Response to Reviewing committee comments: *Interactions between deforestation, landscape rejuvenation, and shallow landslides in the North Tanganyika - Kivu Rift region, Africa*

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March 15, 2021

We would like to thank the referees for their detailed and constructive comments. We believe that their feedback identified some weaknesses in our methodology and discussion. Through completing the suggested edits, the revised manuscript benefits substantially from an improvement in the results, overall presentation, and clarity.
To elaborate our answers to the reviewers' comments, the following color scheme is used: comments of the referees are shown in **blue**, answers are annotated in black and quotes from the revised text in **green**. The lines in the final manuscript are indicated in **purple**, while the lines in the manuscript with tracked changes are in **orange**.

Contents

1 Referee #1

General comments: This study examined landslide activities in response to deforestation in tectonically-rejuvenated and relict landscapes in the North Tanganyika-Kivu rift region, Africa. The authors mapped landslides from Google Earth imagery using a new method to correct for biases in imagery inventories. They found more abundant but smaller landslides after deforestation in rejuvenated landscapes compared to relict landscapes, which were possibly caused by differences in seismicity and regolith stock. This work tackles an interesting and important topic of how land-use changes affect landslide activities in different geomorphic settings, and has potential to make a contribution to Earth Surface Dynamics. However, the current manuscript could be strengthened with improved data presentation and analyses, and clarification of several key technical details

1.1 Major comments

1.1a *Landslide definition and data presentation This manuscript termed the mapped landslides as 'shallow landslides' – I don't get what the authors meant /why the authors emphasized 'shallow'? Do the authors refer to shallow soil landslides that are distinct from bedrock landslides, or have no intention to separate soil versus bedrock landslides? Do the authors exclude deep-seated bedrock landslides?*

Within this work, we opted to look only at shallow landslides, defined here as landslides of which the depth does not exceed more than a couple of meters. We exclude deep-seated landslides because they are less dependent on direct triggers such as rainfall and processes such as deforestation that are at play at the surface of the Earth. We do not expect deforestation to have a (significant) impact on large deep-seated landslides with a rupture plane located deep in the bedrock or regolith of the earth surface. So in conclusion, we chose to only include shallow 'soil' landslides; we exclude deep-seated soil landslides and deep-seated bedrock landslides (shallow bedrock landslides were not observed in the study area). We were more explicit in the revised text:

L158-161/**L169-172** *Moreover, since deforestation mainly affects the stability of the first few meters of the regolith (Sidle and Bogaard, 2016), we only consider shallow landsliding in this study. Deep-seated and bedrock landslides are excluded from the inventory. We apply a maximum depth of a couple of meters for landslides to be inventoried…*

1.1b *If the mapped landslides are all soil landslides, I can see that landslide size is limited by regolith stock – a recent publication, Prancevic et al. (2020), had a nice dataset illustrating this, which could be a useful reference. If the authors do want to highlight those landslides as 'soil' landslides, some discussions are needed then regarding the possible existence of bedrock landslides in the dataset.*

See **comment 1.1a**, we were more explicit about not inventorying deep-seated or bedrock landslides. We added the reference to Prancevic et al. (2020) in the discussion on regolith availability and landslide size.

L322-324/**L377-378**: *…smaller landslides in comparison to relict landscapes, as the size of the shallow landslides is constraint by the regolith availability (Prancevic et al., 2020).*

1c *Meanwhile, it would be helpful to display and discuss the areal size distributions (e.g. Malamud et al., 2004) of the landslide inventories in rejuvenated and relict landscapes, which would be a more effective presentation than just mean landslide source area.*

We illustrate and refer to the size-distribution curves of the landslide inventory, separately for forest and non-forests, and rejuvenated and relict landscapes (there was not enough data to calibrate the curves for deforestation-linked landslides).

Methods:
L174-176/**L196-198**: *Furthermore, we illustrate the potential differences between the landslide areas of rejuvenated and relict landscapes by comparing the frequency density of the landslide areas. The frequency density curves are fitted to the inverse Γ distribution (Malamud et al., 2004).*

Results:

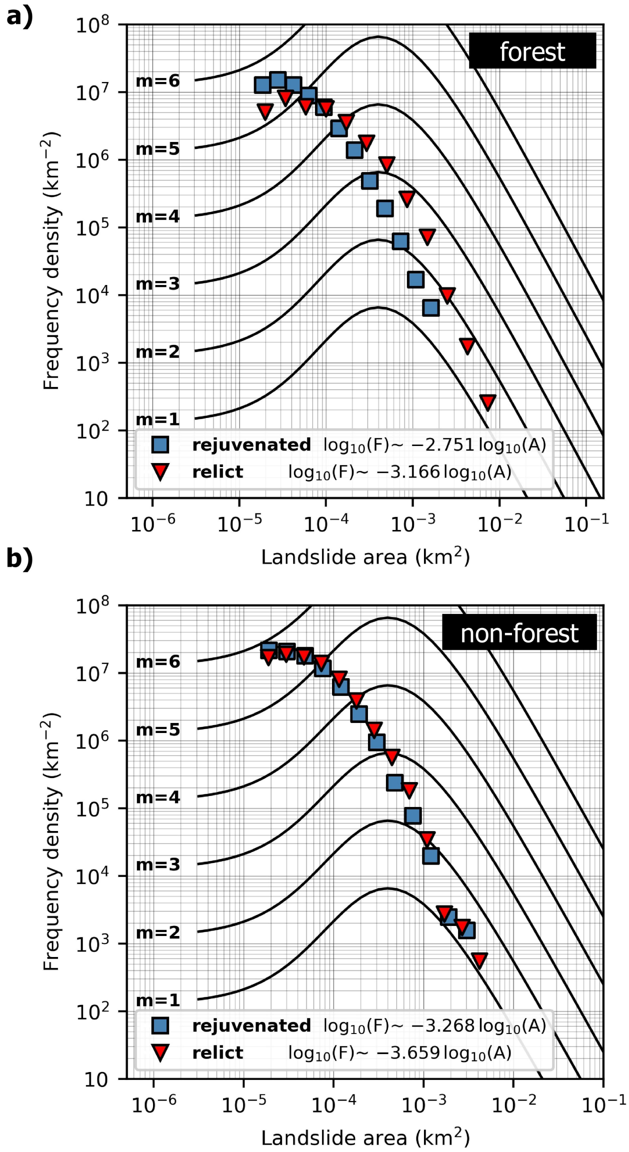

Figure 11. Frequency density in function of the landslide source area. a) *The area frequency density of shallow landslides in forest, separated for rejuvenated and relict landscapes.* ***b)*** *The area frequency density of shallow landslides in non-forest, separated for rejuvenated and relict landscapes. There were not enough landslide observations in deforested land to fit to their area frequency density to the inverse Γ distribution. The general frequency density distributions for inventories of different magnitudes (the black lines on the curves) are derived from Malamud et al. (2004). Note that the frequency is skewed towards smaller landslide sizes due to the omission of deep-seated (and generally larger) landslides from our inventory.*

L281-283/**L321-323**: *This difference in landslide size between rejuvenated and relict landscapes is confirmed in all three land cover types: forests (114 versus 308 m^2, p<0.01,* ***Fig. 11a****), non-forests (111 versus 138 m^2, p<0.01,* ***Fig. 11b****),...*

1.2a *Ambiguity in the method for correcting image biases The description of the method developed to correct for biases in satellite imagery was confusing, and I could not judge whether this method was correct or not. Specifically, in L165-L170, why is Eq. 6 equivalent to Eq. 7? For example, in Eq. 6, assuming A = 1, N = 3, and n1 = r1 = 3, n2 = r2 = 4, n3 = r3 = 5, I would calculate a LSF of 3. In Eq. 7, if A = 1 and ri > 1, I didn't get how Eq. 7 can give the same result of 3.*

We agree that we used a bit of a shortcut in this part, though the mathematics check out, also for the example the reviewer provides; in this example, a database is described consisting of 12 landslides, of which 3 occur in an area with imagery range of 3 years, 4 in an area with imagery range of 4 years, and 5 in an area of imagery range 5 years. To apply Eq. (6), we divide this area in 3 groups (index j); the first group has $n^j = r^j = 3$, the second $n^j = r^j = 4$, the third $n^j = r^j = 5$, hence Eq. (6) gives

$LS_F = \frac{3}{3} + \frac{4}{4} + \frac{5}{5} = 3$, (assuming A=1).

