Review of Wang et al., Rapid Holocene bedrock canyon incision of the Beida River, North Qilian Shan, China for consideration in Earth Surface Dynamics

Summary of study:
Wang et al. use river terraces, geochronology (previously generated cosmogenic depth profiles and new ^{14}C and luminescence data), and river profile analysis to understand the late Quaternary incision history of the Beida River. The authors map several large fill terraces (previously dated with cosmo) and three inset terraces, described as strath terraces, that are dated using ^{14}C measurements of charcoal in and luminescence geochronology. The lowest of the large fill terraces (~24 ka) progrades and merges with an alluvial fan beyond the active mountain front. The three inset terraces are mostly, but not exclusively, found upstream of the active range front and are inferred to have been abandoned at ~9.5, 4.7, and 3.2 ka. Upstream of the mountain front, the river profile shows a large (bedrock?) knickzone. Field observations suggest that after deposition of the 24 kyr fill terrace, the river cut into bedrock, rather than reoccupied the same channel position, a history recorded by the inset terraces. The age and elevation of the inset terraces indicates rapid Holocene incision into bedrock that far outpaces tectonic uplift rates determined from the older, deformed fill terraces by Wang et al. (2020). The authors hypothesize that this rapid pulse of incision is related to the generation of the now headward migrating knickzone. Based on the incision data and using a slope-patch approximation (*sensu* Royden and Perron, 2013), the authors predict paleo-river bottoms and incision histories, absent tectonic uplift. Based on all of this evidence and analysis, the authors argue that the rapid Holocene incision is the result of climatically-induced changes in water discharge and sediment flux that caused the river to rapidly downcut. The formation of the knickzone is argued to be the result of this downcutting as the contrast in erodibility between the erodible alluvial fan downstream of the mountain front and the relatively more resistant bedrock upstream of the mountain front.

Major comments and recommendation:
I think that this is an interesting study that is well suited for publication is E-surf. The study presents new mapping and geochronology and puts them together in a nice way that combines field results with a simple model for bedrock river incision. The results are interesting and highlight the role of climate and variable substrate resistance to erosion as key players in determining incision rate, bedrock channel morphology in response to external perturbations. That said, I think that the presentation can be cleaned up, which will benefit the impact of the study. Specifically, I think the quality of the figures needs to be improved, and perhaps a few figures can be added to help guide the reader through the observations more easily. I also think a little restructuring might help improve the flow of the manuscript. There are some aspects of the analyses that I found unclear and some additional analyses that could be performed to help better support the interpretations forward in the study. Below I highlight these and some other key points that I think need to be addressed before publication.

(1) Figures: Many of the figures are too small and difficult to follow. Some figure additions are needed (e.g., terrace stratigraphy in key locations as in Wang et al. (2020) Figure 4b,c and additional river profiles in the region). I also have several specific suggestions that I think can help improve the presentation of the figures, as well as a couple of suggestions for additional figures. I include the specifics in my detailed line-by-line comments below.

(2) Structure: I struggled quite a bit with the structure and flow of the manuscript. I think that part of this has to do with the legacy of revisions of a previous version of the study. The motivating observation is the observed knickzones in the river profiles, but the key data is the incision history from the terraces. A cleaner presentation of the results might focus on the terraces first and then discuss the

characteristics of the river profile in the context of the incision rates and patterns. This could also serve to streamline the discussion because the terraces record the incision pulse, which presumably generated the knickzone due to variable substrate erodibility. Currently, from the results onward, the terrace and river profile discussions are mixed together, making a really interesting story challenging to follow. I think that this mixing of analyses and discussion makes it more difficult for the reader to clearly visualize the key points of the conceptual model for knickzone formation – climate-driven incision pulse causes downcutting in the alluvial fan downstream of the mountain front and an upstream change in substrate erodibility results in the formation of the knickzone. This is in the paper, but it could be presented more clearly and concisely.

(3) Support for the preferred interpretation (and maybe some additional restructuring): I think the authors can rely more on the geochronology to support their favored interpretation. For example, the emplacement T1 fill terrace corresponds (roughly) with rapid cooling around the LGM and T2 the timing of the preceding full interglacial. This information is not new but presented in Wang et al. (2020), so it is unclear to me why the apparent correlation between fill terrace deposition and climate isn't presented in the background section. Doing so would place the link between climate and incision-aggradation cycles in the reader's mind early and key them into thinking about temporal links between the terrace stratigraphy and climate as they read through the rest of the manuscript. Additionally, I think that the authors can more strongly interpret the timing of inset terrace deposition with regional and global climate records and that this would help their arguments. For example, T1' is abandoned roughly at the Preboreal-Boreal transition. This seems like a missed opportunity in the discussion.

