deposits of the alternating fluvial–lacustrine environments under continental setting (e.g., Chen et al., 2017; Yan et al., 2021). However, variable-thickness evaporate intervals (e.g., anhydrite, gypsum, sylvite, and halite) are commonly sandwiched in these late Cretaceous–early Paleogene terrestrial red beds in these basins. Although discussion still persists, published datasets on sedimentology proxies, biomarkers, element geochemistry, and isotopic geochemistry consistently indicate that late Cretaceous to early Paleogene evaporites are mainly of marine origin (see Figure 6 in Zhang et al., 2013), which has generally been interpreted as the result of a transgression of the Tethys ocean (e.g., Hite and Japakasetr, 1979; El Tabakh et al., 1999; Zhang et al., 2013; Liu et al., 2018; Qin et al., 2020; Wang et al., 2021; Rattana et al., 2021). This suggests that the Simao–Khorat Basins was very close to the ocean and at a relatively low elevation (near sea level) at that time.

In Borneo south of Khorat, a Mississippi- or Amazon- scale submarine fan system persisted from the late Cretaceous to early Cenozoic, a requirement that can be satisfied by an ancient transcontinental river. For details see our response to the comment by the Reviewer #1 above (**Lines 748–779** in this letter).

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(3) Second, I am not expert to modeling, so cannot judge the relibility of the modeling results to support a low relief at sea level before Eocene. But I am curious even there is a continental scale river flow to the Neo-Tethys Ocean, why must be a low relief existed?

Reply: Continental-scale drainage system's longevity in essentially the same path is sustained by the persistence of a stable topographic gradient related to intercontinental setting and lithospheric-scale tectonics (Shephard et al., 2010; Faccenna et al., 2019; Morón et al., 2019). Long-lived large-scale river systems such as the Mississippi and Amazon are major agents for the formation of low-relief landscapes in stable continental interiors. How such low-elevation landscapes with their embedded rivers respond to tectonic deformation and mountain building is of crucial importance for the evolution of continental drainage networks and for reconstructing uplift patterns during continental collision and plateau formation.

Just as the comment from Reviewer #4, "*the key scientific question that this paper addresses is the origin of t**he widespread, high-altitude, low-relief surfaces** that characterize much of the SE Tibetan Plateau: a region where local relief at present is typically quite high, with deeply incised river gorges, and rock-uplift rates are rapid in a global context. The presence of these long-lived, low-relief surfaces at rather high altitudes has long piqued our curiosity: why are they there; how did they form; what are modern analogues of their formative sequence; what data can be used to test various hypotheses?"* And the most significant and novel finding of our study is to link the development and extent of this long-lived paleo-river system to the formation of the low-relief landscape in eastern Tibet before Cenozoic uplift and plateau growth. Specifically, the existence of a large river system that persisted for tens of millions of years intrinsically reflects a prolonged period of tectonic stability, a long-wavelength dynamic topography, and a relatively stable base level. More significantly, the relatively stable base level and long-term tectonic stability required for the maintenance of this large-scale river played a central role in the development of a low-relief region in the continental interior, similar to the formation of modern low-relief landscape in central Australia and Mongolia (Stewart et al., 1986; Jolivet et al., 2007).

Again, as commended by Reviewer #4, *"this provocative, innovative synthesis and interpretation provides a potential resolution to a long-standing problems related to how there low-relief, high-elevation surfaces in SE and Eastern Tibet developed. I believe it is worthwhile to get this data set and interpretation "out there" for the interested audience to contemplate and to try to test with new data or reanalysis."* Moreover, our findings are also of fundamental importance for understanding the relationship between fluvial morphology and topographic signatures and bear implications for future studies investigating the overarching mechanism of surface uplift and landscape evolution.

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(4) Third, in recent studies, Clift et al. (2020) and Zheng et al. (2021) shown that, the Gonjo, Jianchuan, Yuanjiang, and the Northern Vietnam basins have similar provenance in the early Cenozoic, which supporting a southward-flowing river from eastern Tibet to the South China Sea. This is contrast with the figure as suggested in this manuscript. I am wandering how the authors reconcile this inconsistence.