Now, if we apply Eq. (7) we look at the individual landslides (i) and not the groups, applying the equation gives

$LS_F = \frac{1}{3} + \frac{1}{3} + \frac{1}{3} + \frac{1}{4} + \frac{1}{4} + \frac{1}{4} + \frac{1}{4} + \frac{1}{5} + \frac{1}{5} + \frac{1}{5} + \frac{1}{5} + \frac{1}{5} = 3$,

i.e. the same result, but calculated without grouping the landslides first according their imagery range (which would yield a very large number of groups in the real world where the imagery range of landslides has continuous values anywhere between 0 and 20 years). Although Equations 6 and 7 are thus mathematically equivalent, we do recognize the concern of the reviewer and agree that a better description is needed, particularly of how both equations are linked:

L192-193/**L217-218**: *Hence, for the calculation of LS_F, we do not require the size A^j of each subarea j for the calculation of the LS_F. Instead of aggregating the LS_F over all subareas, we can aggregate the LS_F over the individual landslides.* ***Equation (6)*** *then becomes:*

1.2b *I'd suggest add a general paragraph discussing the principles of this correction at the beginning of section 2.2.2, and give some specific examples when doing the derivation. Based on the current description and information, I could not validate this method.*

We agree that this section could be better structured. Given that the concepts of imagery range and density were previously explained in paragraph 2.2.1, we reshuffled this text. For example, by putting the explanation for range and density, and the need to correct for their biases, at the location where they are introduced in the formula (see also **Comments 1.19**, **1.22**, **1.23**, **1.24**, **1.25**). The entire section was rewritten based on these comments and we moved Figure 8a-b (from the original manuscript) towards this section.

L178-218/**L200-244**: *Generally, the LS_F is calculated as:*

$$LS_F = \frac{n}{r\,A} \tag{3}$$

*with n the total number of shallow landslides, A the total area (km^2), and r period of observation (years). The period of observation is equal to the available imagery range in © Google Earth, i.e. the age difference between the oldest and youngest image. However, the imagery range is highly variable throughout the study area due to differences in the availability of © Google Earth imagery (**Fig. 3a**). Since **Eq.** (3) is valid only for areas with a constant r, we divide our study area in subareas j that each have a constant imagery range r^j. The LS_F in each subarea LS_F^j is then:*

$$LS_F^j = \frac{n^j}{r^j\,A^j}, \tag{4}$$

with n^j the number of landslides in subarea j, A^j the surface area of j, and r^j the constant imagery range in j. To calculate the frequency for the entire study area, the frequencies LS_F^j

are averaged out using weights proportional to their corresponding area A^j:

$$LS_F = \sum_{j=1}^{N} \frac{A^j}{A} LS_F^j, \tag{5}$$

*with N the number of subareas j. Substituting **Eq. (4)**, **Eq. (5)** becomes:*

$$LS_F = \frac{1}{A} \sum_{j=1}^{N} \frac{n^j}{r^j}. \tag{6}$$

*Hence, we do not require the size A^j of each subarea j for the calculation of the total LS_F. Instead of aggregating the LS_F over all subareas, we can aggregate the LS_F over the individual landslides. **Equation** (6) then becomes:*

$$LS_F = \frac{1}{A} \sum_{i=1}^{n} \frac{1}{r^i}, \tag{7}$$

*with r^i the time range observed in landslide i. The landslide inventory is expected to be biased due to spatial differences in the imagery density d (**Fig. 3b**), defined as the total number of available images at each location, as vegetation regrowth might erase the spectral signature of landslides before they are captured in imagery. Hence, we expect to detect more landslides in areas with higher imagery density. To compensate for this bias we assume that the probability of identifying a landslide in a certain region increases linearly with imagery density in that specific region. **Equation** (7) then becomes:*

$$LS_F = \frac{1}{A} \sum_{i=1}^{n} \frac{1}{r^i\, d^i}, \tag{8}$$

with d^i the imagery density observed at the location of landslide i. Note that there can be a saturation of the information provided by the imagery: when the imagery density is high, the availability of one extra image will have no to little effect on the observed number of landslides. We validate our assumptions of linearity and saturation by visually assessing the dependency of landslide density (# landslides km^{-2}) on imagery density. If the assumption of linearity does not hold, we have to apply a non-linear transformation on the d^i values. If saturation is problematic to our inventory, we have set a maximum value for d^i.

*Deriving the LS_S equations is analogous to deriving the ones for the LS_F. We only have to slightly modify **Eq. (7)** and **Eq. (11)**:*

$$LS_S = \frac{1}{A} \sum_{i=1}^{n} \frac{a^i_{src}}{r^i}, \tag{9}$$

$$LS_S = \frac{1}{A} \sum_{i=1}^{n} \frac{a^i_{src}}{r^i\, d^i}, \tag{10}$$

whereby a^i_{src} is the source area of landslide i. Note that the calculation of the LS_S will be less accurate than for the LS_F due to biases in the delineation of the landslide source area. These biases are caused by the time lag between the landslide occurrence and the landslide detection in © Google Earth, whereby part of the source area might already have recovered. To avoid biases linked to the interpretation of the source area, all landslides were delineated by the same person.

In order to statistically verify a difference in landslide activity between regions (for example rejuvenated versus relict landscapes), we use the one-sided non-parametric Mann-Whitney U test to compare the different landslide activity measures in fifth order water catchments (calculated with ***Eq. (11)*** *and* ***Eq. (10)*** *to compensate for imagery density differences).*

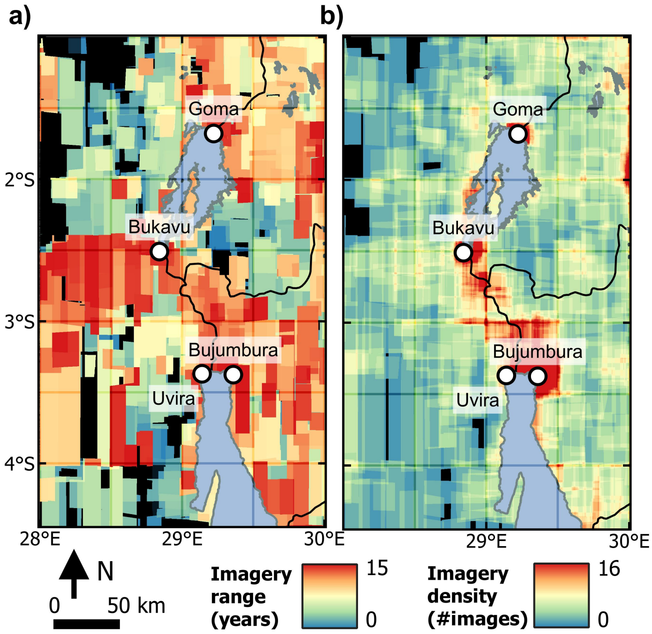

Figure 3. Visualization of the imagery bias in © Google Earth. a) *Imagery range.* ***b)*** *Imagery density. The range and density were calculated by manually identifying 932 imagery footprints. The highest imagery density is available for the major cities in the study area (Goma,Bukavu, and Bujumbura), whereas the northwest and southwest regions have fewer observations.*

1.3a *Point of k_{sn} analysis I didn't get why the authors introduced a new function relationship between slope and ksn and conducted the analysis in Figure 7 - this seems to be irrelevant to the characteristics of landslides in rejuvenated vs. relict landscapes, which are the key points of this study. The ksn analysis seems to be isolated from the remaining discussion of landslide activities as well. I'd suggest either removing this part (or moving to supplement) or linking landslide activities to k_{sn} to enrich discussions.*

In Figure 10 (Fig. 12 in the new manuscript) we look at the impact of slope on landslide activity (accounting for deforestation and rejuvenation). We think that large differences in threshold slope between different areas could have a large impact on the trends observed in this figure; for example, a slope of 30° is expected to display more landslide activity in lithology with lower threshold slope. Such low-threshold-slope-areas are only present in the rejuvenated landscape. Hence, their presence would have clouded our analysis in Fig. 10: the presence of low-threshold-slope lithology would be an extra factor that could explain the higher landslide rates for slopes in relict landscapes compared to equally steep slopes in rejuvenated landscapes. We will highlight this argument better. Yet, precisely because the response of slope to k_{sn} is characteristic of this threshold, we believe this is an important factor for our analysis and an innovative result which would not receive the attention it deserved when placed in the supplementary material

L130-132/**L142-144**: *... Rock strength is a factor that must be taken into account when investigating landslide characteristics; equal slopes with different rock strength properties are expected* *to display different landslide behavior in terms of landsliding and knickpoint retreat (Parker et al., 2016; Baynes et al., 2018; Campforts et al., 2020).*

1.3b *Besides, the classification in Figure 7 was also not convincing – for example, panels a) and f) seem to have similar trends, and do not indicate clearly the existence of a threshold slope (TA).*

The classification of the lithology based on threshold slopes was done automatically by investigating whether or not there was a linear relationship between k_{sn} and slope (if $R^2 > 0$, a linear relationship existed). So while in figure 7a the software detected a positive linear relationship, this was not the case for Figure 7f. We will specify this approach in the manuscript. (also note that both lithology a) and f) are excluded from further analysis, since we only focus on the 'strong' lithology).

