Firstly, we want to thank the reviewers' careful reviewing and good suggestions to the manuscript. The reviewers have wealth of experience in landslide inventory and analysis; their professional comments were very helpful to improve the quality of this paper. We would like to express our sincere thanks to them. We tried our best to reply these comments one by one.

<table>
<tr><th colspan="2">Comments</th><th>Answer</th></tr>
<tr><td colspan="3">Main Comments</td></tr>
<tr><td>1</td><td>The authors better explain the methods and data used to obtain their inventory of RTL. This is an improvement but the author fail to acknowledge the limit of their inventories due to resolution and revegetation issue. This must be stated, with a reference to the recent manuscript by Marc et al., 2019, where in a fraction of the study area of the authors (the Bhothe Koshi valley) hundreds of landslides were mapped between 2010 and 2014, while the authors catalogue (1992-2015) contain only a few in this zone (Fig 3).</td><td>We check our landslide inventory within Bhote Koshi valley; 300 hundred of landslides were interpreted. .During our interpretation, we also found that, the vegetation grows very fast in this area, many landslides regenerated in short time after they occurred, especially for small size of landslides. Also our high resolution images for landslide inventory were not obtained every year during 1992 to 2015. These reasons may cause differences between different inventories. In Fig.3 we used the landslide polygons to draw the map, because the outlines of landslides were very thin, and the scale of the maps was very small, so the landslides were not clearly visible. We adjust the landslide distribution map of Fig. 3.
In our manuscript we added the limitation of RTL inventory in Line 156-158.
Main limitations affecting the landslide inventory are ought to a) revegetation on the areas of the landslides that occurred in 1992 and 2015 that impedes their detection on remote sensing images and b) lack of multi-temporal high resolution images in the region (Marc et al., 2019).</td></tr>
<tr><td></td><td>I think the author present a somehow biased discussion on the two results of their susceptibility maps : I agree that RTL and ETL susceptibility maps are driven by different factor. In large part because PGA is very important for ETL, not RTL. But RTL susceptibility map is in the end almost not controlled by monsoon long term precipitation, that should be a little discussed by the authors. As a result this map does not really differentiate, in its prediction; RTL or ETL.
So I think the authors need to state and discuss more rigorously their results: RTL susceptibility does not depends much on monsoon, thus either meteorology is less important than in situ parameters either other meteorological parameters should be used (e.g., monsooon</td><td>We would like to thank for the reviewer's deep analysis on this issue and suggestions.
In the RTL susceptibility assessment, mean monsoon precipitation was taken as the rainfall triggering factor. This factor can only be used to indicate a general tendency for the landslide distribution at regional scale, Instead, most commonly in other studies, as indicator, the daily rainfall on the date of the event and the antecedent rainfall are correlate better with the landslide occurrence In the discussion part, we tried to discuss this issue base on reviewer's</td></tr>
</table>

	variability, cf Deal et al., 2017). This should be explored in future studies. As a result, the RTL maps give a static susceptibility maps, that does not really discriminate landslide from a trigger or another.	suggestion and some references in Line 411-418: *It should be clarified that although, commonly, the daily and the antecedent rainfall are used to describe the rain effect on the landslide occurrence, in this work, what is used is the mean precipitation during the monsoon season. The use of this value is chosen to provide, at regional scale, a general tendency of the landslide distribution. In the RTL susceptibility assessment model, the weight of the precipitation factor is low, which means this factor was not strongly correlated with the landslide susceptibility. As a suggestion, the use of the daily rainfall instead of the mean precipitation during the monsoon is preferred, in order to take into consideration its variability, as the use of the short-term rainfall variability to study the long term historical landslide inventory and susceptibility assessment may more reasonable (Deal et al. 2017).*
Line by Line Comments		
1	L72 : Please rephrase : They are mostly mapping landslide after an earthquake… But not Marc et al., 2015, that mapped landslide for several years before earthquakes in Taiwan, Papua New Guinea and to a lesser extent Japan. Also do not cite Marc et al., 2015 for the Wenchuan earthquake that is no considered in this study. I think Marc et al., 2019 is very relevant to this part of the introduction as it also focussed on RTL before the earthquake and in zones not affected by the earthquake.	We modified this sentence, and corrected the references. A new phrase was added in Line 72-74 as follows: *There are fewer studies, carried out on multi-temporal RTL inventories in Taiwan, Papua New Guinea and Japan, which focus on the comparison of the RTL considering or not earthquake effects (Marc et al. 2015).*
2	L75-76 : Again you misuse Marc et al., 2015. The post earthquake RTL that are reactivation of coseismic landslides are very limited. There does not seem to be a clear correlation with coseismic pattern. The same is observed in Marc et al., 2019 where only 20-30% of RTL caused in 2015, just after the EQ, are spatially connected to ETL.	We modified this part in Line 75-80 as follow: *The problem with the studies indicated above is that the rainfall-triggered landslides that occur shortly after a major earthquake are generally following the same spatial patterns, due to the availability of large volumes of landslide materials of the co-seismic landslides (Hovius et al., 2011; Tang et al., 2016; Fan et al., 2018a). However, other studies argue that there is not a clear correlation of rainfall-triggered landslides with the co-seismic pattern, as only the 20-30%*