Reply: As reviewer #3 points out correctly, based on regional similarities of provenance signature from a series of sedimentary basins, several recent studies including Clift et al., (2020), Zheng et al., (2021), and He & Zheng et al., (2021) consistently suggest that a paleo-Jinsha River flowed south to the South China Sea from the SE Tibetan Plateau following the initial surface uplift of eastern Tibet in the late Eocene (ca. 35 Ma) (see Figure 14 in Clift et al., 2020). Yet our proposed continental-scale fluvial system of this study existed during late Cretaceous to early Palaeogene (ca. 100–50 Ma), predating the India–Eurasia collision and plateau uplift. Thus, there is no contradiction between the two asynchronous drainage models.

If these interpretations hold, the discrepancy in the drainage pattern before and after plateau uplift would require a river reorganization event that synchronized with a regional tectonic-geomorphological transformation that involved synchronous basin development (Jackson et al., 2018; Li et al., 2020), tectonic uplift (e.g., Hoke et al., 2014; Li et al., 2015), latitudinal crustal shortening (Tong et al., 2017; Todrani et al., 2020), and faulting activities during the late Eocene (e.g., Wang et al., 2012; Zhang et al., 2016). Also, this reorganization event may be related to a change in deformation rate and style along the Ailao Shan–Red River fault system at ~35 Ma (Scharer, 1994; Gilley et al., 2013), which could set up the template for sediment routing that along the Ailao Shan–Red River fault system as shown by the Figure 14 in Clift et al., (2020).

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Reviewer #4

Review of *"Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Palaeogene"*

I am assessing this paper on the basis of its integrated regional data, its innovative interpretations, and the potential impact of those interpretations on our understanding of some large-scale geomorphic anomalies along the eastern and SE margin of the Tibetan Plateau. This approach likely stands in contrast to some other reviewers who are more familiar with details of the local geologic-stratigraphic-petrographic-chronologic data sets that this submission exploits and builds upon. The key scientific question that this paper addresses is the origin of the widespread, high-altitude, low-relief surfaces that characterize much of the SE Tibetan Plateau: a region where local relief at present is typically quite high, with deeply incised river gorges, and rock-uplift rates are rapid in a global context. The presence of these long-lived, low-relief surfaces at rather high altitudes has long piqued our curiosity: why are they there; how did they form; what are modern analogues of their formative sequence; what data can be used to test various hypotheses?

The authors argue that, despite some coeval tectonic uplift, a long-lived, ~north-to-south river system in eastern Tibet during Late Cretaceous to Paleogene times created abundant, low-relief surfaces during a time of relative stability (or in the face of ongoing, but slow rock uplift) prior to the Indo-Aisan collision and the main Himalayan orogeny. The authors support this scenario by comparing different data sets from these terranes: contrasts in cooling histories from the proposed river corridor (versus the bounding terranes); contrasts in detrital mineral compositional abundances and U/Pb zircon cooling ages within the "drainage corridor" versus outside of it; mixing models that optimize inputs from diverse source areas in order to "match" the observed age abundances; etc.

The cooling histories of the compiled thermochronological records (Figure 1b) make a rather persuasive case that slow Late Cretaceous to Early Tertiary cooling in Songpan-Garzi and Yidun terranes contrasts markedly with the regions of significantly more rapid Late Cretaceous-Early Tertiary cooling to the east and west of these terranes. Hence, while considerable rock uplift, erosion, and bedrock cooling was going on to the east and west. during Late Cretaceous to Paleogene times, this north-south corridor in eastern Tibet appears quite stable. To support their interpretation of an integrated fluvial system draining southward to the Neotethyan ocean, the authors combine paleocurrent analysis with detrital mineralogy and detrital zircon U/Pb cooling ages to show a noteworthy consistency among dated sampling sites spanning ~600 km from north to south along the proposed fluvial corridor. For me, the match between (i) the detrital zircon ages from the Songpan-Ganzi and Yidun terranes (proposed source areas in the north) with (ii) the suite of consistent detrital zircon ages from depositional basins spanning 600-750 km from north to south provides critical support to their hypothesized drainage basin geometry and the proposed timing of its existence as an integrated depositional system. To me, this spatial-temporal consistency is a key factor supporting the interpretation offered by these authors.

I suspect that, for some readers, examination of the extensive supplementary data will be needed to convince them of the validity of the authors' hypotheses. I find both (1) their multi-dimensional scaling plots of detrital data sets and potential source areas and (2) their modeled relative contributions from potential source areas quite persuasive.