L151-152/**163-164**: *However, when there is a linear relationship for $S = f(k_{sn})$ in the entire k_{sn} range (when the $R^2 > 0$ for a linear fit), we do not consider the threshold estimate reliable.*

1.4 *Add analysis of seismicity and rainfall data as controls The authors speculated on the roles of rainfall and seismicity in setting landslide abundance and sizes (section 4.1), but did not conduct any thorough analysis. The authors already presented rainfall data in the region (Figure 2c) which could be analyzed in more details to examine the role of rainfall in landslide occurrence. It'd help as well if more seismicity data could be compiled and analyzed to show the differences in rejuvenated versus relict regions. So I'd suggest more quantitative analysis examining the relationships between rainfall (annual rainfall and rainfall variability or extreme events), seismicity (patterns, numbers of small-magnitude events, etc), and landslide occurrence in the rejuvenated and relict landscapes.*

A more thorough analysis of rainfall and seismicity would indeed strengthen this paper. Unfortunately, such an analysis is hampered by the data-scarcity in the region. Specifically, to investigate in-depth the impact of rain on landslides, we would need accurate rainfall data (high spatial and temporal resolution) and accurate timing of the landslides (as of now, we can only locate the timing of a landslide somewhere between two images). Attempts for such an analysis in the region have been conducted by Monsieurs et al. (2019), yet in our case it is impossible to obtain the necessary data to complete our inventory. The same argument can be made for seismicity. Moreover, to our knowledge there were no landslides in the region that were triggered by earthquakes.

Another possibility is to investigate rainfall-landslide or seismicity-landslide trends is to compare the spatial pattern of several rainfall metrics (e.g. threshold exceedance/year) with the spatial occurrence of landslides. Such a study has already been conducted for the study area by Depicker et al. (2020). Here a positive relationships between rainfall and landslides and PGA (peak ground acceleration seismicity) and landslides have been established. We will better highlight this previous work and its results in our discussion.

The link between rainfall and landslides is referred to here:

L108-110/**L116-119**: *Depicker et al. (2020) showed a significant link between landslide occurrence and the frequency of extreme rainfall events. At the same time, field observations and local reports confirm that the majority of recent shallow landslides are rainfall-triggered (Monsieurs et al., 2018a; Depicker et al., 2020; Dewitte et al., 2020).*

The link between earthquakes and landslides is referred to here:

L312-317/**L365-370** *Earthquakes fracture and weaken the bedrock and hence reduce the minimum critical area for landslide initiation (Delvaux and Barth, 2010; Milledge et al., 2014; Vanmaercke et al., 2017). As such, seismic activity may also contribute to a smaller average landslide size. Moreover, a previous study in the NTK Rift established an indirect link between spatial patterns of seismic activity (approximated by a modelled PGA product by Delvaux et al. (2017)) and the spatial pattern of the landslide occurrence, though this study did not differentiate between deep-seated and shallow landsliding (Depicker et al., 2020).*

1.2 Minor comments

1.5 *L1 briefly explain why deforestation increases landslide activity*

L1/**L1**: *Deforestation is associated with a decrease in slope stability through the alteration of hydrological and geotechnical conditions.*

1.6 *L6 'a longer timescale' – over what timescales? Thousands of years?*

L2-3/**L2-3**: *As such, Deforestation increases landslide activity over short, decadal timescales. However, over longer timescales (0.1-10 Ma) the location and timing of landsliding is controlled by the interaction between uplift and fluvial incision.*

1.7 *L7-8 would be useful to define 'rejuvenated' here*

See **Comment 2.9**. We provided a short description of how the rejuvenated landscapes were delineated in the abstract.

L13-14/**L13-14**: *. . . Rejuvenated landscapes were defined as the areas draining towards Lake Kivu or Tanganyika, and downstream of migrating knickpoints. . .*

1.8 *L11-L13 too long, consider rewrite as two shorter sentences?*

See also **Comment 2.1b**. We tried to explain this more clearly.

L14-19/**L14-21**: *We find that erosion rates in these rejuvenated landscapes are roughly 40 % higher than in the surrounding relict landscapes. In contrast, we observe that slope exerts a stronger control on landslide erosion in relict landscapes. These two results are reconciled by the observation that landslide erosion generally increases with slope gradient and that the relief is on average steeper in rejuvenated landscapes. The weaker effect of slope steepness on landslide erosion rates in the rejuvenated landscapes could be the result of three factors:. . .*

1.9 *L20 not consistent with discussions and results? Mentioned the role of regolith stock in the discussions and results but didn't mention them here*

The reviewer is correct for pointing this out, we adapted the text.

L23-25/**L26-28**: *Landslides are not only more abundant in rejuvenated landscapes but are also smaller in size, which may again be a consequence of a thinner regolith stock and/or seismic activity that fractures the bedrock and reduces the minimal critical area for slope failure.*

1.10 *L25 clarify what is 'shallow'*

Within the methods section, we cannot be very specific about the depth because the cited studies (e.g. Montgomery et al., 2000) are not specific either. We mention in the intro that the depth is maximum a couple of meters, but later on in the methods we will specify our criterion in more detail.

L30-31/**L32-33**: *On steep terrain, the erosion caused by shallow landslides (with a depth of maximum a couple of meters) increases significantly as a result of deforestation*

L158-161/**L170-173**: *. . . we only consider shallow landsliding in this study. . . . We apply a maximum depth of a couple of meters for landslides to be inventoried. . . .*

1.11 *Figure 1 possible to add the spatial extent of landslide mapping? Or the whole area – would be helpful to indicate*

The entire area was mapped in terms of landslides. We also added the 2D transects of the rift to figure 1 (as a response to the next comment

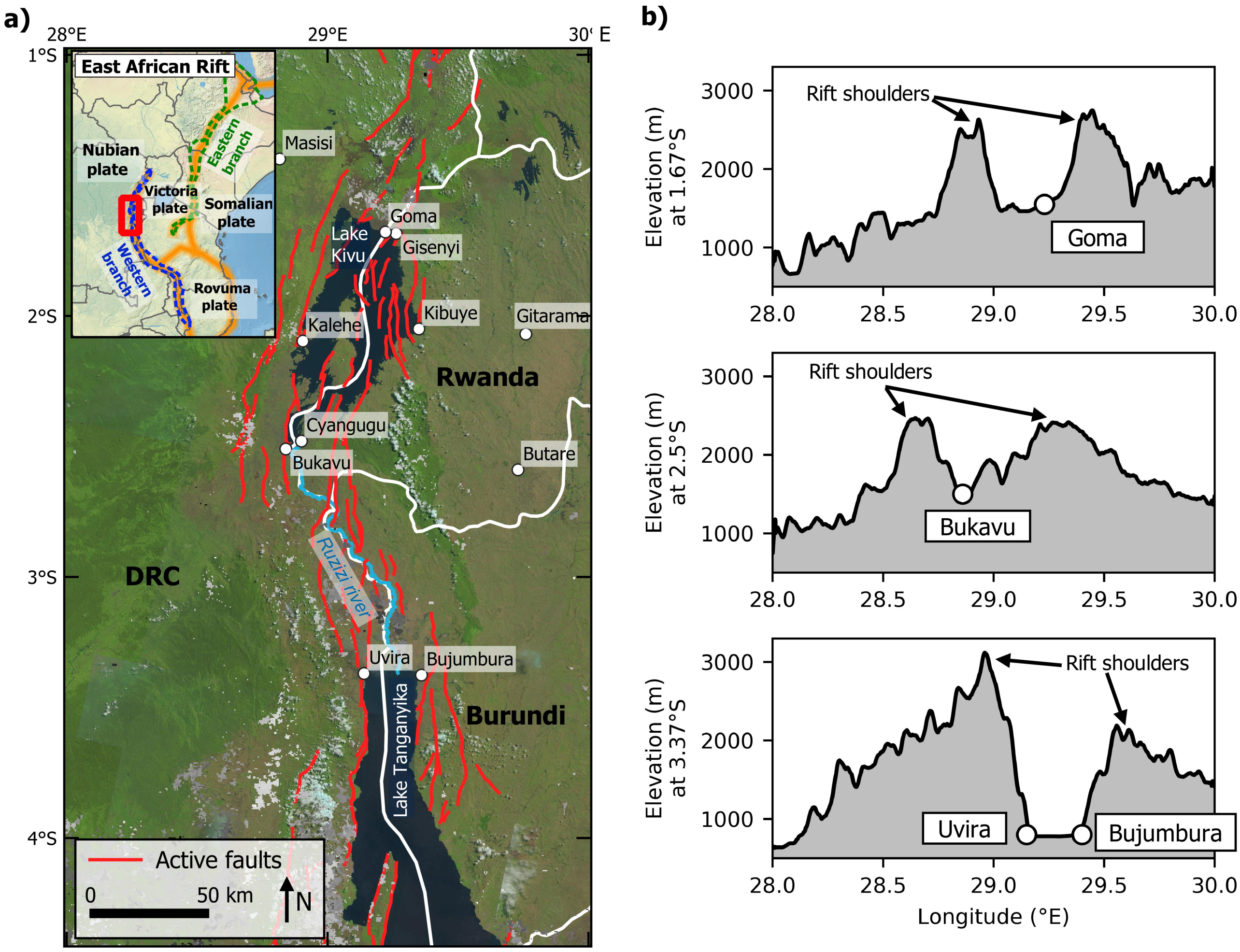

Figure 1. Overview of the NTK Rift. a) *extent of the studied area and active faults (Del-*

vaux and Barth, 2010)). LANDSAT-8 imagery is used as background (USGS, 2018). ***b)*** *The transects of the elevation at different latitudes illustrate the elevated Rift shoulders, being the result of tectonic uplift. The four most populous cities of the NTK Rift (Goma, Bukavu, Uvira, Bujumbura) are located in between the Rift shoulders.*

1.12 *Figure 2 might help to add a 2-D cross-section plot of the Rift zone and illustrate key parts such as 'shoulders'*

See previous comment (**1.11**)

1.13 *L73 is the percentage of tree coverage reported as for each one arc-second grid?*

L82-83/**L89-90**: *The tree cover data shows the percentage of tree coverage per pixel in 2000...*

1.14 *L98 you didn't show the relationships between rainfall metrics and landslide activities in the discussions..., also define what is 'sufficiently large'?*

As to the relationship between rainfall and landslides, see **Comment 1.4**. We defined sufficiently large as 15 mm in the past 2 days. We restructured the text to make this more clear.