		of the RTL that occurred just after an earthquake, are spatially related to the ETL. The post-earthquake RTL that correspond to the reactivation of the co-seismic landslides are very limited (Marc et al. 2019).
3	L95: I think your two questions can be more or less summarized in one : Because if they have the same control, the susceptibility of ETL and RTL should be the same, and one can be used for predicting the other. Opposite is expected if they are controlled by different factor you will not be able to use on to predict the others.	This part was rephrased in line 98-101: *The question that is addressed is whether different landslide sizes are controlled by different sets of contributing factors. Furthermore, it will be investigated whether it is possible to utilize inventories of earthquake-triggered landslides (ETL) as inputs for analyzing the susceptibility of rainfall-triggered landslides (RTL) and vice versa.*
4	L121 : Maybe mentioning Marc et al., 2019 here would be a good addition, as it gives the magnitude of annual landsliding in different High Himalayan valleys	Thanks for the suggestion, we added the reference in Line 127.
5	L141-145 and Fig 3: RTL 1992 – 2015 : I think that you should mention here that the RTL landslide dataset are undersampling the actual amount of landslide during the 1992-2015 period: Because of revegetation is rather rapid on landslide scar and possibly because of resolution. An example of that : Marc et al., 2019, mapped between 2010-2014 ~ 350 landslides in a 25x25km ^{2} of the Bhote Koshi valley, part of your study area. Most, if not all of these landlides are not in you 1992-2015 mapping, while your imagery was just a few years after (so resolution or image quality is also likely at play). If these zones had sustained landsliding at these rates during the last 20 years thousands of landslides are missing in this valley, and likely as much in the other valleys. It is not necessary a problem for your study, but it is a bias that should be acknowledged, so that readers do not think it is a comprehensive representation of the landslide rate and location. (E.g., the sentence L148 is inaccurate and needs to be removed: "the different images for the period between 1992 and 2015 we were able to recognize most of the landslides")	We would like to thank the reviewer for this comment. We agree with the reviewer's suggestion. During our inventory we also found that, the revegetation in this is area is very fast. Given the limitations in the resolution of the remote sensing images quality and obtained period, a complete landslide inventory in the whole area was not feasible. However the images were used to compile as much as possible the landslide database, to use it as a sample for this work. Follow the reviewer's suggestion; we added some more phrases to the discussion part, concerning the limitations of the RTL inventory in Line 151.
6	L 211 : Ok but the size distribution also depends on how you define landslide area : For example Malamud 2004, explicitly state they remove all debris flow with long aspect ratios. Marc et al., 2019, found a power-law starting at 1000-2000 m3, when considering only	It is very difficult to separate the scar area from the runout area in this study. This was not possible for, the landslide inventory of 1992, which was digitized on the original topographic