I note that previous reviews brought up many specific issues and questions, commonly related to the characteristics of a given source area and an alternative interpretation. I am not qualified to judge the merits (or validity) of these objections. But, I did find that this contribution's authors gave quite convincing justifications for their choices and interpretations. Overall, this provocative, innovative synthesis and interpretation provides a potential resolution to a long-standing problems related to how there low-relief, high-elevation surfaces in SE and Eastern Tibet developed. I believe it is worthwhile to get this data set and interpretation "out there" for the interested audience to contemplate and to try to test with new data or re-analysis. I also think that the "problem" that this paper addresses is a long-standing and puzzling one: a problem that has come into clearer focus in recent decades as (i) high-resolution digital topography has become available of even remote or restricted areas (thereby enabling clear topographic syntheses, comparisons across regions, and identification of "anomalies") and (ii) as high-resolution, low-cost, and high-throughput analytical techniques have enable thousands of analyses to be made and synthesized, and (iii) as improved and diversified numerical modeling approaches have enabled more rigorous evaluation of hypotheses. This contribution from the edge of Tibet exploits all of these technologies in a creative synthesis that is sure to inspire (and provoke) further research focused on the evolution of large-scale dynamic orogens

Reply: We sincerely thank reviewer for his/her positive and constructive comments about our work. We have revised the manuscript based on the reviewer' comments and suggestions.

Note to editors/authors: I show my "linguistic/grammatical/clarification" suggestions below in red text.

58 Note that the Yangtze River is not identified / labelled in any figure that I could find!

Reply: Thanks, "the Yangtze River" has been labeled in Fig. 1a.

99 "comprise" means" to be composed of" So eastern Tibet comprises these provinces, not the other way around.

Reply: Yes, it is our original idea that eastern Tibet comprises these provinces (e.g., Songpan-Ganzi and Yidun terranes).

119 with sustained topography topographic relief

Reply: Correction has been made in the revised manuscript.

120 SUCH regional differences in erosion/exhumation rates could be explained by a

Reply: Correction has been made in the revised manuscript.

127 along the foredeep depozone (i.e., SW Sichuan, Xichang, Huili, and Chuxiong basins: Fig. 1a)

Reply: Correction has been made in the revised manuscript.

139-41 Several meters to tens of meters "Thick, cross-stratified sandstone beds with cross-stratification represent channel deposits of southward-flowing, low-energy ("energy" or "gradient"?) rivers and associated floodplains, and/or exorheic lakes. I don't think that crevasse splays or lake deposits should be cited as indicative of overall paleocurrent directions for large river systems, especially for thick sandstone deposits. What indicates that these rivers are "low-energy"? Are there complete channel cross sections and longitudinal sections to enable you to deduce "energy" versus gradient or simply associated grain size?

Reply: Thanks for this valuable comment. We have reorganized this paragraph (revised manuscript, lines 135–145). Grain size of late Cretaceous–early Palaeogene fluvial deposits generally ranges from fine to coarse sand, and only a few thin, clast-supported conglomerate layers were locally found. Cross-beddings within sandstone bodies are commonly low- angle. More importantly, finer facies including floodplains, lacustrine, and crevasse splays are widespread in studied basins (Fig. R1). These observations appear to testify to low-energy current-driven sedimentation. But honestly, we have not done targeted sedimentological work (e.g., detailed analysis of outcrop architecture) to deduce "energy" of river flow, thus the"low-energy" is not mentioned in the revised manuscript.

The paleocurrent orientations presented this study were primarily determined from cross-stratified sandstones and ripple beddings within fluvial sandstone units (please see Deng et al., 2018). Moreover, a dominantly southward paleo-flow direction has also been revealed by limited pebbly sandstones and conglomerate of basal granule lag from late Cretaceous fluvial deposits in the Chuxiong Basin (Xue et al., 2019), further supporting an existence of a south-flowing palaeo-drainage system.

References

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148 "Leshan section" in the Sichuan Basin, (given that the Leshan section is not identified in the figure.

Reply: In Fig. 1a, the abbreviation of "LS" represents "Leshan section" (please see caption of Fig. 1). Please note that we used the "Leshan area" instead of the "Leshan section" in the revised manuscript.

164 "genuine"?? Does this mean "statistically significant"?

Reply: Yes, correction has been made in the revised manuscript as reviewer' suggestion.

173-5 "The consistent provenance signal from the different basins requires the existence of a continuous fluvial system during the late Cretaceous–early Palaeogene." Does it truly"require"? That may well be the most likely scenario, but it doesn't "require" this scenario, in my opinion.

