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Interactive comment on "Topographic changes due to the 2004 Chuetsu thrusting earthquake in low mountain region" *by* Zhikun Ren et al.

Zhikun Ren et al.

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Editor: Solid Earth Dear Editor in chief: I am sending herewith the revised manuscript titled on "Topographic changes due to the 2004 Chuetsu thrust earthquake in a low mountain region" for possible publication in the " Solid Earth". Thanks for your email on 6th Feb, 2019 to inform us that the Referee comment was posted. During the revision, we explained that why our results did not include canopy effects in a separate response file. Meanwhile, we provide a supplementary file to prove it. We also send our manuscript to a professional company to polish the English as the referee point out that our English is not good enough. Hope now our explanation is acceptable. This manuscript has not been previously published and is not and will not be submitted for publication elsewhere when it is in review for the " Solid Earth ".

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Discussion paper

Sincerely yours, Zhikun Ren

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Reply and correspondence to the Reviewer's comments and suggestions: I am grateful to the reviewer for his/her positive and critical comments and suggestions. The following main revisions and answers are made in reply to the reviewer's general comments and suggestions; these changes have also been made in the text.

Major comments: The study is poorly written and many sentences are unclear, but the key issues are methodological errors. Almost at the end of the manuscript the authors admit that the pre DEM contain vegetation and not the post-DEM. In effect this means that where a landslide occur, its estimated depth will be D*=D+Ht , with D its real depth and Ht the tree height. Of course for landslide on grass land (HtÂń1) or very deep slides (say DâĹij20-30) Ht (typically 3-10m) may not matter. But in places with forest yes. The authors dismiss it because they find a difference of about 0 in a sub area of their study. They simply forgot to say that this zone is an alluvial plain covered mainly by towns and field, thus with HtâĹij0 in most places. Any satellite image demonstrate this see the 3 figures of this review. In contrast they "surprisingly" report (in Fig 5 absolutely no comment in the text) that almost all small landslides (>1000m2) are 5 to 10m deep when they should be around 0.5-2m typically (Larsen et al 2010). This strongly suggest a vast majority of measurement is tracking Ht not D. As a result almost all result and consequent discussion are flawed and not worth further consideration until the author make an in-depth analysis of where canopy effect may play, how much, and what are the resulting uncertainties on individual landslide and estimated erosion.

ResponseïijŽ First, the canopy effect is not as serious as the reviewer suggested. The pre-earthquake DEM is not generated simply from the stereo pair of images but also from the field survey GCP. It is not produced by us but is down-

loaded from the Geospatial Information Authority (GSI) of Japan (Freely available at http://fgd.gsi.go.jp/download); thus, this DEM is already calibrated based on field survey data in order to show the bare earth surface DEMïijĹbut not 100%ïijL'. Second, if the results include the canopy part, then the positive and negative values of the DEM difference should not be comparable. The negative values should be much larger than the positive values because the pre-earthquake DEM is assumed to include the forest, i.e., the Ht. However, according to our results, the results clearly show the landslide scarp and toe; hence, positive and negative values are comparable. If there are large errors caused by the canopy, then the values should be mostly large and negative, or at least they should be systematically negative with respect to the Ht value (typically 3-10 m) as the reviewer commented. In the revised manuscript, we provide a supplementary file that indicates the mean error of the difference elevation by summing the positive and negative values, which indicates that the mean difference in the landslide region is mostly on the millimetre to centimetre scales (Please refer to the last column of Supplementary Table 1). This result indicates that the average value is almost zero, which implies that the negative and positive values are comparable; hence, there are no obvious canopy effects. Third, the absolute values of the landslide volumes are not the main contribution of our research. We finally propose a distribution pattern for the erodible material caused by the landslides. By comparison with the geological model, we discuss the role of the earthquake in topographic evolution. Fourth, with respect to the images mentioned in the reviewer comments, even if there is forest cover, there are also many bare earth surfaces, as shown in Figure 1, such as roads and farmlands (with almost no trees at all), and only part of the mountain top (it should be less than 50% from Figures 1 and 3) is covered by forest. The southernmost part of Figure 1 shows the coverage of forest, which is difficult to calibrate to obtain bare earth DEM. However, the mountain top is usually not vulnerable to landslides, and landslides usually occur on mountain slopes. Hence, our results do not calculate the whole region after differential DEM, and we mainly focus on the reliable results (the most seriously forested region is almost all removed and show much less change in elevation), as

shown in Figure 4B. Fifth, theoretically, if as the reviewer suggested, the canopy effect is so serious, then even LiDAR could obtain only the surface of the top of canopy, then there would not be a problem using pre- and post-earthquake DEMs to obtain the landslide volume and topographic changes.

