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Comment on esurf-2021-37

Anonymous Referee #1

Referee comment on "A multi-proxy assessment of terrace formation in the lower Trinity River valley, Texas" by Hima J. Hassenruck-Gudipati et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-37-RC1, 2021

Review of 'A multi-proxy assessment of terrace formation in the lower Trinity River valley, Texas' by Hassenruck-Gudipati et al.

Dear Editors,

I have carefully read the paper by Hassenruck-Gudipati and colleagues on a new method to distinguish between allogenic and autogenic fluvial terrace formation. In this review I will briefly summarize the paper, provide an overall statement, followed by a list of major concerns, minor concerns and line-by-line comments.

Hassenruck-Gudipati and colleagues aim to distinguish between externally (allogenic) and internally driven (autogenic) fluvial terrace formation mechanisms. The identification of terrace formation mechanisms is relevant when terraces are studied as archives preserving information on climatic or tectonic conditions of the past. In order to distinguish between autogenic and allogenic terrace formation, the authors couple statistical analyses of terrace elevations with paleo-hydrological reconstruction from channel remnants preserved on terrace treads. All data were derived from a high resolution (1m) lidar digital elevation model.

The study site is the lower Trinity River valley in Texas, where earlier studies had identified three sets of terraces and related their formation to climatically driven changes in sea-level and/or water discharge. In this study, the authors challenge the earlier interpretation and conclude that most likely only the uppermost terrace level was formed by an allogenic process, while the lower-most level was caused by meander-bend cut-off (=autogenic). For the intermediate level both scenarios are possible.

Overall, we are still lacking methods allowing us to reliably distinguish terraces formed by allogenic processes from those formed by autogenic processes. As such, a new method addressing this problem is relevant. However, in the current version of the manuscript I, in parts, do not follow the reasoning of the chosen approach and, as such, am not convinced yet that the presented method allows to distinguish between different types of terrace formation. As the presented results cannot be validated by independent data and also, at least partly, disagree with previous interpretations, I think a more detailed evaluation of the 'terrace-elevation' method, as well as a discussion on the contradictory conclusions should be included prior to publication. I will explain those points in detail below. Also, I make some suggestions about potential changes in the structure, as I had the impression that results are partly presented in the Data and Methods section and partly in the Results section.

Major points

The approach presented in this study largely depends on the terrace-elevation dataset, derived from a 1m resolution lidar DEM. For each of the 52 preserved terrace segments, the authors extract the median elevation and the according interquartile range (line 134, Fig. 3A). In a second step, the authors calculate the detrended elevation for each segment, by subtracting the elevation from plane fitted to the modern valley floor elevation (extracted from a 10 m resolution DEM). This approach raises several questions. First, from my understanding, this approach assumes that the elevation of a single terrace above the modern channel is constant along the valley. However, this is not necessarily the case, especially not when terraces were formed due to changes in upstream discharge and/ or sediment supply (e.g., Tofelde et al. 2019; Poisson and Avouac 2004). In such cases, the elevation of a single terrace above the river decreases in downstream direction. Because terrace elevation above the modern river can vary through space and terrace width might vary along the valley, the approach of representing each terrace only by a single (median) value might introduces biases, especially since individual terrace segments are up to 10 km in length (lines 278 and 279). Second, the detrending is done using a fitted plane to modern channel elevation data. Why was the modern elevation data extracted from a DEM with a 10m horizontal resolution and not from the 1m DEM? In general, most longitudinal river profiles are characterized by a concave-up shape. Therefore, I am wondering why the authors decide to fit a non-curved plane to the modern channel along a ~ 40 km channel reach? What is the RMSE of the plane fitted to the modern channel elevation and how do the according residuals look like? Wouldn't it be better to fit several planes to individual segments along the river in order to capture any potential curvature? The authors indicate that the modern channel slope is roughly constant along this reach (lines 173-174). However, figure 3 and in particular figure 4 show that a fair amount of datapoints (~half of the lowest terraces) end up with a negative detrended elevation. Negative elevations are not possible and a median detrended elevation for the low Deweyville terraces of 3 cm (line 141) seems very unrealistic. I am not familiar with the study area, but I can't image that the previous studies (Blum et al. 1995; Garvin, 2008) have mapped river segments of less than 3 cm above the modern river as terraces.

Taken together, I am wondering if the presented approach is actually able to capture terrace levels of a few meter elevation difference, given the uncertainty of the method itself? I suggest to add more details on the quality of the fitted plane to the modern channel (e.g., RMSE, residuals, etc.) and discuss limitations of the methods including the negative detrended values. If the error of the methods itself is on the order of a couple of meters, can it still be used to distinguish terrace levels separated only by a couple of meters?

The second main dataset is the reconstruction of paleo discharge from preserved channel geometries on terrace treads and grain size data. I think this is a reasonable approach that was performed carefully. The results indicate that water discharge was about twice as high during the formation of the lower and middle terraces compared to modern times and times when the highest terraces were formed (Fig. 8, lines 228, 233-235, 304-308). Following theory (e.g., Parker 1979; Wickert and Schildgen 2019), a doubling in water discharge (while keeping all other parameter constant) will result roughly in a channel gradient reduced to half, triggering incision and potential terrace formation. Therefore, I do not follow the conclusion that the highest terrace was formed due to an external forcing, while the lower most and potentially the middle terrace level were not. I would argue that the snapshot in time preserved in the terrace surfaces is the switch from aggradation or stable bed elevation to incision. If the lower two terrace levels indicate particularly wet conditions, isn’t this rather indicating that those terraces were formed due to an allogenic forcing (= increase in discharge), while dry conditions during the formation of the abandonment of the highest terrace argue against discharge increase as a driver for those oldest terraces?