Reply: We agree with the comment on the use of "require", and replace it with "argue for" (revised manuscript, line 205).

367 showing main tectonic units "(red text)" (in reference to what represents these units in the

figure) and major river systems (except for the Yangtze! Add a label for this river!)

Reply: Thank you for this helpful comment. Correction of caption has been made in the revised manuscript as reviewer' suggestion; and "the Yangtze River" has been labeled in Fig. 1a.

370 low-relief plateau areas 24, 31–34, 36, 58. New suggested text: Hexagons indicate sites with Cretaceous-Tertiary cooling histories (shown in 1B) Dashed

Reply: This helpful sentence has been added in the revised manuscript, as suggested by reviewer.

Comments on Figures

Figure 1a. Nowhere is the Yangtse River labeled on this figure. Its name should be clearly identified. Note that in the figure caption, no description is given of the light blue vertical band from 40-50 Ma in Fig 1b. What is that? I presume it's a proposed "boundary" between rapid L Cret-Paleocene cooling versus post-50-Ma rapid cooling. Why not add blue or red labels for rapid cooling pre-50 Ma and post-40 Ma, respectively? I also note that the chosen level of transparency of some of the "yellow" low-relief surfaces makes these surfaces appear to be a different color (more orange than yellow, where superimposed above the orange swath), and that there is a strange (inconsistent?) mix of yellow and orange surfaces just above the red "5" in the figure. These issues should all be readily corrected. Could a label be added to the orange region so that it's more self-explanatory? Similarly, a label on the blue-dashed line ("hypothesized drainage divide") would make its significance more obvious.

Reply: The reviewer brings up some important points. As suggested by reviewer, (i) "the Yangtze River" has been labeled in Fig. 1a; (ii) the meaning/definition of light blue vertical band has been included in revised caption of Fig. 1; (iii) the color of low-relief surfaces (yellow) within late Triassic to early Cretaceous foredeep depozone (orange) has appropriately been adjusted; (iv) in order to make its significance more obvious, we also added labels of "hypothesized drainage divide" and "late Triassic to early Cretaceous foredeep depozone" on the modified Fig. 1b.

Figure 1b. Add a legend indicating blue lines for rapid cooling prior to 50 Ma, versus red lines for rapid cooling since 50 Ma.

Reply: The legend showing definitions of blue and red lines has been added in Fig. 1b.

Figure 2. How about helping your readers along with a title box, "Drainage Scenarios" and cryptic summary titles for each scenario's panel?

Reply: Thank you for this valuable comment, succinct summary titles for four scenario's panels have been added in Fig. 2.

Figure 3. The rationale is unstated for the red and blue lines for the probability density functions. Please make that clear!

Reply: We have expressed rationale of the red and blue lines for the probability density functions.

Comments on Supplemental Figures

Supp Figure 3. Illustrative and quite compelling figure!! Spell out the names of the sections (CX, HL, etc) for each group of samples.

Reply: As suggested by reviewer, full names of each studied basin/area were presented.

Reviewers' Comments:

Reviewer #2:
Remarks to the Author:
Dear Editor,

At your request, I revisited the concerns of Reviewer 1 and the authors' rebuttal relating to comments 5, 6, and 15. For all three of the comments, I consider the authors' responses sufficient and sound.

In comment 5, Reviewer 1 raises concerns with biostratigraphic dating techniques and argues that MDAs from DZs should be used instead. This is supported by reference to another study in which there was disagreement and DZ data more closely approximated the true depositional age. I agree with the authors' rebuttal, which argued that the lack of young DZ ages justified their reliance on biostratigraphic data. I found that the justification for the age interpretations in the supplementary files was thorough, transparent, and appropriately referenced. Furthermore, the provenance interpretations made by the authors explain why there is a lack of DZ ages that approximate the true depositional age. My personal experience working with DZ data tells me that MDAs are often in disagreement with the TDA, even when large-n datasets are used. This is mainly a function of the tectonic setting, not the number of analyses.

In comment 6, Reviewer 1 implies that the authors should conduct large-n DZ dating (300 or more analyses) so that DZ data could be used to calculate MDAs. This comment implies that the authors missed the younger age components because they only dated ~100-150 grains per sample. I disagree with Reviewer 1 in this case. According to Vermeesch (2004), DZ studies can be 95% confident that no population of grains constituting 5% or more of the source was missed when a threshold of 117 analyses is achieved. The authors generally met this requirement, indicating that if there were young populations that were missed, they were likely a minor source (<5%). Furthermore, the authors dated several different samples, most of which yielded very similar results. The probability that the young population was missed in each of the datasets would be much less than 5%. Finally, the authors compiled some data from the literature, bringing the number of grains for the composite sample datasets to >300 in most cases.

