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Interactive comment on "Structural variations in basal decollement and internal deformation of the Lesser Himalayan Duplex trigger landscape morphology in NW Himalayan interiors" *by* Saptarshi Dey et al.

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In their paper "Structural variations in basal decollement and internal deformation of the Lesser Himalayan Duplex trigger landscape morphology in the NW Himalayan interiors" authors Dey and others incorporate field observations, measurements of rock strength, and topographic analysis to try to better understand the underlying geometry of the Himalayan sole thrust and the patterns of active uplift in a specific part of the Lesser Himalayan Duplex. The paper is interesting, but it is missing a lot of fundamental detail and it's hard to follow the logic all the way to the conclusion. In my detailed

comments that follow, I try to highlight various portions of the manuscript that need a bit more description or justification. Many of these potentially rise to the level of 'Major Issue', but before I get to these, I highlight what I consider to maybe the largest issue with the manuscript in its current form.

Specifically, at present, it is challenging to see exactly how they arrive at their major conclusion (i.e. the added structural complexity within the sole thrust and the presence of two discrete ramps and flats). For example, in their summation figure 4, they highlight two pairs of low relief and high relief areas, which they relate to the underlying fault geometry corresponding to flats and ramps, respectively. However, in Figure 2 where they are showing the river data, it's not clear where this middle low-relief area is. The high and low relief patterns are similarly not particularly clear in Figure 3. Looking to the supplement (Fig S3), I similarly can't really figure out where the middle low relief area is supposed to be. Does this show up in other metrics (e.g. local relief) etc? Maybe more importantly, the direct jump from these patterns to the hypothesized structure is a bit abrupt. What observations are there to reject active surface breaking faults (as has been proposed by some authors mentioned in this paper)? Are the authors actually rejecting surface breaking faults (i.e. they show what might be one in Figure 4, but it's unclear whether they consider this an active portion of ramp 1 or a passive, dead thrust now be translated on the southern flat)? The main text talks only about ramps but the caption for figure 4 says that maybe there is surface breaking fault? How consistent is this other, more general studies regarding the topography over growing duplexes or movement over flats (references are presented in my detailed comments) Much of the uncertainty and ambiguity in the way data was collected or analyzed and choices made that I highlight in my more specific comments kind of feed into the uncertainty with regards to the final result, but even if all those are addressed, there still needs to be more connection drawn out between the observations they present and the model they propose. Ultimately, this could be a strong contribution showing how topographic observations and field observations can be incorporated to tell us something about deeper structure of orogenic systems, but it's not quite there at present. I hope that my comments are useful to the authors to help them improve their manuscript.

Line-by-line comments:

L35-47 – Without some sort of figure, this opening assumes a fair bit of knowledge on the part of the reader on the location and geometry of the major Himalayan structures. While many are passingly familiar with these, it might be advisable to include a simple cartoon illustrating these structures and there general location with respect to topography. Maybe add a panel to figure 1 that accomplishes this? At least referencing figure 1 as is here could help, but not all of these structures are on here (MFT or STDS) and there are additional structures on Figure 1. Since much of the rest of paper hinges on which structures are active or not, not knowing where they are is kind of a detriment. Especially putting the LHD into structural context with these other structures seems crucial (and again, this can all be in a cartoon, not asking for a balanced cross section or anything). Other worthwhile question to consider which perhaps can put this issue in context, if this was any almost any other mountain range, would it be reasonable to expect a reader to know the relationships between the major local faults in an article published in a widely read, general geology journal like ESurf? You could cite fig S1 / S2 here, but I would argue that the knowledge of this information is sufficiently important to the main point of the paper that such a figure should be in the main text.

L216-217 – You need to explain a little bit more about how you're doing your basin wide statistics. It's not clear from this description, and the representation of it in Figure 3 is confusing (i.e. where are the basin boundaries, etc?).

L246-248 – RE the specific stream power calculation (1) you should state here in your methods that you assume constant discharge (not relegate it to the caption of Table 1) and perhaps more importantly (2) you need to justify that a constant discharge is applicable here. On figure 3, this is traversing >160 km of river distance and potentially traversing some large gradients in precipitation. As a worst case, you should be able to use the available TRMM data for the region to estimate discharge (this is simple with

TopoToolbox that you are already using as you can calculate flow accumulation with a precipitation raster is an optional input). Is there any discharge data in the region to compare this to? Perhaps you have good reason to assume constant discharge, but until that is shown in the paper, it's hard to know how to interpret the SSP data (or whether it should be believed at all).