L110-113/**L120-125**: *To explore any relationship between rejuvenation and rainfall, we analyze two metrics: the average annual rainfall and the number of times where accumulated rainfall was sufficiently large to trigger landsliding. As a proxy of the latter criterion, we use a 2 day-15 mm threshold as it is a conservative estimation for global thresholds set by Guzzetti et al. (2008)). Our intent is not to approximate an actual in-situ threshold, but rather to reflect spatial patterns in intense rainfall capable of triggering landslide events.*

1.15 *L109 modal slope angle?*

In literature the modal slope is indeed used as one possible proxy of the rock strength/threshold slope (e.g. Korup, 2008). Yet, the analysis we conduct (channel slope in function of k_{sn} which is a proxy of the bedrock incision rate) is another option explored in literature (e.g. Safran et al., 2005; DiBiase et al., 2010; Bennett et al., 2016). We prefer this second method, because simply looking at the modal slope would include landscapes that are limited by incision (low k_{sn} and thus low slope) while we are only interested in landscapes limited by rock strength (for the analysis of the threshold slope). Note that we added a reference to Safran et al. (2005) in the manuscript.

1.16 *L125 the introduction of this new function seems somewhat arbitrary, and I really didn't see how this new function adds to the paper...*

The function is arbitrary to some extent, the only criterion for this function is that it should mimic the relationship between slope and k_{sn} (quasi linear for low k_{sn}, constant slope for high k_{sn}). Such a function was necessary to have an objective way of defining the threshold slopes when analyzing the scatter plots of **Figure 7** (**Fig. 8** in the revised manuscript). See **Comment 1.3a** and **1.3b** as to why we deem this analysis relevant for this paper.

1.17 *L135-140 do you mean you excluded some really large landslides here? If so, why you want to exclude the large ones? what's the criteria to exclude large ones? Could expand and add more data to show the results of relative depth? Is there a correlation between landslide* *area and depth so you can estimate landslide volume?*

Indeed, large deep-seated landslides (bedrock or soil), were excluded from the database. See **comment 1.1a**. We were not able to accurately measure landslide depths in Google Earth, therefore we do not make attempts to find a correlation between landslide depth, area, and volume.

1.18 *L140 'a point of initiation' – is this the centroid point of the source area?*

Not necessarily, for example in the case of irregularly shaped source areas (not circular). These initiation points were assigned manually

L166-167/**L179-180**: *we also manually assign a point of initiation used to calculate the landslide frequency LS_F*

1.19 *L148 Figure 8a came too late – suggest to move it earlier to Figure 2.*

See **comment 1.2b**. This Figure was moved towards section 2.2.2 to better illustrate the concepts of imagery density and range

1.20 *L150 how did you tell landslides from mining and quarrying?*

L169-172/**L182-185**: *...linked to mining and quarrying. Such sites were identified either during fieldwork, or in Google Earth imagery through characteristics such as a gradual growth of the affected area over a time span of several years and the presence of mining infrastructure (road tracks, trucks, buildings, spoil tips) within the affected area.*

1.21 *L154 change the title to 'correct for biases in satellite imagery'?*

Although the suggested title is more clear and concise, we think that it is necessary to include the aspect of 'calculating the erosion rates' since this the main topic of the Section.

1.22 *L155 as mentioned earlier, suggest to add an introductory paragraph illustrating the principals of the correction method*

See **Comment 1.2b**.

1.23 *L159 the definition of r is not clear... maybe give an example to illustrate*

See **Comment 1.2b**. We rewrote this text to define this variable at a more appropriate location.

1.24 *L165-170 didn't get why Eq. 6 is equivalent to Eq. 7.*

See **Comment 1.2b**.

1.25 *L174 poor definition and explanation of d, again, might help to give an example*

See **Comment 1.2b**.

1.26 *L198 in order 'to' link…*

done

1.27 *L216 define what is 'tolerance value of 100 m' here*

The tolerance value was introduced in the Methods section (line **L99-104**/**L109-114**)

1.28 *Figure 7: make it clear in the caption what are the points? Values for each individual catchment?*

We tried to increase the clarity of the caption by restructuring as follows:

Figure 8. Threshold slope analysis for the different lithostratigraphical units in the NTK Rift. *Each point represents a first order river catchment over which we averaged the slope gradient S and normalized river steepness index k_{sn}. The black curves represent the $S = f(k_{sn})$ relationship fitted to* ***Eq. (2)****…*

1.29 *L233 briefly explain how the classification in Hungr et al. (2014) works?*

A good suggestion, especially for readers who are not familiar with landsliding. We added definitions:

L266-270/**L300-304**: *Following the classification of Hungr et al. (2014), the observed landslides were mostly debris slides, caused by the sliding of regolith on a planar surface parallel to the ground These debris slides, once initiated, often transform into avalanches, characterized by the flow of (at least partially) saturated debris on a steep slope. Another commonly observed landslide type was the debris/mud flow, defined as the rapid flow of saturated debris in a steep channel.*

1.30 *L253-254 refer to Prancevic et al. (2020)*

Prancevic et al. (2020) indeed illustrate how steep slopes can still display a lower landslide erosion rate

L287-289/**L327-329**: *A decrease is observed for forested slopes >45°, which could be linked to limitations on regolith formation, whereby weathering and sediment deposition are outpaced by erosion (Montgomery, 2001; Dykes, 2002; Prancevic et al., 2020)*

1.31 *L266 suggest to make sub-sections in section 4.1 to discuss in each the role of seismicity, rainfall, and regolith stock…*

See also **Comment 1.4**. Given the fact that our discussion and results for the role of rainfall/seismicity will remain limited, we think that subdividing this section would create too many subsections with each very little information. Hence, we would prefer to keep the structure as it is.

1.32 *Figure 10: in panels g and h, it seems like landslides are centered around similar angles ($\tilde{3}$5 degrees) for both rejuvenated and relict landscapes? Why? Similar threshold angles in both landscapes?*

The different landscapes should have indeed similar threshold angles. See also **Comment 1.3.** We made sure that the landscapes that we compare consist of lithology with similar threshold slopes.

1.33 *L279 what do you mean by 'landslide-triggering earthquakes'? did you mean no earthquakes greater than a specific magnitude? Can make it clear here.*

Rather than saying that 'no earthquakes occurred of large enough magnitude to trigger landslides', we wanted to express that 'no landslides were observed that were triggered by landsliding → no large earthquakes observed during that period'

L309-312/**L359-364**: *However, in our observed period, chances of earthquake-triggered landsliding were very limited (Dewitte et al., 2020). The absence of such events suggests that our window of observation was too short to capture earthquakes that were large enough to trigger landsliding. Over the long term, the contribution of earthquake-induced landsliding to regolith mobilization may nevertheless be important.*

1.34 *L317 expand and explain what do you mean 'the smaller size is likely due to a smaller minimum critical landslide area linked to the absence of tree cover'?*

We tried to better explain this concept:

L362-364/**L422-424**: *The smaller size is likely due to the absence of trees and the associated lower overall root cohesion. Regolith with a lower root cohesion exhibit a smaller minimum critical area needed to initiate landsliding (Milledge et al., 2014; Sidle and Bogaard, 2016).*

1.35 *L322 where does the landslide depth of 2.5 m come from?*

See also **Comment 1.10** and **2.31**. Instead of assuming a fixed depth, we implemented a V∼A scaling relationship presented by Larsen et al. (2010) for soil landslides in Uganda.

L368-370/**L430-432**: *Using the volume∼source area relationships presented by (Larsen et al., 2010) for soil landslides in Uganda, we obtain a rough estimate of the landslide volumes. As such, we find that the LS_S in rejuvenated landscapes corresponds to a denudation rate of 0.006 mm year^{-1}.*

1.36 *L328 how does this 'conservative value of 0.2 mm year^{-1}' relate to Montgomery and Brandon, 2002? Explain.*

The paper of Montgomery and Brandon (2002) presents a global relationship between the mean local relief (ca. 1,300 m) and erosion rates in mountainous areas. Hence, based on the mean local relief (1300 m) we could estimate the erosion rate of 0.6 mm year^{-1}. However, we made a mistake in the original manuscript, as we used the average elevation within their function instead of the mean local relief. This was corrected.

