RC1: 'Comment on egusphere-2023-2651', Anonymous Referee #1, 14 Mar 2024

The paper entitled "Drainage rearrangement in an intra-continental mountain belt: A case study from the central South Tian Shan, Kyrgyzstan" by Gong and co-authors, presents a geomorphic analyze of the Saryjaz drainage basin localized in the Kyrgyz Tianshan. Based on a topographic analyze and the measurement of various geomorphic metrics, the authors identify knickpoints of different origins. They found several transient kick points in tributaries downstream of 180° direction change of the Saryjaz river. Authors argue these transient knickpoints were created by a large river capture which occurred 1.5-4.4 Ma ago and was driven by the overfill of a large intermontane basin.

Reconstructing the drainage evolution of a given river basin through time is often challenging because rivers, being very active and mostly destructive in intramountain area, leave few remains of their past. To tackle this issue, this study presents an interesting geomorphic approach which combine various measurements of several metrics mainly to identify knickpoints along river profiles. The main results of the paper, ie the identification of the transient knickpoints, is mostly robust. However, (1) the general motivation of the study is unclear, (2) the interpretation on the origin of the transient knickpoints (i.e river capture) should be better supported by a robust (statistical?) analysis, and (3) the scenario proposed to explain this supposed river capture (i.e. basin overfill and inverse river flowing) is speculative since not supported by any robust observations/data. More observations, analyses and quantifications are needed to better support this story.

These 3 main issues should be better addressed before final publication.

We thank the reviewer for their detailed and constructive comments, which have shown us where clarifications in the text were needed and helped us to strengthen several of our arguments. We have also considered an alternative to the final capture scenario that addresses inconsistencies that the reviewer identified. To try to address these points, we made the following modifications to the manuscript (***bold, italic text*** is new):

Motivation and goal of this study

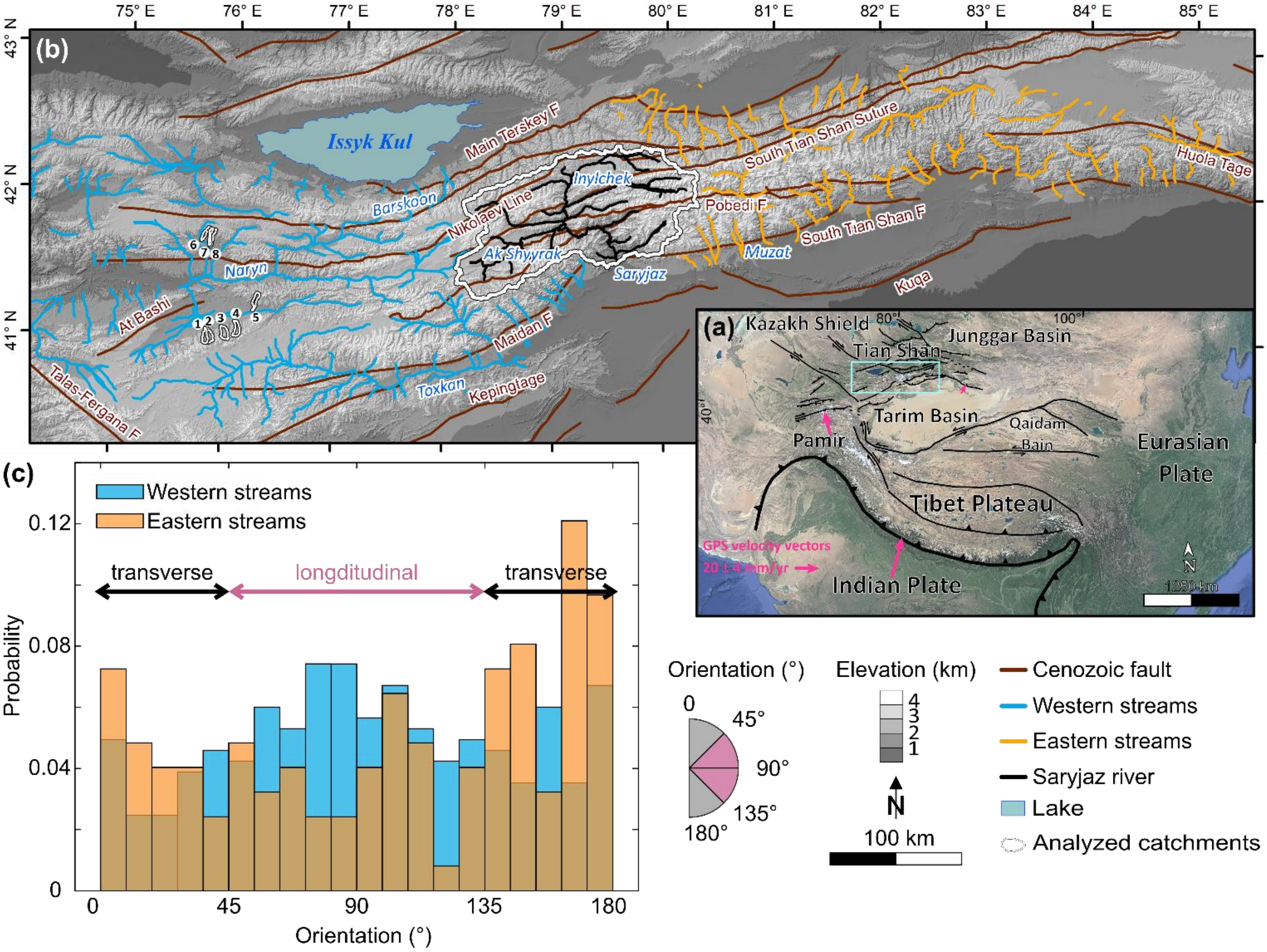
The general motivation of this work and why authors have decided to study the Saryjaz drainage basin is unclear and based on a subjective assertion. It is said lines 52-54 "Within this tectonic and climatic context, the South Tian Shan exhibits a significant contrast between a longitudinal drainage pattern in the west and transverse drainage in the east (Fig. 1)."