	landslide scar area (retrieved applying a correction on landslide runout, cf Marc et al., 2018). >> So maybe mention these separation are relative and maybe improved by removing the area due to runout in individual and stuying landslide scar area only (Marc 2018, 2019)	maps, as high resolution images during this period were unavailable. Then, at regional scale, the differentiation of the boundaries betweenthe scar area and the landslide run-out could not be made,neither due to lack of high resolution images before the sliding., Morever bias related to the experience of technical people who created the landslide inventory would strongly affect the results. Because of these reasons, for this analysis we decided to use the boundary of the whole landslide without further differentiation between scar and run out.
7	L337 and Table 2: I am surprised by the statement the RTL reflects monsoonal control: In Table 2 you find a very small control of monsoon (x10 ~1) much smaller than the other types of control such as elevation (x1=7), slope (x2=6) or curvature (x3=-10). So the pattern of RTL	The rainfall factor used in the RTL susceptibility assessment was the mean precipitation during the monsoon season. But for the occurrence of landslides, the rainfall intensity is better correlated with the landslide occurrence.
8	L366 : Also limited because they are not comprehensive : See earlier comment on resolution and revegetation.	We add the limitation on RTL in this part in Line 377-379. *Another limitation for this landslide inventory was related to the temporal and spatial resolution of the satellite images, as well as the revegetation the impedes the landslide detection for a complete historic landslide inventory*
9	L368 : Yes event trigger is a challenge. Note that a first database of RTL event inventories was published recently in Marc et al., 2018.	One sentence was added in line 379-381: *There has been an increasing number of researchers working on the development of event-based landslide inventories and databases (Marc et al., 2018), which may be used to supply more samples for the comparison between RTL and ETL.*
10	L386 : "consensus"	This word in the manuscript was corrected.
11	L387-389 : Not only topographic, also mechanical properties (as underlined in Frattini and Crosta 2013 or Stark and Guzzetti 2009). Although the methods are somewhat different you could also mention that Marc et al., 2019 found similar Beta values between ETL and RTL, and also relatively similar to your (2.45-2.55). The cutoff value is much smaller because a correction to remove runout was applied.	New sentence was added in line 400-404 as follow: *Our findings regarding similar cutoff values obtained from different inventories created for the same area are also supporting this argument. This conclusion is also supported by Marc et al., 2019, who found similar Beta values between*

	This ask the question whether a landslide with a long runout is a large landslide ?	*ETL and RTL, but also a cutoff value which is much smaller, as the result of a correction to remove the runout areas from the landslide boundaries.* For the question: This ask the question whether a landslide with a long runout is a large landslide? Landslides with long runout present certain peculiarities as the initial part of the landslide (Source area) is respectively very small, but further material sources exist within its runout area. If we only use the initial source area to define the size of landslide, the landslide size will not be representative.
12	L390 : " precipitation in the Monsoon for RTL, and PGA distribution for ETL) have major influence on the distribution of landslides and susceptibility zones " You cannot state that ! This is true for earthquake but NOT for monsoon, see my comment on Table 2 where the effect of monsoon is very small compared to topography, slope, curvature etc. This is also demonstrated by Fig 10 : ETL susceptibility separate quite well ETL in high susceptibility and RTL in low susceptibility. In contrast, RTL susceptibility does not really distinguish RTL and ETL. This is consistent with the fact that RTL susceptibility is mainly driven by static factor and thus relates to both trigger types.	We would like to delete the phrase.
13	L402: Ok ETL map is specific, but your statement about RTL are not correct : You state that the RTL predict modestly the ETL… But it predict the small ETL almost as well as the small and large RTL. So you cannot say that RTL has much specificity.	We would like to delete the phrase.
14	L414 : "cocoseismic" > coseismic	Corrected.
15	L415 : No Marc et al., 2015 did not say that post-earthquake RTL was following coseismic landslides, see other comments.	Considering the controversy between different studies, we modified this part.
16	Fig 10 : I think some of the values in % are wrong (relative to curve positions...)	We would like to thank the for reviewer for this careful review. We modified the figure and corrected the position of the values.
17	References used and not in the manuscript : Deal, E., Favre, A. C., & Braun, J. (2017). Rainfall variability in the H imalayan orogen and its relevance to erosion processes. Water Resources Research, 53(5), 4004-4021. Marc, O., Behling, R., Andermann, C., Turowski, J. M., Illien, L.,	We added these references in the manuscript.

	Roessner, S., and Hovius, N.: Long-term erosion of the Nepal Himalayas by bedrock landsliding: the role of monsoons, earthquakes and giant landslides, Earth Surf. Dynam., 7, 107-128, https://doi.org/10.5194/esurf-7-107-2019, 2019. Marc, O., Stumpf, A., Malet, J.-P., Gosset, M., Uchida, T., and Chiang, S.-H.: Initial insights from a global database of rainfall-induced landslide inventories: the weak influence of slope and strong influence of total storm rainfall, Earth Surf. Dynam., 6, 903-922, https://doi.org/10.5194/esurf-6-903-2018, 2018. Stark, C. P. and Guzzetti, F.: Landslide rupture and the probability distribution of mobilized debris volumes, J. Geophys. Res.-Earth, 114, F00A02, https://doi.org/10.1029/2008JF001008, 2009.	