L375-378/**L436-441**: *. . . Based on a global relationship between mean local relief and erosion rate, formulated by Montgomery and Brandon (2002), we obtain a more conservative value of 0.6 mm year^{-1} for the average erosion rate in in landscapes with a similar mean local relief as the rejuvenated landscapes in the NTK Rift (ca. 1,300 m). In this scenario, shallow landslide erosion accounts for 1.0 % of the total erosion. . . .*

1.37 *L341-343 sentence too long and reads confusing. Rewrite to shorter sentences?*

Similarly to **Comment 1.8** and **2.1b**, we rewrote this part of the text:

L388-392/**L451-457**: *. . . Rejuvenated landscapes display a higher shallow landslide erosion rate than relict landscapes. Contrarily, the effect of slope steepness on landslide erosion rates is smaller in rejuvenated landscapes. These two seemingly contradicting results are reconciled by the observations that erosion generally increases with slope gradient, and that the average slope is much steeper in the rejuvenated landscapes. The lower impact of slope steepness on landslide erosion in the rejuvenated landscapes could be the result of three factors:. . .*

2 Referee #2

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

2.1 Major comments

2.1a *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.*

We added a paragraph to show that our results are conform with previous literature:

L335-339/**L389-394**: *Deforestation drastically increases the landslide frequency and landslide erosion rate. The observed landslide erosion and frequency increases two- to eight-fold after deforestation, which is the same order of magnitude as has been reported in literature (Jakob, 2000; Glade, 2003; Guthrie, 2002). The effect of deforestation on landslide erosion and frequency is temporary, lasting approximately 15 years (Sidle et al., 2006; Sidle and Bogaard, 2016). However, a longer period of observation would be useful to confirm if this effect persists after 15 years.*

2.1b *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.*

We agree that this main finding is a rather complex mechanism that is not easily explained in a few sentences. Basically, we present the three following observations: 1) the erosion rate increases with slope steepness, 2) the overall erosion rate is higher in rejuvenated landscapes, but 3) when there are two slopes of equal steepness, one in rejuvenated landscapes and one in relict landscapes, then the slope in rejuvenated landscape is expected to have lower erosion rate than the slope in the relict landscape. In other words, slope exerts a weaker effect on landslide erosion in rejuvenated landscapes. The only way these 3 observations can be reconciled, is when the average slope steepness in rejuvenated landscapes is higher than in relict landscapes (and this is backed by the data). We do not think that the overall large number of landslides in rejuvenated landscapes can be used as an explanation for the higher erosion rate due to the fact that the landslides in rejuvenated landscapes are much smaller. Figure 10 (Fig. 12 in the revised manuscript) shows that, if the rejuvenated and relict landscapes had the same slope distributions, the relict landscapes would have a higher erosion rate. However, in reality, the slopes are on average much steeper in the rejuvenated landscapes. We rephrased this finding in the abstract, discussion, and conclusion.

L14-18/**14-21**: *We find that erosion rates in these rejuvenated landscapes are roughly 40 % higher than in the surrounding relict landscapes, upstream of retreating knickpoints and outside of the Rift shoulders. In contrast, we find that slope exerts a stronger control on landslide erosion in relict landscapes. These two results are reconciled by the observation that landslide erosion generally increases with slope gradient and that the relief is on average steeper in rejuvenated landscapes. The weaker effect of slope steepness on landslide erosion rates in the rejuvenated landscapes could be the result of three factors:...*

L301-304/**L341-348**: *While the landslide erosion rate (approximated by the LS_S) is higher in rejuvenated landscapes due to a steeper relief, the relative effect of slope steepness on landslide erosion appears to be weaker in rejuvenated landscapes: we found that steep (>35°) forested slopes display higher shallow landslide erosion rates in relict landscapes than in rejuvenated landscapes (**Fig. 12c**). We propose three mechanisms that could potentially explain this difference: seismic activity, regolith availability, and climate.*

L388-392/**L451-457**: *Rejuvenated landscapes display a higher shallow landslide erosion rate than relict landscapes. Contrarily, the effect of slope steepness on landslide erosion rates is smaller in rejuvenated landscapes. These two seemingly contradicting results are reconciled by the observations that erosion generally increases with slope gradient, and that the average slope is much steeper in the rejuvenated landscapes. The lower impact of slope steepness on landslide erosion in the rejuvenated landscapes could be the result of three factors:...*

***2.2** Role of mean annual precipitation: The manuscript refers to a difference in precipitation 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 variability across the large mapped area.

Indeed, the MAP is quite large in all types of landscapes and as such is expected to not cause a large difference in landsliding. However, analogous to the MAP, there is a difference in the frequency of high-intensity rainfall-events (∼storms) and thus a difference in the frequency of landslide triggers. We still believe that this discrepancy can explain (at least part) of the difference between relict and rejuvenated landscapes. We better stress that the frequency of threshold exceedance is an explanatory factor for the difference, rather than the MAP.

L327-334/**L381-388**: *A difference in the frequency of landslide-triggering rainfall events could be a third explanation for the lower impact of slope steepness on landslide erosion in rejuvenated landscapes. Based on the global rainfall threshold proposed by Guzzetti et al. (2008), we observe that the rainfall threshold for landsliding is exceeded more often in relict landscapes. However, due to these differences in rainfall, we would not only expect a higher erosion rate in relict landscapes, but also a higher LS_F. The latter is not the case: slope steepness appears to have a lower effect on LS_F in relict landscapes than in rejuvenated landscapes (**Fig. 12f**). This discrepancy between erosion and frequency could be linked to two factors: differences in regolith thickness which allow for larger landslides in the relict landscapes, and seismic fracturing allowing for smaller (and more) landslides in the rejuvenated landscapes.*

Yet, we did not longer present the 'rainfall' hypothesis as the prime explanation for the differences between relict and rejuvenated landscapes. Instead, we first introduce the 'earthquake' hypothesis.

L305-306/**L349-350**: *Seismic activity is a first factor that could explain why slope has a different impact on landslide erosion in rejuvenated and relict landscapes. . .*

We specified the difference in trigger frequency (17 %) in the results. However, we believe that reporting the exact numbers of threshold exceedance averaged over relict and rejuvenated landscapes (24,891 versus 21,209), would have little meaning to the reader.

L253-255/**L287-289**: *Similarly, we find that within the rejuvenated landscapes the 2 day 15 mm threshold is exceeded less often compared to the relict landscapes (17 % difference, $p<0.01$), indicating that intense (potentially landslide-triggering) rainfall events occur less frequently.*

2.3 *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.*

Within Figure 6 (Figure 7 in the revised manuscript), we now make a differentiation between landslides in forest, non-forest and deforested land. Moreover, we added a panel in the figure illustrating some shallow landslides and the delineation of their total and source area, a panel to show the distinction between rejuvenated and relict landscapes zoomed in for a smaller portion of the landscape, and a panel with a schematic distinction between the relict and rejuvenated landscape. Note that we only show stationary knickpoints, so we do not make a distinction between knickpoint types in the legend. Within Fig. 12 of the revised manuscript, there is already a presentation of the slope distribution in different landscapes and the distribution of landslides in function of slope.