Also L83-85: "Drainage basins in the east are mostly characterized by transverse streams, whereas to the west, they show longitudinal patterns. The transition zone between these two drainage patterns hosts both the highest topography and the Saryjaz River catchment (Figs. 1, 2)."

So, as far as I understood, the starting point of this study is a supposed contrast in river drainage pattern between East and West of the South Tianshan and the fact that the Saryjaz basin is located at the transition zone and is, therefore, of particular interest. **First, what means East and West ? With respect of what? Longitudes should be given.**

We added the longitudinal ranges of the 'eastern' and 'western' streams in the South Tian Shan to the text: Drainage basins in the east ***(between 80-85°E, see streams colored in orange in Fig. 1b)*** are mostly characterized by transverse streams, whereas to the west, they show longitudinal patterns ***(between 74-78°E, see streams colored in blue in Fig. 1b)***.

We also updated ***Fig. 1b*** (see below) which now shows the 'eastern streams' and 'western streams' in different colours, as well as the Saryjaz river in black to give a comparison of the two sides.



More importantly, this assertion of a contrasted drainage pattern is qualitative and may not be supported by the data. Indeed, the only argument presented by the authors is a rapid interpretation of Fig. 1 which displays the main rivers around the Saryjaz basin. Yet, on this figure several large longitudinal rivers are also present to the East of the Saryjaz basin as well as transverse rivers to the West. Moreover, why focusing only on this part of the South Tianshan? This range is much larger. Farther East (longitude >E83° which is the limit of Fig.1) the range is dissected by a long longitudinal river that goes down to the Bayanbulak intramountainous basin.

In conclusion, the statement that the drainage pattern of the South Tianshan exhibits a longitudinal contrast is rather vague, subjective though important for the study. This needs to be strengthened for instance using a more quantitative approach. Below is a suggestion of a quantitative analyze of the flowing directions of rivers in the South Tianshan (see screen shot in Fig. A that show the region that I have selected). This analyze could likely be refined/improved.

Using GIS and 90m SRTM DEM I extracted the flow directions of each cells of the main rivers (defining an arbitrary accumulation threshold). The figure B shows the frequency distributions of the NS, EW, NW-SE and NE-SW directions along longitudes. The frequency values are normalized to the total number of cells found for each bins of the histograms. We see from this figure that the change of East-West flowing directions in South Tianshan is tenuous. May be, it increases a little from E79° to E77° of longitude?

NB: I restricted my analyze to the main reliefs and excluded the large Tarim flat foreland basins

This is a very good point. The plots from the reviewer nicely show the normalized frequency of flow orientation along strike. To highlight the differences in drainage patterns to the west and east of the Saryjaz drainage, we now include a histogram of flow directions for these two sets of rivers in ***Fig. 1*** (see above).

The procedure we followed to create this histogram is the following: by using MATLAB, Topotoolbox 2 and the Copernicus GLO-90 Digital Elevation Model, we define a base-level elevation at 1500 m (piedmont elevation to the Tarim basin), extract the main rivers (accumulation threshold = 10 km) flowing through the South Tian Shan (excluding basins such as Tarim and Issyk Kul). We extract the flow orientation of rivers following cardinal directions; 0: N-S, 45°: NE-SW, 90° E-W, 135° NW-SE, and 180°: S-N. We also plotted the normalized frequency of river orientation from the 'eastern' (in orange) and 'western' (in blue) in a histogram (bin = 9°).

Considering that the strike direction of the South Tian Shan is mostly W-E (or slightly (5-10°) to the north), the orientation of streams in the directional ranges [0, 45º] and [135º, 180º] can be classified as mostly transverse, while those oriented between [45º, 135º] are longitudinal. The histogram shows that the majority of the 'eastern streams' have an orientation between [0, 45º] or between [135º, 180º] (i.e., transverse), while the 'western streams' are more variably oriented but show a maximum around 90º (i.e., longitudinal). Please see our updated ***Fig. 1*** for complete information.

