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Interactive comment on "Interactions between deforestation, landscape rejuvenation, and shallow landslides in the North Tanganyika – Kivu Rift region, Africa" *by* Arthur Depicker et al.

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Review of Depicker et al., "Interactions between deforestation, landscape rejuvenation, and shallow landslides in the North Tanganyika - Kivu Rift region, Africa" for ESurf.

Please let me apologise for the delay in obtaining the second review of your manuscript. The peer review process is under considerable strain at the moment, we're having to allow reviewers more time. In this case, I have stepped in to act as reviewer (and another AE will take over).

This manuscript seeks to explore how deforestation, and subsequent land use change,

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Discussion paper

has impacted rates and patterns of landsliding in the North Tanganyika-Kivu Rift region. Landslides are mapped from Google-based satellite imagery across a wide study area (inventory updated from a previous publication) and their spatial and temporal patterns analysed. The paper does a good job of trying to deal with variable bedrock geology (through slope-based correction), and variable image coverage. The results are important – deforestation appears to greatly increase landslide total area and frequency (not a new observation, but novel for this area), but there is an interesting observation about landslide size, which differs in landscapes based on their recent geomorphic disturbance (influence of the rift-associated topographic uplift) (this is new and intriguing). The paper is well set up, and the theme and results are certainly of interest to this field, and the readership at Earth Surface Dynamics.

However, I found that in the abstract, conclusions and discussions, the main findings were not always easy to follow, and that some of the explanations for the patterns could be more convincing.

1) Drawing out the main findings: I found that the main results from the work were quite hard to follow. I wonder if there is a better way to explain what was found, and work through them in the discussion.

In some ways, these key messages can be seen in Figure 9. This firstly shows that deforested landscapes have much larger numbers and rates of landsliding in this setting. I'm aware that other studies have made this point, but I found the discussion of this rather limited.

Second, Figure 9 shows that for all classes, the rejuvenated landscapes have a higher number of landslides. Overall, this translates to an overall greater landsliding area per year. But, in the deforested landscapes the landslides tended to be larger in the relict landscapes, and so covered more total area. This way of explaining the patterns seems a little clearer than the abstract/discussion/conclusion has it.

2) Role of mean annual precipitation: The manuscript refers to a difference in precip-

itation between the relict and rejuvenated landscapes, but it is rather minor, because in both areas it is close to ∼2 m/yr. I think if a 300 mm/yr difference was seen where the MAP was ∼1 m/yr, this might make more of a difference, but its impact seemed overstated.

The paper also mentions a precipitation intensity proxy. But the output of that analysis was less well explained. It was also unclear how well these climate patterns were constrained for the region, and their spatial variailty across the large mapped area.

3) Landslide maps: The figures are well made, and good on the whole, but the study could benefit from having examples of the landslide polygon maps and spatial patterns of landslide metrics shown in the paper.

Otherwise, my final main comment is similar to point 1 raised by Referee #1 (note I completed my review prior to reading their comments). Namely, the "shallow" part of the landslide description. I don't think this is necessary to add throughout. While the discussion of landslide depth needs to be more complete and considered.

Other comments: 10: at this stage the "relict" and "rejuvenated" terms need more context (perhaps link them to the discussion above).

10 – 13: I have read these two sentence a few times – I find it hard to follow. It starts with "40% higher" erosion rates, and ends with "lower landslide erosion rate".

17: would it make sense to start with these observations (about how deforestation impacts), before then talking about the link to landscape metrics and longer-term patterns (previous text)?

47: Have a new section here, on study area?

76: I suppose there may be reasons why land is "non forest" – i.e. grassland (e.g. precip, temp, seasonal climate) – apart from anthropogenic factors? But perhaps not true in this case, but that could be clarified.

80: a brief summary of some of this would be welcome in the abstract (see comment above).

103: some more information on this climate model output would be welcome. Particularly the link to measurements on the ground, and calibration/testing of model outputs.

108: again, somehow mentioning the attempts to control for lithology could be useful in the abstract (and perhaps mentioned as part of the challenges in this type of assessment in the introduction?)

137: more info is going to be needed on the depth aspect here.

139: what is the normalising area here (km2), and how does it become a per year? – this would benefit from an extra sentence to explain – basically that this is the processing of the landslide maps once each has an initiation year.

148: it would be useful to explain the satellite data source if possible.

170: what is "imagery density"? do you mean more frequent imagery? And/or resolution of imagery? What is the basis to assume that identifying landslides increases linearly with imagery density?

191: I found this section hard to follow. It needs to refer to the necessary equations, and make clear what the issue here is.

208: I see what you're getting at, but this sentence is a bit awkward. Basically you don't know exactly when a landslide occurred between satellite image dates – so just say that.

213: It would be useful to explain for how many landslides this is an issue. Essentially, if the landslide is mapped the same year (or time window) as deforestation. Also, is there not another way to assess this? Could you not examine the values of forest cover pixels proximal to the landslide polygon? If the landslide caused the deforestation, the wider area should still be deforested?

Figure 5 – this is helpful, but it could better draw out the key part of the story (i.e. the middle of part b), perhaps with different annotation?

220: It might be significantly different in a statistical sense, but the value is pretty similar in terms of MAP. . .

221: give the values here.

Figure 6 – the separation of the rejuvenated/relict landscapes is not very clear in this figure. This could be just a display thing (i.e. useful to have an example zoomed in), and/or could be a method thing – how are the knickpoints being used – the non-stationary ones need to have a different symbol.

Figure 8 c – I'm not sure what is happening here – is this across the whole dataset, or a selection of an area, with incremental increases in the image density? The inflection point marked, its not clear to me why this should happen. Is it not because the places with highest image density (fig. 8b) are the cities, with fewer landslides? In other words, is part c the best way to show the role of the image availability on the results?

267: what does the "equal steepness" mean.

269: its not much drier! Both have almost or greater than 2 m of rain per year!

270: in fact, this is about landslide area, not number, so I'm not sure about the explanation here. Why would slightly less rainfall (1.9 m/yr vs 2.2 m/yr) lead to smaller landslides? Is it anything to do with how landslide size is linked to lithology? And/or the geomorphology – are the rejuvenated landscapes typically smaller catchments, and so landslides are generally smaller (constrained by hillslope length)?

Figure 11 – how many landslides? What are the fainter lines and grey zone? What is the landscape average (perhaps as a line?)

290: yes – this makes sense (link to 270 above)

302: That is not what that figure shows, in my opinion – the rate of landsliding seems

to be pretty constant across the window.

306: again, “fading out” is not what I can see in Figure 11.

322: where does this depth come from?! It needs more discussion.

338: I would remove the word “shallow” from here, as you don’t actually know how deep they are. Figure A1- this would be better than Figure 11 (and add the number of landslides and explanation of the fine lines and grey shading).

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