L256-265 – Part 1 : Your measurements are your measurements, so you'll need to make do with what you've got, but it is worth discussing/addressing why you only collected 8-10 per location as this is ∼1/2 to 1/3 of the number of measurements thought to be needed to be robust (e.g. Niedzielski, T., Migon, P., Placek, A., 2009. A minimum sample size required from Schmidt hammer measurements. Earth Surface Processes and Landforms 34, 1713–1725. https://doi.org/10.1002/esp.1851). Perhaps it would be worth while considering pooling results from units/lithologies you consider to be similar and to get a larger N and seeing how those compare to the small N individual sites (i.e. if you have 5 sites all in the same unit with 10 measurements each, look at the statistics of the aggregate population of 50 values and see how those compare to the statistics of each of the 5 measurement sites). Such an analysis might help to alleviate some concerns, but there will remain lingering issues with a small N for each site. Similarly reporting of the raw values in a supplemental table and considering the standard deviation on the means when you're using them would be warranted (i.e. are the apparent differences in mean rebound values in Figure 3 'real'? how much of those differences would disappear or be less extreme if you considered the uncertainty?). Whether you follow my specific recommendations here or not, there needs to be more transparency with regards to these values and how reliable they may (or may not) be.

L256-265 – Part 2 : In general, a lot more detail is required to interpret your Schmidt hammer data. Later you describe significant fabrics in the rocks in the field (plus some nice field photos in the supplement). How did you consider this when taking your Schmidt hammer readings? Were you consistent with taking readings parallel or perpendicular to fabrics? Generally, where did you take these readings? Were they in the active channels? On the banks? Were the measured faces wet (which can bias readings)? Did you evenly space your measurements? Did you avoid fractures (which can bias readings)? Did you try to measure near fractures (which can bias readings)? There is a rich literature on concerns related to Schmidt hammer readings, like the Niedzielski paper above, but Aydin, A., Basu, A., 2005. The Schmidt hammer in rock material characterization. Engineering Geology 81, 1–14. https://doi.org/10.1016/j.enggeo.2005.06.006 is a good source for why knowing the answers to at least some of the questions posed above are relevant to interpreting your results.

L319-322 – The knickpoints might be easier to visualize if you included a chi (integral of drainage area) vs elevation plot as a companion to the long-profile.

L338-339 – More discussion / consideration of the influence of dams and reservoirs on your width measurements might be warranted in your methods (it wasn't clear until now that there were dams on the profile).

L342-343 – Similar to the comments earlier with regards to the SSP calculation, it is worth considering how the widths you measure compare to drainage area / discharge. This also gets to the constant discharge assumption, i.e. what is the change in drainage area along the portion of the river you're examining? Obviously your width measurements are not varying smoothly as a function of drainage area, but this is an important contributor to channel width that appears to be largely ignored.

L354 – But assuming a constant discharge right? That's what Table 1 indicates.

L384- An alternative / complimentary approach might be thinking about patterns in cosmogenic erosion rates with topography. At least based on a quick browsing of the OCTOPUS database (https://earth.uow.edu.au/) there are no cosmo basins directly in your area, but there are some not that far away (Olen et al, 2016, Munack, 2014, Dortch 2011). There is good evidence of relationships between erosion rates and ksn (e.g. Kirby, E., Whipple, K.X., 2012. Expression of active tectonics in erosional

landscapes. Journal of Structural Geology 44, 54–75.) so you could explore what an aggregate of ksn vs E data in the surrounding regions imply for your area (need to consider complicating factors like precip and rock type when transporting relationships and restrict your analysis to locally equilibriated basins, but maybe at least another option you could consider). These would also be more on a complimentary timescale compared to thermochron, which might be giving you a longer term average.

L452 – Are these cooling ages on any of your figures / maps? It might help for spatial context. Could probably add them to Figure 4 without cluttering too much.

L492 – Worth considering how consistent your observations are with other studies focused on the surface / geomorphic expression of a growing duplex. The paper from Adams et al, 2016 (Adams, B.A., Whipple, K.X., Hodges, K.V., Heimsath, A.M., 2016. In situ development of high-elevation, low-relief landscapes via duplex deformation in the Eastern Himalayan hinterland, Bhutan. Journal of Geophysical Research: Earth Surface 121, 294–319. https://doi.org/10.1002/2015JF003508) might be a relevant one to consider. Similarly, you are ultimately arguing for motion over a series of ramps and flats. You may want to think about the role that lateral advection could play in the observed topographic patterns (e.g. Eizenhöfer, P.R., McQuarrie, N., Shelef, E., Ehlers, T.A., 2019. Landscape Response to Lateral Advection in Convergent Orogens Over Geologic Time Scales. J. Geophys. Res. Earth Surf. 124, 2056–2078. https://doi.org/10.1029/2019JF005100). In general, need to think / talk much more about how your observations lead to the model you propose, because at present, this is not clear at all.

Figure 2 – In a, what is the jagged blue line floating above the profile? Is it fluvial relief? A blow up the profile? There's no mention of it in the caption. In general, applying some amount of smoothing to the profile (and its derivatives) would be appropriate as it is hard to see the signals you're trying to highlight with the noise from the DEM superimposed.

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