The flow accumulation threshold should be better defined as the limit between the hillslope and fluvial processes (log plot of slope vs. drainage area).

On a regional scale, it is impractical to define the flow accumulation threshold for fluvial processes in each drainage, given that functions that operate on a DEM to define the flow accumulation network take the argument of a single minimum drainage area. Instead, we follow a fairly standard approach of choosing a minimum drainage area of 1 km^2 to initially define our drainage networks. We found that this value was sufficient to remove portions of the catchment dominated by hillslope processes, both by examining k_{sn} maps (no systematic lowering of ksn in the uppermost reaches) and chi plots (no systematic lowering of slopes in the uppermost reaches).

We modify the sentence in the main text that mentions the cut-off to the following: "Streams used for longitudinal profile and χ analysis were extracted with a minimum drainage area of 10^6 m^{2,} ***which we found was sufficient to exclude portions of the basin dominated by hillslope processes***."

Also, the strike of the range is not purely E-W while the flow directions were calculated based on cardinal points.

We agree with the reviewer that the strike of the South Tian Shan is not purely W-E, from around ENE-SWS between 73-83°E, to ESE-WNW between 83-86°E. Here we use cardinal directions as a standard to compare stream orientation on both sides of the Saryjaz river, and we feel this sufficiently clearly shows the differing trends of these two sets of rivers.

Authors also need to clarify their zone of interest. What means "South Tianshan"? Why not considering longitude >E83°?

Following previous studies (e.g., Yin et al., 1998; Jourdon et al., 2017; Morin et al., 2019; see complete reference below) and Cenozoic topography, we added a definition of the "South Tian Shan" as ***between the Talas-Fergana fault to the west (around 75°E) and Huola Tage Mountain to***

the east (around 85°E), separated from the North Tian Shan by the Main Terskey fault, and from the Tarim Basin by the Maidan fault and South Tian Shan fault.

Yin, A., Craig, P., Harrison, T. M., and Ryerson, F. J.: Late Cenozoic tectonic evolution of the southern Chinese Tian Shan, Tectonics, 17, 1–27, 1998.

Jourdon, A., Petit, C., Rolland, Y., Loury, C., Bellahsen, N., Guillot, S., Le Pourhiet, L., and Ganino, C.: New structural data on Late Paleozoic tectonics in the Kyrgyz Tien Shan (Central Asian Orogenic Belt), Gondwana Res., 46, 57–78, https://doi.org/10.1016/j.gr.2017.03.004, 2017.

Morin, J., Jolivet, M., Barrier, L., Laborde, A., Li, H., and Dauteuil, O.: Planation surfaces of the Tian Shan Range (Central Asia): Insight on several 100 million years of topographic evolution, J. Asian Earth Sci., 177, 52–65, https://doi.org/10.1016/j.jseaes.2019.03.011, 2019a.

Moreover, while in the introduction the authors describe an EW contrast of drainage pattern across the South Tianshan, most of the paper focuses only on a U-turn made by the main stem of the Saryjaz river.

Yes, the Introduction focusses on a general pattern we observe (transverse flow in the west, longitudinal in the east), whereas the paper focuses on an approach we believe will help us to identify whether a change in drainage pattern resulted from capture of a longitudinally flowing portion of the landscape by a transverse flowing portion. **The introductory sentences are meant to set up a more regional scope: why studying the evolution of the Saryjaz catchment might be significant beyond the local peculiarity it presents.** We hope that our revisions of some of the sentences in the Introduction (described below) make this sufficiently clear now.

The motivation of the paper being unclear, the goals and the reasons why authors have chosen the methods they used are also unclear.

It is said L53-54 “However, it is not clear how or if the drainage pattern responded to Cenozoic structural reactivation and the uplift of individual ranges, a major change in climate, or the impacts of locally intense glacial erosion.”and L55-59: “To unravel these complexities, we investigated the transition area between the regions of longitudinal and transverse drainage: the anomalously large Saryjaz catchment, which drains the highest part of the South Tian Shan, and includes two Neogene intermontane basins: the Ak Shyyrak and Saryjaz basins (Figs. 1, 2).

What means “unraveling these complexities”? Is the goal to explain the supposed East West difference in drainage pattern (assuming it is real)? If yes what are the hypotheses to explain this difference and how these hypotheses can be tested? For instance, what would be an impact of a Cenozoic reactivation? How would it differ from a change in climate and/or glacial erosion? Consequently, what are the methods to test these hypotheses, why, how do they help to test these hypothesis/the different scenari? Why in particular focusing on knickpoints?

We agree some of these sentences near the end of the Introduction were insufficiently clear, and caused confusion regarding the main motivations of the work. To try to address this, we modified the sentence L53-54 to the following:

“However, it is not clear how or if the drainage pattern responded to Cenozoic structural reactivation and the uplift of individual ranges, a major change in climate, or the impacts of locally intense glacial erosion***, any of which may have had an impact on how regional drainage patterns have evolved***.”