Taken together, I think the presented approach is interesting. But due missing information on the reliability of the elevation dataset and the reconstruction of wettest conditions during the formation of the lower two terrace levels, I currently can’t follow the conclusion of the authors (e.g., lines 17-18) that only the uppermost terraces were formed by an allogenic mechanism and the lower terraces by an autogenic mechanism. As there is no independent dataset to validate the conclusion drawn in this study and the conclusion also differs from previous work (lines 56-59), I think it would be useful to discuss those discrepancies and provide arguments for why the conclusion presented here is more reliable than the previously proposed mechanism.

Minor points

Introduction: In my opinion the introduction is in parts incomplete. The main goal of the study is to distinguish between autogenic and allogenic terrace formation from topographic analyses. As such, the introduction should contain a complete description of the different terrace formation mechanisms and the resulting topographic differences. Lines 38 to 45 briefly explain the two general mechanisms of allogenic forcings – downstream variability in base level elevation and upstream variability in the water-to-sediment ratio. And lines 51 to 54 briefly list some, but not all the mechanism of autogenic terrace formation. For example, the complex response/internal feedback mechanism described by Schumm (1973) and further studies (Slingerland and Snow 1988; Stanley A Schumm and Parker 1973) could be added here. Later in the manuscript, the authors investigate the number of preserved meander bends on each terrace. If this is a topographic indicator of terrace formation, I suggest to introduce it as well in the beginning of the manuscript. Same for paired versus unpaired terraces, which is only introduced as an indicator during the discussion (lines 317-318). So, what I miss in the introduction is a comprehensive description of driver-dependent topographic differences in the resulting terraces. What are expected differences, for example, in terrace surface slope and according differences in terrace elevation above the modern channel, channel width, terrace surface age, the predominant formation of paired or unpaired terraces, number of preserved bends, average size of terrace segements etc. for each mechanism of terrace formation (upstream allogenic, downstream allogenic, autogenic). For example, Poisson and Avouac (2004) investigated terrace in the Tien Shan formed by an increase in water discharge through time. The resulting terraces decrease in gradient, resulting in higher elevation differences between terraces and the modern channel at the upstream end. Tofelde et al. (2019) have investigated topographic differences in terrace and channel geometries in response to upstream and downstream perturbations using lab experiments. Those experiments also show a decrease in river channel gradient relative to terrace surface gradient for terraces formed by a decrease in the sediment-to-water discharge ratio. Malatesta et al. (2017) have modeled autogenic terrace formation numerically, and showed that those terraces were mostly unpaired. These are only some examples, but I think introducing all investigated parameters and summarizing expected differences caused by autogenic and allogenic drivers will help to later argue for or against one or the other driving mechanism.

Data and methods & Results: My impression was that some of the results were presented in the Data and Methods section (e.g., lines 258-259, 262, 265-267) and other results in the Results section. When reading it felt like I was going through the same plots twice. I think this mainly results from the fact that most figures showing results are already referenced in the Methods section and some of them again in the Results section. Figure 10 (elevation difference between terraces and number of bends), in contrast, is only presented in the methods section, but not in the Result section. I suggest to reduce the Data and methods section to a brief description of all applied methods (without any references to the figures), and concentrate the description of all datasets in the Results section.

Figure 1&2: I would find a regional map in addition to the map of the study site very helpful. Currently it is hard to estimate, for example, the distance to the ocean. Also, figure 1 and 2 miss coordinates, which makes it complicated to find the study site for example in google earth.

Line-by-line

Lines 14-16: I would expect a clustering of terrace elevation for some allogenic drivers, but not for all (e.g. not necessarily for changes in water-to-sediment discharge ratios); see comment above.

Lines 126-127: This long list of numbers is a little confusing. In which order are the numbers given? For clarity, I suggest to separate them in two or four lists, respectively.

Line 133: Why was a 5m grid used here and not the higher resolution 1m grid?

Line 140: Comma between ‘elevations’ and ‘is plotted…’?

Lines 158-162: But don’t autogenically formed terrace also preserve the slope? Or is the point to stress that autogencially formed terraces are on average shorter than allogenically formed ones? If so, it might be interesting to include a plot showing the size distribution of the different terrace segments for each level?

Lines 184-185: This sentence is confusing. I suggest rephrasing.

Line 221: A legend could be added indicating which datapoint size relates to what terrace length.

Line 248-249: Remove ‘decrease in…’?

Lines 279-281: I suggest to introduce this parameter (paired vs. unpaired terraces) already in the introduction.

Lines 322-325: Also, the number of channel bends used as an argument to distinguish between allogenic and autogenic drivers could be introduced earlier, e.g. in the introduction.

Lines 257-258: The sentence sounds strange, I suggest rephrasing.

Lines 383-384: If those terraces are formed by meander-bend cut-off would they expected to be up to 10 km long? This again makes me think that a plot showing the distribution of terraces lengths might be helpful.