

Dear Editor and Corresponding Authors,

It was a pleasure reading this contribution from Wang et al. The manuscript uses field mapping, ^{14}C , and OSL data collected from river terraces to constrain the incisional history of the Beida River since ~ 24 ka. The resulting chronology is then coupled with a numerical incision model to constrain the duration and magnitude of an incision rate increase responsible for creating a prominent knickzone on the river. Finally, the authors argue that variations in the Beida River incision rate that have occurred during the Holocene were primarily driven by increases in precipitation related to the increasing influence of the Southeast Asian Monsoon. The incision rate began to increase at ~ 9.5 ka due to the expansion of the monsoon into the North Qilian Shan, and a higher intensity increase responsible for creating the knickzone occurred sometime after ~ 4.7 ka due to strengthening of the monsoon. The numerical model suggests that the knickzone was created over a maximum duration of ~ 700 years at a minimum incision rate of 50 m/yr.

This is a very interesting study containing novel geochronology data, modeling techniques, and insights into the relationships between climatic forcing, lithologic variations, and river incision. I believe that the manuscript can be suitable for publication after some minor revisions. I have only one suggestion that rises to the level of a major comment concerning the veracity of the implied model result, and I think that all my comments can be addressed without any major modifications to the overall structure of the manuscript. I'd also like to echo the suggestion from RC1 and RC2 that the authors include chi plots of the Beida River and its tributaries to strengthen their argument.

Overall, I find this study to be an extremely interesting example of how regional climate forcing can significantly impact the pace of river incision, and I look forward to reading the revised manuscript. If the authors have any specific questions regarding my comments, please feel free to contact me.

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Major Comment:

My only major comment concerns some of the assumptions that go into the coupled incision model and the discussion of its results. The model assumes a simplified Beida River incisional history occurring in three stages: Stage 1 (~9.5 – 4.7 ka, ~5.63 m/kyr), Stage 2 (~4.7 – 3.2 ka, ~18.6 m/kyr), and Stage 3 (~3.2 ka to present, ~11.56 m/kyr). Using the incision model, these discrete stages suggest that the knickzone was created over a duration of ~700 years with a minimum incision rate of ~50 m/kyr. The actual incision history was likely much more complex, but I think that simplifying it into three stages for the sake of this analysis is perfectly justifiable.

However, I am curious to know how slightly different yet plausible incision scenarios might affect the model results. In particular, the boundary between Stages 2 and 3 is inferred to be 3.2 ka. However, this is only based on a single datapoint (a single OSL sample from sampling site B). One could imagine a more complex incisional history between 4.7 ka (the boundary between Stages 1 and 2) and present. For example, perhaps the incision rate has gradually decreased since ~4.7 ka, implying that Stages 2 and 3 would display an exponential curve on Figure 7 rather than two linear segments. Alternatively, perhaps the terrace at site B was abandoned much later than implied by the ~3.2 ka depositional age, justifying the inclusion of a fourth Stage in the model.

I think the authors should add a few sentences in Section 5 that discuss how their model results might vary under different circumstances. Importantly, they should address how their conclusion that the knickzone was created over ~700 years at a minimum rate of ~50 m/kyr might vary. I don't think they need to perform a full sensitivity analysis (though that would be interesting!), nor do they need to quantify the duration and magnitude of knickzone formation under specific, alternative conditions. Rather, I think they should just briefly list some plausible scenarios that could either increase or decrease the implied duration of knickzone formation and qualitatively discuss the effects. I think this could be done just by adding a few sentences to Section 5.1.

I listed this as a major comment because the 700-year duration and 50 m/kyr incision rate are a major takeaway of this study (I expect that they will immediately grab the readers' attention in the abstract). Therefore, I think it is very important to put them in context.

Minor Comments:

1. I think that Table 1 could benefit from two changes. First, it is extremely difficult to tell which sample name the 1-sigma and 2-sigma results correspond to. I recommend realigning the data in the calibrated age column so that their corresponding sample names are unambiguous. Second, I recommend adding a column showing the elevational position of each sampling site relative to T1. This will allow the readers to quickly reference the relative age of each terrace.
2. I have an issue with one of the authors' arguments against the knickzone being a lithologic feature. I do agree with the authors' interpretation that the knickzones on the Beida River and its neighboring rivers were created by climatic forcing and are not likely the result of lithologic variations (i.e. variations in K in equations 1-3). The strongest evidence ruling out a lithologic origin is the lack of a knickzone preserved on the T1 strath (lines 240-241) and the continuous projection of terraces across patch 2 (Figure 6).

However, consider this excerpt from Lines 236-239: **“In the neighboring Maying and Hongshuiba River, similar to the Beida River, present river channels also incised into Late Pleistocene fill terraces..., forming prominent knickzones 10-15 km upstream of the mountain front (Figure 2). This suggests these knickzones share similar origins, reflecting regional forcing. Local variations of lithology would thus be an unlikely cause for knickzone formation.”** If I am interpreting this correctly, the authors argue that if the Beida River knickzone was a lithologic feature,

we would not expect to see similar knickzones on the Maying or Hongshuiba rivers because the lithologic variations along the Beida River would not likely be present along the other two rivers.