In comment 13, Reviewer 1 requested weighted mean ages for the standards so that results from this study can be compared to standard ages. The authors sufficiently addressed this request and comparison indicates the data are of high precision and accuracy.

Respectfully,
Dr. Andrew K Laskowski

Reviewer #3:
Remarks to the Author:

The authors provided a detailed response to my previous comments, but unfortunatelly, I am not fully convinced by the authors in two aspects.

First, the authors presented some evidence to support a fluvial origin of the deposit in the studied basins, but those sedimentary structures could also develop in shallow lake or floodplain environment, which do not require a perennial river as the author suggested.

Second, regarding the southward-flowing river to the South China Sea (Clift et al., 2020) or to the Neo-Tethyan Ocean (This study), the authors’ response is that “a paleo-Jinsha River flowed south to the South China Sea from the SE Tibetan Plateau following the initial surface uplift of eastern Tibet in the late Eocene (ca. 35 Ma) (Fig. R15). Yet our proposed continental-scale fluvial system of this study existed during late Cretaceous to early Paleogene (ca. 100–50 Ma), predating the India–Eurasia collision and plateau uplift." And therefore no contradiction between each other. However, this argument largely relies on the reliability of the "late Cretaceous-early Paleogene" age of the studied basins. I understand the difficulty to precisely constrain the age of these red bed basins, and I acknowledge that the authors have tried their best to constrain the age of these basins, but as acknowledged by the authors, the exact ages of these deposits are not yet known, and we cannot preclude the possibility that the deposits in the basins, e.g., Huili, Xichang, can extend into late Eocene. If this was the case, then the southward-flowing river from the Sichuan basin would join the paleo-Red River to the South China Sea as Clark et al. (2004) suggested.

At last, I am also curious what is the difference between "a dendritic paleo-Red River to the South China Sea" and "a continental-scale paleo-drainage system to the Neo-Tethyan Ocean" on the formation of a low-relief landscape?

Reviewer #4:
Remarks to the Author:
Review of "Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Palaeogene"
I am assessing this paper on the basis of its integrated regional data, its innovative interpretations, and the potential impact of those interpretations on our understanding of some large-scale geomorphic anomalies along the eastern and SE margin of the Tibetan Plateau. This approach likely stands in contrast to some other reviewers who are more familiar with details of the local geologic-stratigraphic-petrographic-chronologic data sets that this submission exploits and builds upon. The key scientific question that this paper addresses is the origin of the widespread, high-altitude, low-relief surfaces that characterize much of the SE Tibetan Plateau: a region where local relief at present is typically quite high, with deeply incised river gorges, and rock-uplift rates are rapid in a global context. The presence of these long-lived, low-relief surfaces at rather high altitudes has long piqued our curiosity: why are they there; how did they form; what are modern analogues of their formative sequence; what data can be used to test various hypotheses?
The authors argue that, despite some coeval tectonic uplift, a long-lived, ~north-to-south river system in eastern Tibet during Late Cretaceous to Paleogene times created abundant, lowrelief surfaces during a time of relative stability (or in the face of ongoing, but slow rock uplift) prior to the Indo-Aisan collision and the main Himalayan orogeny. The authors support this scenario by comparing different data sets from these terranes: contrasts in cooling histories from the proposed river corridor (versus the bounding terranes); contrasts in detrital mineral compositional abundances and U/Pb zircon cooling ages within the "drainage corridor" versus outside of it; mixing models that optimize inputs from diverse source areas in order to "match" the observed age abundances; etc.
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99 "comprise" means "to be composed of" So eastern Tibet comprises these provinces, not the other way around.

119 with sustained topography topographic relief

120 SUCH regional differences in erosion/exhumation rates could be explained by a

127 along the foredeep depozone (i.e., SW Sichuan, Xichang, Huili, and Chuxiong basins: Fig. 1a)

139-41 Several meters to tens of meters "Thick, cross-stratified sandstone beds with crossstratification
represent channel deposits of southward-flowing, low-energy ("energy" or "gradient"?) rivers and associated floodplains, and/or exorheic lakes I don't think that crevasse splays or lake deposits should be cited as indicative of overall paleocurrent directions for large river systems, especially for thick sandstone deposits. What indicates that these rivers are "low-energy"? Are there complete channel cross sections and longitudinal sections to enable you to deduce "energy" versus gradient or simply associated grain size?

