

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

### **Anonymous Referee #1**

Received and published: 3 April 2019

Dear Editor,

I have read the rebuttal letter of the authors. I see the authors have improved the english and clarified a number of minor points.

However they have refused to include any substantial change or additional discussion on their methods and results about landslide volume. I am not convinced by the different arguments of the authors and still consider that the study cannot be published without a rigorous assessment of the methods uncertainties. The authors suggest some of my suggestions are impossible but in the contrary I think the authors have access to the data and just need to perform a series of test and analysis that should not take longer than a few weeks.

I discuss below the points raised with the authors with which I am still disagreeing. I have also tried to clarify my current issues and suggestion with the current analysis of the authors.

### Major comment

The author argue canopy effects is not important but I do not see their arguments as very convincing. On my opinion they present several conflicting or poorly supported arguments (I briefly summarize them before commenting on them): > 1/ The GSI has GCP ground control point. Ok but unless the author show a map of them and show that they are included in the occasional forested land I do not think it exclude that the digital surface will go over forested lands.

2/ Negative and positive part should not be comparable (difference by tree removal should always be much bigger than positive deposits ). I agree and this is a good approach to validate their methods and data. However I do not see a good support in the current revised paper. I currently cannot understand what was done with the Suppl. Table : There are ~300 entries with area (of the landslide ?) and a fraction of them have indicated a Min Max Mean Std and Average Error. Presumably the result of the DEM difference. This table is not describe din the revision. I do not know why most data are not reported, or how was obtained the average error. I do not understand why some landslides have a MEAN change large and positive (5m or more) that would suggest the deposit is much larger and thicker than the eroded area, that does not make sense from a mass balance point of view. . .

3/ The absolute value of landslide volume is not the main contribution. » I think this is clearly contradicted by the whole abstract that can be summarized as: “ Landslide volume matter to understand the impact of EQ on topography. It is hard to estimate. We propose a new method. We use landslide volume to obtain denudation and thus EQ mass balance and discuss it”. I think the authors cannot escape a more detailed discussion on their landslide volume estimate. If biased the whole tectonic discussion

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would be too.

4/ This argument is mixing several points : forest is not everywhere ; Not all landslides are on forested slopes ; Lidar may be blocked by forest when very dense; I agree that forest is not everywhere, but it does not remove the need to examine quantitatively the errors caused by the presence of forest. Again, the authors have aerial imagery available, it would be easy to select a number of forested zone where landslide have occurred and another set of landslides occurring on grassland. Then plot the 2 different dataset (could be just colored point in Fig 5), and discuss possible difference bias. It would be especially important for the medium size landslides.

If Lidar is also blocked by forest, it would explain they find overall similar DEM where landslide did not occurred but the difference between Pre and Post landsliding would still be dominated by the removal of the tree so this point does not answer my worries (although the author should check and write in the methods whether or nor the LIDAR is expected to have passed through the canopy.

The authors conclude that : “At least, our results are much more reliable than the volume information from scaling laws; we use real pre- and post-earthquake data.” This is currently not supported. First, using “data” rather than a model does not mean that there cannot be biased in the data. Then the scaling laws are also based on real (field) measurements, from around the world, and show a strong convergence (with many different type of landslides through out the world having the same trend (Guzzetti et al., 2009, Larsen et al., 2010). Of course there is some statistical noise, but the trend and order of magnitude of the parameters have been validated many times. The data of the authors (for their so called shallow landslide) do not follow the expected trend and that is worrying.

Response to some Line By Line Comments L118/ L217 : The author reply that their landslide depth is validated in Fig 4B and 4C.

This cannot be currently judged. As I said in my review, it is unsurprising that the

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biggest landslide show reasonable volume change (erosion/deposit) as its depth is big relative to tree height. Obviously as seenable in Fig 5, many slides have reasonable depth and many have very ssuspicious depth. One working example cannot help to judge the whole dataset.

L208: I previously suggested that some more validation should be performed about the uncertainties and bias of the DEM difference. The authors reply to me that study using scaling laws also have uncertainties but have been published, and somehow suggest the best analysis have already been done and that more testing would be “mission impossible”. » 1/ errors in other papers do not justify to reduce the quality of your own paper. And there are a number of evidence from the manuscript figure that cast serious doubt on the volume obtained in this study, explaining why I am currently doubting the overall discussion and conclusions. 2/ My point still hold and I make precise suggestion that are totally doable (the authors have all the data) : Take any imagery distinguishing forest and land, subset landslide in forested areas and in grass lands, analyze and plot the negative and positive volume in these 2 subsets. Compare the Area-Volume-Depth in these 2 zones. This would be a first easy step to assess the impact of canopy.

L212 : Definition of the deep-seated landslide. The authors did not respond to my point. I am sorry I have not mentioned my point is based on Fig 5B : IN that we see clearly that about 30 “Deep Seated” slides (black crosses) have depth below 1-2 m. The smallest with 10cm. This is average depth according to the authors and it is preety worrying. Similarly the “shallow” slides according to the independent inventories (from satellited and field if I understand from the reply letter) have very frequently ~10m or more depth. . . This is also suggesting there are problems either in the DEM volume estimate or in the landslide classification or in both.

Fig3C : I do not think that the authors reply makes sense: Currently we see nothing else than the big landslides (i.e. a couple of zones) and then many patches around 0. If the authors use a color scale saturating at -10 and 10, the shallow slides will be more visible AS WELL AS the deep ones (only they will have uniform blue and red

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erosion/deposit saturated at -10/10). Fig 4C can still use the -30/30 scale to show the detail of course.

Anyway to answer my comments about the evaluation of landslide depth retrieval, it would be essential that a main text figure OR a supplementary figure, show a few zones (with and without forest ) with the color map of DEM changes, and the landslide polygon superposed. 2-3 Zooms containing a dozens of polygons with different size in different setting would be perfect. Again this is extremely easy to do and I do not understand why the authors refuse to show that in the supplement.

Fig 5 : Well there are hundreds of points (grey crosses) indicating shallow landslides, with area between 10 and 10,000 m<sup>2</sup> and with depth around 10m. I just looked at the data (Of course I also do see many shallow landslides with depth around 0.5-2m, as you expect from scaling relationships).

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-3>, 2019.

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