We also modified the sentence about “unraveling these complexities”; it now states: ***The transition zone between longitudinal and transverse drainage, the anomalously large Saryjaz catchment, might offer some clues as to whether the pattern has changed through time, and if so, what might have triggered the change.”***

We hope these two changes, although somewhat minor, better explain how our study of the Saryjaz catchments helps us address questions about whether the regional drainage pattern evolved over time, and if so, why.

Authors only said they combine series of topographic analyses and metrics without explaining in detail their goals and what each method will provide “We combine quantitative analysis of topography using fluvial metrics (i.e. slopes, channel steepness, integral proxy χ), with mapping and characterization of knickpoints throughout the catchment.”

We added a motivation for studying the knickpoints, together with the two changes outlined above, and hope that will make the goal of the topographic analyses clearer.

Many conspicuous knickpoints occur throughout the Saryjaz catchment, but these can have variable origins; they can be tectonic (related to active faulting), lithologic (related to boundaries between lithologies with different erosional resistance), glacial (inherited from glacial processes affecting the topography) etc. We aim to characterize these knickpoints and understand the processes at their origin.

The methodological part is very technical and does not provide much more information regarding the goals and what each method will provide to address the general question and hypotheses of the study, which are, I recall, unclear.

We understand the reviewer’s concerns, but a separate section on the theory behind ***knickpoint migration and transient landscape evolution*** comes between the Introduction and the Methods – which we believe addresses both concerns raised by the reviewer. Why do we have a separate section for this? We found that it was not practical to include the theory needed to explain the details of our approach in the Introduction. If we did, the Introduction would be exceedingly long, and move away from the main point of an Introduction, which is mainly to motivate the work and provide a brief overview of the approach taken. Likewise, if the theory were included in the Methods, the Methods section would be quite long, and would mix important background (which we want all readers to understand) with technical details (which likely only a subset of readers will care about). We understand that a separate section on theory is not a part of a “standard” manuscript structure, but we believe it is the most effective way to organize our manuscript.

Similar structure can be found in some *ESurf* papers in our reference, for example:

Gallen, S. F. and Wegmann, K. W.: River profile response to normal fault growth and linkage: an example from the Hellenic forearc of south-central Crete, Greece, Earth Surf. Dyn., 5, 161–186, https://doi.org/10.5194/esurf-5-161-2017, 2017.

Interpretation of the knickpoints

First, it is said L244 that knickpoint at distance <250m from a fault are identified as “tectonic knickpoints” and hence disregarded. But why 250m? please justify this value.

The value was chosen somewhat arbitrarily, based on our consideration that in such a remote area with common DEM errors due to the very high relief of the topography, the exact mapped position of faults could be inaccurate. We added the phrase ***"(chosen somewhat arbitrarily, considering that there could be some error in the fault positions in such remote regions)"***.

Moreover, could tectonic knickpoints migrate upstream and hence be at a distance >250m from the fault they originated from? For instance, in the Apenin in Italy, tectonic knickpoints are localized several kilometers upstream of the faults (see Whitaker et al., 2008). This point should be better discussed/argued.

The reviewer is correct that in the case of a recent change of activity along a fault, there could be a knickpoint that has transiently migrated upstream from the fault contact. However, these would be classified as "transient knickpoints" (see old Figs. 3b,3d in the main text). In such cases, there would still be a stationary knickpoint expected at the fault trace, which we refer to as a "structural" or "lithologic" knickpoint, separating areas of differential rock uplift and/or erodibility along a fault trace. To clarify this point, we have modified the text:

"We identified lithologic and structural knickpoints as those lying within 250 m horizontal distance ***(chosen somewhat arbitrarily, but considering that there could be some error in the fault positions in such remote regions)*** from the boundaries of lithologic contacts or Cenozoic faults, respectively. ***We expect these knickpoints to be stationary, marking a spatial change in erodibility and/or rock-uplift rates."***