148 "Leshan section" in the Sichuan Basin, (given that the Leshan section is not identified in the figure.

164 "genuine"?? Does this mean "statistically significant"?

173-5 "The consistent provenance signal from the different basins requires the existence of a continuous fluvial system during the late Cretaceous–early Palaeogene." Does it truly "require"? That may well be the most likely scenario, but it doesn't "require" this scenario, in my opinion.

367 showing main tectonic units "(red text)" (in reference to what represents these units in the figure) and major river systems (except for the Yangtze! Add a label for this river!)

370 low-relief plateau areas24,31–34,36,58. New suggested text: Hexagons indicate sites with Cretaceous-Tertiary cooling histories (shown in 1B) Dashed

Comments on Figures

Figure 1a. Nowhere is the Yangtse River labeled on this figure. Its name should be clearly identified. Note that in the figure caption, no description is given of the light blue vertical band from 40-50 Ma in Fig 1b. What is that? I presume it's a proposed "boundary" between rapid L Cret-Paleocene cooling versus post-50-Ma rapid cooling. Why not add blue or red labels for rapid cooling pre-50 Ma and post-40 Ma, respectively? I also note that the chosen level of transparency of some of the "yellow" low-relief surfaces makes these surfaces appear to be a different color (more orange than yellow, where superimposed above the orange swath), and that there is a strange (inconsistent?) mix of yellow and orange surfaces just above the red "5" in the figure. These issues should all be readily corrected. Could a label be added to the orange region so that it's more self-explanatory? Similarly, a label on the blue-dashed line("hypothesized drainage divide") would make its significance more obvious.

Figure 1b. Add a legend indicating blue lines for rapid cooling prior to 50 Ma, versus red lines for rapid cooling since 50 Ma.

Figure 2. How about helping your readers along with a title box, "Drainage Scenarios" and cryptic summary titles for each scenario's panel?

Figure 3. The rationale is unstated for the red and blue lines for the probability density functions. Please make that clear!

Comments on Supplemental Figures

Supp Figure 3. Illustrative and quite compelling figure!! Spell out the names of the sections (CX, HL, etc) for each group of samples.

Response letter

In this letter we provide our detailed response (in blue text) to the comments of the three reviewers (in black) and explain all changes performed on the manuscript.

Response to the comments of the three reviewers

Reviewer #2

Dear Editor,

At your request, I revisited the concerns of Reviewer 1 and the authors' rebuttal relating to comments 5, 6, and 15. For all three of the comments, I consider the authors' responses sufficient and sound. In comment 5, Reviewer 1 raises concerns with biostratigraphic dating techniques and argues that MDAs from DZs should be used instead. This is supported by reference to another study in which there was disagreement and DZ data more closely approximated the true depositional age. I agree with the authors' rebuttal, which argued that the lack of young DZ ages justified their reliance on biostratigraphic data. I found that the justification for the age interpretations in the supplementary files was thorough, transparent, and appropriately referenced. Furthermore, the provenance interpretations made by the authors explain why there is a lack of DZ ages that approximate the true depositional age. My personal experience working with DZ data tells me that MDAs are often in disagreement with the TDA, even when large-n datasets are used. This is mainly a function of the tectonic setting, not the number of analyses.

In comment 6, Reviewer 1 implies that the authors should conduct large-n DZ dating (300 or more analyses) so that DZ data could be used to calculate MDAs. This comment implies that the authors missed the younger age components because they only dated ~100-150 grains per sample. I disagree with Reviewer 1 in this case. According to Vermeesch (2004), DZ studies can be 95% confident that no population of grains constituting 5% or more of the source was missed when a threshold of 117 analyses is achieved. The authors generally met this requirement, indicating that if there were young populations that were missed, they were likely a minor source (<5%). Furthermore, the authors dated several different samples, most of which yielded very similar results. The probability that the young population was missed in each of the datasets would be much less than 5%. Finally, the authors compiled some data from the literature, bringing the number of grains for the composite sample datasets to >300 in most cases.