I disagree with this reasoning. I am unfamiliar with the regional geology of the North Qilian Shan, but I can see on Google Earth that there are SW-dipping lithologic contacts along the Beida River corridor. These contacts are roughly parallel along their NW-SE strike, and so without a more detailed geologic map, it seems entirely reasonable that the Beida, Maying, and Hongshuiba rivers might cross the same bedrock units in each of their knickzones. Also, it appears on Google Earth that the Beida River crosses lithologic contacts (marked by stark color contrasts) at the transition from patch 1-2 and patch 2-3.

I recommend that the authors examine the spatial relationships between the Beida River patches and the underlying lithologic contacts. I still agree with their interpretation that the knickzone is most likely a transient feature created by climatic forcing, but if it happens that the knickzone is underlain by a low-erodibility rock type, then this might be a contributing factor worthy of a brief discussion. Alternatively, if the boundaries between the three patches do not correspond with major lithologic transitions, then this will strengthen their interpretation. The authors could also extend this analysis to the other two rivers.

Since I am unfamiliar with this region, I do not know if the authors can obtain a geologic map with enough detail to perform this analysis. If they cannot, I would be satisfied if they just removed their argument in Lines 236-239 and relied on their evidence in Lines 240-241.

3. Between Lines 363 to 371, it is unclear to me whether some sentences are information from Tan et al., 2018 (cited in Line 362) or the authors' own interpretations. I think that these require clarification. Specifically, these lines:

- Lines 363- 364: **“Between 24 kyr and 9.5 kyr B.P., the Beida River drainage was under the dominant influence of the arid westerly moisture source.”**
- Lines 365-367: **“During the Early Holocene, the humid Asian monsoon expanded to the central North Qilian Shan, where it affected the Hei He main stem and filled Juyanze lake to its highest lake level.”**
- Lines 368-371: **“During the mid-Holocene, the Asian monsoon grew stronger, starting in the Hei He drainage around 5.4 to 5.1 kyr B.P., and then expanding further to the western North Qilian Shan a few hundred years later. This peak of monsoon influence lasted less than 700 ± 340 yr, which led to an increase of precipitation therefore an increase of water discharge and incision rate.”**

If previous research demonstrated that the Asian monsoon expanded into the North Qilian Shan in the early Holocene and then strengthened during the mid Holocene, then this independently supports the authors' conclusions that the knickzone was created by mid Holocene climatic forcing. However, it is unclear to me whether this was previous research or a novel idea presented here.

4. In Lines 335-346, the authors argue that a discharge contribution from melting glaciers would not have been enough to trigger knickzone formation. While this intuitively seems correct to me, I'm not sure that I agree with their reasoning. They cite metrics of modern glacial melt and discharge contributions, compare these to possible conditions in the past, and conclude that mid Holocene melt contributions could not have triggered knickzone formation. However, the authors have not quantified the actual discharge increase necessary to create the knickzone. Without this information, I don't think that the other metrics presented here definitively rule out glacial melt as the primary driving mechanism. I recommend modifying the sentence in Lines 345-346 to clarify this.
5. Can the authors comment on how the long-term (i.e. averaged over several glacial / interglacial cycles) bedrock incision rate may compare to the tectonic uplift rate? Beida River incision clearly outpaces uplift during the transition from glacial to interglacial conditions, but averaged over 10^5 -year timescales, is the river more or less in steady-state? Quantifying this long-term trend requires dating

several generations of strath terraces (see [Pederson et al., 2006](#)), and so this may not be possible to do for the Beida River. Still, it might be useful for the authors to add a few sentences discussing this in Section 5.

Line-by-line comments:

- Line 114: “insect” should be “inset”.
- Line 128: “A is drainage area” is unnecessary. Drainage area does not appear in this particular formulation of the stream power equation (Q is used instead), and so the variable “A” does not need to be defined.
- Line 135: “drainage area of Beida River” should be “drainage area of the Beida River”.
- Lines 136 and 137: “km²” should be “km²”.
- Lines 158, 245, and 384: “Beida River” should be “the Beida River”.
- Lines 169 and 170: “above present riverbed” should be “above the present riverbed”.
- Lines 172-173: Is site C on the T1’ terrace? If so, shouldn’t the samples imply that T1’ was abandoned AFTER 9.5 ± 0.16 ka (not prior)? The changes that I recommended for Table 1 could help clarify this.
- Lines 172-180: While reading this section, I found it difficult to construct a mental map of the terrace position / age relationship due to their heights being listed relative to different surfaces. This is later clarified very well in Figures 6 and 7, but those figures are neither shown nor referenced for another 5 pages. I think the changes I recommended for Table 1 will be very useful here, because the readers will have already seen these data, and they can flip back to the table for a visual aid if necessary.
- Lines 198 199: Does this mean that none of the inset terraces below T1’ have exposed straths?
- Line 219: “Incision rate” should be “The incision rate”.
- Line 220: I think there may be unwanted spaces after some periods in this line. I think “between 4. 7 to 3. 2” should be “between 4.7 to 3.2”.
- Line 229-230: Tying into my previous comment concerning long-term incision rates, over what period of time is the average uplift rate ~ 0.6 ?
- Line 246: “It is likely that knickzone” should be “It is likely that the knickzone”.
- Line 381: “correspond” should be “corresponding”.
- Line 382: “dating of T2 terrace” should be “dating of the T2 terrace”.
- Lines 382-384: This sentence is awkward. I recommend breaking it into two sentences and changing “similar to that T1” to “similar to how T1”.
- Line 387: “similar age as T2 terrace” should be “similar age as the T2 terrace”.
- Line 388: “Shagou River” should be “the Shagou River”.