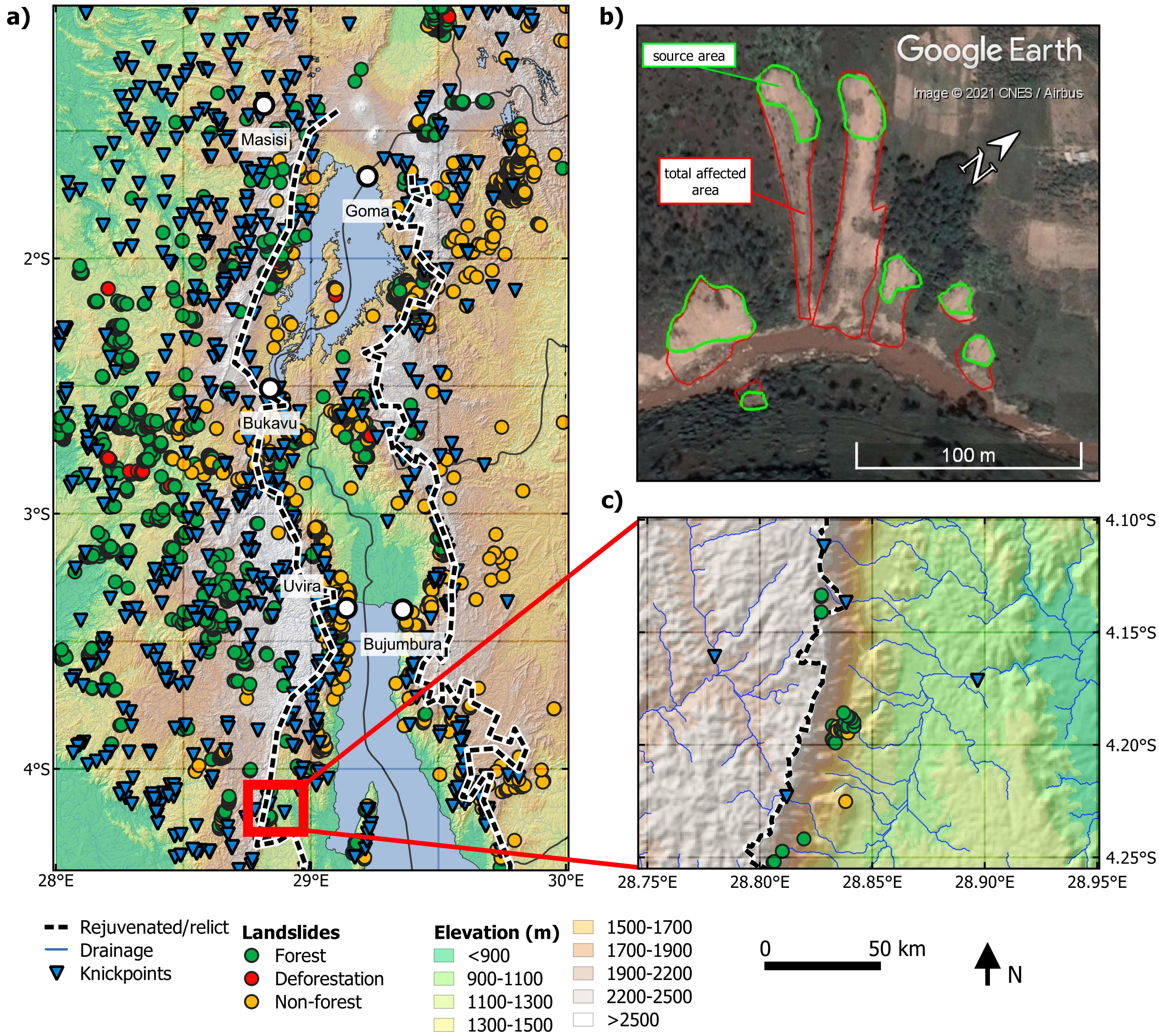

Figure 6. Landslide and knickpoint inventory for the NTK Rift. a) *We identified 7,994 shallow recent landslides that occurred either in forest, non-forest, or after deforestation, and 673 non-stationary knickpoints. These knickpoints were used to separate the rejuvenated landscapes between the Rift shoulders from the surrounding relict landscapes (black-and-white line).* ***b)*** *Example of shallow landslides in Rwanda (29.7909 °E, -1.7151 °S) and the delineation of their total area (red) and source area (green). c) Close-up of the Rift shoulder West of Lake Tanganyika. The method for delineating the rejuvenated landscapes inside the Rift shoulders is precised in Section XX.*

2.4 Otherwise, my final main comment is similar to ***1.1a*** *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.*

See **Comment 1.1a**. The goal of this study is to investigate the impact of deforestation on landslides, while deforestation only affects regolith stability in the first couple of meters. For that reason, we want exclude deep-seated/bedrock landslides on which deforestation is expected to have much less impact. We think it is important to stress that we focus on shallow landslides, but we acknowledge that this choice was poorly justified in the manuscript. We adapted the methods section (see **Comment 1.1a**).

2.2 Minor comments

2.5 *L10-13 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".*

See **Comment 2.2b**.

2.6 *L17 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)?*

This is a good suggestion. However, the structure of the abstract as it is now mirrors the structure in the methods, results, and discussion sections. If we would change the structure of the abstract, we would have to rewrite large parts of the main text as well (we cannot simply switch the order of paragraphs in the later sections due to the way certain hypothesis and concepts are presented). Therefore, we would like to keep the structure of the abstract as it is, unless the reviewer does not mind that the abstract structure is not an exact copy of the structure in the other sections.

2.7 *L47 Have a new section here, on study area?*

We added an extra section title.

L54/**L59**: *2. The North Tanganyika - Kivu Rift region*

2.8 *L76 I suppose there may be reasons why land is "non forest" – i.e. grassland (e.g. precipitation, temporal, seasonal climate) – apart from anthropogenic factors? But perhaps not true in this case, but that could be clarified.*

Indeed, we wrongly assumed that all landscapes in the NTK Rift were forest before human intervention. Yet this was not the case, parts of Rwanda and Burundi have a seasonal climate where savanna is the expected natural vegetation (Beck et al., 2018). Another example is the Virunga volcanic province, where grassland was omnipresent until the large herbivores disappeared and trees started to grow (Verschuren, 1987). We adjusted the text accordingly (in the introduction and in the methods).

L65-69/**L70-74**: *In the natural context, prior to widespread human activity, forests covered most of the DRC and the mountainous Rift shoulders in Rwanda and Burundi, while the vegetation transitioned to woodland savanna towards the east of our study area (Ellis et al., 2010; Aleman et al., 2018;* **?***). Only since the beginning of the 20th century, large scale deforestation has taken place, especially along the Rift shoulders and in Rwanda and Burundi (Ellis et al., 2010; Aleman et al., 2018).*

L85-90/**L93-98**: *Both deforested and non-forest land encompass land use classes such as bare land, cropland, grassland, and urban land. Historically, current 'non-forest´ land used to be either savanna grassland or forest (***?***). The difference between 'non-forest' land that used to be forested in the past and 'deforested' land is the elapsed time since deforestation. Thus, the 'non-forest' land either underwent deforestation before the year 2000, or was never forest in the* *first place. 'Deforested' land experienced deforestation over the last two decades.*

2.9 *L80 a brief summary of some of this would be welcome in the abstract (see comment above).*

See also **Comment 1.7**. We added one sentence to the abstract to clarify how we delineated the rejuvenated landscapes.

L13-14/**L13-14**: *Rejuvenated landscapes were defined as the areas draining towards Lake Kivu or Tanganyika, and downstream of migrating knickpoints.*

2.10 *L103 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.*

The climate model is a purely physical model, hence no ground data was used for calibration. However, it was evaluated with satellite data and one other climate model (ERA5). generally, the model predicts a higher precipitation than what the satellite products suggest, yet this is no surprise as satellite products typically underestimate the real amount of rainfall (Dinku et al., 2011; Monsieurs et al., 2018b). Moreover, the climate research group at the Vrije Universiteit Brussel (in cooperation with the KU Leuven) will soon submit a paper where the climate model is compared to ground truth data, and one of their conclusions is that the model used in this paper is the most accurate for the region. We rewrote the paragraph to better discuss this

118-123/**L129-134**: *The rainfall pattern is derived from a regional climate simulation with COSMO-CLM, a physical model, for the period 2005-2015 and using the ERA5 reanalysis product for the initial and boundary conditions of the atmosphere (Van de Walle et al., 2019; Hersbach et al., 2020). Due to in-situ data scarcity, evaluation of the simulated precipitation amounts is restricted to a comparison with a set of satellite products. Generally, the satellite data suggests lower amounts of rainfall compared to the simulated model output (Van de Walle et al., 2019), yet this was expected as satellite products tend to underestimate the actual precipitation (Dinku et al., 2011; Monsieurs et al., 2018b)...*

2.11 *L108 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?)*

We agree that this result could be better highlighted. We tried to keep the reference in the abstract to this rather short, since we do not want to make the abstract too long.

L11-13/**L12-13**: *Moreover, to account for the impact of rock strength on both landslide occurrence and knickpoint retreat, we limit our analysis to rock types with threshold angles of 24-28°.*

2.12 *L137 more info is going to be needed on the depth aspect here.*

See **Comment 1.1a**. We better clarified why and how we identified 'shallow' landslides

2.13 *L139 what is the normalising area here (km^2), 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.*

The normalizing area is dependent on the type of landscape and/or the slope interval we calculate the LS_S for, hence we cannot provide one value. The conversion to an annual rate (year^{-1}) is quite complex for our context (given the uneven Google Earth imagery). We therefore chose to dedicate a separate subsection to the development of these calculations (3.2.2). We added a reference to this section

L164-165/**L177-178**: *Each landslide is manually assigned a polygon delineating the source area so that the total source area LS_S can be calculated (m^2 km^{-2} year^{-1}; Section 3.2.2)*

L166-167/**L179-180**: *To each landslide, we also manually assign a point of initiation used to calculate the landslide frequency LS_F (#LS km^{-2} year^{-1}; Section 3.2.2)*

2.14 *L148 it would be useful to explain the satellite data source if possible.*

We added information about the timing of the used images and the companies that produced them.

L162-164/**L175-177**: *All images used in the analysis are of very-high spatial resolution, ranging from 30 to 60 cm. The images in © Google Earth are provided by either © DigitalGlobe or © CNES/© Airbus and they were captured between 2000 and 2019.*

2.15 *L170 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?*

See **Comment 1.2b**. we tried to write the methodology more clearly, and we added the definition of imagery density close to where it is mentioned in the equations.