In comment 13, Reviewer 1 requested weighted mean ages for the standards so that results from this study can be compared to standard ages. The authors sufficiently addressed this request and comparison indicates the data are of high precision and accuracy.

Respectfully,
Dr. Andrew K Laskowski

Reply: We thank Dr. Andrew Laskowski for his detailed comments on the three points. In the "Methods" section, we added the following sentence for clarification: "In this study, all samples from the different basins show consistent detrital zircon components, and each sample yielded 68–123 concordant ages, which generally meets statistical requirements" (lines 349–351 of revised manuscript).

Reviewer #3

The authors provided a detailed response to my previous comments, but unfortunatelly, I am not fully convinced by the authors in two aspects.

First, the authors presented some evidence to support a fluvial origin of the deposit in the studied basins, but those sedimentary structures could also develop in shallow lake or floodplain environment, which do not require a perennial river as the author suggested.

Reply: Indeed, sedimentary structures such as small-scale cross stratifications or climbing ripple lamination can also be observed in siltstone to mudstone beds of shallow-lacustrine or floodplain facies. However, the presence of meter to tens of meter thick amalgamated sandstone beds which are laterally continuous over scales of hundreds of meters, indicate a fluvial origin for these sedimentary deposits. Characteristic fluvial sedimentary structures include medium- to large-scale cross stratifications, upward fining sequences, basal granule lags, and lenticular or tabular sandstone beds with erosional contacts. Moreover, paleocurrent directions based on trough and tabular cross-beds are consistently indicating a flow to the south or southeast, further corroborating the existence of a perennial fluvial system. Likewise, the sedimentological descriptions and interpretations of the depositional environment from geological surveys (as summarized in the Supplementary Information) indicate that the fluvial-lacustrine facies associations and the freshwater fossil assemblages of the studied basins are best interpreted as typical components of a large river system. We re-iterate that hydrologically open lake basins (Carroll and Bohacs, 1999; Bohacs et al., 2000) and floodplains are integral parts of large river systems, as can be seen along the largest rivers of the world (Miall et al., 1996; Chen et al., 2007; Ashworth et al., 2012). These arguments, together with the lack of thick evaporite sequences, make the existence of a large throughgoing river system very likely. As we already addressed this issue in the last revision of the manuscript (lines 135–153 of revised manuscript), we did not make further changes.

References:

Ashworth, P. J. & Lewin, J. How do big rivers come to be different? *Earth Sci Rev.* **114**, 84–107 (2012).

Bohacs, K. M., A. R. Carroll, J. E. Neal, P. J. Mankiewicz. Lake-basin type, source potential, and hydrocarbon character: an integrated-sequence-stratigraphic–geochemical framework, in E. H. Gierlowski-Kordesch and K. R. Kelts, eds., Lake basins through space and time: AAPG Studies in Geology 46, p. 3–34 (2000).

Carroll, A. R., Bohacs, K.M. Stratigraphic classification of ancient lakes: balancing tectonic and climatic controls. *Geology* **27**, 99–102 (1999).

Chen, Z., Xu, K., Watanabe, M. Dynamic hydrology and geomorphology of the Yangtze River. In: Gupta, A. (Ed.), Large Rivers: Geomorphology and Management. Wiley, Chichester, pp. 457–469, (2007).

Miall, A. D. The Geology of Fluvial Deposits: Sedimentary Facies, Basin Analysis, and Petroleum Geology, *Springer* (1996).

Second, regarding the southward-flowing river to the South China Sea (Clift et al., 2020) or to the Neo-Tethyan Ocean (This study), the authors' response is that "a paleo-Jinsha River flowed south to the South China Sea from the SE Tibetan Plateau following the initial surface uplift of eastern Tibet in the late Eocene (ca. 35 Ma) (Fig. R15). Yet our proposed continental-scale fluvial system of this study existed during late Cretaceous to early Paleogene (ca. 100–50 Ma), predating the India–Eurasia collision and plateau uplift." And therefore no contradiction between each other. However, this argument largely relies on the reliability of the "late Cretaceous-early Paleogene" age of the studied basins. I understand the difficulty to precisely constrain the age of these red bed basins, and I acknowledge that the authors have tried their best to constrain the age of these basins, but as acknowledged by the authors, the exact ages of these deposits are not yet known, and we cannot preclude the possibility that the deposits in the basins, e.g., Huili, Xichang, can extend into late Eocene. If this was the case, then the southward-flowing river from the Sichuan basin would join the paleo-Red River to the South China Sea as Clark et al. (2004) suggested.