Hence, we think that the reviewer's comments about the canopy effect do not address a main problem in our manuscript. At least, our results are much more reliable than the volume information from scaling laws; we use real pre- and post-earthquake data.

Detail CommentsïijŽ L41: This sentence is very vague and confusing. The idea that earthquake can contribute to mean topographic base level increase, as to the formation of relief is pretty old (see King et al., 1988, Avouac 2007). We also know other tectonic processes than earthquake redistribute mass and affetc topography (e.g., interseismic processes). These facts are not very well introduced by the authors overall in the whole introduction.

ReplyïijŽThis sentence explains our main idea about the co-seismic effects caused by strong earthquakes in topographic evolution. We cannot consider the old view of earthquakes contributing only to the topographic base level increase (this concept is only partially correct); actually, this view is not true because we use high-resolution pre- and post-earthquake DEMs, InSAR studies, GPS observations, etc. Only a small region along the co-seismic surface rupture zone is uplifted, and only when a thrust earthquake occurs. If the fault is normal, even the region around the co-seismic surface rupture zone is depressed. Regarding a strike-slip fault, if it causes many co-seismic landslides, it also mainly generates an elevation decrease rather than an increase. The study by McPhilips, "the Millennial-scale record of landslides in the Andes consistent with earthquake trigger" also shows the role of earthquakes in topographic evolution. We actually do not want to confuse the readers about some old incomplete views of the role of earthquakes in topographic evolution; therefore, we do not include these quite old references.

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Discussion paper

L46 – 48 : Several of this reference are erroneous : some work do not relate earthquake to topography : e.g.; Montgomery and Larsen 2012 Hovius 2011 is about Taiwan , not the LongMenShan Reply. We apologize for the wrong insertion of references. We have modified the citation.

L49: confusing wording: demonstrated that Ldsl are tought to ? Reply: Thank you for the reviewer's valuable comments. The sentence has been revised to clarify what we would like to express to the readers. "Previous studies have demonstrated that landslides limit the slope"

L51 I would suggest to specify first the location : Recent study in the arid foothills of Peru Reply: Thank you for the reviewer's valuable comment. The sentence is revised by showing the location as the reviewer suggested. "Recent study in the arid foothills of Peru found that erosion caused by landslides did not change much in response to climatic changes;"

L63 end of the sentence unclear Reply: Thank you for the reviewer's valuable comments. This sentence demonstrates the use of scaling laws to obtain co-seismic landslides; there are large uncertainties. The sentence is revised to "However, using scaling laws to obtain the co-seismic landslide volumes has large uncertainties in different regions."

L64 what mean totally different volumes ? Which different methods ? Again wrong reference Marc 2015 does not present anything about coseismic landslide volume. Reply: Thank you for the reviewer's valuable comments. The sentence is revised as follows: "Different co-seismic landslide volume results have been reported for the 2008 Wenchuan earthquake"

L112 : Change the wording Reply: Thank you for the reviewer's comments. The sentence is revised as follows: "Hence, the topographic evolution in the epicentral area should be closely related to the co-seismic landslides caused by strong earthquakes."

L118 : If pre eartquake DEM is from stereo pairs, the height of vegetation will be included. So how do you account for it? Is there a correction on the pre-DEM ? Then it need to be explain and its uncertainties described. Or is the Lidar giving you the post-DEM with elevation including tree height ? But in this case many landslide “depth” will be driven by tree height. For me this is a likely explanation of why most of the small landslides (10-1000m2) depth is between 5-10m in Fig 5. If you look at the global database of landslide (Larsen et al., 2010) in ths size range the mean depth should be around 1m (with significant scatter). Reply: Please refer to the reply to the main comments above. First, the pre-earthquake DEM is corrected. Meanwhile, there is actually a large area of bare earth in the epicentral area that is not densely forested. Even if we assume that there are serious canopy effects, they would not be consistent with our results from the differential DEM (negative values should not be comparable with positive values). It is also possible that the LiDAR data, including the forest canopy, are too dense. Overall, we believe our DEM shows real results for the topographic difference, as indicated by the landslide scarp and landslide toe area in one landslide in Figure 4c. Theoretically, the global landslide dataset could not show the pattern of co-seismic landslides at all; it is of different scale. Meanwhile, the landslide inventory data are from the formally available data (compiled not by one researcher but by a group of Japanese researchers, which also includes much field work; we believe the results are reliable because the deep-seated and shallow landslides are from the combination of remote sensing image interpretation and field validation).