L195-200/**L220-226**: *The landslide inventory is expected to be biased due to spatial differences in the imagery density d (**Fig. 3b**), defined as the total number of available images at each location, as vegetation regrowth might erase the spectral signature of landslides before they are captured in imagery. Hence, we expect to detect more landslides in areas with higher imagery density. To compensate for this bias we assume that the probability of identifying a landslide in a certain region increases linearly with imagery density in that specific region. **Equation** (7) then becomes:*

$$LS_F = \frac{1}{A}\sum_{i=1}^{n}\frac{1}{r^i\, d^i}, \tag{11}$$

In the follow-up text, we specify that we are not certain this linearity assumption will hold, and that we will check (visually) whether or not we have to take extra measures:

L201-206/**L227-232**: *with d^i the imagery density observed at the location of landslide i. Note that there can be a saturation of the information provided by the imagery: when the imagery density is high, the availability of one extra image will have no to little effect on the observed number of landslides. We validate our assumptions of linearity and saturation by visually assessing the dependency of landslide density (# landslides km^{-2}) on imagery density. If the assumption of linearity does not hold, we have to apply a non-linear transformation on the d^i values. If saturation is problematic to our inventory, we have set a maximum value for d^i.*
Note that we specify that this assessment is visually:

L203-204/**L229-230**: *We validate our assumptions of linearity and saturation by visually assessing the dependency of landslide density (# landslides km^{-2}) on imagery density.*

2.16 *L191 I found this section hard to follow. It needs to refer to the necessary equations, and make clear what the issue here is.*

We rewrote this paragraph to better express our objectives and our methods to reach it

L220-226/**L246-254**: *In order to assess the impact of slope steepness on the LS_S (a proxy for landslide erosion), we first reclassify the slope values between 0-50° into 10 classes of equal width, and subsequently apply **Eq. (10)** to each slope class and the landslides therein. Similarly, to assess the impact of slope steepness on LS_F, we apply **Eq. (8)** to each slope class and its landslides. Furthermore, we estimate the degree to which our LS_S and LS_F calculations are affected by outliers and/or extreme landslide events. First, we divide the study area in 50 East-West bands of equal width. Second, we calculate the LS_S and LS_F for each slope class 50 times, each time leaving out the slope and landslide data for a single east-west band. In other words, for each run we slightly perturbate the landslide inventory.*

2.17 *L208 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.*

We agree that the phrasing a bit complex. We tried to restructure the text by making it a bit longer (allowing for more explanation) while using shorter paragraphs.

L233-246/**L261-280**: *Determining the LS_S in function of the time elapsed since deforestation (t_{def}) is necessary to characterize the post-deforestation landslide wave. Because t_{def} is temporally dynamic this analysis requires two components: i) the total area $A_{t_{def}}$ in which we can observe land that was deforested t_{def} years ago, and ii) the total affected area of landslides that happened t_{def} years after deforestation. The first component, $A_{t_{def}}$, entails all areas where the sum of t_{def} and the year of deforestation lies in the time range between the age of the oldest and newest post-deforestation image in © Google Earth. For the second component, we only include landslides for which the time between deforestation and landsliding ($t_{def \rightarrow LS}$) is equal to t_{def}.*

There is a considerable degree of uncertainty associated to $t_{def \rightarrow LS}$ since we do not know the exact timing of the landslides (the occurrence is situated between the capture times of the image where it was initially observed and the preceding image). Similarly, we know the year of deforestation but not the exact date. To assess the uncertainty on the timing of deforestation and landslide occurrence, we calculate the LS_S 100 times, each time sampling a new $t_{def \rightarrow LS}$ for each landslide. For each sample of $t_{def \rightarrow LS}$, we make two assumptions: i) the exact moment of deforestation (the lower limit of $t_{def \rightarrow LS}$) is assumed to be distributed uniformly in the reported deforestation year. ii) The timing of landslide occurrence (the upper limit of $t_{def \rightarrow LS}$), is assumed to be distributed uniformly between the capture times of the image where it was initially observed and the preceding image.

2.18 *L213 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?*

We found that this was potentially an issue for 495 landslides (while 378 were directly linked to deforestation). We think this is the most accurate way of making sure that the landslides were caused by deforestation. Looking at the surrounding forest is certainly an option, but we have two reasons not to apply this strategy: 1) landslides, especially in the DRC, can lay bare soil and potentially minerals, which will incite people further deforest the surroundings. Potentially, trees around the clear patch can be more susceptible to windfall. We do not know much about these processes, but we think that this strategy would add an extra layer of uncertainty and complexity to our methods/results. 2) Moreover, the omission of these 495 landslides, and thus potentially of some deforestation-induced landslides, does not mean we underestimate the deforestation effect. This is because the omitted landslides fall within a time range we do not consider in the calculations of the deforestation effect.

L270-272/**L304-305**: *In total, we found 873 landslides in deforested land, yet for only 378 of those landslides we could be sure that they were preceded by deforestation (Section 3.2.4)...*

2.19 *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?*

We simplified the figure and its caption to make the story more clear and stress the uncertainty related to the data of deforestation (we only know the year, but not the exact date).

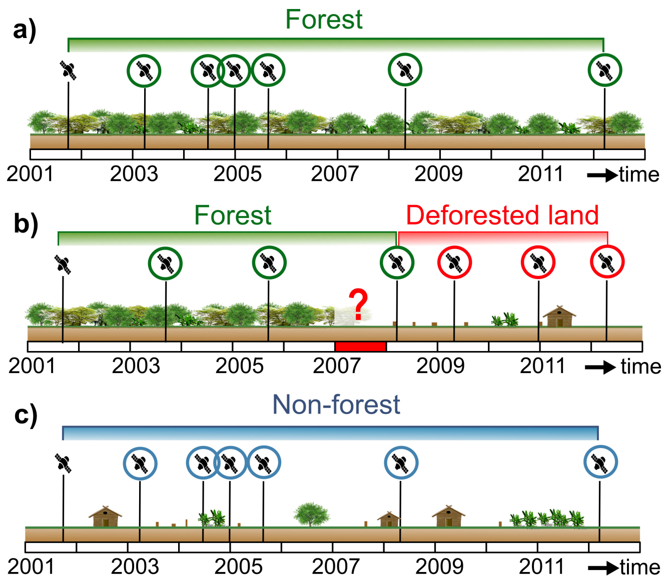

Figure 6. Schematic overview of the three considered forest cover scenarios in © Google Earth. *The satellite icons signal the availability of a © Google Earth image and the colored circles indicate whether we can observe recent landslides in the concerned image.* ***a)*** *Forest scenario: each landslide observed in these areas is linked to forest cover.* ***b)*** *Deforestation scenario: only landslides observed starting from the second © Google Earth image after the year of deforestation are considered to be linked to deforestation (in other words, we can only observe deforestation-induced landslides in imagery that is encircled in red on the figure). Note that we do not know the exact moment of deforestation, only the year (indicated with the red bar) is reported.* ***c)*** *Non-forest scenario: every landslide observed in these regions is linked to non-forest.*

2.20 *L220 It might be significantly different in a statistical sense, but the value is pretty similar* *in terms of MAP...*

See **Comment 2.2.** We no longer put the climate hypothesis as the main explanation, and we better stress that it is the frequency of landslide-triggering events, rather than the MAP, that could cause differences in landslide frequency/erosion.

2.21 *L221 give the values here.*

See Comment 2.2. We think that reporting the frequency of threshold exceedance (24,891 in relict landscapes and 21,209 in rejuvenated landscapes) does not have an added value for the reader. The size of this number depends heavily on the temporal resolution of the data and the period of observation, hence it cannot be interpreted intuitively by the reader. Therefore, we only changed the text to mention the relative difference (17 %)

L253-255/**L287-289**: *Similarly, we find that within the rejuvenated landscapes the 2 day 15 mm threshold is exceeded less often compared to the relict landscapes (17 % difference, p<0.01), indicating that intense (potentially landslide-triggering) rainfall events occur less frequently.*

2.22 *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.*

See **Comment 2.3**. We changed the caption of the figure to refer to the text where the delineation of the rejuvenated landscapes is explained. We also show a close-up of the boundary between rejuvenated and relict landscapes. We only present the non-stationary knickpoints (otherwise the figure becomes too crowded).

2.23 *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?*

Fig. 8 (Fig. 9 in the revised manuscript) is valid for the entire area that falls under the rocks of Cat. III (the strong lithology to which we narrow our analysis). Due to the exclusion of the 'weaker' rock categories, we automatically exclude the largest city areas (Bukavu, Bujumbura, Uvira)? Concerning the figure and the lower landslide concentration for imagery density≥12, we think it is rather caused by the small areas available for these large densities. We added a line of explanation to the results and changed our caption of the figure to better describe its contents:

L275-276/**L313-315**: *The proportion of the study area with a higher density than 12 images is negligible (1.5 %) and contains merely 1 % of all landslides in the inventory. Hence,...*
Figure 9. The impact of imagery density (the number of available images in © Google Earth) on the number of observed landslides. *We only show the results for the rocks of Category III (Section 4.1). Therefore, major cities that are characterized by a high imagery density like Bukavu, Bujumbura, and Goma are excluded from this figure.* ***a)*** *Impact of the imagery density on the number of observed landslides. The number of landslides seems to increase linearly with imagery density up to 12 images. The cumulative landslide proportion for a*

certain value shows the % of the landslide inventory contained in areas with an imagery density equal or lower than that value. ***b)*** *The evolution of imagery availability between 2000 and 2018.*

2.24 *L267 what does the "equal steepness" mean.*

We adapted the text to make this part more clear (also in line with **Comment 2.1b**).