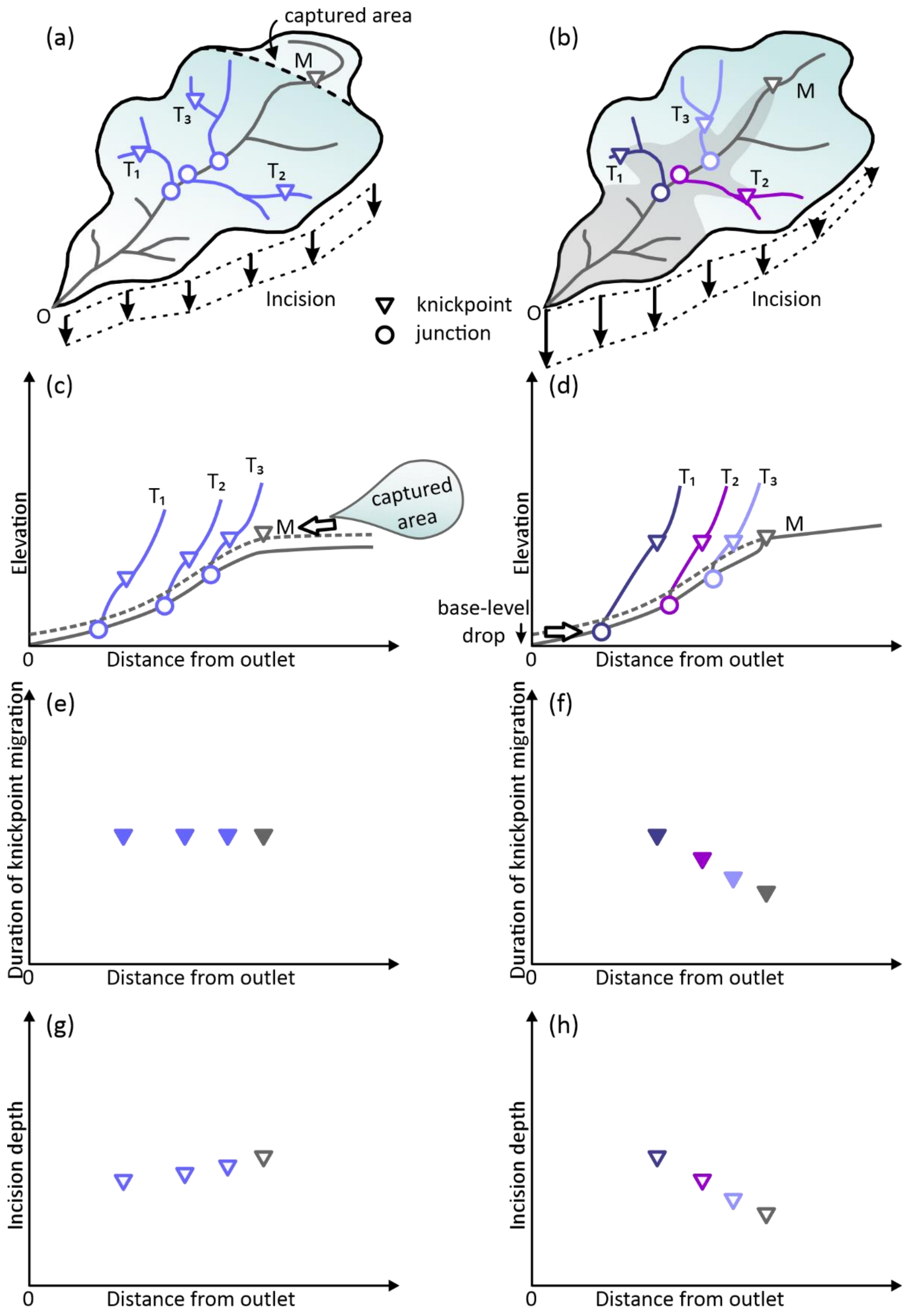
These knickpoints clearly differ from transient knickpoints that have migrated upstream, either due to a change in base-level fall rate or drainage capture.

More importantly, it is said L410: "our observation are **clearly** more consistent with transient knickpoint migration triggered by drainage capture". To support this conclusion, according to authors (see Fig. 3a), the knickpoints should "clearly" have the same Incision Depth and have initiated all at the same time. First and so far, the Onset time vs. Distance from outlet is not shown anywhere. This is surprising.

I understand that the Onset time are shown in Fig. 7b, where, indeed they seems similar in all studied basins (whatever the glacial model is). But Fig. 7b is a "Caltech plot" while an indisputable demonstration would be to show the onset time as function of the Distance from outlet as suggested in Fig 3.

Figure 6d shows χ versus distance, which is nearly equivalent to "Onset time" versus distance; the two are related through the erodibility K (assumed constant). We use to term "Onset time" earlier on in Figure 3 because we wanted to explain the general concept to readers before getting into the technical details of chi, which are only given in the Methods section, and requires several steps of derivation. Moreover, we realized that "Onset time" is probably not the best term to use, and a better equivalent to chi would be "***Duration of knickpoint migration***". To transform chi into actual "Duration of knickpoint migration", we need to calibrate the erodibility, which is what is illustrated in Figure 7. **We have added another sentence to the caption of Fig. 6 to explain how χ relates to**

"Duration of knickpoint migration" to help clarify this point, and we changed all instances of "Onset time" in Fig. 3 to "*Duration of knickpoint migration*".



Yet, I wanted to plot the Onset time vs. Distance from outlet to check authors conclusions but I couldn't because the data are not provided. The onset time of knickpoint migration should be given in Table 1 and not only shown in a "Caltech" plot in Fig. 7b.