Furthermore, in looking at Google Earth, I think observations from adjacent drainages can be used to the benefit of the climate and discharge interpretations. Importantly, it is evident from Google Earth that the only rivers that incise into the alluvial fan/fill north of the range front are those that drain high elevations. This is clearly recognized by the author's and mentioned in their interpretations, but what is missing are observations from satellite imagery (or Google Earth) and river profiles (not just from the trunk channels) from adjacent rivers to help support this kind of interpretation. This would be a small added figure/analysis but could help convince a skeptical reader.

(4) Slope Patch: I was a little confused by some aspects of the slope patch approach used here. I understand the formulation in Royden and Perron (2013), and I see what the authors are trying to do. My main question has to do with the observation of variable channel width that is not included in the analysis here. In the slope patch formulation in Royden and Perron (2013), they use the general stream power model (E = KA^mS^n). This equation presumably accounts for/assumes hydrologic (Q) and hydraulic (W) scalings with A encapsulated by exponent m. To handle these along stream changes, Royden and Perron (2013) use a coordinate transformation of distance, introducing the non-dimensional distance chi. From chi, along with other non-dimensionalization, Royden and Perron (2013) come up with their elegant slope patch solutions.

As I understand it for this application, Q is assumed to be uniform (or approximately so) over the length of the study area; however, W is shown to vary. It seems that equation 3 only works as applied in the manuscript if W is also uniform with only S and I varying along the channel length. It seems like the observed variations in W make it difficult to apply this simplified version of the slope patch approach as presented in equations 2-5.

That said, I do think that this analysis is useful, but I think that the caveats and assumptions made in this simple analysis need to be discussed and explained fully. As written, I think many readers might not realize that this analysis is limited to channel sections where Q and W are assumed to be uniform or that one needs independently constrained incision records for this kind of analysis. These requirements make the approach limiting and not general. I think these limitations/data requirements needed to be more clearly articulated. I also think the slope patch discussion could be rounded out by linking the modeled changes in incision rate and river profile geometry through time back to the observed slope patches in section 5.2. For example, how close are the observed and modeled slope patch gradients when run to the modern?

I realize this is a long comment that can probably be handled with the addition of a few sentences in the methods and discussion; I just wanted to make sure that I completely communicated my questions and tried to articulate where and why I was a little confused.

(5) Terrace stratigraphic relationships: It is really hard to understand the general terrace stratigraphy without photos and figures. One suggestion is to add a composite terrace stratigraphy for several locations along the river to illustrate the relationships between the fill terraces and the inset terraces. What I have in mind is something like figure 4b and c in Wang et al. (2020). It was hard for me to understand the context of the terrace observations without a figure like this. Also, from figure 4 in Wang et al. (2020), it looks like the T1' terrace is a cut terrace (meaning it is cut into the T1 fill terrace) and is not a strath terrace. Is this correct? If so, does it become a strath terrace up valley? Also, how might this play into the interpretations forwarded in the study (i.e., strath vs. cut terraces)?

In summary, I think this is a cool study, well suited for publication in E-surf. With some moderate revisions, the study can be strengthened, and the arguments in support of the preferred interpretation strengthened.

If any of my major or detailed comments are unclear, please reach out for clarification.

Best,

- Sean F. Gallen

Detailed line-by-line comments.

L 13: incision accelerated from what value to 25 m/ky?

L 14: "faster" rather than "larger"?

L 17: "We interpret that this period of increased…"?

L 28-29: The role of lithology is critical here and probably deserved more discussion in section 5 somewhere. Without the change in erodibility due to the lithology change at the range front, the knickpoints would be causing the profile to relax (assuming elevated incision is due to an increase in Q, as suggested). It seems worth explaining this to the reader in some detail later.