Reply: As acknowledged by the reviewer, the exact ages of these deposits are difficult to constrain, but young (i.e., late Eocene or younger) zircons are completely lacking in our samples. Given that late Eocene plutons are common across southeastern Tibet (e.g., Lu et al., 2012; Deng et al., 2014), the absence of late Eocene zircon ages implies that the studied continental red-beds are older than late Eocene. Still, we cannot preclude the possibility mentioned by the reviewer that the large-scale river discharged to the Proto-South China Sea at a late stage of the K_2–E_1 time interval. However, in the current depositional area of the Red River (the Yinggehai-Song Hong Basin of the South China Sea), many boreholes revealed that the Cenozoic deposits at the bottom of the boreholes are not older than late Eocene (~35 Ma) (e.g., Clift et al., 2006, 2008). Regionally, the Proto-South China Sea during the late Cretaceous to early Cenozoic period is characterized by a series of deep, rapidly-subsiding small-scale rift basins that formed during back-arc extension (see review of Morley et al., 2012). This tectonic setting makes it difficult to test the hypothesis suggested by the reviewer. Considering these uncertainties and the reviewer's concern, we added the following sentence to the manuscript (lines 255–259) "**It is also possible that the paleo-drainage network discharged into the Neo-Tethyan Ocean during the late Cretaceous and Palaeocene, but later changed its course as a result of the India-Asia collision, and flowed into the proto-South China Sea starting in late Eocene.**"

References:

Clift, P. D., Blusztajn, J. & Nguyen, A. D. Large- scale drainage capture and surface uplift in eastern Tibet- SW China before 24 Ma inferred from sediments of the Hanoi Basin, Vietnam. *Geophys. Res. Lett.* **33**, L19403 (2006).

Clift, P. D., Long, H. V., Hinton, R., Ellam, R. M., Hannigan, R., Tan, M. T. et al. Evolving east Asian river systems reconstructed by trace element and Pb and Nd isotope variations in modern and ancient Red Rive-Song Hong sediments. Geochem., Geophys., Geosyst. 9, Q04039 (2008).

Deng, J., Wang, Q. F., Li, G. J. & Santosh, M. Cenozoic tectono-magmatic and metallogenic processes in the Sanjiang region, southwestern China. *Earth-Science Reviews* **138**, 268–299 (2014).

Lu, Y.J., Kerrich, R., Cawood, P.A., McCuaig, T.C., Hart, C.J.R., Li, Z.X., Hou, Z.Q., Bagas, L. Zircon SHRIMP U–Pb geochronology of potassic felsic intrusions in western Yunnan, SW China: constraints on the relationship of magmatism to the Jinsha su-ture. *Gondwana Res.* **22**, 737–747 (2012).

Morley, C.K. Late Cretaceous–Early Palaeogene tectonic development of SE Asia. *Earth Sci Rev. 115*, 27–75 (2012).

At last, I am also curious what is the difference between "a dendritic paleo-Red River to the South China Sea" and "a continental-scale paleo-drainage system to the Neo-Tethyan Ocean" on the formation of a low-relief landscape?

Reply: A dendritic palaeo-Red River to the South China Sea, as suggested by Clark et al. (2004), would be largely controlled by a regionally eastward-tilt of the topography, possibly resulting from plateau uplift. Such a tectonic setting makes it difficult to develop a large-scale low-relief surface. In contrast, our proposed continental-scale paleo-drainage system to the Neo-Tethyan Ocean follows an inherited long-wavelength topographic depression (as explained on lines 105–111 of the revised manuscript), which provides more favourable boundary conditions for the formation of the extensive low-relief landscape.

Reference: Clark, M. K., Schoenbohm, L. M., Royden, L. H., Whipple, K. X., Burchfiel, B. C., Zhang, X., Tang, W., Wang, E. & Chen, L. Surface uplift, tectonics, and erosion of eastern Tibet from large–scale drainage patterns. *Tectonics* **23**, TC1006 (2004).

Reviewer #4

Review of *"Existence of a continental-scale river system in eastern Tibet during the late Cretaceous–early Palaeogene".*

Reply: As explained in the Cover Letter, the comments we received from Reviewer #4 were those of the last round of reviews. We believe that we satisfactorily addressed these comments in our last revision and did therefore not change anything in this current revision of the manuscript.