According to ***equation (9)*** in our main text, τ ('onset time' or 'duration of knickpoint migration') is linearly related to χ through the erodibility *K*. To help the manuscript flow better, we thought that τ should come after ***Table 2 Erodibility calculation***. Therefore, we added an extra table in the supplementary information (***Table S1***) that reports τ calculated from the three scenarios used to infer denudation rates from cosmogenic-nuclide data.

Second and similarly, in Fig. 6b, giving the scattering of the data, the fact that incision depths are similar in all basins is far from being obvious and "clear" as argued by authors.

The lack of a trend is the most important result of this particular plot, given that a number of factors can contribute scatter to the actual migration rate of a knickpoint (e.g., anything that affects the erodibility of the rock between the tributary junction and the location of the knickpoint, and any change in erosion process that could affect n). **We have added linear regressions to these plots, which show a high p value (0.977), demonstrating that there is no significant trend. We also now explain in the main text the importance of this lack of a statistically significant trend.**

To support their conclusion about the origin of the transient knickpoints, authors could better statistically analyze their data (Online Isoplot R, F-Test ??).

We greatly appreciate this suggestion from the reviewer; as noted above, we have added linear regressions with reported statistics that show significant trends (or lack thereof), and these additions have helped to strengthen our arguments.

Scenario to explain the supposed drainage capture

But let's assume that the drainage capture is robust and supported by a better statistical analyze of the data. The scenario that is proposed to explain this capture also questions and may request more quantitative analyzes/observations.

The mechanism that is proposed is "overtopping of the divide: during the Pliocene-Pleistocene period, the Ak Shyyrak Basin gradually filled with sediment, until river aggradation caused the west-flowing channel to eventually reach and overtop the drainage divide." To support this mechanism the authors claim that "the sedimentary remnants, inferred from satellite imagery" occur "east of and high above the Saryjaz river in the vicinity of the "U-turn" (Fig. S6). It is very challenging to see sediments on the Google Earth capture presented in Fig S6. I zoomed on google Earth on the crest where authors claim sediments remain (see Fig. C). I don't see any evidence for sediments here. The crest is darker but I think it is just a shade and a misfit between the image and the DEM. And we can see the bedrock everywhere on both flanks. To better support the presence of sediment high above the river, do authors have any other observations? Like field photo? Geological map?

The Quaternary Geological Map of the Khan Tengri Massif (ISTC project No. KR-920) actually shows mid-Pleistocene deposits on the right side of the 'U-turn', exactly where we showed them on the Google Earth image. We also include an extract of this map in the supplementary figure for revision.

Mikolaichuk, A.V., Charimov, T. A., Zubovich, A., Gubrenko, M., Bobrovski, A.: Quaternary Geological Map of the Khan Tengri Massif (Kyrgyzstan), ISTC Project No. KR-920, 2008.

Moreover, those supposed sediments lie at >3200m more than 950m above the modern river. What is the elevation of sediments in the Ak Shyyrak basin, is it consistent with such elevation downstream above the U-turn ? Do you have any Geological map of Ak Shyyrak basin ?

I understand it is difficult to do field work in this region but I think a better geomorphic analyze of the different valleys/basins and their sediment remains/filling is still possible using high resolution satellites images and DEM. It is important to better support the proposed scenario for instance focusing on recent alluvial deposits that could be found in the different valleys/basins. What are their elevation? Do they support sediment overfill of the basin and a major incision since 2-4Ma?

Indeed, these middle Pleistocene deposits were mapped at 3200 m elevation, which would be consistent with overflow of the Ak Shyyrak basin being at the origin of the capture.

Also, in the proposed scenario “in Miocene time, a west-flowing river connected two intermontane basins, Saryjaz and Ak Shyyrak, and likely continued westward to join the current Naryn River.” Then “The capture event would also have reversed the Ak Shyyrak river to flow east into the Saryjaz catchment rather than west into the Naryn catchment”. **I think a more quantitative analyze of the implication of this inverse flowing is requested. The modern water divide between the river that flows West toward the Naryn basin and the Ak Shyyrak river that flows East lies around 3500m. In the proposed scenario the elevation of the divide should not have changed (assuming negligible uplift and sediment filling?). If we assume that the Miocene East flowing Ak Shyyrak river had a similar slope (<0.5-1%) than today in the Ak Shyyrak basin, then the elevation of this river at the U-turn location (90 km east from the modern divide) should be >600m above 3500m. The valley bottom being now at 2300m at the U-turn, this would require >1800m (3500+600-2300) of incision in 2-3 Ma (>0.6-0.9 mm/a), and even more upstream.** Is it realistic for the Tianshan? How does this value compare with other region where drainage basin captures have been documented? **Is it consistent with the order of magnitude of incision depth observed with the transient knickpoints?** Any observations upstream of the U-turn to support more than 1800m of incision? A figure with the paleo-profiles vs modern profiles to see the implication in terms of incision would be helpful.