L 43 [Figure 1]: Make the map of figure 1 larger, label some of the key geographic features mentioned in the text on the map (e.g., Hexi corridor), provide a zoomed-in shaded relief map of the location with the three highlighted rivers, and combine the current figure 1 and 2. Doing this would introduce the reader to all of the general key observations without having to flip back and forth and look in google earth to understand much of what is written. This might also be a good location to show some google earth images of the studied and adjacent rivers (see part of my major comment 3 above). Also, are the glaciers in this map active or from the Pleistocene?

L 51 [Figure 3]: The photos in figure 3 are very small but really important. Please make them bigger. It would also be useful to add some photos showing the key relationship that the river didn't reoccupy the same valley filled with T1.

L 66-72: Is there any information on Pleistocene ELAs or glacial extents in this region? It might be relevant.

L 85-94: The typical convention in name/labeling terraces is that the older units have lower numbers and the younger units have higher numbers. This is the same convention as bedrock map units. It appears that in this study and in Wang et al. (2020) the opposite is used (older have higher numbers than younger). Because of this, I found this section very confusing the first time I read it until I saw figure 5. Also, why not label the inset terraces with something like letters? The lack of naming for them makes reading a little awkward.

L 99: I can't see the reverse fault offsets in figure 1.

L 114: inset rather than insect.

L 119 [Table 1]: Can a row be added to link these ages to the terraces they were collected from? Having a composite terrace stratigraphy figure preceding this table and naming/labeling the inset terraces will make this pretty easy to do.

L 124: see my major comment 4 regarding this section.

L 128: Need to link A and Q in some kind of statement.

L 136: km^2

L 137: km^2

L 162-164: These changes in width impact hydraulic geometry and thus shear stress imparted by a flow of a given magnitude. How might these changes in channel width impact the assumptions made in the simplified slope patch calculations?

L 171-182: This section would be easier to follow if the inset terraces were labeled or named, and there were some figures showing their composite stratigraphy in a few key locations along the river.

L 184 [Figure 4]: can the y-axis label be oriented as in figure 2 with the labels reading from bottom to top? Also, I don't understand panel b, and I assume the y-axis is mislabeled (should be elevation rather than width). How does the channel bed have negative elevation?

L195-198: Photos of these critical relationships would really help!

L 204 [Figure 5]: I don't find panel **a** very helpful because the fill terraces (besides T1) aren't the main part of this manuscript, but the inset terraces are. I can't see the distribution of the inset terraces here with respect to T1, so I don't get much out of this panel. Panel c should be enlarged to make it easier to read.

L216-218: What about tectonic subsidence? No correction is needed for that?

L235-237: It would be nice to see some images of this from Google Earth or something. These observations are very helpful to the interpretations presented here.

L239: Be careful here. The change in erodibility of the alluvial fan and the bedrock is essential to explain the knickpoint. The driver of incision might be a temporal increase in Q, but if that occurs in uniform substrate, the river profile will relax, forming an "inverted" knickpoint that migrates upstream. Here, the inferred elevated Q forces the river gradient to relax in the alluvial fan (incision), and this base level fall steepens the "harder" bedrock rock reach upstream. The interaction of the inferred climate change along with the spatial change in substrate erodibility explains the observations. Both are required.

L245-249: Yes, changes in substrate erodibility are needed as stated here.

L305-324: It seems like this would be better placed before the slope patch discussion. It also makes me wonder about the utility/generality of the slope patch approach used here. The Royden and Perron (2013) formulation implicitly takes changes in width into account via the calculation of chi, here that doesn't work. It might be helpful to highlight this point earlier in section 3.3 and mention that this point will be discussed in detail in section 5.

L 325: It seems like showing the relaxation of the river profiles beneath the incised alluvial fan surface for a few of the rivers would help support the point that there is a reduction in the river channel gradient beyond the range front. In the framework used here, the two most obvious explanations are changes in water discharge and/or sediment flux. Considering that the rivers that show this behavior all drain high elevations, the interpretations forwarded here seem reasonable to me. The key observation that isn't explained well in my mind is that the river gradients have declined downstream of the knickzones in the alluvial fans. The gradient decline, coupled with the change in substrate erodibility, generates the knickzones, correct?

L 378-395: I am left wondering if it might be helpful to explain the possible links between climate and the previously dated T1 and T2 fill terraces earlier in the study. This is all based on published data and will help establish a link between climate and geomorphology in the study area before diving into the inset terraces. It can be revisited here, but much of this section is related to previous studies and knowledge gained, so it seems better suited for the background.