L207 : Where can we see that ?The authors need to support this claim with a supplementary figure at least. Reply: Thank you for the reviewer’s comments. Figure 4b shows the reliable results for the overall topographic changes. By applying the landslide inventory polygon, we clearly show the landslide scarp and landslide toe area in Figure 4c.

L208: I disagree with its claim. Where there is no landslides the precision of the difference DEM may be high (low noise level) but this noise level may very well change

between the landslide zone (with steep topography even if not very tall) and the valley to the North. Additionally the author compare the biggest landslides to the mean noise. . .They should compare to mean landslide depth. Reply: Thank you for the reviewer's comments. There are indeed different precisions according to the DEMs. The studies using scaling laws to derive landslide volume include many large errors, but many papers have been published. We are using higher-resolution data and results, which is clearly shown in the manuscript, but the reviewer seems not to believe it. We have done our best to explain our study and try to show more reliable results to the reader. However, there are also many "mission impossible" tasks in scientific research.

L212 : How were shallow and deep-seated landslide classified ? Clearly I would not call deep seated a slide with a 1-2 m depth, while some of them have 10cm. . . And 10m is not shallow and except in some place most likely much deeper than the soil layer. Reply: Thank you for the reviewer's comments. I am not sure where the reviewer found that deep-seated landslides are 10 cm. Our results show the overall topographic changes. Even if the reviewer's point is true, it is possible that a deep-seated landslide may have some less deformed shallow parts at the edges.

L234-238 : Here the author acknowledge, very late, the problem of canopy (this should be done in the method !). And then dismiss it on the argument that no systematic error are observed and that low elevation difference exist in the "blank area". Well the reason is simple enough : in the carefully selected area of fig 4b, there is only city, agricultural fields and river flood plain. SO very limited high ranging vegetation, and if the images were taken when fields were denuded it would make sense to obtain overall no elevation change. Reply: Thank you for the reviewer's comments. However, the sequence of the figures should match the contents of the manuscript; from the overall structure of the manuscript, we could not first show the results of Figure 4 and then explain the details in the method section. We need to first show Figure 3. If we included this part in the method, maybe other reviewers would comment that the methods and results are mixed. We hope that the reviewer can understand.

Authors figures Fig 1-4 Overall the author show lithological maps everywhere that are almost not discussed. IN contrast a map differentiating agricultural lands, forest , grass land, and shrub or medium height vegetation would be much more useful and a potential place to start to evauate methodological error related to canopy.

Fig 1: could be an inset. Reply: Thank you for the reviewer's comments. This figure shows the overall tectonic setting of the Chuetsu area within a larger geological context to allow readers to better understand the background of mountain building in this region. Meanwhile, the DEM data covering Japan also show the orogenic features and mountains clearly. We actually tried to show this information as an inset figure, but a very small inset figure could not show the geological background clearly.

Fig 3C : The color scale need to be re adjusted to something like +10 / -10 Same for Fig 4, this stretch hides all details and just show the biggest slides. Reply: Thank you for the reviewer's comments. However, we do not agree with the reviewer. This disagreement is because there are deep-seated landslides, very shallow landslides and non-deformed regions. The scale should not be stretched to show false information by adjusting the real elevation change to +10 to -10. This action would mean that we removed other deformations between +30 to +20 and -30 to -20.

Fig 5: We need to see the uncertainties on the parameters of the V-A relationships and the associated confidence interval on the plot (along the fitted lines). In any case the biggest issue is that the trend of size with depth do not exist for "shallow" landslides. They tend to be randomly distributed around 10m, that is most likely a methodological error, not acknowledged nor discussed by the authors. Reply: Thank you for the reviewer's comments. We do not understand the point of this comment. The figure is in a log plot because the scatter of landslide depths is so large, but how could the figure be interpreted by the reviewer to indicate $\sim$10 m?

Fig 8B : No idea what the points are or the shade line and how they are drawn... Reply: Thank you for the reviewer's comments. The points show the average denudation

depths of the catchments. This figure is just a very simple plot using X (distance to the fault) and Y values (denudation depth of the catchment).

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-3/se-2019-3-AC1-supplement.zip

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