This is a very helpful thought experiment from the reviewer. While we will avoid speculating on whether ca. 2 km of incision could be realistic for the Tian Shan generally, we can examine how much incision is recorded in the transient profiles in the vicinity (just downstream) of the U-turn. We see a lot of scatter (Fig. 6c), but the range is around 200 to 600 m, not 1800 m. This raises two possibilities: (1) our scenario for the paleo-drainage flowing into the upper Naryn River basin is incorrect, or (2) the upper Naryn River basin had substantial sedimentary infill since the capture time to raise its elevation. Absent any direct age constraints on the basin fill, we cannot rule out (2). But the possibility of (1) remains. A simple overtopping of a divide in the vicinity of the U-turn could similarly result in a drainage capture. That scenario would suggest that ***the modern upper Saryjaz catchment was a closed basin, not one that flowed into the upper Narin Basin***. This scenario would explain all of our observations without requiring what appears to be an unrealistic amount of incision. We have modified the ***Discussion*** to include these possibilities, and we modified the text to highlight the potential inconsistency between low incision depths downstream from the U-turn and the scenario of the upper Saryjaz flowing into the Naryn Basin.

Other comments

Part 2.3 should go in the method (3.3) and not along the Geological/climate background of the Tianshan. This would avoid several useless repetitions and would make the reading more straightforward and easier.

We tried this in an early draft of the manuscript, but we found that (1) it made the methods section extremely long, and (2) it created a mix of general theory and detailed explanations/equations that was not effective. The latter in particular was problematic, as it is important that all readers understand the theory and predictions, but not that they follow the detailed explanations and equations. In re-examining the text, we do not believe that the very minor reduction in material that is repeated is worth the risk that readers skip over critical aspects of the theory and our predictions for different hypotheses.

The calibration of the erodibility coefficient is limited to 8 unpublished 10Be derived basin average denudation rates located around the Naryn basin, >200km west of the studied area. Why not using the largest 10Be dataset of Charreau et al. 2023 which includes much more larger drainage basins in the Eastern Tianshan? Lithology, climate history etc is likely similar in Eastern Tianshan.

The location of data from *Charreau et al. 2023* is further east between 83 and 87 °E, at a lateral distance of more than 300 km from the Saryjaz catchments. We carefully selected the 8 catchments from *Kudriavtseva et al., 2023,* which has now been published, to resemble as closely as possible the situation we would have in the Saryjaz in terms of tectonics and main lithologyies. Therefore, we would rather stick to this dataset. See the answer below for the reference.

By the way, the reference of Kudriavtseva et al., even if in review, is not provided in the list of publication at the end of the paper. Instead of this unpublished paper I would rather quote the PhD thesis.

The paper is now published and added to the list of references.

Kudriavtseva, A., Codilean, A. T., Sobel, E. R., Landgraf, A., Fülöp, R. H., Dzhumabaeva, A., Abdrakhmatov, K., Wilcken, K. M., Schildgen, T., Fink, D., Fujioka, T., Gong, L., Rosenwinkel, S., Merchel, S., and Rugel, G.: Impact of Quaternary Glaciations on Denudation Rates in North Pamir—Tian Shan Inferred From Cosmogenic 10Be and Low-Temperature Thermochronology, J. Geophys. Res. Earth Surf., 128, 1–23, https://doi.org/10.1029/2023JF007193, 2023.

L112: In the Eastern part the Tianshan is bounded by the Junggar basin to the north. Not the Kazakh plateform

We rephrased to “The Tian Shan is bounded by the Tarim Basin to the south, Kazakh platform ***and Junggar block*** to the north.”

L116: what about the Kazakh plateform mentioned before?

We included Kazakh into the sentence “The ancestral Tian Shan was formed by ***several large-scale*** collisions between the Tarim***, Kazakh*** and Junggar blocks, and continental accretion during the Paleozoic.”

Besides, we clarified the location of the South Tian Shan after general geological evolution of the whole Tian Shan, to focus on the main research target of this work.

The South Tian Shan is the south-most part of the Tian Shan, lying between Talas-Fergana fault to the west (around 75°E), Huola Tage Mountain to the east (around 85°E), separated from North Tian Shan by the Main Terskey fault, and from Tarim Basin by the Maidan fault and South Tian Shan fault.

L142: those rates are derived from GPS. This should be said.

We added the “***GPS data indicate that the***” in the main text.

L142-145: several studies have also constrained the Quaternary deformation rates across the South Tianshan using geomorphic markers.

We agree with the reviewer, and add this sentence to the beginning of this paragraph.

L145-148: these values are for the entire Eastern Tianshan, not only for the South range.

We deleted the “South”.

L213: some explanation in sup info of how fig S1 has been made would be helpful. Not everyone is a specialist of the power law approach.

In the caption of Fig. S1, we added more explanation: ***The Bayesian Optimization algorithm attempts to minimize a scalar objective in a bounded domain. The function ‘mnoptim’ in Topotoolbox 2 uses χ analysis to linearize river long profile, and pick a random subset of channels/basins to calculate the best-fit concavity and test with other channels/basins.***

For further information, please see the function ‘mnoptim’ in Topotoolbox 2: https://github.com/wschwanghart/topotoolbox/blob/master/%40STREAMobj/mnoptim.m.

