Review comments and answers

ESD manuscript (esurf-2020-37)

In their revised manuscript, ***'Implications of the ongoing rock uplift in NW Himalayan interiors'*** Dey et al., combine topographic analyses with a variety of field observations, dating of Quaternary deposits, and synthesis of previous work to explore the extent to which the surface geology and geomorphology helps to constrain the subsurface structural geometry in the northwestern Himalaya. The paper is improved from the original submission and the authors have addressed some of the questions I had on the first draft. There are still some issues with following the arguments, though mostly all the information is there, there just needs to be a little more thought given to organization or explanation as to where the authors are taking the readers. In a few places the authors need to provide a bit more explanation that was asked for in the previous review as well (e.g., the specific stream power calculation, though ultimately these values are barely used in the paper, so the details might not matter that much).

Thanks for your thorough suggestions in the first round and in the second round, too. We acknowledge that the discussion has been a bit wayward than we could/should have done before. We spent time on how to improve the discussion and revised the discussion section considerably. For example, now we evaluate our observation and results in light of two competing models we refer: duplex-growth model and out-of-sequence fault-ramp model. At present, our data show similarity with long-term exhumation rates, but. Honestly it cannot resolve which model is perfect for the setting. This is because we have field observations/ results which partially agree to both the models. And, that's why we didn't favour any model in the end, but, tried to keep it open. We have explained the revisions/ changes made in the following paragraphs.

1. This comment is mainly on the discussion as a whole, but it kind of extends to the results as well. At present, it's kind of hard to follow the connections between the various data being presented, i.e. what is the relevance of the different parts to each other. You go through some of this in the very

beginning of the paper, but by the time a reader gets to the results and discussion, it gets harder to see these connections. You have the start of a description of what you're going to do near the beginning of your discussion (L483-491), but I think this could benefit from a little expansion and a bit more specificity before you launch into the rest of the discussion.

We revised the starting paragraphs of the discussion section and made it more concise (line #470-488), so that readers can follow our arguments provided in sub-sections 5.1-5.5.

2. Where every thing comes together is Section 5.5, but as highlighted in some of my line by line comments below, this section is hard to follow and it's not clear what model you prefer or if you prefer any of them. It seems like you are arguing against the Gallivot duplex model at the beginning, but then you agree with parts of it. In the end, it seems like the data you have assembled can't discriminate between some of these options or clarify some of the details. This is fine, but you just need to be much more explicit about it, i.e. explain from the beginning that you can't differentiate on the basis of the data you have and then go through the relevant data and how it does or does not support various models. You tell us this is the case in the introduction, but this final discussion section meanders around this point a bit.

Section 5.5 is now split into two sub-sections 5.4 Our new results in context with the previously-published data and 5.5 Two competing models: duplex-growth model vs. out-of-sequence fault-ramp model. Section 5.4 deals with the comparison of our data with the data presented by Gavillot et al., (2018) from Kishtwar window and data from Dhauladhar Range (Thiede et al., 2017), Kullu-Rampur Window (Stuebner et al., 2018). Section 5.5 contains the competency of the two models with our results and field observations. We hope that this should make the manuscript/ discussion to be pointier.

Alongside this, we have removed some parts of the discussion, which we believe is either repetition of earlier statements or are speculative at this moment as those don't have independent data to support the claims. So, the revised text is more concise.

3. Knickpoints vs knickzones: There is some lack of consistency between defining something as knickpoint vs a knickzone which leads to confusion. Both K1 and K2 seem to both be knickpoints and knickzones. In figure 7, they are both (i.e., there are steepened zones highlighted named K1 and K2 and also discrete point named K1 and K2). In the text, K1 is described more as knickzone, but K2 is described as a knickpoint and a knickzone in section 5.1.2. Are the points for K1 and K2 just the upstream beginning of the knickzones? If that is the case, maybe distinguishing them with different names (e.g. K1_s, K2_s or something). It makes it challenging to follow how we are suppose to interpret these features.

Now in revised manuscript, knickzones are marked as KZ and knickpoints (discrete ones and the upstream head of the knickzones) are marked as KP. This will clear the confusion. All the occurrences including figures are revised.

Line by line:

L273-283: Could you provide a little more detail on this calculation, kind of just fill in the gaps. Specifically, I assume you use the width measurements from the previous section in your calculation of SSP? How are you estimating discharge? Again, I would assume you're routing the TRMM rainfall, but saying this is necessary. Do you assume a runoff ratio of 1? Do you have any data that would allow you to estimate if a constant runoff ratio is warranted (regardless of the specific value), etc?

Revised and new text added at line#279-283. In short, as we see TRMM data, we see no variation in rainfall across the study area, that is why we assume a constant Q. And, at this moment, we don't have any data on run-off ratio, so we assume that Q translates completely into run-off.

L392-402: Some of this text is repeated from your background section. Maybe keeping the previous observations in the background and your new observations here in the results would be better.

Text revised accordingly.

L652-653: It's not clear which field observations don't work with the duplex model or what the differences would be. Later (L658) you mention that the Gavillot model suggest no surface-faulting, but in the sentence that precedes this, you describe that the observed deformation could be a product of active faulting, crustal buckling, or internal folding, so the incompatibility of these observations with this model is not clear.

Discussion is revised and a new sub-section is added (line # 650-686) titled 'Two competing models: duplex-growth model vs. out-of-sequence fault ramp model'. In this section we categorize our observations and their match and mismatch with either models.

L669-672: This seems out of place and more like something that belongs in your conclusion.

Revised and deleted from the text. The conclusion is also revised accordingly.