L301-303/**L341-344**: *While the landslide erosion rate (approximated by the LS_S) is higher in rejuvenated landscapes due to a steeper relief, the relative effect of slope steepness on landslide erosion appeared to be weaker in rejuvenated landscapes: we found that steep (>35°) forested slopes display higher shallow landslide erosion rates in relict landscapes than in rejuvenated landscapes (**Fig. 12c**)*

2.25 *L269 it's not much drier! Both have almost or greater than 2 m of rain per year!*

See **Comment 2.2** and **2.21**. This was nuanced, and the 'climate' hypothesis is no longer the main hypothesis for explaining the difference response of landslides to slope steepness in rejuvenated versus relict landscapes.

2.26a *L270 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?*

This observation is correct, and we tried to discuss this somewhat by mentioning that if the erosion rate is higher in relict landscapes, we would also expect a higher frequency in relict landscapes, yet this is not the case. So, although a difference in rainfall-triggering events of 17 % is likely to contribute to differences in erosion, it is not a sufficient explanation (otherwise we would also expect a higher frequency in the relict landscape). The discrepancy between the erosion and frequency observations can then be caused by two factors: a thicker regolith cover than allows for larger landslide sizes in the relict landscapes, and seismic fracturing that decreases the critical area for failure in rejuvenated landscapes, thus allowing for more shallow landslides.

For the updated paragraph, see the response to **Comment 2.2**.

2.26b *And/or the geomorphology – are the rejuvenated landscapes typically smaller catchments, and so landslides are generally smaller (constrained by hillslope length)?*

Since we only look at the source area (and not the total area, which is the source area+run-out), we do not expect that hillslope length plays a constraining role with regard to the landslide size in rejuvenated landscapes.

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

A good suggestion. Especially the illustration of the background erosion rates in rejuvenated and relict landscapes would help to 'visualize' how after 15 years the situation seems to 'normalize'.

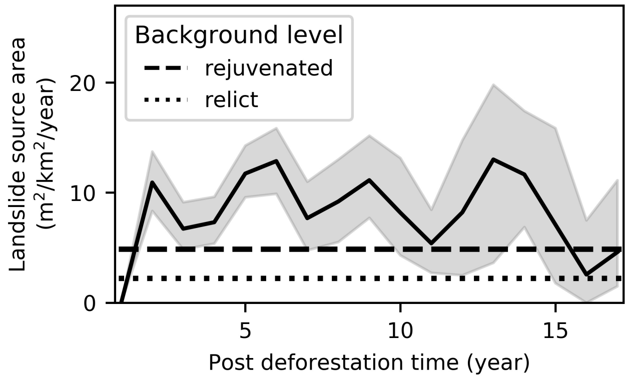

Figure 13. Deforestation-induced landslide wave. *Overall landslide source area (LS_S, m^2 km^{-2} $year^{-1}$) in function of time elapsed since deforestation, based on the analysis of 374 post-deforestation landslides in rocks of category III (Section 4.1). The grey area is the 90 % confidence interval, derived from 100 iterations of LS_S calculations (Section 3.2.4). The dashed and dotted line represent the overall erosion rates in rejuvenated and relict landscapes. There are not enough observations to make two separate consistent plots for rejuvenated and relict landscapes (**Fig. A1**)*

2.28 *L290 yes – this makes sense (link to 270 above)*

See **Comment 2.2** and **2.26**, we fine-tuned this discussion part.

2.29 *L302 That is not what that figure shows, in my opinion – the rate of landsliding seems to be pretty constant across the window.*

See **Comment 2.27**. The rates seem to fall back to the background erosion rate levels. Though, we agree that more observations (or at least a longer period of observation) would be useful to confirm this. We add some nuance to the discussion.

L338-340/**L393-394**: *After the wave has passed, landslide erosion rates decrease to a level similar to the average erosion rate in the region (**Fig. 13**). However, a longer period of observation would be useful to confirm whether or not this effect persists after 15 years.*

2.30 *L306 again, "fading out" is not what I can see in Figure 11.*

This is indeed confusing, we did not mean that the activity becomes 0, but rather that the surplus in activity due to deforestation disappears. We will better describe this.

L348-352/**L405-411**: *The landslide erosion rates in non-forest land are much lower than in deforested areas and, in fact, similar to or lower than what has been observed in forests (**Fig. 12a-b**). Thus, even when there is no regrowth of forest vegetation, the landslide erosion rate returns to normal levels some time after deforestation. A possible hypothesis that might explain this result is that once the effect of deforestation on landsliding has worn out, the regolith mantle is protected as efficiently by forest cover as by grassland and crops, despite the presence of human practices such as terracing that could promote landsliding (Sidle and Ochiai, 2006).*

2.31 *L322 where does this depth come from?! It needs more discussion.*

See **Comment 1.36**. This 'depth' raises indeed too many questions. Instead of assuming this fixed depth, we think it will be better if we use a V∼A scaling relationship from (Larsen et al., 2010) (namely the one proposed for soil landslides in Uganda, as this is expected to be the most relevant for study area).

L368-370/**L430-432**: *Using the volume∼area relationships presented by (Larsen et al., 2010) for soil landslides in Uganda, we obtain a rough estimate of the landslide volumes. As such, we find that the* LS_S *in rejuvenated landscapes corresponds to a denudation rate of ca. 0.006 mm* $year^{-1}$.

2.32 *L338 I would remove the word "shallow" from here, as you don't actually know how deep they are.*

See **Comments 1.1a** and **2.4**. We think that it is important to stay focused on shallow landslides, given our objective to study the impacts of deforestation (which should not have much impact on deep-seated landslides). Hence, within the conclusion and title, the 'shallow' aspect should be mentioned to make sure that readers do not generalize our findings to all types of landslides (e.g. deep-seated)

2.33 *Figure A1- this would be better than Figure 11 (and add the number of landslides and explanation of the fine lines and grey shading).*

See **Comment 2.27**. The grey area represented the range of observations (from our 100 iterations), whereby each black line presented one of these iterations. We simplified the figures by only showing the interval containing 90 % of the iterations.

it's a reasonable request to split up the graph for rejuvenated and relict landscapes, but we think that in the separate landscapes there might be not enough observations (∼185 in each landscape type) to properly characterize the post-deforestation landslide wave.

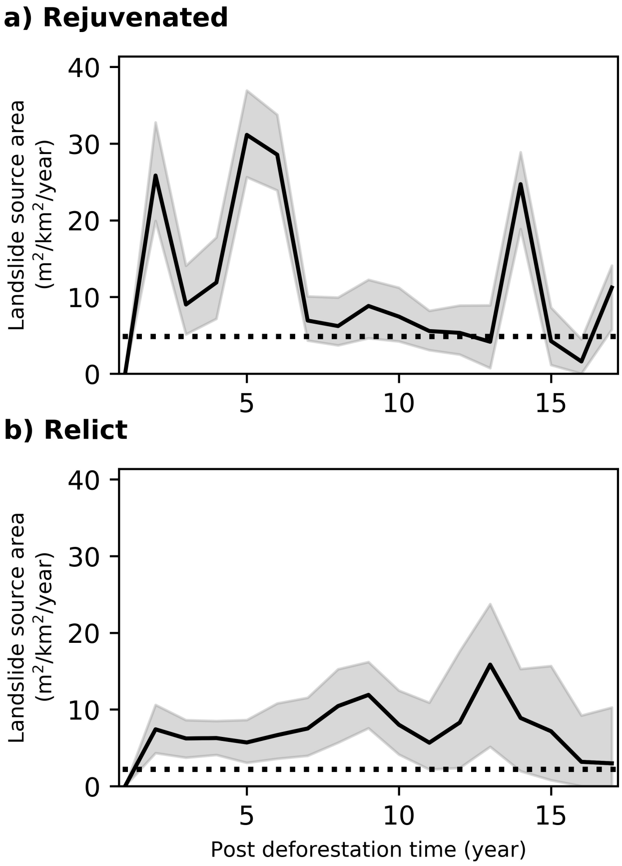

__Figure A1. Deforestation-induced landslide wave in different geomorphic contexts.__ Overall landslide source area (LS_S, m^2 km^{-2} $year^{-1}$) in function of time elapsed since deforestation. The grey area represents the 90 % confidence interval, derived from 100 iterations of LS_S calculations (Section 3.2.4). The dotted line shows the overall erosion rates in the different contexts. __a)__ Landslide response to deforestation in rejuvenated landscapes, characterized with 193 landslide instances. The irregular curve suggests that, in order to better characterize the deforestation-induced landslide wave, we need more landslide and deforestation observations and over a larger area. __b)__ Landslide response to deforestation in relict landscapes, characterized with 181 landslide instances. The dotted line represents the background erosion rate in relict landscapes.

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