L245: again why 1000000m2 ? justify this choice

The choice was somewhat arbitrary, but it is a common choice, since it often excludes hillslope portions of the catchment. Indeed, we do not see any evidence of systematic decreases of ksn in the uppermost portions of the river network, which suggests that the cut-off we used was sufficient.

We changed the text to read “Streams used for longitudinal profile and χ analysis were extracted with a minimum drainage area of 10^6 m^2, ***which we found was sufficient to exclude portions of the basin dominated by hillslope processes***.”

L308-309: it is very hard from Fig 2b to see this gradian. May be a plot of slope vs distance along the basin and/or a zoomed map would be helpful (could be given in sup info)

In our updated supplementary information, we added an additional figure to show the slope along the stream that flows through the Ak Shyyrak basin to the outlet. Notice that the range of slope seems quite big (light blue shade), considering the noise in DEM data and swift change of direction along the profile; while the mean value (thick blue line) can be a good proxy to show the change of slope within a certain distance (in this case 5 km) to the channel.

L313-314: how can we see the change in Ksn in the Ak Shyyrak basin from fig 2b while it shows the slope?

We split Fig. 2 into 2 separate figures, one showing the geologic and topographic information (new ***Fig. 2***); the other one showing river steepness (k_{sn}) with distribution of knickpoints (new ***Fig. 3***).

L348-349: please provide a figure (map, plot) that shows this “slight” increase.

Please see ***Table 1*** where we included 'k_{sn} downstream' and 'k_{sn} upstream'. To better show the trend, we also added a plot in the supplementary information.

L381-382: useless repetition. This was already said in the method part.

We deleted the sentence.

L315-316: what is the width of the swath profile? This is important since it could bias the data and derived interpretation. The distinction between the so-called low-relief vs high-relief region is arbitrary and qualitative. The change in reliefs along the main stem is in general very gradual and I don't see two zones marked by a sharp change between them. Only the region around the U-turn shows a rapid change in relief but if we overlook this area, reliefs gradually increase from 130km to 60km, then remain high from 60 to 20km and then decrease slowly up to the outlet of the basin

For instance, in Fig6a, reliefs are relatively high from 90 to 100km (>3000m) while authors call this region low-relief. Similarly, reliefs are low near the outlet (<1000m) while called high-relief.

We agree with the reviewer, that the boundary between 'high-relief' and 'low-relief' topography should be changed. Please see ***Fig. 7*** (old Fig. 6) for updates.

We chose 15 km as the width of the swath to fully cover the topography surrounding the trunk of the Saryjaz river, which we added to ***Fig. 7***, and location of the swath to supplementary information.

L474-476: this is wrong. 10Be derived paleo-denudation rates reconstructed in the Eatsern Tianshan increase from 9 to 4Ma and then remained steady (see fig 11 of Puchol et al., 2017). Moreover, Kudriavtseva et al. worked in western Kyrgyz Tianshan, not in the eastern region.

We appreciate this correction and changed the text to:

In the eastern Tian Shan, denudation rates increased from around 9 to 4 Ma, and then remained relatively steady, based on estimates derived from in-situ ^{10}Be in dated sedimentary records.

Figures

Figre 2c: it is very hard to see the Ksn "lines" and their values

Please see ***Fig. 3*** in our updated manuscript with both knickpoints and streams colored by k_{sn} values. Besides, ***Fig. 5*** (old Fig. 4) also shows a zoomed view of k_{sn} values especially downstream of the 'U-turn'.

Figure 3: the definition of the Onset time is unclear. Is it an absolute time? A duration with respect to the onset of base level drop ? or the time since knickpoint started to migrate (ie time before present)?

This is a very good point. The "onset time" here indicates the inferred time since knickpoints started to migrate. We rephrased to "***Duration of knickpoint migration***", which is more precise.

Figure 6: as said before, what is the width of the swath profile? How were defined the low- vs high-relief zones? Reliefs >2000m can be found in the so-called low-relief zone while reliefs <500m are present in the "high-relief" zone.

Please see response to the comment above.

Figs. 6a-c: horizontal scales should be the same for all these 3 figures and the same than figure 6a. Moreover, I would put those figures below each other and aligned to figure 6a. It is unclear which type of knickpoints is shown here. I guess transient ones but please clarify this in the caption.

Distance along the river is needed to do a proper linear regression of the data points, but swath profiles necessarily show distances along the swath. We add the reference point of the U-turn to help readers align the data. Repeating the plots based on distance along the swath would take substantially more space for very little added value.

I see only 3 “upstream knickpoints” in figure 6a while 5 are shown on figs 6b and c.

We updated the figure to correct this omission. Please see the new ***Fig. 7*** (old Fig. 6) for the locations of upstream knickpoints in the swath profile, and new ***Fig. 3*** for a map view.